



Technical Supplement to the Health Profession Opportunity Grants (HPOG) Impact Study Interim Report: Report Appendices

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Technical Supplement to the Health Profession Opportunity Grants (HPOG) Impact Study Interim Report: Report Appendices

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¹ This report was originally released in May 2018. Errors in the analysis of subgroup impacts were identified after publication. These errors were corrected, and the report was reissued in November 2019. See footnote 22 in this appendix for more information.

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Appendix A: Sample Details and Approach to Missing Data

This appendix provides additional information on data collection for the Interim Report and the related issues of sample size and treatment of missing data. We begin with a detailed description of the Health Profession Opportunity Grants (HPOG) grantees and programs, followed by a description of sample attrition from the time of randomization. Last, we describe the data sources used throughout the Interim Report and the analytic techniques we used to approach instances of missing data.

A.1 Sample

As described in Chapter 1, this report estimates the impact of the HPOG funding stream across a diverse set of 23 grantees which operated 42 distinct programs in 92 distinct administrative divisions. Exhibit A.1 lists the 42 programs, including state, name of grantee and program operator, local program name, and program operator type.

Of the 42 programs in Exhibit A.1, six were also part of the Pathways for Advancing Careers and Education (PACE) project:

- Pima County Community College District,
- San Diego Workforce Partnership—MAAC South,
- San Diego Workforce Partnership—Metro CTS,
- San Diego Workforce Partnership—North County Lifeline,
- Will County WIB—Instituto del Progreso Latino, and
- Workforce Development Council of Seattle-King County.

In these programs, the PACE project collected baseline and individual survey data, using slightly different instruments than the HPOG Impact Study used. We include data from these HPOG/PACE programs in as many analyses as possible. However, some outcome measures could not be comparably constructed from PACE survey data; we restricted the sample to the 36 HPOG-only programs for these analyses. Table notes throughout the report indicate the sample size, which distinguishes between the full HPOG/PACE sample and the HPOG-only sample.²

² Further, Exhibit A.6 below lists the source for all survey outcomes analyzed for the Interim Report. The HPOG and PACE follow-up surveys are listed separately.

Appendix A: Sample Details and Approach to Missing Data

Exhibit A.1: HPOG Programs and Operator Type

State	Grantee—Program Operator	Local Program Name	Program Operator Type
AZ	Pima County Community College District	Pathways to Healthcare	Institution of Higher Education
CA	San Diego Workforce Partnership—Metropolitan Area Advisory Committee South	Bridge to Employment in the Health Care Industry	Government Agency or Non-profit
CA	San Diego Workforce Partnership—Metro Comprehensive Training Services	Bridge to Employment in the Health Care Industry	Government Agency or Non-profit
CA	San Diego Workforce Partnership—North County Lifeline	Bridge to Employment in the Health Care Industry	Government Agency or Non-profit
CT	The WorkPlace	Southwestern Connecticut Health CareerRx Academy	Workforce Agency
FL	Pensacola State College	Pensacola State College Health Profession Opportunity Grant	Institution of Higher Education
IL	Will County Workforce Investment Board—Central States SER	Central States SER Healthcare Bridge Program	Government Agency or Non-profit
IL	Will County Workforce Investment Board—College of Lake	College of Lake County Health Care Bridge Program	Institution of Higher Education
IL	Will County Workforce Investment Board—Instituto del Progreso Latino	Carreras en Salud	Government Agency or Non-profit
IL	Will County Workforce Investment Board—Jewish Vocational Services	Jewish Vocational Service Pharmacy Technician Training Program	Government Agency or Non-profit
IL	Will County Workforce Investment Board—Joliet Junior College	Joliet Junior College Healthcare Bridge Program	Institution of Higher Education
KS	Kansas Department of Commerce—Heartland Works, Inc.	Kansas Health Profession Opportunity Project	Workforce Agency
KS	Kansas Department of Commerce—Southeast KANSASWORKS, Inc.	Kansas Health Profession Opportunity Project	Workforce Agency
KS	Kansas Department of Commerce—Workforce Alliance of South Central Kansas	Kansas Health Profession Opportunity Project	Workforce Agency
KS	Kansas Department of Commerce—Workforce Partnership	Kansas Health Profession Opportunity Project	Workforce Agency
KS	Kansas Department of Commerce—WorkforceOne	Kansas Health Profession Opportunity Project	Workforce Agency
KY	Gateway Community and Technical College	Gateway Health Profession Opportunity Grant	Institution of Higher Education
LA	Workforce Investment Board SDA-83 Inc.	Northeast Louisiana Professional Healthcare Opportunities—Careers And Support	Workforce Agency
MO	Full Employment Council	21st Century Healthcare Works Program	Workforce Agency
NE	Central Community College	Health Education Laddering Program	Institution of Higher Education
NH	New Hampshire Office of Minority Health	New Hampshire Health Profession Opportunity Project	Government Agency or Non-profit
NJ	Bergen Community College—Bergen Community College	Northern New Jersey Health Professions Consortium	Institution of Higher Education
NJ	Bergen Community College—Brookdale Community College	Northern New Jersey Health Professions Consortium	Institution of Higher Education

Appendix A: Sample Details and Approach to Missing Data

State	Grantee—Program Operator	Local Program Name	Program Operator Type
NJ	Bergen Community College—Community College of Morris	Northern New Jersey Health Professions Consortium	Institution of Higher Education
NJ	Bergen Community College—Essex County College	Northern New Jersey Health Professions Consortium	Institution of Higher Education
NJ	Bergen Community College—Hudson County Community College	Northern New Jersey Health Professions Consortium	Institution of Higher Education
NJ	Bergen Community College—Middlesex County College	Northern New Jersey Health Professions Consortium	Institution of Higher Education
NJ	Bergen Community College—Passaic County Community College	Northern New Jersey Health Professions Consortium	Institution of Higher Education
NJ	Bergen Community College—Sussex County Community College	Northern New Jersey Health Professions Consortium	Institution of Higher Education
NJ	Bergen Community College—Union County College	Northern New Jersey Health Professions Consortium	Institution of Higher Education
NJ	Bergen Community College—Warren County Community College	Northern New Jersey Health Professions Consortium	Institution of Higher Education
NY	Research Foundation of City University of New York- Hostos Community College	Allied Health Career Pipeline Program	Institution of Higher Education
NY	Buffalo and Erie County Workforce Development Consortium	Buffalo and Erie County Health Professions Collaborative	Workforce Agency
NY	Schenectady County Community College	Health Profession Opportunity Demonstration Project	Institution of Higher Education
NY	Suffolk County Department of Labor	Suffolk County Healthcare Occupational Opportunity for Learning	Workforce Agency
OH	Eastern Gateway Community College	Project HOPE	Institution of Higher Education
PA	Central Susquehanna Intermediate Unit	Work Attributes Toward Careers in Health Project	Government Agency or Non-profit
SC	South Carolina Department of Social Services	Project Health Occupations Preparation for Employment	Government Agency or Non-profit
TX	Alamo Community College District and University Health System	Healthcare Professions Training Initiative	Institution of Higher Education
WA	Edmonds Community College	Creating Access to Careers in Health Care	Institution of Higher Education
WA	Workforce Development Council of Seattle-King County	Health Careers for All	Workforce Agency
WI	Milwaukee Area Workforce Investment Board	CareerWorks Healthcare Training Institute	Workforce Agency

Source: Werner et al. (2014).

Appendix A: Sample Details and Approach to Missing Data

Data Sources

The Interim Report drew on multiple quantitative data sources. Exhibit A.2 lists these data sources and describes what measures were constructed from each source.

Exhibit A.2: Data Sources

	HPOG-only Programs			HPOG/PACE Programs			Program Offerings	Staff Characteristics and Opinions
	Baseline Covariates	Outcomes	Training and Service Participation	Baseline Covariates	Outcomes	Training and Service Participation		
National Directory of New Hires (NDNH)	X	X		X	X			
HPOG Follow-up Survey		X	X					
PACE Follow-up Survey					X	X		
Performance Reporting System (PRS)	X		X ^a			X ^a		
PACE Basic Information Form (BIF)				X				
Grantee Survey							X	
Management and Staff Survey								X

Notes:

^a The PRS includes data on HPOG training and HPOG service participation only. It does not include data for control group members or data on training or services received by treatment group members from non-HPOG sources.

Sample Sizes for Impact Analyses at Randomization and Follow-up

This section provides additional detail on the sample used to estimate the overall impacts of HPOG on training and service participation (Chapter 3) and on outcomes (Chapter 4), disaggregating the overall sample of 13,802 randomized individuals described in Chapter 1 of the Interim Report. This same sample was used to estimate impacts for subgroups (Chapter 5). Because this section focuses on impact analyses that pooled the enhanced and standard HPOG treatment groups into a single treatment group, we present sample sizes for the pooled treatment group and the control group in Exhibit A.3.

Across the full HPOG/PACE Sample, 13,802 individuals were randomized. As of December 2016, when the study sample for this report was constructed, 85 individuals had withdrawn consent for inclusion in the analysis, leaving 13,717 individuals in the study sample. We used this full study sample to analyze outcomes constructed from NDNH data. A total of 10,450 individuals responded to either the HPOG follow-up survey or the PACE follow-up survey. This sample is used to analyze survey outcomes that can be constructed from both surveys.

Some outcomes could only be constructed from the HPOG follow-up survey. For analyses of those outcomes, we restricted the sample to the 36 programs that were not part of the PACE study. We refer to this as the HPOG-only sample. The key difference between HPOG-only and HPOG/PACE programs is the project overseeing random assignment and data collection; this predated random assignment and can be considered exogenous.

In the HPOG-only sample, 10,693 individuals were randomized. As of December 2016, when the study sample for this report was constructed, 76 individuals had withdrawn consent. A total

Appendix A: Sample Details and Approach to Missing Data

of 8,091 responded to the HPOG follow-up survey. This sample is used to analyze survey outcomes that cannot be constructed from the PACE follow-up survey.

Exhibit A.3: Sample Sizes for Impact Analyses

	Total Sample	Treatment	Control
HPOG and PACE Sample: 23 Grantees; 42 Programs; 92 Divisions			
Randomized	13,802	8,699	5,103
Study Sample	13,717	8,673	5,044
Survey Respondents	10,450	6,801	3,649
HPOG-only Sample: 20 Grantees; 36 Programs; 86 Divisions			
Randomized	10,693	7,140	3,553
Study Sample	10,617	7,116	3,501
Survey Respondents	8,091	5,566	2,525

Source: PRS.

Sample Size for Impact of Program Enhancement Analyses

This section provides additional detail on the sample used to estimate the impact of adding specific program enhancements to the standard HPOG program (Chapter 6). Some sites randomly assigned people to two treatment arms, allowing for a comparison of the offer of a standard treatment to the offer of an enhanced treatment. In Exhibit A.4 we report the number of individuals randomized, in the study sample, and survey respondents separately for each of the types of enhancements. As above, the study sample excludes the individuals who withdrew consent before the study sample was constructed in December 2016. We used the full study sample to analyze NDNH outcomes and the sample of survey respondents to analyze survey outcomes. Note that none of the enhanced sites were PACE sites, so the distinction between the HPOG/PACE sample and HPOG-only sample is not applicable in Exhibit A.4.

Exhibit A.4: Sample Sizes for Experimental Analyses of Program Enhancements

	Total Sample	Standard Treatment	Enhanced Treatment	Control
Emergency Assistance: 3 Grantees; 11 Programs; 15 Divisions				
Randomized	2,372	910	676	786
Study Sample	2,366	910	675	781
Survey Respondents	1,773	707	529	537
Non-cash Incentives: 5 Grantees; 5 Programs; 10 Divisions				
Randomized	1,930	891	401	638
Study Sample	1,923	890	399	634
Survey Respondents	1,480	704	309	467
Peer Support: 3 Grantees; 3 Programs; 7 Divisions^a				
Randomized	1,394	543	391	460
Study Sample	1,389	541	390	458
Survey Respondents	1,132	448	330	354

Notes:

^a NY Buffalo and Erie County WDC offered peer support to certain tracks within divisions: licensed practical nurse and records and health information technicians.

Source: PRS.

A.2 Survey Response

The HPOG and PACE follow-up surveys were fielded, using similar procedures, to all randomized individuals who had not withdrawn from the study by the same survey research firm (Abt/SRBI). The survey team grouped all individuals randomized in the same month together and released the group for follow-up on the first day of the 16th month after the month of random assignment. For example, individuals randomly assigned in December 2013 were released for follow-up on April 1, 2015, which ranges from 15 to 16 months after random assignment depending on when during the month of December the individual was randomized.³ The survey team initially attempted to complete the survey by telephone and transitioned to in-person interviews if phone efforts were unsuccessful. In Exhibit A.5 we present the distribution of the timing of survey responses. The median survey response was 18 months after random assignment.

Exhibit A.5: Timing of Survey Response

Follow-up Month	Respondents		
	Number	Percent	Cumulative Percent
13	87	0.8	0.8
14	307	2.9	3.8
15	2,368	22.7	26.4
16	1,155	11.1	37.5
17	1,182	11.3	48.8
18	1,133	10.9	59.6
19	1,054	10.1	69.7
20	786	7.5	77.2
21	579	5.5	82.8
22	453	4.3	87.1
23	395	3.8	90.9
24	329	3.2	94.0
25	297	2.8	96.9
26	169	1.6	98.5
27	85	0.8	99.3
28 or more	54	0.5	99.8
Unknown	17	0.2	100.0
Summary of Follow-up Months			
Median	18		
Mean	18		

Sources: HPOG PRS; HPOG and PACE follow-up surveys. Follow-up month is calculated as the difference in days between random assignment and survey response divided by 30.

³ Individuals randomized during the last months of random assignment (October-December 2014) were “crunched” and released for survey follow-up 1 to 2 months early to reduce data collection costs.

A.3 Overview of Approach to Missing Data

Because sources are missing some data, the analysis team undertook efforts to handle both *item nonresponse*, where a particular item is missing, and *unit nonresponse*, where the data are missing entirely for a study sample member. Because unit nonresponse is only a problem in the survey, we use rereighting to address missing items due to unit nonresponse only for the survey data. We use multiple imputation for missing items due to item nonresponse in the PRS, follow-up surveys, and NDNH data, regardless of whether the missing item is intended to be a covariate or an outcome measure.

To place this discussion in a greater context, we begin with a description of the magnitude of unit and item nonresponse for survey outcomes. We then discuss the prevalence of missing data from administrative data sources.

On average, 76 percent of the study's sample responded to either the follow-up survey, be it for HPOG or PACE. Although this is relatively high, it is possible that the sample of survey respondents differs somewhat from the overall study sample. In addition, the response rate to the survey differs for the treatment and control groups (the difference in response rates is statistically significant at the one percent level). Our rereighting procedure—described further in Appendix section A.4—corrects for this nonrandom selection by estimating weights separately by treatment status. The weights place more emphasis on respondents who were less likely to have completed the survey based on observable characteristics such as age and education, increasing the likelihood that the weighted sample is representative of the original sample.

Next, Exhibit A.6 lists each of the outcomes from the survey along with a summary of response rates to those items. As described in Appendix A, section A.1, some variables are not available for study sample members in the PACE programs.⁴ For each variable we note the domain, a description of the variable, the source of the variable, and the “type” of variable (i.e., binary, continuous, or discrete). Next, we report the number of missing observations—the number of respondents who were asked the question and either responded don't know, refused to respond, or have no response to the question—followed by the number of non-missing responses. For all items, the sum of the missing and non-missing will equal the sample size for respondents of the follow-up survey (10,450 for variables available for both HPOG and HPOG/PACE or 8,091 for variables available for HPOG-only). We use the sample size as the denominator to calculate the missing rate, which appears in the last column.

For some outcomes and covariates, the number of missing observations is zero. This is because we used the question's response to assign a logical value to the missing item prior to any imputation. For these measures, the analysis team interpreted the response “don't know” or refusals to answer as indicating a negative response. For example, if individuals did not positively indicate that they completed a college degree, we infer that they did not complete a college degree (even if they responded “don't know” or refused to answer the question). That is, if a person does not know whether she has a college degree, then she probably does not have a degree. We believe it is justified, and used this strategy for the following measures:

⁴ We do not impute values for PACE participants for these variables.

Appendix A: Sample Details and Approach to Missing Data

- completion of training or ongoing enrollment in training,
- obtained a professional, state or industry certificate, license or credential since random assignment,
- earned any college credits since random assignment,
- completed a college degree (Associates, Bachelors or higher),
- completed a degree (AA, BA or higher) or obtained a credential (professional, state or industry certificate, license or credential) since random assignment,
- currently enrolled in course for credit,
- currently enrolled in training,
- perception of progress towards long-range educational goals,
- currently employed in a healthcare job or (if unemployed) worked for pay at some point after random assignment and most recent job was in healthcare, and
- currently employed in a healthcare job.

After this adjustment these items are no longer treated as missing. Because item level non-response rates were low for the items used to construct these measures, relatively few individual outcomes were affected by this choice. The nonresponse rates of all but one of these items ranges from 0.1 percent to 1.3 percent.⁵

⁵ All but three of these items had nonresponse rates of 1 percent or lower. Two of items addressed completion of occupational training and college credits earned since random assignment, and both had nonresponse rates of 1.3 percent. One item on the PACE follow-up survey, asking about the highest level of education expected to complete, had a non-response rate of 7.4 percent. However, because the questions was only asked of a subset of PACE survey respondents, this translates to 174 people with missing data.

Appendix A: Sample Details and Approach to Missing Data

Exhibit A.6: Item Nonresponse for Analysis Variables from Survey

Domain	Variable	Source	Variable Type	Number Missing	Number Non-Missing	Treatment	Control	Rate Missing
<i>Education-related Outcomes</i>								
Educational Progress	Completion of training or ongoing enrollment in training	HPOG and PACE follow-up surveys	Binary	0	10,450	6,801	3,649	0.0%
Education	Obtained a professional, state or industry certificate, license or credential since random assignment	HPOG and PACE follow-up surveys	Binary	0	10,450	6,801	3,649	0.0%
Education	Earned any college credits since random assignment	HPOG and PACE follow-up surveys	Binary	0	10,450	6,801	3,649	0.0%
Education	Completed a college degree (Associates, Bachelors or higher)	HPOG and PACE follow-up surveys	Binary	0	10,450	6,801	3,649	0.0%
Education	Completed a degree (AA, BA or higher) or obtained a credential (professional, state or industry certificate, license or credential) since random assignment	HPOG and PACE follow-up surveys	Binary	0	10,450	6,801	3,649	0.0%
Educational Progress	Currently enrolled in course for credit	HPOG and PACE follow-up surveys	Binary	0	10,450	6,801	3,649	0.0%
Educational Progress	Currently enrolled in training	HPOG and PACE follow-up surveys	Binary	0	10,450	6,801	3,649	0.0%
Educational Progress	Perception of progress towards long-range educational goals	HPOG and PACE follow-up surveys	Binary	70	10,380	6,760	3,620	0.7%
<i>Employment-related Outcomes</i>								
Employment in Healthcare	Currently employed in a healthcare job or (if unemployed) worked for pay at some point after random assignment and most recent job was in healthcare	HPOG and PACE follow-up surveys	Binary	0	10,450	6,801	3,649	0.0%
Employment in Healthcare	Currently employed in a healthcare job	HPOG follow-up survey	Binary	0	10,450	6,801	3,649	0.0%
Job Benefits	Current or most recent job offers health insurance ^a	HPOG follow-up survey	Binary	0	8,091	5,566	2,525	0.0%

Appendix A: Sample Details and Approach to Missing Data

Domain	Variable	Source	Variable Type	Number Missing	Number Non-Missing	Treatment	Control	Rate Missing
Barriers to Employment	Childcare arrangements very often interfere with school, work, job search, or family responsibilities	HPOG and PACE follow-up surveys	Binary	16	10,434	6,794	3,640	0.2%
Barriers to Employment	Transportation very often interferes with school, work, job search, or family responsibilities	HPOG and PACE follow-up surveys	Binary	11	10,439	6,796	3,643	0.1%
Barriers to Employment	Alcohol or drug use very often interferes with school, work, job search, or family responsibilities	HPOG and PACE follow-up surveys	Binary	10	10,440	6,796	3,644	0.1%
Barriers to Employment	An illness or health condition very often interferes with school, work, job search, or family responsibilities	HPOG and PACE follow-up surveys	Binary	18	10,432	6,790	3,642	0.2%
Barriers to Employment	Another situation very often interferes with school, work, job search, or family responsibilities	HPOG and PACE follow-up surveys	Binary	17	10,433	6,792	3,641	0.2%
Barriers to Employment	Number of barriers that very often interfere with school, work, job search, or family responsibilities	HPOG and PACE follow-up surveys	Discrete	6	10,444	6,799	3,645	0.1%
Self-Efficacy and Motivation	General Self-Efficacy Scale (GSE) based on Schwarzer and Jerusalem (1995) ^a	HPOG follow-up survey	Continuous	10	8,081	5,558	2,523	0.1%
Income-related Outcomes								
Public Assistance Benefits	Individual receipt of cash public assistance (TANF) in the prior month ^a	HPOG follow-up survey	Binary	31	8,060	5,545	2,515	0.4%
Public Assistance Benefits	Number of major welfare programs (TANF, SNAP, Medicaid) from which the individual received benefits in the prior month ^a	HPOG follow-up survey	Discrete	20	8,071	5,553	2,518	0.2%
Public Assistance Benefits	Number of major welfare programs (TANF, SNAP, Medicaid) from which the household received benefits in the prior month	HPOG and PACE follow-up surveys	Discrete	62	10,388	6,762	3,626	0.6%
Public Assistance Benefits	Household received ANY government assistance in the prior month	HPOG and PACE follow-up surveys	Binary	69	10,381	6,760	3,621	0.7%

Appendix A: Sample Details and Approach to Missing Data

Domain	Variable	Source	Variable Type	Number Missing	Number Non-Missing	Treatment	Control	Rate Missing
Economic Status	Personally received ANY government assistance in the prior month ^a	HPOG follow-up survey	Binary	32	8,059	5,544	2,515	0.4%
Economic Status	Personal income received from all sources	HPOG and PACE follow-up surveys	Continuous	129	10,321	6,740	3,581	1.2%
Economic Status	Household income received from all sources	HPOG and PACE follow-up surveys	Continuous	127	10,323	6,742	3,581	1.2%
Economic Status	Used loans in either own name or parents name to pay for school or living expenses	HPOG follow-up survey	Binary	2,723	7,727	4,934	2,793	26.0%
Economic Status	Used loans in parents name to pay for school or living expenses	HPOG follow-up survey	Binary	2,727	7,723	4,930	2,793	26.1%

Notes: This table focuses on item nonresponse rates and excludes unit nonrespondents (people who did not complete the survey at all).

^a These variables were not collected for PACE participants, so the sample size for the variable reflects only HPOG participants.

Appendix A: Sample Details and Approach to Missing Data

Exhibit A.7 lists outcomes and baseline covariates obtained from administrative data sources. Administrative data from NDNH, PRS, and BIF are available for survey non-respondents. For each row in the exhibit, the number of missing and non-missing sum to 13,717. This is the full study sample, including all randomized individuals who had not withdrawn consent.

In the NDNH data, we observe individual quarterly earnings from state Unemployment Insurance (UI) records and data from some employers not included in the UI program (e.g., the federal government). Each quarter, we submit sample members' social security numbers (SSN) and names to the federal Office of Child Support Enforcement (OCSE) in the Administration for Children and Families (ACF), which maintains the NDNH data, to match to the NDNH database and receive quarterly wage data for the previous eight quarters. Generally, individuals for whom we do not observe quarterly earnings in a particular quarter were not employed in that quarter. However, some of these individuals may have been employed and the observations missing due to issues matching administrative records. The analysis plan (Harvill, Moulton and Peck 2015) provides additional details on how we distinguish between unemployment and missing data. Two percent of NDNH outcomes data and 11 percent of NDNH baseline data are missing.^{6,7} As described in the analysis plan, these missing data are treated as item non-response and addressed using multiple imputation.

Unit nonresponse is not a concern for baseline measures from the PRS, BIF, and SAQ as completion of these forms was required prior to random assignment. However, as some individuals chose not to answer specific items, item non-response for baseline covariates from these sources is as high as 5 percent. To reduce the run time of this computationally intensive analysis, we reduced the number of covariates included in the impact models from our original plan. To select covariates to retain, we used an empirical specification approach. We analyzed two outcomes—educational progress and earnings in the fifth quarter after random assignment—and used R^2 as a criterion to select measures for inclusion.⁸ To validate the model identified through this process, we compared the list of selected covariates to the list of confirmatory and secondary outcomes and noted that the empirical selection approach identified the most relevant pre-intervention measures of outcomes for inclusion in the model.⁹

⁶ The rate of missing data is higher for baseline measures because some PACE control group members were added to the match file more than a quarter after their study enrollment. For the PACE control group members randomly assigned before September 30, 2013, the eight quarters of NDNH data covered the quarter of random assignment but did not include a sufficient number of pre-random assignment quarters to calculate baseline covariates.

⁷ For a few wage records, the quarterly wages were unreasonably large and appeared to be entered incorrectly. We therefore considered earnings to be missing if reported wages were \$25,000 per quarter or larger. In the fifth quarter after random assignment, wages for 15 individuals were missing for this reason. We did not treat employment data as missing in these cases because wages were reported, even if the reports were inaccurate. Therefore, there are 15 more missing observations for earnings than for employment in the fifth quarter after random assignment.

⁸ We used a modified stepwise selection procedure. In each step, we added two new covariates: one that increased the R^2 statistic for the educational progress regression the most and another that increased the R^2 for the earnings regression the most.

⁹ For example, we include baseline measures of educational attainment, completion of an occupational skills license or credential, employment, earnings, and participation in WIC or SNAP. Each of these baseline measures is directly related to a key outcome.

Appendix A: Sample Details and Approach to Missing Data

Exhibit A.7: Item Nonresponse for Analysis Variables from Administrative Data

Domain	Variable	Source	Variable Type	Number Missing	Number Non-Missing	Treatment	Control	Rate Missing (%)
<i>Employment Outcomes</i>								
Employment	Employment in Q5	NDNH	Binary	287	13,430	8,508	4,922	2.1
Employment	Employment in Q4	NDNH	Binary	287	13,430	8,508	4,922	2.1
Employment	Employment in Q3	NDNH	Binary	287	13,430	8,508	4,922	2.1
Employment	Employment in Q2	NDNH	Binary	287	13,430	8,508	4,922	2.1
Employment	Employment in Q1	NDNH	Binary	287	13,430	8,508	4,922	2.1
Employment	Cumulative employment (Q1-Q5)	NDNH	Binary	287	13,430	8,508	4,922	2.1
<i>Income Outcomes</i>								
Earnings	Earnings in Q5	NDNH	Continuous	302	13,415	8,494	4,921	2.2
Earnings	Earnings in Q4	NDNH	Continuous	293	13,424	8,505	4,919	2.1
Earnings	Earnings in Q3	NDNH	Continuous	295	13,422	8,503	4,919	2.2
Earnings	Earnings in Q2	NDNH	Continuous	291	13,426	8,504	4,922	2.1
Earnings	Earnings in Q1	NDNH	Continuous	291	13,426	8,505	4,921	2.1
Earnings	Cumulative earnings (Q1-Q5)	NDNH	Continuous	314	13,403	8,487	4,916	2.3
<i>Baseline Covariates</i>								
Dependent Children	Parent to one or more dependent children	PRS/BIF	Binary	746	12,971	8,296	4,675	5.4
Race/Ethnicity	Ethnicity (Hispanic/Latino) and race	PRS/BIF	Binary	266	13,451	8,566	4,885	1.9
Born Outside U.S.	Born outside the U.S.	PRS/BIF	Binary	482	13,235	8,427	4,808	3.5
Education	Attained postsecondary degree prior to random assignment	PRS/BIF	Binary	0	13,717	8,673	5,044	0.0
Education	Occupational Skills License or Certification prior to random assignment	PRS/BIF	Binary	0	13,717	8,673	5,044	0.0
Earnings	Average quarterly wage received during the four quarters prior to the quarter of random assignment	NDNH	Continuous	1,502	12,215	8,494	3,721	10.9
Employment	Proportion of quarters employed during the four quarters prior to the quarter of random assignment	NDNH	Continuous	1,500	12,217	8,495	3,722	10.9
Public Assistance	WIC/SNAP Indicator	PRS/BIF	Binary	396	13,321	8,484	4,837	2.9

Overall, the percentages in the final columns of Exhibits A.6 and A.7 suggest a low incidence of item nonresponse in both the survey and administrative data sources. There are two items with noticeably higher nonresponse rates. These two questions—whether respondents used loans in their parents’ name to pay for school or living expenses and used loans in either their own name or parents name to pay for school or living expenses—have unusually high nonresponse rates because of an administrative error in the survey that resulted in an illogical skip pattern for these questions. As a result, we are unable to properly differentiate between skips and nonresponse. However, this mistake was corrected while the survey was still being collected, and in the subset of data with the proper skip pattern the nonresponse rate for both questions is less than one percent.¹⁰

A.4 Nonresponse Weighting to Address Unit Nonresponse

The primary concern with unit nonresponse is sample selection resulting in the risk of bias. The nonrandom selection of nonresponses results in a sample that may not be representative of study sample members. Furthermore, if the selection process differs between the treatment and control groups, then nonresponse can generate differences between the two groups among the subset of study sample members who completed the survey, and these differences that can bias the impact estimates.¹¹ Therefore, the goal of the procedure is to reweight the sample of respondents to look like the original study sample. This is accomplished by determining the probability of response to the survey and assigning a larger weight to the responses from individuals who were least likely to respond.

Technical Description

Missing data can be classified as being missing completely at random (MCAR), missing at random (MAR), or missing not at random (MNAR) (Rubin 1987). The differences among these categories of missing data lie in the assumptions about the relationship between the probability that the data are missing and covariates, either observed or unobserved. More specifically, data are MCAR if the process that leads to missing data is completely random: the probability of data being missing is constant and does not depend on either observed or unobserved data. Data are MAR if the probability of missing data depends on observed covariates but not on unobserved variables. Finally, data are MNAR if the probability of missing data depends on both observed covariates and unobserved variables.

The classification of the missing data has implications for the potential bias in impact estimates that stems from ignoring the missing data. When data are MCAR, no bias results from dropping observations with missing data. When data are MAR, no bias arises due to dropping missing data *as long as the covariates are used appropriately*. But when data are MNAR, covariates cannot be used to completely eliminate bias in impact estimates.

¹⁰ We use our knowledge of what precipitated the incorrect skip pattern to improve the accuracy of our imputations for these outcomes. Specifically, our imputation procedure includes an indicator for the individuals who were incorrectly skipped to differentiate them from other nonrespondents.

¹¹ Because we treat all missing data from administrative sources as item nonresponse, the techniques described here apply only to survey data.

Appendix A: Sample Details and Approach to Missing Data

Our approach models the probability of responding to the survey as a function of observable characteristics, which removes the bias due to these characteristics. If the data are truly MAR (which is unknowable) then there remains no bias due to unobservable characteristics. As recommended by Puma et al. (2009), we estimate the models for treated and control observations separately. To properly implement this model we need data for both respondents and nonrespondents, so we use the baseline characteristics from the PRS listed in Exhibit A.8 as well as program indicators as the covariates in our model of response, which we estimate by logistic regression. One complication with the baseline data is item-level nonresponse in some of the variables (see Exhibit A.7). Because nearly all of the covariates in our model are either binary or discrete, we address this problem by treating missing values as their own category of response when we transform the covariates into binary variables.¹²

Exhibit A.8: Covariates from PRS Used in Nonresponse Weighting

Domain	List of Covariates
Baseline demographics	Sex, Parent to one or more dependent children, Ethnicity and race, Age, Born outside the U.S.
Baseline education background measures	Attained postsecondary degree prior to random assignment, Occupational Skills License or Certification prior to random assignment, Skills course attendance
Baseline income and benefits measures	Average quarterly wage received during the four quarters prior to the quarter of random assignment, Public assistance use (by source)
Baseline employment and expected time use measures	Proportion of quarters employed during the four quarters prior to the quarter of random assignment, Expect to be working for pay in the next few months
Baseline life challenges measures	Limited English proficiency, Number of barriers that fairly often interfere with school, work, job search or family responsibilities

The goal is to weight survey respondents' contribution to the impact analysis by the inverse of their probability of response. However, using predicted probabilities can create individuals with very large weights. To create more stable weights, we follow common practice and stratify survey respondents into five bins based on their probability of response to the survey (Cochran 1968; Baker et al. 2006). We generate the weight for each stratum by dividing the overall probability of response (within the entire sample) by the average predicted probability of response for that stratum. This implies that those who are least likely to respond will have the largest weights.

Once we have computed the appropriate weights to use for this procedure, we can generate weighted impact estimates. We estimate weighted impact estimates for each of our multiply imputed datasets we describe in Appendix A, section A.5.

Operationalization

Our statistical analysis for the HPOG Impact Study uses The SAS System to estimate the probability and calculate weights separately by treatment status using the approach described above. We use PROC LOGISTIC to estimate this model. This SAS procedure includes an option (P=) in the OUTPUT statement that stores the probability of response for further use. Next, we use PROC RANK to stratify the observations based on their probability of response to

¹² The only covariate in Exhibit A.7 that is continuous is age. To make our treatment consistent with the other covariates, we create a series of seven binary variables that indicate categories of age and include an additional indicator for age missing.

the survey. Then we divide the overall response probability by the average response probability for each stratum to determine the weights. We include these weights in our impact estimation by adding WEIGHT statements to the appropriate procedures.

A.5 Multiple Imputation to Address Item Non-response

Item nonresponse can pose challenges to analysis because the analyst has less information on the problematic item(s) than the remainder of the data. Ignoring incomplete observations, or letting these observations “drop out” of a regression analysis can bias results if the mechanism determining item nonresponse is nonrandom. Alternatively, another simple imputation strategy is to replace missing items with the observed mean among respondents; however, doing so affects the underlying characteristics of the data such as the variance of the item or the covariance between the item and other items. As a result, this strategy is not appropriate for the regression analysis we propose to use, which relies on these statistics to estimate standard errors.

Multiple imputation addresses these concerns by using a model-based approach to randomly generate a number (m) of imputed values. Thus, this procedure results in a plausible replacement value for the missing item and maintains the variation that comes from the uncertainty in the procedure. The analyst combines the results from the m datasets into one overall impact estimate and can estimate standard errors that reflect the use of multiple data sets.

Technical Description

As with nonresponse weighting, the multiple imputation procedure removes bias under the assumption that data are MAR. To implement this procedure, we first generate a predetermined number (m) of imputed data sets. Each of these datasets uses a regression model to assign a plausible value to the missing observation, and the stochastic element in each dataset accounts for the uncertainty inherent in the imputation procedure. As in the previous section, we apply multiple imputation separately to treatment and control observations (Puma et al. 2009). To generate the imputed values we employ multiple imputation with a “fully conditional specification.” This procedure is similar to multiple imputation using chained equations, also known as MICE. A succinct description of the MICE procedure, paraphrased here as a list of steps, is given in White, Royston, and Wood (2011, pg. 378):

1. Initially, all missing values are filled in by simple random sampling with replacement from the observed values.
2. The first variable with missing values, x_1 say, is regressed on all other variables x_2, \dots, x_k , restricted to individuals with the observed x_1 .
3. Randomly selected values from the posterior predictive distribution (based on the original non-missing values and the model-predicted values in (2)) replace the missing values of x_1 .
4. The next variable with missing values, x_2 say, is regressed on all other variables x_1, x_3, \dots, x_k , restricted to individuals with the observed x_2 , and using the imputed values of x_1 .

Appendix A: Sample Details and Approach to Missing Data

5. Again, missing values in x_2 are replaced by randomly selected values from the posterior predictive distribution (based on the original non-missing values and the model-predicted values in (4)).
6. The process is repeated for all other variables with missing values in turn: this is called a cycle.
7. In order to stabilize the results, the procedure is usually repeated for several cycles (e.g., 10 or 20) to produce a single imputed data set. Following Royston and White (2011), we use ten cycles.
8. The whole procedure is repeated m times to give m imputed data sets. There is a large literature on the proper choice for m ; given our large sample size and small set of covariates, we follow Rubin (1987) and Schafer (1999) and choose to set m equal to ten.¹³

An important decision in using this approach is the selection of variables to include in the regression model. White et al. (2011) recommend including every variable that will be included in any analysis model, including outcomes, predictors of the incomplete variable, and predictors for whether or not the variable is missing. However, the fully conditional specification procedure cannot handle a large number of variables in the algorithm. Therefore, to ensure our computations were tractable, we decided to include only the covariates included in the model and those listed in Exhibit A.9 in our multiple imputation procedures. In addition, we grouped outcomes by table and perform independent imputations for each table we produce. This means that for each table we will impute only the outcomes pertinent to the table along with all covariates.

Exhibit A.9: Covariates Used in Multiple Imputation

Domain	Source	List of Covariates
Baseline demographics	PRS/BIF	Parent to one or more dependent children, Ethnicity and race, Age, Born outside the U.S.
Baseline education background measures	PRS/BIF	Attained postsecondary degree prior to random assignment, Occupational Skills License or Certification prior to random assignment
Baseline income and benefits measures	NDNH	Average quarterly wage received during the four quarters prior to the quarter of random assignment
Baseline employment and expected time use measures	NDNH	Proportion of quarters employed during the five quarters prior to the quarter of random assignment
Baseline economic status	PRS/BIF	Participation in either WIC (Special Supplemental Nutrition Program for Women, Infants, and Children) or SNAP (Supplemental Nutrition Assistance Program)
Baseline life challenges measures	PRS/BIF	Number of barriers that fairly often interfere with school, work, job search or family responsibilities

¹³ This is also consistent with the suggestion in White et al. (2011) that m should be at least equal to the percentage of incomplete cases in the dataset.

Once we have constructed the ten datasets, we proceed by analyzing each of the datasets independently.¹⁴ Because each of these datasets has imputed values instead of missing observations, item nonresponse is no longer a concern, and analysis of each dataset is straightforward. In addition, because the imputed values in each dataset differ, the impact estimates for each analysis differ. We discuss combination of these estimates in Appendix A, section A.6.

Operationalization

SAS has established procedures that can impute ten datasets via the fully conditional specification method separately by treatment status and perform the impact analyses. For the first step of the multiple imputation procedure, generation of the ten datasets, we use PROC MI, where “MI” refers to multiple imputation. In addition to the standard syntax, we include an FCS statement.¹⁵ We specify that the imputation procedure begin with the variables with the lowest rate missing and end with the variables with the highest rate missing. Once we have ten datasets, we use standard SAS procedures (e.g., PROC MIXED or PROC SURVEYREG) to analyze each dataset using the unit nonresponse weights from Appendix A, section A.4 and saving the output separately.

A.6 Combining Nonresponse Weights and Multiple Imputation to Estimate Impacts

Our goal is to report a single estimate for the impact and the standard error. Therefore, we need to combine the estimates from the ten datasets we created in the multiple imputation process. The rules for this combination across multiply imputed datasets are commonly called Rubin’s rules, based on Rubin (1987).¹⁶ To properly describe these rules, we begin with some notation:

$\widehat{Q}_1 \dots \widehat{Q}_{10}$: Each of the impact estimates from the ten datasets

Using this notation, the impact of the intervention is estimated by calculating the simple average of the impact estimates from the ten datasets:

$$\bar{Q} = \frac{1}{10} \sum_{i=1}^{10} \widehat{Q}_i$$

Properly calculating standard errors requires that we account for the interaction between nonresponse weighting and the multiple imputation procedure (Shao and Sitter 1996). For analyses that use both nonresponse weights and multiple imputation, we bootstrap our standard errors rather than applying Rubin’s formulae for combining standard error estimates across imputations.¹⁷ To implement the bootstrap, we resample a predetermined number of replications from our original data set (resampled with replacement such that each replicate is different). In

¹⁴ This analysis includes unit nonresponse weights, described in detail in Appendix A, section A.3.

¹⁵ We use FCS LOGISTICS for binary variables, FCS DISCRIM for categorical variables, and FCS REG for continuous variables. For bounded continuous variables, like age, we use FCS REGPMM.

¹⁶ Rubin’s rules are typically more general for m imputations. We replace m with 10 in this formula to reflect our intent to set $m=10$.

¹⁷ Specifically, we do not use nonresponse weighting in our analysis of administrative data. Therefore, we use Rubin’s rules rather than bootstrapping to estimate these standard errors.

theory, the procedure is to replicate the analysis exactly on each dataset to create an empirical distribution of estimated impacts; the standard deviation of impacts from this procedure is the bootstrapped standard error. We deviate from standard bootstrap theory in two ways: (1) rather than resampling with uniform probability, which would require nonresponse weights to be re-estimated for each bootstrap iteration and add to the computational intensity of the procedure, we use our nonresponse weights as resampling weights and do not include nonresponse weights to estimate impacts; and (2) within each replicate dataset we impute only one value in the multiple imputation procedure, as opposed to ten (this approach is appropriate because the bootstrap formulas are asymptotic in the number of replicates). The only requirement for consistency of the variance estimate is that the imputation procedure (though not the number of imputations) within each bootstrapped sample is the same as the procedure for the overall data (Shao and Sitter 1996). These steps are described in the operationalization section.

Operationalization

For estimating impacts we use the SAS procedure PROC MIANALYZE to combine the individual impact estimates according to Rubin's rules above. In order to implement the bootstrap, we use a program that randomly samples from our original data with replacement, and use PROC MI to re-impute the missing variables with a single imputation using the same models as in Appendix A, section A.5, and uses the same regression technique as our impact model (i.e., PROC MIXED) to estimate impacts for this bootstrapped sample. The program saves the output for each bootstrapped sample and repeats the procedure 100 times. Last, we use PROC MEANS to estimate the variance of impacts from all of the bootstrapped samples. We report the square root of the resulting variance estimate as the standard error of the impacts. This process is restated as an itemized list below:

Start with the original data that have not yet been imputed. For each bootstrap iteration:

1. Sample from the data with replacement using our estimated nonresponse weights as sampling weights.
 - a. Perform a single imputation using the same procedure described in Appendix A, section A.4.
 - b. Analyze the data. (Weights are not necessary in this analysis because of the weighting in the sampling.)
 - c. Save the impact estimate.
2. Create 100 bootstrapped estimates, which the literature considers to be sufficient for standard-error estimation (Efron & Tibshirani, 1993; Cameron & Trivedi, 2009).
3. Calculate the variance of the bootstrapped estimates.

Appendix B: Analytic Details

This appendix provides a brief summary of the analytic models used to produce the estimates reported in Chapters 3 through 6 of the Interim Report. Many of the details provided here were made publicly available at the design phase, before any analysis of outcomes was performed. We refer readers to these documents for a full description of data sources, measures operationalization, the models used to produce the estimates in this report, and other analytic details:

- *Health Profession Opportunity Grants Impact Study Design Report* (Peck et al. 2014) provides a high-level overview of the design of the study and includes the data collection instruments in the appendix volume; and
- *Health Profession Opportunity Grants Impact Study Technical Supplement to the Evaluation Design Report: Impact Analysis Plan* (Harvill, Moulton and Peck 2015, 2017) provides technical details on measure construction and the analytic approach.

We refer to the first of these as the Design Report and the second as the Analysis Plan.

B.1 Estimating HPOG's Impact

In this subsection, we describe the analytic model used to estimate HPOG's impact on training and service participation (as presented in Chapter 3) and education, employment, and income (as presented in Chapter 4). We estimate intention-to-treat (ITT) impacts of *being given access* to the basic HPOG program using a multi-level regression model that adjusts the impact (i.e., the difference between average outcomes for treatment and control group members) by controlling for exogenous characteristics measured at baseline.

We estimate a three-level model, where the unit of analysis for level one is the individual sample member (indexed by i); the unit of analysis for level two is the division (indexed by j); and the unit of analysis for level three is the program (indexed by k). The analysis combines all individuals randomly assigned to the standard or enhanced treatment group into a "pooled" treatment group. This is justified by the fact that HPOG programs are diverse, and the program enhancements tested in the programs with two treatment groups were represented across other programs that had just one treatment group. We label this pooled treatment group TE because it combines standard treatment, T , and enhanced treatment, E . The following model was used to estimate the impact of HPOG using the pooled treatment group and the control group from across all 42 programs:

$$Y_{kji} = \alpha_0 + \beta_0 TE_{kji} + \sum_c \delta_c IC_{ckji} + \gamma R_k + \{\varepsilon_{kji} + v_k + v_{kj} + u_k TE_{kji} + u_{kj} TE_{kji}\} \quad (\text{eq. B-1})$$

The treatment indicator, TE_{kji} , is defined at the individual level to take on a value of 1 if the individual was assigned to receive HPOG services and 0 if the individual was assigned to the control group. The primary coefficient of interest, β_0 , captures the average impact of being offered access to HPOG relative to the counterfactual condition of no access to HPOG.

The model controls for the following individual characteristics, IC_{ckji} :

- Average quarterly earnings in the year prior to intake

- Number of quarters employed in the year prior to intake
- Attainment of a postsecondary degree or certificate prior to intake
- Attainment of occupational skills license or credential prior to intake
- Race/ethnicity
- Foreign birth
- Parent of one or more dependent children
- Participation in either WIC (Special Supplemental Nutrition Program for Women, Infants, and Children) or SNAP (Supplemental Nutrition Assistance Program)

The construction of these measures is described by Harvill, Moulton and Peck (2015).

The indicator R_k identifies programs participating in the PACE study. The coefficient γ accounts for the two ways the PACE programs differ from the other programs in the sample: slight differences in data collection instruments and different probabilities of assignment to treatment. For programs in the PACE study, half of the sample was assigned to treatment and half to control; in the HPOG-only programs, two-thirds was assigned to treatment (either standard or enhanced) and one-third to control. The Analysis Plan details slight differences in measure construction (Harvill et al. 2015).

The error term includes elements that capture variation in impacts across divisions and programs (u_k, u_{kj}) and variation in the level of outcomes across divisions and programs (v_k, v_{kj}). We use maximum likelihood procedures (which assume joint normal distributions for the random components) to estimate the above model.

We use a model with similar structural components for all impact analyses. In subsequent sections, we explain how we adapt this model to address a range of research questions. Exhibit B.1 provides a table that summarizes, in one place, the notation used in these models.

Exhibit B.1: Definitions of Model Terms

Name	Definition
Outcome and Covariates	
Y_{kji}	The outcome measure for individual i from division j and program k
TE_{kji}	The HPOG program treatment group indicator (1 for those individuals assigned to the standard HPOG treatment or enhanced HPOG treatment groups; 0 for the control group individuals; this is labelled “TE” for the combination of standard “treatment” and “enhanced” treatment groups)
E_{kji}	The HPOG enhanced treatment group indicator (1 for those individuals assigned to the enhanced HPOG treatment groups; 0 for those individuals assigned to the standard HPOG treatment or control groups; this is labelled “E” for “enhanced” treatment group)
IC_{ckji}	Individual baseline characteristic c for individual from division j and program k (grand mean centered), $c = 1, \dots, C$ (this is labelled “IC” for “individual characteristics”)
R_k	Indicator for programs participating in the PACE study, which had a different probability of random assignment to treatment (this is labelled “R” for “random assignment”)
S_{skji}	Subgroup s for individual i from division j and program k , $s = 1, \dots, S$ (this is labelled “S” for “subgroup”)
P_{mk}	Program component m for program k , $m = 1, \dots, M$
LC_{qk}	Local context measure q for program k , $q = 1, \dots, Q$
I_{gkj}	Implementation feature g for division j and program k , $g = 1, \dots, G$
PC_{dkj}	Participant composition measure d for division j and program k , $d = 1, \dots, D$
Model Coefficients	
α_0 (alpha)	The grand mean control group outcome in non-PACE programs
α_s (alpha sub s)	The mean control group outcome for subgroup s in non-PACE programs
β_0 (beta)	The grand mean impact of HPOG
β_0^S (beta)	The grand mean impact of standard HPOG treatment
β_s (beta sub s)	The mean impact of HPOG for subgroup s
π_e (pi)	The grand mean impact of enhanced HPOG treatment
δ_c (delta)	The effect of individual characteristic c on the mean outcome, $c = 1, \dots, C$
γ (gamma)	The difference in grand mean control group outcomes in PACE and non-PACE programs
π_m (pi)	The effect of program component m on the mean outcome, $m = 1, \dots, M$
ζ_q (zeta)	The mean impact of HPOG for programs with local context measure q , $q = 1, \dots, Q$
φ_g (phi)	The mean impact of HPOG for programs with implementation feature g , $g = 1, \dots, G$
τ_d (tau)	The mean impact of HPOG for programs with participant composition measure d , $d = 1, \dots, D$

Name	Definition
Error Terms	
ε_{kji} (epsilon)	A random component of the outcome for each individual
v_{ki}	A random component of control group mean outcome for each division
v_k	A random component of control group mean outcome for each program
u_{kj}	A random component of the impact for each division
u_k	A random component of the impact for each program
ω_{kj}	A random component of the enhancement impact for each division
ω_k	A random component of the enhancement impact for each program

B.2 Estimating Subgroup Impacts

To estimate the subgroup impacts presented in Chapter 5, we expand the impact model described in Appendix B, section B.1 to include an interaction term between the treatment indicator and the subgroup categories of interest, as follows:

$$Y_{kji} = \sum_s \alpha_s S_{skji} + \sum_s \beta_s S_{skji} TE_{kji} + \sum_c \delta_c IC_{ckji} + \gamma R_k + \{\varepsilon_{kji} + v_k + v_{kj} + u_k TE_{kji} + u_{kj} TE_{kji}\} \quad (\text{eq. B-2})$$

In this equation, subgroups are identified by S_{skji} and there is no omitted reference category. The regression directly calculates a separate control group mean (α_s) and impact (β_s) for each subgroup. In addition to testing for the significance of impacts, we statistically test whether the differences in impacts are different from zero across subgroups using a Wald test under the assumption that the differences in impacts are approximately normally distributed.

B.3 Estimating the Impact of HPOG Program Enhancements

In Chapter 6, we present estimated impacts of three program enhancements: emergency assistance, non-cash incentives, and facilitated peer support groups. The model is a slight variation of the standard impact model, adding a term to identify members of the enhanced treatment group (E_{kji}):

$$Y_{kji} = \alpha_0 + \beta_0^S TE_{kji} + \pi_e E_{kji} + \sum_c \delta_c IC_{ckji} + \{\varepsilon_{kji} + v_k + v_{kj} + u_k TE_{kji} + u_{kj} TE_{kji} + \omega_k E_{kji} + \omega_{kj} E_{kji}\}, \quad (\text{eq. B-3})$$

The primary coefficient of interest, π_e , provides an estimate of the impact of being offered a specific enhancement component in addition to the standard HPOG program. Individuals who received the enhanced program are identified by E_{kji} . The impact of the standard program is denoted by β_0^S , where the superscript s , for standard, differentiates it from the impacts in other estimates and emphasizes that the impact does not reflect the enhancement components. The sample for each analysis is restricted to focus on programs that offered the specific enhancement component being investigated. The Interim Report's Exhibit 1.2 lists which programs offer which enhancements. Because none of the PACE programs offered enhancement components, the PACE indicator is not included in this model. Conducting the analysis separately for emergency assistance enhancement programs, non-cash incentives enhancement programs, and facilitated peer support enhancement programs provides the experimental estimates of the contribution of those program components to the overall impact magnitude.

How to Read the Impact Tables in This Report Appendix

All the impact tables in this report appendix contain similar content, which is more detailed than the impacts tables in the main body of the Interim Report. For example, the sample table below presents HPOG's impact on study participants' enrollment in training.

The first five columns are structured in the same way as the impact tables in the main body of the interim report.

- The table reports the level of the outcome for both the treatment and control groups. The numbers in the table below show that 71 percent of the treatment group and 62 percent of the control group enrolled in training during the follow-up period.
- The difference between the two mean outcomes is the impact of being in the treatment group.
- Impacts marked with one or more asterisks are statistically significant, indicating that it is unlikely that the impact is due to chance. The number of asterisks indicates whether the impact is statistically significant at the 10 percent (*), 5 percent (**), or 1 percent (***) level. In the table, the impact is statistically significant at the 1 percent level, providing strong evidence that it was HPOG that increased the percentage of students enrolled in any training since random assignment.
- The relative impact in the next column is computed as the impact divided by the control group mean, and it places in context the magnitude of the treatment-control difference. In the example below, the treatment group mean of 71 percent is 15 percent larger than the control group mean of 62 percent.

The next five columns provide technical details which are not available in the body of the report.

- The standard error quantifies the precision of the impact estimate. The standard error reflects the size of the sample, the multi-level structure of the model, and the variability of the outcome after controlling for baseline covariates. A smaller standard error indicates a more precise estimate.
- The 90 percent confidence interval summarizes the precision of the impact estimate in a different way. Values within the interval are possible alternative values of the impact. Values outside the interval are statistically different from the impact. This column is particularly useful for findings that are not statistically significant, because it places bounds on possible impacts.
- The minimum detectable effect (MDE) is the smallest true impact we expect to be able to detect given the variability we observe in the impact estimate. To calculate the MDE, we set the level of significance to 10 percent and power to 80 percent. This column is particularly useful for findings that are not statistically significant, as it indicates how large the effect would have needed to be to be detected 80 percent of the time.
- Finally, the two right most columns report the sample sizes for the treatment and control groups.

	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Enrollment in Training or Classes (%)									
Enrolled in Any Training or Pre-Training Activities since Randomization	71.4	62.1	9.3***	15.0	0.9	(7.9, 10.7)	2.2	6,801	3,649

Appendix B provides technical details related to the analytic model used to estimate the impacts reported in these exhibits.

Appendix C: Expanded Results for Chapter 3

This appendix presents more detailed versions of the tables in Chapter 3, which describe impacts on training and service participation.

C.1 Expanded Impacts of HPOG on Enrollment in Training**Exhibit C.1: Expanded Impacts on Enrollment in Training and Pre-Training Activities**

Enrollment in Training or Classes (%)	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Enrolled in Any Training or Pre-Training Activities since Randomization	71.4	62.1	9.3***	14.9	1.0	(7.6, 11.0)	2.5	6,801	3,649
Any enrollment in credit classes	35.6	36.9	-1.3	-3.5	3.4	(-6.9, 4.3)	8.5	6,801	3,649
Any enrollment in occupational classes	38.6	27.3	11.3***	41.5	3.3	(5.9, 16.8)	8.3	6,801	3,649
Any enrollment in ESL classes	3.5	2.5	1.0	40.9	2.0	(-2.3, 4.4)	5.1	6,801	3,649
Any enrollment in basic skills classes	12.1	9.9	2.2	21.9	3.2	(-3.0, 7.4)	7.9	6,801	3,649

Notes: All findings are exploratory. They are intended to support interpretation of impact findings.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources: HPOG and PACE follow-up surveys.

C.2 Expanded Impacts on Service Receipt

Exhibit C.2: Expanded Impacts on Receipt of Support Services

Service Measure (%)	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Academic Support Services									
Received Any Academic Support Services^a	56.6	47.4	9.2***	19.4	1.2	(7.2, 11.2)	3.0	5,566	2,525
Received Financial Aid Advising Services ^b	27.1	24.4	2.7**	10.9	1.2	(0.8, 4.5)	2.9	6,801	3,649
Received Academic Advising Services ^b	30.9	25.1	5.7***	22.8	1.2	(3.8, 7.7)	3.0	6,801	3,649
Received Assessment Services ^a	27.1	20.2	6.9***	34.1	1.2	(5.0, 8.8)	2.9	5,566	2,525
Received Tutoring Services ^b	17.2	14.6	2.6***	18.0	0.7	(1.4, 3.8)	1.8	6,801	3,649
Received Peer Support Services ^a	9.5	5.7	3.8***	66.1	0.6	(2.8, 4.7)	1.5	5,566	2,525
Career Support Services^b									
Received Any Career Support Services	39.3	26.1	13.1***	50.3	1.1	(11.3, 15.0)	2.7	6,801	3,649
Received Career Counseling Services	25.2	15.4	9.8***	63.6	0.8	(8.4, 11.2)	2.1	6,801	3,649
Received Job Search Services	30.8	19.2	11.7***	61.0	1.0	(10.1, 13.3)	2.4	6,801	3,649
Other Support Services									
Received Any Other Support Services^a	39.4	27.0	12.4***	46.1	1.0	(10.7, 14.1)	2.6	5,566	2,525
Received Help Arranging Supports ^b	19.7	11.2	8.4***	75.3	0.8	(7.1, 9.8)	2.0	6,801	3,649
Received Counseling Services ^a	12.2	9.8	2.4***	24.9	0.7	(1.2, 3.7)	1.8	5,566	2,525
Received Noncash Incentives ^a	10.1	2.2	7.9***	358.5	0.6	(6.9, 8.9)	1.5	5,566	2,525
Received Emergency Assistance Services ^a	14.4	10.8	3.6***	33.2	0.8	(2.3, 4.9)	2.0	5,566	2,525

Notes: All findings are exploratory. They are intended to support interpretation of impact findings.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources:

^a HPOG follow-up survey.

^b HPOG and PACE follow-up surveys.

Appendix D: Expanded Results for Chapter 4

This appendix presents more detailed versions of the tables in Chapter 4, as follows:

- Section D.1 presents expanded results for impacts on education.
- Section D.2 presents expanded results for impacts on employment.
- Section D.3 presents expanded results for impacts on income.
- Section D.4 presents impacts on weekly hours, weekly earnings, and wages.
- Section D.5 presents an analysis of program moderators.

D.1 Expanded Results for Impacts on Education

Exhibit D.1: Expanded Results for Impacts on Educational Progress

Outcome (%)	Level of Evidence	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Completed Training or Currently Enrolled in Training	Confirmatory	67.6	60.3	7.4###	12.2	1.0	(5.6, 9.1)	2.2	6,801	3,649
Perception of progress toward long-range educational goals	Exploratory	0.8	0.8	0.1***	9.4	0.0	(0.1, 0.1)	0.0	6,801	3,649
Obtained certificate, license, or credential	Exploratory	49.5	39.5	10.0***	25.3	1.1	(8.2, 11.8)	2.7	6,801	3,649
Earned any college credits	Exploratory	28.5	28.6	-0.1	-0.3	1.2	(-2.1, 1.9)	3.0	6,801	3,649
Completed college degree	Exploratory	17.9	17.9	-0.1	-0.4	0.7	(-1.2, 1.1)	1.7	6,801	3,649
Completed college degree or obtained certificate, license, or credential	Exploratory	58.2	49.1	9.0***	18.4	1.0	(7.5, 10.6)	2.4	6,801	3,649
Currently enrolled in course for credit	Exploratory	15.9	16.7	-0.8	-4.8	0.8	(-2.1, 0.4)	1.9	6,801	3,649
Currently enrolled in occupational training	Exploratory	4.4	4.4	0.0	0.6	0.4	(-0.6, 0.7)	1.0	6,801	3,649

Notes: Confirmatory and secondary findings use a one-sided hypothesis test, and exploratory findings use a two-sided hypothesis test.

Statistical significance levels for one-sided tests are indicated with hashtags, as follows: ### = 1 percent; ## = 5 percent; # = 10 percent.

Statistical significance levels for two-sided tests are indicated with asterisks, as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources: HPOG and PACE follow-up surveys.

Note Regarding Measurement of the Outcome Completed College Degree or Obtained Certificate, License, or Credential

Partway through the survey fielding period, we revised the survey items asking about completion of academic degrees or occupational training. Prior to the revision these items asked about any completion of academic degrees or occupational training. The revision to the items clarified that respondents should report completion of academic degrees or occupational training *since random assignment*. We analyzed the impact of HPOG separately using data from before and after this edit and detected no difference in impacts. Despite the edit, it remains possible that survey respondents who received the edited text interpreted the question as being about educational attainment and training completion regardless of its timing. Such a misinterpretation would not affect the estimated impact of HPOG on educational progress, as long as the assignment groups interpreted the question in the same manner. The experimental design ensures that baseline level of education is unrelated to treatment status; therefore, any differences in attainment between the treatment and control groups at follow-up were caused by the HPOG program. There is no reason to expect assignment groups would interpret this item differently, either before or after the edit.

If survey items intended to only capture training after random assignment instead captured degree or training completion at any point in time, then the proportions with educational progress in the treatment and control groups would be biased upward. To get a sense of how far off the proportions might be if they were affected at all, we calculated an alternative measure of educational progress, assuming that individuals who reported completion of an occupational skills license both at baseline and at survey follow-up had completed all training prior to random assignment. Analyses of this alternative measure showed that the proportions with educational progress in the treatment and control groups could have been overstated by as much as 12 to 13 percentage points, so that the proportion of the treatment group making educational progress was 55 percent (rather than 68 percent) and the proportion of the control group making educational progress was 47 percent (rather than 60 percent). However, the analysis confirmed that differences between the treatment and control group outcomes were not misstated; as a result, readers can be confident that the impacts presented in the report represent accurately treatment-control differences in educational progress.

D.2 Expanded Results for Impacts on Employment**Exhibit D.2: Expanded Results for Impacts on Employment-Related Outcomes**

Outcome	Level of Evidence	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Employment										
Employment in Q5 (%) ^a	Secondary	69.7	69.4	0.2	0.3	0.9	(-1.3, 1.8)	2.0	8,673	5,044
Employment in Q4 (%) ^a	Exploratory	68.9	67.5	1.4	2.1	0.9	(-0.0, 2.9)	2.3	8,673	5,044
Employment in Q3 (%) ^a	Exploratory	65.9	66.5	-0.6	-0.9	0.9	(-2.0, 0.9)	2.2	8,673	5,044
Employment in Q2 (%) ^a	Exploratory	62.2	63.7	-1.5*	-2.4	0.9	(-3.0, -0.1)	2.2	8,673	5,044
Employment in Q1 (%) ^a	Exploratory	55.7	60.3	-4.6***	-7.7	0.9	(-6.2, -3.1)	2.3	8,673	5,044
Cumulative employment (Q1-Q5) (ranges from 0-5) ^a	Exploratory	3.2	3.3	-0.1	-1.5	0.0	(-0.1, 0.0)	0.1	8,673	5,044
Survey measure of employment (%) ^b	Exploratory	70.1	68.4	1.7	2.5	1.1	(-0.1, 3.5)	2.8	6,801	3,649
Employment in Healthcare^b										
Current or Most Recent Job in Healthcare (%)	Secondary	52.6	41.4	11.2###	27.1	1.0	(9.6, 12.8)	2.1	6,801	3,649
Currently employed in a healthcare job (%)	Exploratory	43.7	33.8	9.9***	29.5	1.0	(8.2, 11.7)	2.6	6,801	3,649
Job Quality^c										
Current or Most Recent Job Offers Health Insurance (%)	Secondary	57.9	55.7	2.2##	3.9	1.1	(0.4, 4.0)	2.3	5,566	2,525
Barriers to Employment^b										
Childcare arrangements (%)	Exploratory	0.15	0.17	-0.02**	-10.1	0.01	(-0.03, -0.00)	0.02	6,801	3,649
Transportation (%)	Exploratory	11.6	11.4	0.1	1.2	0.6	(-0.8, 1.1)	1.5	6,801	3,649
Alcohol or drug use (%)	Exploratory	0.3	0.4	-0.1	-31.8	0.1	(-0.3, 0.1)	0.3	6,801	3,649
An illness or health condition (%)	Exploratory	6.5	6.5	0.0	0.0	0.5	(-0.8, 0.8)	1.2	6,801	3,649
Another situation (%)	Exploratory	5.6	6.1	-0.5	-7.8	0.5	(-1.3, 0.4)	1.3	6,801	3,649
Number of barriers (range is 0-5)	Exploratory	0.39	0.41	-0.02*	-5.4	0.01	(-0.04, -0.00)	0.03	6,801	3,649
Self-Efficacy^c										
Generalized Self-Efficacy Scale (range is 1-4)	Exploratory	3.17	3.15	0.03**	0.8	0.01	(0.01, 0.05)	0.03	5,566	2,525

Notes: Confirmatory and secondary findings use a one-sided hypothesis test, and exploratory findings use a two-sided hypothesis test. Statistical significance levels for one-sided tests are indicated with hashtags, as follows: ### = 1 percent; ## = 5 percent; # = 10 percent. Statistical significance levels for two-sided tests are indicated with asterisks, as follows: *** = 1 percent; ** = 5 percent; * = 10 percent. Employment outcomes list the follow-up quarter: Q5 refers to the fifth follow-up quarter after random assignment.

Sources:

^a National Directory of New Hires.

^b HPOG and PACE follow-up surveys.

^c HPOG follow-up survey.

Under-Reporting of Quarterly Wage Data from Washington to the NDNH

Between 2014 and 2016 the state of Washington underreported employment and earnings data to the NDNH.¹⁸ This affected this study's sample members in the Washington grantees (WDC Seattle – King County and Edmonds Community College) whose fifth quarter after random assignment was one of these under-reported quarters and accounts for approximately 51 percent of the sample for Washington State and 3 percent of the total impact study sample.

When constructing NDNH measures of employment and earnings, we interpreted an absence of quarterly wages reported to the NDNH to mean that the individual was not employed. The underreporting in Washington state implies that we might have mistakenly identified some people as unemployed, with no earnings, who were instead employed, with earnings. This underreporting leads to underestimating the *employment rate* and the average level of *earnings* in affected quarters. We expect that the distribution between treatment and control groups is the same. Therefore, although the magnitude of the study's impacts might be affected, this underreporting should not affect the study's impacts relative to the control group mean.

To determine whether this underreporting affected impact findings, we estimated findings treating fifth quarter earnings and employment as missing for all participants at Washington grantees whose fifth quarter after random assignment was one of the underreported quarters. In this sensitivity analysis, we applied our standard approach to missing data (described in Appendix A) which involved imputing these missing outcomes. The results appear in Exhibit D.3 alongside the main analytic results. According to the sensitivity analysis, 71 percent of the control group was employed in the fifth quarter after random assignment, and control group members on average earned \$3,433 in that quarter. The main analysis found that 69 percent of the control group was employed, and the group earned \$3,345 in that quarter. As expected, the sensitivity analysis shows slightly higher rates of employment and earnings in the control group in the fifth quarter after random assignment.

Treating fifth quarter earnings and employment outcomes as missing for all affected participants did not meaningfully affect estimated impacts. Neither the standard analysis nor the sensitivity analysis found evidence that HPOG increased employment in the fifth quarter after random assignment, and the point estimates from the two analyses differ by a tenth of a percent. Compared to the standard analysis' estimated impact of \$137 in the fifth follow-up quarter, the sensitivity analysis found that HPOG increased earnings in the fifth quarter after random assignment by \$131 dollars. Both estimates are statistically significant at the same level.

¹⁸ NDNH employment and earnings in Washington appear to be under-reported in 2014Q4, 2015Q2, and 2015Q4 through 2016Q4. The underreporting was significant: in several quarters, we observe about 90 percent fewer wage records than expected. After analysis for this report was complete, the state of Washington recovered employment and earnings data from 2015Q4 through 2016Q4 and provided these data to the NDNH.

Exhibit D.3: Sensitivity Analyses for Employment and Earnings Impacts

Outcome	Level of Evidence	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Employment in Q5 (%), main analysis	Secondary	69.7	69.4	0.2	0.3	0.9	(-1.3, 1.8)	2.0	8,673	5,044
Employment in Q5 (%), sensitivity analysis	Secondary	71.3	71.0	0.3	0.5	1.0	(-1.2, 1.9)	2.0	8,673	5,044
Earnings in Q5 (\$), main analysis	Secondary	3,482	3,345	137##	4.1	80	(6, 268)	169	8,673	5,044
Earnings in Q5 (\$), sensitivity analysis	Secondary	3,564	3,433	131##	3.8	78	(2, 261)	167	8,673	5,044

Notes: NDNH employment and earnings data were under-reported by the state of Washington in several quarters between 2014 and 2016. This affected sample members associated with the Washington grantees (WDC Seattle – King County and Edmonds Community College) whose fifth quarter after random assignment was one of these under-reported quarters. This sensitivity analysis treated fifth quarter earnings and employment as missing for all affected participants.

Statistical significance levels for one-sided tests are indicated with hashtags, as follows: ### = 1 percent; ## = 5 percent; # = 10 percent.

Employment and earnings outcomes list the follow-up quarter: Q5 refers to the fifth follow-up quarter after random assignment.

Sources:

National Directory of New Hires

D.3 Expanded Results for Impacts on Income

Exhibit D.4: Expanded Results for Impacts on Income-Related Outcomes

Outcome	Level of Evidence	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Quarterly Earnings^a										
Earnings in Q5 (\$)	Secondary	3,482	3,345	137 ^{##}	4.1	80	(6, 268)	169	8,673	5,044
Earnings in Q4 (\$)	Exploratory	3,173	3,070	103	3.4	75	(-19, 226)	186	8,673	5,044
Earnings in Q3 (\$)	Exploratory	2,779	2,772	7	0.3	63	(-95, 110)	156	8,673	5,044
Earnings in Q2 (\$)	Exploratory	2,317	2,477	-160 ^{***}	-6.5	53	(-247, -73)	132	8,673	5,044
Earnings in Q1 (\$)	Exploratory	1,806	2,065	-259 ^{***}	-12.6	44	(-331, -188)	108	8,673	5,044
Cumulative earnings (Q1-Q5) (\$)	Exploratory	13,544	13,713	-170	-1.2	252	(-584, 244)	628	8,673	5,044
Public Assistance Benefits										
TANF Receipt (%) ^c	Secondary	8.5	8.8	-0.4	-4.1	0.6	(-1.4, 0.7)	1.3	5,566	2,525
Number of major welfare programs (range is 0-3) ^c	Exploratory	1.0	1.0	-0.0	-1.6	0.0	(-0.1, 0.0)	0.1	5,566	2,525
Number of major welfare programs (household) (range is 0-3) ^b	Exploratory	1.1	1.1	-0.0	-0.6	0.0	(-0.0, 0.0)	0.0	6,801	3,649
Any government assistance (%) ^c	Exploratory	66.3	66.3	0.0	0.0	1.1	(-1.7, 1.7)	2.6	5,566	2,525
Any government assistance (household) (%) ^b	Exploratory	73.1	72.4	0.7	1.0	0.8	(-0.6, 2.0)	2.0	6,801	3,649
Economic Conditions^b										
Personal monthly income (\$)	Exploratory	1,445	1,473	-29	-2.0	20	(-62, 4)	50	6,801	3,649
Household monthly income (\$)	Exploratory	2,440	2,439	1	0.0	31	(-49, 52)	77	6,801	3,649
Loans in either own or parents' name for school/living expenses (%)	Exploratory	21.9	28.1	-6.2	-22.0	12.9	(-27.3, 14.9)	32.0	6,801	3,649
Loans in parents' name for school/living expenses (%)	Exploratory	1.5	4.8	-3.4	-69.6	8.3	(-17.0, 10.3)	20.6	6,801	3,649

Notes: Confirmatory and secondary findings use a one-sided hypothesis test, and exploratory findings use a two-sided hypothesis test.

Statistical significance levels for one-sided tests are indicated with hashtags, as follows: ### = 1 percent; ## = 5 percent; # = 10 percent.

Statistical significance levels for two-sided tests are indicated with asterisks, as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Earnings outcomes list the follow-up quarter: Q5 refers to the fifth follow-up quarter after random assignment.

Sources:

^a National Directory of New Hires.

^b HPOG and PACE follow-up surveys.

^c HPOG follow-up survey.

D.4 Impacts on Weekly Earnings, Weekly Hours, and Wages

In this section, we consider the earnings impact further, decomposing it into its component parts. The \$137 impact on earnings in the fifth quarter after random assignment could reflect a change in employment, a change in hours worked conditional on employment, a change in hourly wage, or a combination of the three. We did not find evidence that HPOG increased employment in general, though we did observe an impact on employment in the healthcare sector. This observation suggests that an earnings impact could have arisen through changes in hours and wages particularly related to the shifting composition of employment toward the healthcare sector.

Measuring impacts on hour and wages poses a methodological challenge because we observe (non-zero) earnings, wages and hours only for people who are employed. In analyses of earnings and hours, we considered individuals who were not employed to have zero earnings and zero hours worked. However, this approach cannot be extended to wages: if we treat people who are not employed as having zero earning and zero hours and we try to calculate hourly wages from these data, we divide zero earnings by zero hours and do not obtain a meaningful wage because the quotient is undefined. Instead, we produce earnings and hours values at the aggregate level for each experimental group and use those to create what is labelled a “pseudo-wage” (Martinson et al, 2017).

To construct the pseudo-wage, we relied on survey data on weekly hours and weekly earnings.¹⁹ We used our standard analysis approach to estimate impacts separately on weekly hours and on earnings.²⁰ Then, we computed the:

- Control group mean wage as the average control group weekly earnings divided by the average control group weekly hours;
- Treatment group mean wage as the average treatment group weekly earnings divided by the average treatment group weekly hours; and
- The impact on hourly wages as the difference between the treatment group mean wage and the control group mean wage, at the group-aggregate level.

We bootstrapped the standard errors as described in Appendix A. The results of this decomposition analysis are presented in Exhibit D.5.

There is no evidence of an impact on weekly earnings. Based on the 90 percent confidence interval, we infer that likely values of the impact on weekly earnings range from a decrease of about \$3 to an increase of about \$17. These findings are not inconsistent with the NDNH findings: an impact of \$137 per quarter is equivalent to an impact of \$10.54 per week, which is well within the 90 percent confidence interval.

¹⁹ Measures of hours worked per week were only available from survey data. We constructed weekly earnings from survey data by multiplying reported hours worked and reported hourly wage.

²⁰ The models do not include NDNH baseline earnings and employment covariates because the particular survey items measuring weekly earnings and hours were not linked to the NDNH administrative data.

There is some evidence that HPOG increases hours: the treatment group worked, on average, 23.8 hours per week, and the control group worked 23.3 hours. This represents a small, half hour per week increase.

Both treatment and control groups had hourly wages of about \$13 per hour. There is no evidence of an impact on hourly wage. Further, we can rule out the possibility of large impacts on hourly wage: the 90 percent confidence interval indicates that, if HPOG increases hourly wage, then that increase is less than 15 cents per hour, and, if HPOG decreases hourly wage, then the decrease is less than 13 cents per hour.

Exhibit D.5: Impacts on Weekly Earnings, Weekly Hours and Hourly Wages

Outcome ^a	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Weekly Earnings	307.02	299.99	7.03	2.3	6.04	(-2.87, 16.92)	15.01	6,801	3,649
Weekly Hours	23.81	23.28	0.52*	2.2	0.31	(0.01, 1.04)	0.78	6,801	3,649
Hourly Wage (Pseudo Wage)	12.90	12.88	0.01	0.1	0.08	(-0.13, 0.15)	0.21	6,801	3,649

Notes: Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Source:

^a HPOG and PACE follow-up surveys.

D.5 Program Moderator Analyses

Some types of programs may be more effective than other types. We investigate whether program impacts differ for programs based on:

- the contrast in services available to treatment group members through HPOG and those available in the community;
- the type of institution that operates the program; and
- the typical duration of completed trainings.

To estimate these impacts, we use the same statistical model as we did for subgroup analyses based on the sample's baseline characteristics (and reported in Chapter 5). In this case, however, the subgroup is defined at the program level. We also estimate differences in impacts between the types of programs. These comparisons are non-experimental and should be interpreted with caution. It is possible an observed difference in impacts was caused by the type of programs being compared. However, it is also possible that some other factor was driving the difference in impacts.

Impacts Excluding Programs Operating in a Service-Rich Environment

Section 2.3 in the report describes control group conditions and the experimental contrast. Exhibit 2.15 depicts the number of programs that offer services not available to control group members in four areas: training courses, training access, financial supports, and supportive services. Programs with strong contrasts in none or in only one of these areas had low contrast. We separately estimate the average impact for the eight programs that have low contrast and for the remaining programs with typical and high contrast.

As shown in Exhibit D.6, treatment group members in programs with low contrast experienced positive impacts from HPOG in educational progress and employment in healthcare of 4 and 8 percentage points, respectively. Treatment group members in programs with typical and high contrast also had positive impacts in educational progress and employment in healthcare, though the magnitudes of the impacts were larger (8 and 12 percentage points, respectively). Impacts on educational progress and employment in healthcare were statistically different between the types of programs, as shown in Exhibit D.7.²¹

The exhibit shows that those programs with typical or high contrast also had more favorable impacts: 4 percentage points larger impacts on educational progress, 5 percentage points larger on employment in healthcare. Although impacts on TANF were not statistically different from zero for either low contrast or typical and high contrast programs, the difference between the impacts for the two groups was statistically significant, again favoring those programs with typical and high contrast: their programs decreased TANF receipt an additional 2 percentage points relative to the set of low contrast programs.

²¹ The impact on educational progress for typical and high contrast programs of 8 percentage points is not statistically different from the overall impact of HPOG on educational progress for all sites, which was 7 percentage points.

Exhibit D.6: Impacts by Service Contrast

Outcome	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Low Contrast									
Educational Progress (%)^a	68.3	63.9	4.4**	6.9	1.9	(1.3, 7.5)	4.8	1,889	847
Program completion (%) ^a	48.5	40.2	8.3***	20.6	2.2	(4.6, 11.9)	5.5	1,889	847
Employment (%) ^b	76.4	75.0	1.4	1.9	2.0	(-1.9, 4.7)	4.9	2,369	1,157
Employment in healthcare (%) ^a	57.1	49.5	7.7***	15.5	2.2	(4.0, 11.3)	5.5	1,889	847
Job quality (%) ^a	58.3	55.7	2.6	4.7	2.0	(-0.7, 5.9)	5.0	1,889	847
Earnings (\$) ^b	3,936	3,729	206	5.5	164	(-63, 476)	409	2,369	1,157
TANF (%) ^a	8.9	7.9	1.0	12.8	1.1	(-0.7, 2.7)	2.6	1,889	847
Typical and High Contrast									
Educational Progress (%)^c	67.5	59.2	8.3***	14.1	1.4	(6.0, 10.6)	3.5	4,912	2,802
Program completion (%) ^c	50.0	39.3	10.6***	27.1	1.3	(8.5, 12.8)	3.2	4,912	2,802
Employment (%) ^d	67.5	67.4	0.1	0.1	1.1	(-1.8, 1.9)	2.8	6,304	3,887
Employment in healthcare (%) ^c	51.4	38.9	12.4***	32.0	1.1	(10.6, 14.3)	2.8	4,912	2,802
Job quality (%) ^e	57.8	55.8	2.0	3.5	1.6	(-0.6, 4.5)	3.9	3,677	1,678
Earnings (\$) ^d	3,346	3,225	121	3.7	91	(-29, 270)	227	6,304	3,887
TANF (%) ^e	8.2	9.3	-1.1	-11.4	0.7	(-2.2, 0.0)	1.7	3,677	1,678

Notes: Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Employment and earnings are measured in the fifth quarter after random assignment.

Sample Sizes and Sources:

^a 8 programs. HPOG follow-up survey.

^b 8 programs. National Directory of New Hires.

^c 34 programs. HPOG and PACE follow-up surveys.

^d 34 programs. National Directory of New Hires.

^e 28 programs. HPOG follow-up survey.

Exhibit D.7: Differences in Impacts by Level of Service Contrast

Outcome	Group with More Favorable Impact	Group With Less Favorable Impact	Difference	Standard Error	Confidence Interval	Sample Size: Treatment	Sample Size: Control
Educational Progress^a	Typical or High Contrast	Low Contrast	3.9 *	2.4	(0.0, 7.8)	6,801	3,649
Program Completion^a	None	None	N/A				
Employment^b	None	None	N/A				
Employment in Healthcare^a	Typical or High Contrast	Low Contrast	4.8 *	2.6	(0.5, 9.1)	6,801	3,649
Job Quality^c	None	None	N/A				
Earnings^b	None	None	N/A				
TANF^c	Typical or High Contrast	Low Contrast	-2.1 *	1.2	(-4.1, -0.1)	5,566	2,525

Notes: Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Employment and earnings are measured in the fifth quarter after random assignment. Subgroup differences are listed in the table if they are significant at the 10 percent level. Subgroup differences that are not listed are not significantly different than zero.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

Impacts by Type of Institution Operating the Programs

The report's Exhibit 2.1 maps HPOG programs and displays different markers for the three different types of programs. Out of 42 programs:

- 12 programs were operated by workforce development agencies (stars),
- 21 programs were operated by institutions of higher education (circles), and
- 9 programs were operated by other government agencies, non-profit institutions or quasi-governmental entities (squares).

Exhibit D.8 below reports impacts separately for each type of program operator.

Treatment group members in **workforce development agencies** had favorable impacts of HPOG in educational progress and employment in healthcare of 5 and 11 percentage points, respectively. The impact on TANF receipt was favorable as well: the treatment groups served by workforce development agencies experienced a 2 percentage point decrease in TANF receipt relative to the control group.

Treatment group members from **institutes of higher education** saw favorable impacts on educational progress and employment in healthcare (7 and 10 percentage points, respectively), but no impact on TANF.

Treatment group members from programs run through **government agencies** experienced favorable impacts on educational progress (12 points) and employment in healthcare (14 points); and unfavorable impacts on receipt of TANF (an increase of 4 percentage points).

Exhibit D.9 identifies the differences in impacts across these institution types that are statistically significant. It shows that the impacts on educational progress and program completion were more favorable for programs run by government agencies than for those run by either by institutes of higher education or workforce development agencies; but that there was no difference in the impacts between the programs run by institutes of higher education and workforce development agencies.

Differences in impacts on TANF receipt are inconsistent with the pattern of impacts on other outcomes. Impacts in institutes of higher education and workforce development agency programs were more favorable in terms of TANF receipt, with a 4 to 6 point relatively larger decrease being observed in those programs relative to government agencies. The TANF outcome was measured on the HPOG-only sample, while the other outcomes were measured on the full sample. Because four HPOG/PACE programs were operated by government agencies, excluding these programs reduced the number of programs operated by government agencies from nine to five, and may have significantly changed the composition of programs operated by government agencies.

Exhibit D.8: Impacts by Type of Program Operator

Outcome	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
WIB/One Stop									
Educational Progress (%)^a	67.9	62.8	5.1***	8.1	1.6	(2.5, 7.7)	4.0	2,182	1,120
Program completion (%) ^a	50.4	43.5	6.9***	15.9	1.9	(3.7, 10.1)	4.8	2,182	1,120
Employment (%) ^b	69.4	68.9	0.6	0.8	1.7	(-2.3, 3.4)	4.3	2,852	1,579
Employment in healthcare (%) ^a	59.1	48.3	10.8***	22.4	1.7	(8.0, 13.6)	4.2	2,182	1,120
Job quality (%) ^c	58.3	55.7	2.5	4.6	2.3	(-1.2, 6.3)	5.7	1,936	900
Earnings (\$) ^b	3,637	3,428	209	6.1	141	(-23, 440)	351	2,852	1,579
TANF (%) ^c	8.5	10.7	-2.2**	-20.5	1.0	(-3.8, -0.6)	2.5	1,936	900
Institute of Higher Education									
Educational Progress (%)^d	65.0	58.3	6.7***	11.6	1.7	(4.0, 9.5)	4.1	3,272	1,708
Program completion (%) ^d	45.9	36.4	9.5***	26.2	1.6	(7.0, 12.1)	3.9	3,272	1,708
Employment (%) ^e	68.7	68.4	0.3	0.4	1.5	(-2.1, 2.7)	3.6	4,144	2,332
Employment in healthcare (%) ^d	48.6	38.3	10.3***	26.8	1.6	(7.6, 13.0)	4.1	3,272	1,708
Job quality (%) ^f	50.7	48.3	2.4	4.9	1.7	(-0.4, 5.1)	4.2	2,772	1,231
Earnings (\$) ^e	3,281	3,145	136	4.3	119	(-59, 331)	296	4,144	2,332
TANF (%) ^f	8.4	8.7	-0.3	-3.3	0.9	(-1.7, 1.1)	2.2	2,772	1,231
Government Agency									
Educational Progress (%)^g	73.0	60.9	12.1***	19.9	2.2	(8.4, 15.8)	5.6	1,347	821
Program completion (%) ^g	56.7	40.7	16.0***	39.4	2.2	(12.5, 19.6)	5.4	1,347	821
Employment (%) ^h	71.4	71.1	0.2	0.3	2.1	(-3.2, 3.7)	5.3	1,677	1,133
Employment in healthcare (%) ^g	52.0	38.1	13.9***	36.5	2.1	(10.5, 17.4)	5.2	1,347	821
Job quality (%) ⁱ	55.6	54.7	1.0	1.8	2.7	(-3.4, 5.3)	6.6	858	394
Earnings (\$) ^h	3,680	3,622	58	1.6	174	(-228, 344)	434	1,677	1,133
TANF (%) ⁱ	8.5	4.8	3.8**	78.6	1.6	(1.1, 6.4)	4.0	858	394

Notes: Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Employment and earnings are measured in the fifth quarter after random assignment.

Sample Sizes and Sources:

^a 12 programs. HPOG and PACE follow-up surveys.

^b 12 programs. National Directory of New Hires.

^c 11 programs. HPOG follow-up survey.

^d 21 programs. HPOG and PACE follow-up surveys.

^e 21 programs. National Directory of New Hires.

^f 20 programs. HPOG follow-up survey.

^g 9 programs. HPOG and PACE follow-up surveys.

^h 9 programs. National Directory of New Hires.

ⁱ 5 programs. HPOG follow-up survey.

Exhibit D.9: Differences in Impacts by Institution Type

Outcome	Subgroup with More Favorable Impact	Subgroup With Less Favorable Impact	Difference	Standard Error	Confidence Interval	Sample Size: Treatment	Sample Size: Control
Educational Progress^a							
	GovernmentAgency	WIB/One Stop	7.0 **	2.7	(2.5, 11.5)	3,529	1,941
	GovernmentAgency	Institute of Higher Ed	5.3 **	2.7	(0.8, 9.8)	4,619	2,529
Program Completion^a							
	GovernmentAgency	WIB/One Stop	9.1 **	2.8	(4.5, 13.7)	3,529	1,941
	GovernmentAgency	Institute of Higher Ed	6.5 **	2.6	(1.9, 11.1)	4,619	2,529
Employment^b							
	None	None	N/A				
Employment in Healthcare^a							
	None	None	N/A				
Job Quality^c							
	None	None	N/A				
Earnings^b							
TANF^c							
	WIB/One Stop	GovernmentAgency	-6.0 ***	2.0	(-9.3, -2.7)	2,794	1,294
	Institute of Higher Ed	GovernmentAgency	-4.0 **	1.9	(-7.1, -0.9)	3,630	1,625

Notes: Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Employment and earnings are measured in the fifth quarter after random assignment. Subgroup differences are listed in the table if they are significant at the 10 percent level. Subgroup differences that are not listed are not significantly different than zero.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

Program Impacts by Typical Duration of Completed Programs

Each of the 42 programs offered trainings that ranged in length. Given the wide range of possible training durations—anywhere from 6 weeks to several years—the average duration of training does not necessarily describe the typical experience of HPOG participants. To characterize the length of trainings offered by the program, we focus on the proportion of trainings completed that were short, i.e., less than 3 months. We consider a program to be:

- **short** duration if more than 80 percent of completed trainings were completed within 3 months of enrollment in training,
- **medium** duration if between 20 and 80 percent of trainings were completed within 3 months of enrollment in training, and
- **long** duration if less than 20 percent of completed trainings were completed within 3 months of enrollment in training.

This definition of long duration programs is consistent with the average duration of trainings for specific occupations (Exhibit 2.9), which shows that, on average, HPOG participants completed training for an RN in less than eight months, presumably because they had already completed many of the requirements.

Treatment group members in programs of short duration experienced positive impacts of HPOG (see Exhibit D.10). On average their educational progress improved by 7 percentage points, employment increased by 4 percentage points, employment in healthcare increased by 11 percentage points, and quarterly earnings increased by \$449. Treatment group members in programs of medium duration had positive impacts for educational progress and employment in healthcare (8 and 12 percentage points, respectively). Treatment group members in programs of long duration had positive impacts for employment in healthcare (8 percentage points).

These estimates should be interpreted cautiously. We used experiences of the HPOG program participants who completed training to create these groups of programs, and we do not observe the duration of programs that were not completed. Because of this, it is possible that we have mischaracterized the duration of trainings that programs offered.

Next, Exhibit D.11 reports which of these impacts statistically differ across the duration-defined groups of programs. It shows that treatment group members in programs of short duration experienced significantly larger impacts on employment (5 points), job quality (8 points) and earnings (\$406) than treatment group members in programs of medium duration. They also had larger impacts on educational progress (9 points), program completion (7 points) and job quality (8 points) than treatment group members in programs of long duration. Treatment group members in programs of medium duration also had statistically larger impact on educational progress (10 points) than treatment group members in programs of long duration. In brief, this analysis shows that those programs whose typical training duration were shorter had relatively more favorable impacts.

One possible explanation for this pattern of findings is that individuals in longer duration programs were more likely to be in school at baseline (see Exhibit D.12). Sixty-two percent of individuals in long-duration programs were in school at baseline. This stronger and more stable starting position likely put control group members at a less disadvantaged position relative to treatment group members than were control group members in shorter-duration courses. In effect, the contrasts for the three subgroups were different, contributing to the observed differences in impacts on educational progress.

Exhibit D.10: Impacts by Typical Duration of Completed Programs

Outcome	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Short Duration									
Educational Progress (%)^a	62.1	54.9	7.2***	13.1	2.4	(3.3, 11.1)	5.9	1,366	596
Program completion (%) ^a	42.7	31.7	11.0***	34.6	2.4	(7.0, 15.0)	6.0	1,366	596
Employment (%) ^b	74.6	70.4	4.2**	6.0	2.1	(0.7, 7.7)	5.3	1,779	871
Employment in healthcare (%) ^a	53.6	42.4	11.2***	26.5	2.5	(7.2, 15.2)	6.1	1,366	596
Job quality (%) ^a	63.9	55.8	8.2***	14.6	2.2	(4.6, 11.7)	5.4	1,366	596
Earnings (\$) ^b	3,564	3,115	449**	14.4	183	(148, 750)	456	1,779	871
TANF (%) ^a	11.6	12.6	-1.0	-7.7	1.4	(-3.3, 1.4)	3.6	1,366	596
Medium Duration									
Educational Progress (%)^c	68.7	60.3	8.4***	14.0	1.3	(6.2, 10.6)	3.3	4,874	2,813
Program completion (%) ^c	51.7	41.0	10.7***	26.1	1.3	(8.6, 12.8)	3.2	4,874	2,813
Employment (%) ^d	68.1	68.7	-0.7	-1.0	1.1	(-2.4, 1.1)	2.7	6,196	3,841
Employment in healthcare (%) ^c	52.2	40.5	11.7***	28.9	1.1	(9.8, 13.6)	2.8	4,874	2,813
Job quality (%) ^e	64.5	64.0	0.5	0.8	1.5	(-2.0, 3.0)	3.8	3,639	1,689
Earnings (\$) ^d	3,411	3,368	43	1.3	89	(-104, 189)	222	6,196	3,841
TANF (%) ^e	8.1	8.0	0.1	1.6	0.7	(-1.0, 1.3)	1.7	3,639	1,689
Long Duration									
Educational Progress (%)^f	71.3	73.1	-1.8	-2.4	3.7	(-7.8, 4.3)	9.2	561	240
Program completion (%) ^f	45.3	41.5	3.9	9.4	4.0	(-2.6, 10.4)	9.9	561	240
Employment (%) ^g	72.5	70.6	1.8	2.6	3.1	(-3.2, 6.9)	7.6	698	332
Employment in healthcare (%) ^f	56.6	48.8	7.7**	15.8	3.8	(1.5, 13.9)	9.4	561	240
Job quality (%) ^f	65.7	65.1	0.6	0.9	3.4	(-4.9, 6.2)	8.4	561	240
Earnings (\$) ^g	3,959	3,614	345	9.5	253	(-70, 760)	629	698	332
TANF (%) ^f	3.3	5.2	-2.0	-37.3	1.7	(-4.7, 0.8)	4.2	561	240

Notes: Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Employment and earnings are measured in the fifth quarter after random assignment.

Sample Sizes and Sources:

^a 6 programs. HPOG follow-up survey.

^b 6 programs. National Directory of New Hires.

^c 31 programs. HPOG and PACE follow-up surveys.

^d 31 programs. National Directory of New Hires.

^e 25 programs. HPOG follow-up survey.

^f 5 programs. HPOG follow-up survey.

^g 5 programs. National Directory of New Hires.

Exhibit D.11: Differences in Impacts by Typical Duration of Completed Programs

Outcome	Subgroup with More Favorable Impact	Subgroup With Less Favorable Impact	Difference	Standard Error	Confidence Interval	Sample Size: Treatment	Sample Size: Control
Educational Progress^a							
	Short Duration	Long Duration	9.0 **	4.2	(2.1, 15.9)	1,927	836
	Medium Duration	Long Duration	10.2 ***	3.8	(3.9, 16.5)	5,435	3,053
Program Completion^a							
	Short Duration	Long Duration	6.8 *	4.1	(0.0, 13.6)	1,927	836
Employment^b							
	Short Duration	Medium Duration	4.9 **	2.4	(0.9, 8.9)	7,975	4,712
Employment in Healthcare^a							
	None	None	N/A				
Job Quality^c							
	Short Duration	Medium Duration	7.7 ***	2.7	(3.2, 12.2)	5,005	2,285
	Short Duration	Long Duration	7.6 *	4.0	(1.0, 14.2)	1,927	836
Earnings^b							
	Short Duration	Medium Duration	\$406 **	204	(69, 743)	7,975	4,712
TANF^a							
	None	None	N/A				

Notes: Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Employment and earnings are measured in the fifth quarter after random assignment. Subgroup differences are listed in the table if they are significant at the 10 percent level. Subgroup differences that are not listed are not significantly different than zero.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

Exhibit D.12: Baseline Characteristics by Typical Duration of Completed Programs

Baseline Characteristics (%)	Short Duration	Medium Duration	Long Duration	Short vs. Medium Duration	Short vs. Long Duration	Medium vs. Long Duration	Sample Size
Enrolled in school ^a	18.4	23.1	62.1	-4.8***	-43.7***	-39.0***	9,910
Expect to participate in HPOG full-time ^b	68.6	74.6	87.4	-6.0***	-18.8***	-12.9***	12,729
Some college or college degree ^b	47.5	54.0	64.8	-6.6***	-17.4***	-10.8***	13,086
No barriers to school and work ^b	66.8	60.3	57.8	6.5***	9.1***	2.6	13,271
Employed ^b	43.3	41.5	51.3	1.8	-8.0***	-9.8***	12,571

Notes: Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources:

^a HPOG PRS

^b HPOG PRS, PACE Baseline Information Form

Appendix E: Expanded Results for Chapter 5

This appendix presents more detailed versions of the tables in Chapter 5, as follows:

- Section E.1 presents expanded results for impacts on demographic subgroups.
- Section E.2 presents expanded results for impacts on policy-relevant subgroups.
- Section E.3 presents impacts for subgroups defined by public assistance receipt at baseline.²²

For the report's exhibits that graphically display the differences in subgroup impacts, this appendix includes tables that show the underlying results that provide the data for those graphical exhibits, along with some additional detail, including standard errors, confidence intervals of the difference in impacts, and group sample sizes.

²² While preparing a subsequent report the analysis team discovered an error in the code that produced estimates of impacts by subgroup. Those errors were corrected and are reflected in the reissued report, as of November 2019. The errors are localized to analyses of subgroups. As a result, this chapter was heavily edited and references to subgroup impacts were revised throughout the report.

E.1 Expanded Results for Impacts on Demographic Subgroups

Exhibit E.1: Expanded Results for Impacts on Educational Progress, by Demographic Subgroup

Subgroup	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Age									
Under 25	62.4	54.9	7.6***	13.8	1.9	(4.5, 10.6)	4.7	2,180	1,068
25 or older	69.8	62.5	7.3***	11.7	1.1	(5.6, 9.1)	2.6	4,598	2,568
Race/Ethnicity									
Hispanic/Latino of any race	63.5	54.5	9.0***	16.5	2.3	(5.2, 12.7)	5.7	1,517	889
Black/African American, non-Hispanic	63.7	57.6	6.1***	10.5	1.6	(3.4, 8.7)	4.0	2,383	1,188
Other (includes white/Caucasian), non-Hispanic	73.4	65.9	7.6***	11.5	1.5	(5.1, 10.0)	3.7	2,828	1,460
Dependent Children									
No dependent children	70.0	62.6	7.4***	11.8	1.6	(4.7, 10.0)	4.0	2,497	1,238
One or more dependent children	66.3	58.9	7.4***	12.5	1.3	(5.2, 9.6)	3.3	4,021	2,166

Notes: Subgroup-specific sample sizes for treatment and control do not sum to analytic sample size due to missing data. The analysis model uses the full sample of individuals and imputes subgroup membership where it is missing.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources: HPOG and PACE follow-up surveys.

Although there are statistically significant impacts for all of these demographic subgroups, there are no detectable *differences in impacts between subgroups* defined by age, race/ethnicity, or presence of dependent children. As a consequence, there is no supplemental exhibit showing between-subgroup differences (as exist for some of the other subgroups examined).

E.2 Expanded Results for Impacts on Policy-Relevant Subgroups**Exhibit E.2: Expanded Results for Impacts by School Enrollment at Baseline**

Outcome	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Not Enrolled at Baseline – 72% of Sample									
Educational progress (%)^a	60.4	52.6	7.7***	14.7	1.4	(5.4, 10.1)	3.5	3,902	1,733
Program completion (%) ^a	43.7	33.1	10.7***	32.3	1.7	(8.0, 13.4)	4.1	3,902	1,733
Employment (%) ^b	74.3	72.5	1.8	2.5	1.3	(-0.3, 4.0)	3.3	5,056	2,428
Employment in healthcare (%) ^a	53.6	40.3	13.3***	32.9	1.5	(10.7, 15.8)	3.8	3,902	1,733
Job quality (%) ^a	56.8	53.1	3.7**	7.0	1.5	(1.3, 6.1)	3.6	3,902	1,733
Earnings (\$) ^b	3,661	3,429	232**	6.8	102	(64, 400)	255	5,056	2,428
TANF receipt (%) ^a	9.7	10.7	-1.0	-9.7	0.8	(-2.4, 0.3)	2.0	3,902	1,733
Enrolled at Baseline – 28% of Sample									
Educational progress (%)^a	78.1	74.8	3.3	4.3	2.5	(-0.8, 7.3)	6.2	1,459	622
Program completion (%) ^a	46.6	39.3	7.3***	18.5	2.4	(3.4, 11.1)	5.9	1,459	622
Employment (%) ^b	74.2	75.6	-1.3	-1.8	1.9	(-4.5, 1.9)	4.8	1,789	816
Employment in healthcare (%) ^a	58.3	49.7	8.6***	17.3	2.4	(4.7, 12.5)	6.0	1,459	622
Job quality (%) ^a	61.1	63.1	-2.0	-3.2	2.3	(-5.8, 1.8)	5.7	1,459	622
Earnings (\$) ^b	4,105	3,988	117	2.9	158	(-143, 376)	393	1,789	816
TANF receipt (%) ^a	5.0	3.5	1.5	42.9	1.0	(-0.1, 3.1)	2.5	1,459	622

Notes: Subgroup-specific sample sizes for treatment and control do not sum to analytic sample size due to missing data. The analysis model uses the full sample of individuals and imputes subgroup membership where it is missing. Employment and earnings are measured in the fifth quarter after random assignment.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources:

^a HPOG follow-up survey.

^b National Directory of New Hires.

Exhibit E.3: Differences in Impacts by School Enrollment at Baseline

Outcome	Subgroup with More Favorable Impact	Subgroup with Less Favorable Impact	Difference	Standard Error	Confidence Interval	Sample Size: Treatment	Sample Size: Control
Educational progress (%) ^a	None	None	N/A				
Program completion (%) ^a	None	None	N/A				
Employment (%) ^b	None	None	N/A				
Employment in healthcare (%) ^c	Not Enrolled	Enrolled	4.7*	2.7	(0.2, 9.1)	5,361	2,355
Job quality (%) ^a	Not Enrolled	Enrolled	5.7**	2.7	(1.8, 10.8)	5,361	2,355
Earnings (\$) ^b	None	None	N/A				
TANF receipt (%) ^a		Enrolled	-2.6*	1.5	(-5.0, -0.1)	5,361	2,355

Notes: Subgroup-specific sample sizes for treatment and control do not sum to analytic sample size due to missing data. The analysis model uses the full sample of individuals and imputes subgroup membership where it is missing. Employment and earnings are measured in the fifth quarter after random assignment.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent. Subgroup differences are listed in the table if they are significant at the 10 percent level. Subgroup differences that are not listed are not statistically significantly different than zero.

Sources:

^a HPOG follow-up survey.

^b National Directory of New Hires.

Exhibit E.4: Expanded Results for Impacts by Baseline Expectations for Participation in HPOG

Outcome	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Part-Time – 26% of Sample									
Educational progress (%)^a	62.8	56.5	6.2***	11.0	2.0	(3.0, 9.5)	4.9	1,682	868
Program completion (%) ^a	47.3	35.9	11.4***	31.8	2.2	(7.8, 15.0)	5.4	1,682	868
Employment (%) ^b	72.5	71.4	1.1	1.5	1.7	(-1.6, 3.8)	4.1	2,183	1,181
Employment in healthcare (%) ^a	52.3	39.3	13.0***	33.1	2.0	(9.7, 16.4)	5.1	1,682	868
Job quality (%) ^c	58.6	55.3	3.3	6.0	2.2	(-0.4, 7.0)	5.6	1,408	633
Earnings (\$) ^b	3,606	3,568	38	1.1	131	(-177, 254)	326	2,183	1,181
TANF receipt (%) ^c	5.8	6.7	-0.9	-13.2	1.4	(-3.1, 1.4)	3.4	1,408	633
Full-Time – 74% of Sample									
Educational progress (%)^a	69.3	61.5	7.8***	12.7	1.1	(6.0, 9.6)	2.8	4,845	2,614
Program completion (%) ^a	50.3	40.7	9.5***	23.4	1.3	(7.4, 11.7)	3.2	4,845	2,614
Employment (%) ^b	68.5	68.4	0.1	0.1	1.1	(-1.7, 1.9)	2.8	6,142	3,631
Employment in healthcare (%) ^a	52.6	42.0	10.5***	25.1	1.3	(8.3, 12.7)	3.3	4,845	2,614
Job quality (%) ^c	57.7	55.9	1.8	3.2	1.4	(-0.5, 4.1)	3.5	3,926	1,761
Earnings (\$) ^b	3,444	3,265	179**	5.5	88	(34, 324)	220	6,142	3,631
TANF receipt (%) ^c	9.4	9.6	-0.2	-2.1	0.7	(-1.4, 1.0)	1.8	3,926	1,761

Notes: Subgroup-specific sample sizes for treatment and control do not sum to analytic sample size due to missing data. The analysis model uses the full sample of individuals and imputes subgroup membership where it is missing.

Employment and earnings are measured in the fifth quarter after random assignment.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

There are no detectable differences in impacts between subgroups defined by baseline expectations for participation in HPOG. As a consequence, there is no supplemental exhibit showing between-subgroup differences.

Exhibit E.5: Expanded Results for Impacts by Educational Attainment at Baseline

Outcome	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Less than 12th Grade – 12% of Sample									
Educational progress (%)^a	57.1	52.7	4.5*	8.5	2.7	(0.0, 8.9)	6.7	661	515
Program completion (%) ^a	49.6	37.5	12.1***	32.3	2.5	(8.0, 16.2)	6.2	661	515
Employment (%) ^b	63.1	65.0	-2.0	-3.0	2.3	(-5.7, 1.8)	5.7	883	737
Employment in healthcare (%) ^a	47.0	38.0	9.0***	23.7	2.8	(4.3, 13.7)	7.1	661	515
Job quality (%) ^c	55.5	51.9	3.6	6.9	5.1	(-4.9, 12.0)	12.8	269	130
Earnings (\$) ^b	2,604	2,668	-64	-2.4	179	(-358, 230)	446	883	737
TANF receipt (%) ^c	11.8	13.8	-2.0	-14.8	3.1	(-7.2, 3.1)	7.7	269	130
High School or Equivalent – 34% of Sample									
Educational progress (%)^a	54.3	44.9	9.4***	21.0	1.8	(6.5, 12.4)	4.5	2,383	1,047
Program completion (%) ^a	44.0	33.3	10.7***	32.3	1.7	(8.0, 13.4)	4.1	2,383	1,047
Employment (%) ^b	72.2	70.9	1.3	1.8	1.5	(-1.2, 3.7)	3.7	3,094	1,516
Employment in healthcare (%) ^a	50.0	41.0	9.0***	21.9	1.8	(6.1, 11.9)	4.4	2,383	1,047
Job quality (%) ^c	56.3	57.4	-1.0	-1.8	1.9	(-4.1, 2.0)	4.6	2,259	931
Earnings (\$) ^b	3,365	3,223	142	4.4	114	(-45, 329)	284	3,094	1,516
TANF receipt (%) ^c	10.2	10.4	-0.2	-1.7	1.1	(-2.0, 1.6)	2.8	2,259	931
Some College – 36% of Sample									
Educational progress (%)^a	74.1	66.3	7.8***	11.7	1.7	(5.0, 10.6)	4.2	2,387	1,334
Program completion (%) ^a	54.1	44.5	9.7***	21.7	1.9	(6.6, 12.7)	4.7	2,387	1,334
Employment (%) ^b	69.2	68.1	1.1	1.6	1.4	(-1.3, 3.4)	3.5	3,021	1,785
Employment in healthcare (%) ^a	55.1	42.3	12.8***	30.3	1.5	(10.4, 15.3)	3.7	2,387	1,334
Job quality (%) ^c	59.8	56.1	3.8*	6.7	2.1	(0.3, 7.2)	5.2	1,790	813
Earnings (\$) ^b	3,563	3,385	179	5.3	112	(-6, 363)	279	3,021	1,785
TANF receipt (%) ^c	6.4	7.7	-1.3	-16.6	1.1	(-3.1, 0.5)	2.8	1,790	813
Degree – 18% of Sample									
Educational progress (%)^a	84.4	79.5	5.0***	6.3	1.8	(2.1, 7.9)	4.4	1,284	635
Program completion (%) ^a	49.6	41.5	8.0***	19.4	2.4	(4.1, 12.0)	5.9	1,284	635
Employment (%) ^b	70.5	71.5	-1.0	-1.4	2.0	(-4.3, 2.2)	4.9	1,558	829
Employment in healthcare (%) ^a	56.1	42.6	13.5***	31.6	2.5	(9.3, 17.6)	6.2	1,284	635
Job quality (%) ^c	58.4	53.1	5.3**	10.0	2.4	(1.4, 9.2)	6.0	1,173	541
Earnings (\$) ^b	4,237	4,052	186	4.6	154	(-67, 438)	383	1,558	829
TANF receipt (%) ^c	7.6	6.6	1.0	15.7	1.2	(-0.9, 3.0)	2.9	1,173	541

Notes: Subgroup-specific sample sizes for treatment and control do not sum to analytic sample size due to missing data. The analysis model uses the full sample of individuals and imputes subgroup membership where it is missing.

Employment and earnings are measured in the fifth quarter after random assignment.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

Exhibit E.6: Differences in Impacts by Educational Attainment at Baseline

Outcome	Subgroup with More Favorable Impact	Subgroup with Less Favorable Impact	Difference	Standard Error	Confidence Interval	Sample Size: Treatment	Sample Size: Control
Educational progress (%) ^a	None	None	N/A				
Program completion (%) ^a	None	None	N/A				
Employment (%) ^b	None	None	N/A				
Employment in healthcare (%) ^a	None	None	N/A				
Job quality (%) ^a	None	None	N/A				
Earnings (\$) ^b	Degree	High School or Equivalent	6.3**	3.1	(1.1, 11.5)	1,442	671
	Some College	High School or Equivalent	4.8*	2.7	(0.4, 9.2)	4,049	1,744
TANF receipt (%) ^c	None	None	N/A				
	None	None	N/A				

Notes: Subgroup-specific sample sizes for treatment and control do not sum to analytic sample size due to missing data. The analysis model uses the full sample of individuals and imputes subgroup membership where it is missing.

Employment and earnings are measured in the fifth quarter after random assignment.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent. Subgroup differences are listed in the table if they are significant at the 10 percent level. Subgroup differences that are not listed are not statistically significantly different than zero.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey

Exhibit E.7: Expanded Results for Impacts by Barriers to School and Work at Baseline

Outcome	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Two or More Barriers – 10% of Sample									
Educational PROGRESS (%)^a	66.4	58.4	8.0**	13.8	3.4	(2.5, 13.6)	8.5	619	385
Program completion (%) ^a	49.7	38.3	11.5***	30.0	3.1	(6.5, 16.5)	7.6	619	385
Employment (%) ^b	64.0	63.7	0.3	0.5	2.6	(-4.0, 4.6)	6.5	779	526
Employment in healthcare (%) ^a	50.4	39.3	11.1***	28.2	3.5	(5.4, 16.8)	8.7	619	385
Job quality (%) ^c	56.1	56.1	-0.1	-0.1	3.5	(-5.7, 5.6)	8.6	488	240
Earnings (\$) ^b	2,813	2,997	-184	-6.1	188	(-493, 125)	468	779	526
TANF receipt (%) ^c	11.2	9.7	1.5	15.5	2.1	(-2.0, 5.0)	5.3	488	240
One Barrier – 24% of Sample									
Educational progress (%)^a	66.6	59.2	7.4***	12.4	1.9	(4.3, 10.4)	4.6	1,468	833
Program completion (%) ^a	49.6	38.7	10.8***	28.0	2.0	(7.5, 14.1)	5.0	1,468	833
Employment (%) ^b	67.3	67.0	0.3	0.5	1.8	(-2.6, 3.2)	4.4	1,850	1,146
Employment in healthcare (%) ^a	51.4	40.2	11.1***	27.7	1.9	(8.0, 14.3)	4.8	1,468	833
Job quality (%) ^c	55.8	55.6	0.3	0.5	2.5	(-3.8, 4.3)	6.1	1,185	557
Earnings (\$) ^b	3,156	3,168	-11	-0.4	129	(-223, 201)	322	1,850	1,146
TANF receipt (%) ^c	9.4	10.0	-0.6	-5.9	1.5	(-3.0, 1.9)	3.7	1,185	557
No Barriers – 66% of Sample									
Educational progress (%)^a	68.3	61.2	7.1***	11.6	1.1	(5.3, 8.9)	2.8	4,239	2,156
Program completion (%) ^a	49.5	40.2	9.3***	23.1	1.2	(7.3, 11.3)	3.0	4,239	2,156
Employment (%) ^b	71.2	70.9	0.3	0.4	1.1	(-1.6, 2.2)	2.8	5,430	2,992
Employment in healthcare (%) ^a	53.3	42.3	11.0***	26.0	1.4	(8.8, 13.2)	3.4	4,239	2,156
Job quality (%) ^c	58.7	55.5	3.2**	5.8	1.5	(0.8, 5.6)	3.6	3,553	1,580
Earnings (\$) ^b	3,705	3,465	240***	6.9	92	(90, 391)	228	5,430	2,992
TANF receipt (%) ^c	7.7	8.2	-0.5	-6.4	0.7	(-1.6, 0.6)	1.7	3,553	1,580

Notes: Subgroup-specific sample sizes for treatment and control do not sum to analytic sample size due to missing data. The analysis model uses the full sample of individuals and imputes subgroup membership where it is missing.

Employment and earnings are measured in the fifth quarter after random assignment.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

Exhibit E.8: Differences in Impacts by Barriers to School and Work at Baseline

Outcome	Subgroup with More Favorable Impact	Subgroup with Less Favorable Impact	Difference	Standard Error	Confidence Interval	Sample Size: Treatment	Sample Size: Control
Educational progress (%) ^a	None	None	N/A				
Program completion (%) ^a	None	None	N/A				
Employment (%) ^b	None	None	N/A				
Employment in healthcare (%) ^a	None	None	N/A				
Job quality (%) ^a	None	None	N/A				
Earnings (\$) ^b	None	None	N/A				
	No Barriers	Two or More Barriers	\$425**	197	(101, 748)	6,209	3,518
	No Barriers	One Barrier	\$251*	141	(19, 484)	7,280	4,138
TANF receipt (%) ^c	None	None	N/A				

Notes: Subgroup-specific sample sizes for treatment and control do not sum to analytic sample size due to missing data. The analysis model uses the full sample of individuals and imputes subgroup membership where it is missing.

Employment and earnings are measured in the fifth quarter after random assignment.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent. Subgroup differences are listed in the table if they are significant at the 10 percent level. Subgroup differences that are not listed are not statistically significantly different than zero.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

Exhibit E.9: Expanded Results for Impacts by Employment at Baseline

Outcome	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Employed – 43% of Sample									
Educational progress (%)^a	71.0	63.7	7.3***	11.4	1.4	(5.0, 9.6)	3.5	2,803	1,453
Program completion (%) ^a	52.9	41.5	11.4***	27.4	1.7	(8.6, 14.2)	4.2	2,803	1,453
Employment (%) ^b	81.7	82.7	-1.0	-1.3	1.4	(-3.3, 1.2)	3.4	3,521	1,968
Employment in healthcare (%) ^a	63.3	54.1	9.2***	17.1	1.6	(6.6, 11.8)	4.0	2,803	1,453
Job quality (%) ^c	66.6	67.2	-0.5	-0.8	1.8	(-3.6, 2.5)	4.6	2,367	1,054
Earnings (\$) ^b	4,364	4,292	72	1.7	108	(-105, 248)	268	3,521	1,968
TANF receipt (%) ^c	5.1	5.9	-0.7	-12.7	0.8	(-2.0, 0.5)	2.0	2,367	1,054
Not Employed – 57% of Sample									
Educational progress (%)^a	65.2	57.7	7.5***	13.0	1.2	(5.4, 9.5)	3.1	3,660	1,951
Program completion (%) ^a	47.1	38.0	9.1***	23.9	1.4	(6.9, 11.3)	3.4	3,660	1,951
Employment (%) ^b	60.8	59.4	1.5	2.5	1.2	(-0.6, 3.5)	3.1	4,683	2,718
Employment in healthcare (%) ^a	44.8	32.0	12.8***	39.8	1.4	(10.5, 15.0)	3.5	3,660	1,951
Job quality (%) ^c	50.8	46.3	4.5***	9.7	1.6	(1.9, 7.1)	3.9	2,877	1,239
Earnings (\$) ^b	2,862	2,660	202**	7.6	97	(43, 361)	241	4,683	2,718
TANF receipt (%) ^c	11.2	11.3	-0.1	-0.9	1.0	(-1.8, 1.6)	2.5	2,877	1,239

Notes: Subgroup-specific sample sizes for treatment and control do not sum to analytic sample size due to missing data. The analysis model uses the full sample of individuals and imputes subgroup membership where it is missing.

Employment and earnings are measured in the fifth quarter after random assignment.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

Exhibit E.10: Differences in Impacts by Employment at Program Entry

Outcome	Subgroup with More Favorable Impact	Subgroup with Less Favorable Impact	Difference	Standard Error	Confidence Interval	Sample Size: Treatment	Sample Size: Control
Educational progress (%) ^a	None	None	N/A				
Program completion (%) ^a	None	None	N/A				
Employment (%) ^b	None	None	N/A				
Employment in healthcare (%) ^a	Not Employed	Employed	3.5*	2.1	(0.1, 6.9)	6,463	3,404
Job quality (%) ^a	Not Employed	Employed	5.0**	2.4	(1.1, 9.0)	5,244	2,293
Earnings (\$) ^b	None	None	N/A				
TANF receipt (%) ^c	None	None	N/A				

Notes: Subgroup-specific sample sizes for treatment and control do not sum to analytic sample size due to missing data. The analysis model uses the full sample of individuals and imputes subgroup membership where it is missing.

Employment and earnings are measured in the fifth quarter after random assignment.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent. Subgroup differences are listed in the table if they are significant at the 10 percent level. Subgroup differences that are not listed are not statistically significantly different than zero.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

E.3 Expanded Results for Impacts on Public Assistance Subgroups**Exhibit E.11: Expanded Results for Impacts by Public Assistance Receipt at Baseline**

Outcome	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Received TANF – 12% of Sample									
Educational progress (%)^a	65.1	53.6	11.5***	21.4	2.8	(6.9, 16.1)	7.0	725	403
Program completion (%) ^a	51.4	40.1	11.3***	28.2	3.0	(6.3, 16.3)	7.6	725	403
Employment (%) ^b	59.6	57.1	2.4	4.2	2.3	(-1.4, 6.2)	5.7	940	559
Employment in healthcare (%) ^a	46.5	33.9	12.6***	37.3	2.9	(7.9, 17.3)	7.1	725	403
Job quality (%) ^c	53.3	42.3	11.0***	26.1	3.2	(5.8, 16.2)	7.9	526	225
Earnings (\$) ^b	2,390	2,296	95	4.1	177	(-195, 384)	439	940	559
TANF receipt (%) ^c	32.6	33.4	-0.8	-2.4	3.4	(-6.3, 4.7)	8.4	526	225
Received WIC/SNAP Only – 46% of Sample									
Educational progress (%)^a	65.5	56.9	8.6***	15.1	1.5	(6.2, 11.0)	3.6	3,021	1,670
Program completion (%) ^a	49.6	38.2	11.4***	30.0	1.7	(8.7, 14.2)	4.1	3,021	1,670
Employment (%) ^b	67.0	67.5	-0.4	-0.6	1.3	(-2.5, 1.7)	3.2	3,858	2,239
Employment in healthcare (%) ^a	51.9	39.8	12.2***	30.6	1.4	(9.9, 14.5)	3.5	3,021	1,670
Job quality (%) ^c	56.2	56.5	-0.4	-0.7	1.9	(-3.5, 2.8)	4.8	2,480	1,133
Earnings (\$) ^b	3,155	3,061	93	3.0	103	(-75, 261)	255	3,858	2,239
TANF receipt (%) ^c	8.5	9.5	-0.9	-9.9	1.0	(-2.5, 0.6)	2.4	2,480	1,133
No Assistance – 42% of Sample									
Educational progress (%)^a	71.3	66.4	4.9***	7.4	1.4	(2.6, 7.3)	3.5	2,823	1,378
Program completion (%) ^a	49.1	40.9	8.2***	19.9	1.6	(5.6, 10.8)	3.9	2,823	1,378
Employment (%) ^b	75.4	74.8	0.6	0.8	1.4	(-1.7, 2.9)	3.4	3,570	1,964
Employment in healthcare (%) ^a	55.3	45.6	9.7***	21.3	1.6	(7.1, 12.4)	4.0	2,823	1,378
Job quality (%) ^c	61.0	58.1	2.9	4.9	1.8	(-0.1, 5.9)	4.5	2,397	1,020
Earnings (\$) ^b	4,192	3,982	210**	5.3	105	(37, 383)	262	3,570	1,964
TANF receipt (%) ^c	2.4	2.2	0.2	9.7	0.6	(-0.8, 1.2)	1.6	2,397	1,020

Notes: Subgroup-specific sample sizes for treatment and control do not sum to analytic sample size due to missing data. The analysis model uses the full sample of individuals and imputes subgroup membership where it is missing.

Employment and earnings are measured in the fifth quarter after random assignment.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources: ^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

Exhibit E.12: Differences in Impacts by Public Assistance Receipt at Baseline

Outcome	Subgroup with More Favorable Impact	Subgroup with Less Favorable Impact	Difference	Standard Error	Confidence Interval	Sample Size: Treatment	Sample Size: Control
Educational progress (%)^a							
	Received TANF	No Assistance	6.6**	3.1	(1.4, 11.7)	3,548	1,781
	Received WIC or SNAP	No Assistance	3.7*	2.1	(0.3, 7.0)	5,844	3,048
Program completion (%)^a	None	None	N/A				
Employment (%)^b	None	None	N/A				
Employment in healthcare (%)^a	None	None	N/A				
Job quality (%)^a							
	Received TANF	No Assistance	8.2**	3.9	(1.7, 14.6)	2,923	1,245
	Received TANF	Received WIC or SNAP	11.4***	3.9	(5.0, 17.8)	3,006	1,358
Earnings (\$) ^b	None	None	N/A				
TANF receipt (%)^c	None	None	N/A				

Notes: Subgroup-specific sample sizes for treatment and control do not sum to analytic sample size due to missing data. The analysis model uses the full sample of individuals and imputes subgroup membership where it is missing.

Employment and earnings are measured in the fifth quarter after random assignment.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent. Subgroup differences are listed in the table if they are significant at the 10 percent level. Subgroup differences that are not listed are not statistically significantly different than zero.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

Appendix F: Expanded Results for Chapter 6

This appendix provides additional details for the analysis of the impact of adding specific enhancement components to the standard HPOG program, reported in Chapter 6 of the Interim Report. Chapter 1 of the Interim Report describes the experimental design that supports the tests of three enhancement components: emergency assistance, non-cash incentives, and facilitated peer support.

For each enhancement, this appendix includes: a textbox describing the guidance provided to programs on how to implement the enhancement, detailed comparisons of training and service participation between the enhanced and standard treatment groups, and expanded versions of the impact tables included in the Interim Report. In addition, this appendix compares the overall impacts of programs that tested emergency assistance, tested non-cash incentives, tested facilitated peer support, and did not test any enhancements.

Emergency Assistance was tested in the following 11 programs:

- Full Employment Council (MO),
- Bergen Community College (NJ)-Bergen Community College,
- Bergen Community College (NJ)-Brookdale Community College,
- Bergen Community College (NJ)-Community College of Morris,
- Bergen Community College (NJ)-Hudson County Community College,
- Bergen Community College (NJ)-Middlesex County College,
- Bergen Community College (NJ)-Passaic County Community College,
- Bergen Community College (NJ)-Sussex County Community College,
- Bergen Community College (NJ)-Union County College,
- Bergen Community College (NJ)-Warren County Community College, and
- NY Research Foundation of CUNY-Hostos Community College.

Non-cash Incentives were tested in the following five programs:

- Gateway Community and Technical College (KY),
- Bergen Community College (NJ)-Essex County College,
- Suffolk County (NY) Department of Labor,
- Alamo (TX) Community College District and University Health System, and
- South Carolina Department of Social Services.

Facilitated Peer Support was tested in the following three programs:

- The WorkPlace (CT),
- New Hampshire Office of Minority Health, and
- Buffalo and Erie County (NY) WDC.

F.1 Implementation of Emergency Assistance and Expanded Results**Rules for Configuring Emergency Assistance in Practice***Defining an emergency*

Programs defined the emergencies and/or barriers to participation for which they would make payments. Examples included:

- Transportation assistance, including car repair and public transit subsidy;
- Housing, including eviction prevention and utilities assistance;
- Uninsured medical emergencies;
- Childcare; and
- Food.

Qualifying for assistance

Programs developed a transparent and consistent application process for emergency assistance.

Frequency/access to assistance

All enhanced treatment group members had the opportunity to receive the emergency non-cash assistance, above and beyond regular support services provided by the program, at least once per year. Programs had discretion to allow multiple payments for the same emergency. This was not an entitlement support; programs were required to develop an application and approval process that, at minimum, required participants to demonstrate need. Programs also developed a payment structure that was accountable and provided direct payments to vendors, rather than participants, whenever possible.

Maximum budget

\$1,200 per student per year.

Exhibit F.1: Impacts of the Emergency Assistance Enhancement on Training and Service Participation

Training or Service Measure (%)	Enhanced Treatment Group Mean	Standard Treatment Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Enhanced Treatment	Sample Size: Standard Treatment	Sample Size: Control
Enrollment in Training or Classes										
Enrolled in Any Training since Randomization	68.1	67.9	0.2	0.2	2.7	(-4.3, 4.6)	6.8	529	707	537
Any enrollment in credit classes	24.2	23.1	1.1	4.6	2.8	(-3.6, 5.7)	7.0	529	707	537
Any enrollment in occupational classes	41.6	41.1	0.5	1.3	3.3	(-4.8, 5.9)	8.1	529	707	537
Any enrollment in ESL classes	1.8	1.0	0.8	78.2	0.8	(-0.6, 2.1)	2.1	529	707	537
Any enrollment in basic skills classes	6.5	4.9	1.7	34.7	1.4	(-0.7, 4.0)	3.6	529	707	537
Academic Support Services										
Received Any Academic Support Services	44.2	48.2	-4.1	-8.4	3.1	(-9.2, 1.0)	7.7	529	707	537
Received financial aid advising services	20.7	23.7	-3.0	-12.7	2.9	(-7.8, 1.8)	7.2	529	707	537
Received academic advising services	23.0	24.1	-1.0	-4.2	2.5	(-5.1, 3.1)	6.2	529	707	537
Received assessment services	19.8	21.5	-1.6	-7.6	2.2	(-5.3, 2.0)	5.5	529	707	537
Received tutoring services	11.8	13.7	-2.0	-14.3	2.0	(-5.2, 1.3)	4.9	529	707	537
Received peer support services	7.2	6.5	0.6	10.0	1.4	(-1.7, 3.0)	3.5	529	707	537
Career Support Services										
Received Any Career Support Services	48.7	47.1	1.6	3.4	3.0	(-3.3, 6.5)	7.5	529	707	537
Received career counseling services	29.6	29.2	0.4	1.4	2.7	(-4.0, 4.8)	6.6	529	707	537
Received job search services	42.7	40.0	2.7	6.7	2.6	(-1.6, 7.0)	6.5	529	707	537
Other Support Services										
Received Any Other Support Services	38.8	29.8	9.0***	30.2	2.9	(4.3, 13.7)	7.1	529	707	537
Received help arranging supports	20.6	18.5	2.2	11.7	2.1	(-1.3, 5.6)	5.3	529	707	537
Received counseling services	12.8	10.0	2.8	27.7	1.8	(-0.1, 5.7)	4.4	529	707	537
Received non-cash incentives	5.8	4.9	0.9	18.0	1.8	(-2.1, 3.9)	4.5	529	707	537
Received emergency assistance services	17.7	8.3	9.4***	112.5	2.2	(5.7, 13.0)	5.6	529	707	537

Notes: Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Source: HPOG follow-up survey.

Exhibit F.2: Expanded Results for Estimates of the Contribution of Emergency Assistance to Impact Magnitude

Outcome	Enhanced Treatment Group Mean	Standard Treatment Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Enhanced Treatment	Sample Size: Standard Treatment	Sample Size: Control
Educational Progress (%)^a	66.3	68.0	-1.8	-2.6	2.7	(-6.2, 2.7)	6.7	529	707	537
Program completion (%) ^a	45.9	48.0	-2.1	-4.4	2.9	(-6.9, 2.7)	7.3	529	707	537
Employment (%) ^b	72.8	72.0	0.8	1.1	2.4	(-3.1, 4.7)	5.9	675	910	781
Employment in healthcare (%) ^a	48.4	50.8	-2.4	-4.7	3.0	(-7.4, 2.6)	7.6	529	707	537
Job quality (%) ^c	52.8	53.9	-1.1	-2.1	3.0	(-6.0, 3.8)	7.4	529	707	537
Earnings (\$) ^b	4,075	3,921	154	3.9	190	(-158, 466)	472	675	910	781
TANF (%) ^c	9.8	11.1	-1.3	-11.4	1.7	(-4.1, 1.5)	4.2	529	707	537

Notes: Employment and earnings are measured in the fifth quarter after random assignment. Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

F.2 Implementation of Non-Cash Incentives and Expanded Results**Rules for Configuring Non-Cash Incentives in Practice***Determining incentive awards*

Incentive awards could not be cash, but could include material goods and gift cards. Examples included:

- School supplies, including pens, pencils, binders, textbooks, scrubs, lab coats, stethoscope;
- Household materials, including cleaning supplies, household necessities/furnishings;
- Personal items, including child needs (e.g., baby diapers) or clothing needed/appropriate for interviews; and
- Gift cards for gas (limited to the purchase of gasoline) or grocery stores (limited to allowable grocery or household items).

Benchmarks for incentives

Programs were encouraged to incentivize both results- and behavior-based benchmarks. Results-based benchmarks included:

- Completing soft-skills, basic skills, GED, and occupational training courses/certificates/degrees; and
- Securing employment or retaining employment for a set time period.

Behavior-based benchmarks included such things as attending class; achieving perfect attendance; completing course assignments on time; and creating budgets for managing household finances, transportation, or childcare.

Maximum budget

\$1,000 per participant over the course of HPOG enrollment.

Exhibit F.3: Impacts of the Non-Cash Incentives Enhancement on Training and Service Participation

Training or Service Measure (%)	Enhanced Treatment Group Mean	Standard Treatment Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Enhanced Treatment	Sample Size: Standard Treatment	Sample Size: Control
Enrollment in Training or Classes										
Enrolled in Any Training since Randomization	74.0	68.7	5.3*	7.8	3.1	(0.2, 10.5)	7.8	309	704	467
Any enrollment in credit classes	26.3	27.8	-1.5	-5.2	3.5	(-7.3, 4.3)	8.8	309	704	467
Any enrollment in occupational classes	45.0	37.5	7.5**	20.0	3.3	(2.1, 12.9)	8.2	309	704	467
Any enrollment in ESL classes	3.0	1.1	1.8	163.9	1.4	(-0.4, 4.1)	3.4	309	704	467
Any enrollment in basic skills classes	11.7	8.0	3.6	45.2	2.5	(-0.5, 7.8)	6.2	309	704	467
Academic Support Services										
Received Any Academic Support Services	51.2	53.9	-2.7	-5.1	3.3	(-8.2, 2.7)	8.3	309	704	467
Received financial aid advising services	25.6	29.2	-3.7	-12.6	3.6	(-9.7, 2.3)	9.1	309	704	467
Received academic advising services	26.4	32.0	-5.6*	-17.4	3.0	(-10.4, -0.7)	7.4	309	704	467
Received assessment services	25.3	26.0	-0.7	-2.9	3.3	(-6.2, 4.7)	8.2	309	704	467
Received tutoring services	12.9	17.5	-4.6*	-26.3	2.7	(-9.1, -0.2)	6.7	309	704	467
Received peer support services	9.9	7.5	2.4	31.2	2.0	(-1.0, 5.7)	5.1	309	704	467
Career Support Services										
Received Any Career Support Services	47.1	46.2	0.9	2.0	3.9	(-5.4, 7.3)	9.6	309	704	467
Received career counseling services	28.5	29.4	-0.9	-3.2	3.3	(-6.3, 4.5)	8.2	309	704	467
Received job search services	41.2	37.6	3.7	9.8	3.6	(-2.2, 9.6)	9.0	309	704	467
Other Support Services										
Received Any Other Support Services	50.2	31.5	18.8***	59.7	3.6	(12.8, 24.7)	9.0	309	704	467
Received help arranging supports	18.9	18.0	1.0	5.4	3.4	(-4.6, 6.5)	8.4	309	704	467
Received counseling services	12.3	10.8	1.6	14.4	2.0	(-1.7, 4.8)	5.0	309	704	467
Received non-cash incentives	36.8	7.1	29.7***	415.4	3.8	(23.5, 35.8)	9.4	309	704	467
Received emergency assistance services	12.4	9.3	3.1	33.7	2.6	(-1.2, 7.4)	6.5	309	704	467

Notes: Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources:

HPOG follow-up survey.

Exhibit F.4: Expanded Results for Estimates of the Contribution of Non-Cash Incentives to Impact Magnitude

Outcome	Enhanced Treatment Group Mean	Standard Treatment Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Enhanced Treatment	Sample Size: Standard Treatment	Sample Size: Control
Educational Progress (%)^a	54.2	56.3	-2.1	-3.7	3.3	(-7.5, 3.3)	8.2	309	704	467
Program completion (%) ^a	34.7	38.1	-3.4	-9.0	3.3	(-8.8, 2.0)	8.2	309	704	467
Employment (%) ^b	69.5	72.1	-2.6	-3.5	3.1	(-7.7, 2.6)	7.8	399	890	634
Employment in healthcare (%) ^a	47.8	44.4	3.4	7.6	3.4	(-2.2, 8.9)	8.4	309	704	467
Job quality (%) ^c	54.4	53.3	1.1	2.0	3.4	(-4.4, 6.6)	8.3	309	704	467
Earnings (\$) ^b	3,245	3,349	-104	-3.1	191	(-417, 209)	474	399	890	634
TANF (%) ^c	7.8	7.2	0.6	8.9	1.7	(-2.2, 3.5)	4.3	309	704	467

Notes: Employment and earnings are measured in the fifth quarter after random assignment. Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

F.3 Implementation of Peer Support and Expanded Results

Rules for Configuring Facilitated Peer Support Groups in Practice

Structure of peer support groups

Study guidelines required grantees to construct facilitated peer support groups that adhere to the following framework.

- *Facilitated:* A professional facilitator or an HPOG staff member with facilitation skills was to run the peer support group meetings. The facilitator needed to be familiar with a grantee's HPOG program offerings, client skills and needs, and community resources. Additionally, the facilitator had to be familiar with adult learning theory.
- *Convenient:* Peer support group meetings were to occur at a time and location that was convenient for group members. For example, they sometimes took place directly before or after scheduled classes.
- *Active participation:* Peer support group meetings were supposed to be interactive and participatory. The meetings could not be structured like a traditional course or lesson in which the staff member delivered didactic instruction to group members.
- *Manageable size:* Groups could include no more than 20 students, with 10-12 being the preferred size.

Content of peer support group meetings

Each peer support group meeting had to have an agenda. Grantees submitted a list of proposed meeting topics to the study team as part of their peer support enhancement plan. Examples of meeting topics included:

- Discussions of challenges that may impede students' academic success and information about available HPOG services or additional community resources;
- Study group sessions that encourage collaborative learning, student-led discussions, and self-reflective "lessons learned" to supplement content learning; and,
- Events for group members or group members' families and friends to expand participants' social networks.

Frequency of peer support group meetings

Peer support group meetings were required to occur at least twice per month, and preferably weekly. Programs that found it infeasible to meet weekly could meet in-person frequently (e.g. biweekly) and on the off weeks use alternative forms of communication such as conference calls, emails, or texting. These non-face-to-face activities were to be led by a facilitator and could reference topics from prior weeks. For example, the facilitator could communicate with participants via email or text and ask if goals from the previous week had been accomplished (e.g., practicing a new, desirable behavior) to which participants were asked to respond. The exception to the relaxed guideline for frequency of meetings was short-duration courses. To maximize effectiveness, groups in courses lasting eight weeks or less were required to meet weekly; courses of longer duration (12-16 weeks) were to meet at least eight times over the course of enrollment. Group meetings needed to take place before, during, and after enhanced treatment group members were enrolled in occupational trainings. Meeting length needed to be at least one hour.

Participation requirements/attendance policy

The enhancement evaluation required programs to assign students to participate in the support groups. Support groups could not be structured on a walk-in basis. However, programs could choose whether to make participation in the meeting mandatory or voluntary. In either instance, programs were to establish participant buy-in by discussing the benefits of peer support groups and working with group members to establish consensus around group norms and expectations. Although the specific requirements for attendance remained a program decision, it was strongly recommended that programs require participants to attend a minimum of 80 percent of the group sessions. If deemed appropriate by the grantee, new participants could replace those who had left the group, with the caveat that a minimum number of sessions had to still be available (e.g., at least half the sessions still remaining or the participant could roll over to the next peer support group). Grantees were also able to offer incentives to increase attendance rates. Acceptable incentives included transportation vouchers, food and beverages at meetings and/or gift cards for such items as gas, transportation, food, or clothing for each meeting students attended. Incentives were capped at \$25/student/meeting, with an annual limit of \$600 per student.

Maximum Budget

Grantee budgets allowed for up to \$600 per attending student per year and up to \$1,000 per meeting for staff or outside professional facilitators.

Exhibit F.5: Impacts of the Facilitated Peer Support Enhancement on Training and Service Participation

Training or Service Measure (%)	Enhanced Treatment Group Mean	Standard Treatment Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Enhanced Treatment	Sample Size: Standard Treatment	Sample Size: Control
Enrollment in Training or Classes										
Enrolled in Any Training since Randomization	81.0	79.6	1.4	1.7	3.2	(-3.8, 6.6)	7.9	330	448	354
Any enrollment in credit classes	29.0	26.9	2.1	7.6	3.6	(-3.9, 8.0)	9.0	330	448	354
Any enrollment in occupational classes	54.1	46.5	7.6*	16.2	4.0	(1.0, 14.1)	9.9	330	448	354
Any enrollment in ESL classes	2.9	2.0	0.9	42.7	1.0	(-0.8, 2.5)	2.5	330	448	354
Any enrollment in basic skills classes	10.2	8.4	1.8	21.1	2.4	(-2.2, 5.7)	6.0	330	448	354
Academic Support Services										
Received Any Academic Support Services	68.5	60.1	8.4**	13.9	3.9	(2.0, 14.7)	9.6	330	448	354
Received financial aid advising services	31.2	26.2	5.0	19.2	4.1	(-1.7, 11.7)	10.2	330	448	354
Received academic advising services	36.2	36.1	0.1	0.4	3.7	(-5.9, 6.2)	9.1	330	448	354
Received assessment services	30.9	23.4	7.5**	32.3	3.3	(2.2, 12.9)	8.1	330	448	354
Received tutoring services	17.7	13.1	4.6	35.3	3.0	(-0.4, 9.6)	7.6	330	448	354
Received peer support services	32.9	15.3	17.6***	114.5	3.4	(12.0, 23.1)	8.4	330	448	354
Career Support Services										
Received Any Career Support Services	55.1	51.2	3.9	7.6	4.0	(-2.6, 10.4)	9.8	330	448	354
Received career counseling services	34.9	32.9	2.1	6.3	4.5	(-5.3, 9.4)	11.1	330	448	354
Received job search services	45.6	40.3	5.3	13.2	3.7	(-0.7, 11.3)	9.1	330	448	354
Other Support Services										
Received Any Other Support Services	55.6	44.5	11.2***	25.1	3.8	(4.9, 17.4)	9.5	330	448	354
Received help arranging supports	24.7	27.2	-2.5	-9.2	3.4	(-8.1, 3.1)	8.4	330	448	354
Received counseling services	18.7	17.3	1.4	7.9	3.1	(-3.7, 6.5)	7.8	330	448	354
Received non-cash incentives	28.7	5.9	22.8***	387.7	2.7	(18.4, 27.3)	6.7	330	448	354
Received emergency assistance services	16.5	12.6	3.9	31.1	2.7	(-0.5, 8.3)	6.7	330	448	354

Notes: Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Source: HPOG follow-up survey.

Exhibit F.6: Expanded Results for Estimates of the Contribution of Facilitated Peer Support to Impact Magnitude

Outcome	Enhanced Treatment Group Mean	Standard Treatment Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Enhanced Treatment	Sample Size: Standard Treatment	Sample Size: Control
Educational Progress (%)^a	62.8	68.7	-5.8	-8.5	4.3	(-13.0, 1.3)	10.8	330	448	354
Program completion (%) ^a	42.1	48.6	-6.5	-13.5	4.3	(-13.5, 0.4)	10.6	330	448	354
Employment (%) ^b	75.7	77.3	-1.6	-2.0	2.8	(-6.2, 3.0)	7.0	390	541	458
Employment in healthcare (%) ^a	58.2	55.3	2.9	5.3	4.5	(-4.4, 10.2)	11.1	330	448	354
Job quality (%) ^c	53.4	56.6	-3.2	-5.7	3.8	(-9.5, 3.1)	9.5	330	448	354
Earnings (\$) ^b	4,186	4,618	-432*	-9.4	251	(-844, -20)	625	390	541	458
TANF (%) ^c	12.1	9.5	2.6	27.8	2.3	(-1.1, 6.4)	5.7	330	448	354

Notes: Employment and earnings are measured in the reflect impacts for fifth quarter after random assignment. Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources:

a HPOG and PACE follow-up surveys.

b National Directory of New Hires.

c HPOG follow-up survey.

F.4 Impacts by Enhancement Offered²³

The programs that implemented one of the three enhancement components to test its relative effectiveness could not already have had that component in place. Because of this, some observers might wonder whether these selected programs are meaningfully different from those programs either that had the components in place (and so were ineligible to add it) or otherwise chose not to participate in the three-armed experiment. In response, we investigated whether the overall impacts of programs varied based on whether the programs tested emergency assistance, non-cash incentives, peer support, or did not test any enhancements. Exhibit F.7 reports the results.

We find that HPOG increased educational progress in programs that added non-cash incentives or did not operate a three-armed experiment; and HPOG did not have a detectable effect on educational progress in programs that offered emergency assistance or peer support. That said, there are no statistically significant differences in impacts across these groupings of programs, not only for educational progress but for the other outcomes, as well; as a consequence, there is no supplemental exhibit showing differences between groupings.

In sum, the key take-away is this: the best evidence we have on the effectiveness of these three program components—offered as enhancements in 19 selected programs—comes from the experimental analysis of their impact. That evidence shows that none of emergency assistance, non-cash incentives, or facilitated peer support adds importantly to HPOG's overall impact. The additional analysis that compares impacts across these subsets of programs is helpful in identifying whether we should be comfortable *generalizing* the impacts across HPOG as a whole, rather than limiting it to the programs that ran the three-armed experiments.

Because there are no differences in impacts across these groupings of programs, it might be reasonable to infer that the findings of the enhancement tests could be generalized more broadly. That said, there are important descriptive differences across the groupings that relate to program implementation and their rationales for adding a program enhancement later that had not been intentionally integrated from the grant's beginning. Subsequent analysis by Walton, Harvill, and Peck (2019) offers additional insight on the extent to which generalizing from these results is warranted.

²³ The analysis and description in this section was revised in November 2019. See footnote 22 for more information.

Exhibit F.7: Impacts by Enhancement Tested

Outcome	Treatment Group Mean	Control Group Mean	Impact	Relative Impact	Standard Error	90% Confidence Interval	Minimum Detectable Effect	Sample Size: Treatment	Sample Size: Control
Emergency Assistance: 11 programs									
Educational progress (%)^a	66.5	61.7	4.8	7.8	3.1	(-0.3, 9.8)	7.7	1,236	537
Program completion (%) ^a	45.6	35.4	10.2***	28.8	3.2	(4.9, 15.5)	8.1	1,236	537
Employment (%) ^b	71.9	70.9	1.0	1.5	2.3	(-2.7, 4.8)	5.7	1,585	781
Employment in healthcare (%) ^a	50.6	40.8	9.8***	24.0	3.2	(4.6, 15.0)	7.8	1,236	537
Job quality (%) ^c	52.9	48.8	4.1	8.5	3.0	(-0.8, 9.1)	7.5	1,228	533
Earnings (\$) ^b	3,947	3,564	384**	10.8	175	(97, 671)	435	1,585	781
TANF (%) ^c	10.6	9.5	1.2	12.3	1.7	(-1.7, 4.0)	4.3	1,228	533
Non-cash Incentive: 5 programs									
Educational progress (%)^a	55.8	48.4	7.4**	15.4	3.0	(2.4, 12.4)	7.6	1,013	467
Program completion (%) ^a	36.3	25.8	10.4***	40.4	2.9	(5.7, 15.2)	7.2	1,013	467
Employment (%) ^b	71.5	71.8	-0.3	-0.4	2.7	(-4.7, 4.2)	6.8	1,289	634
Employment in healthcare (%) ^a	45.2	32.4	12.8***	39.6	2.8	(8.3, 17.4)	6.9	1,013	467
Job quality (%) ^c	54.0	54.5	-0.5	-0.8	3.0	(-5.4, 4.5)	7.5	1,009	464
Earnings (\$) ^b	3,287	3,276	11	0.3	205	(-325, 346)	509	1,289	634
TANF (%) ^c	7.4	7.0	0.4	6.0	1.7	(-2.4, 3.2)	4.2	1,009	464
Peer Support: 3 programs									
Educational progress (%)^a	64.8	62.1	2.6	4.3	4.2	(-4.3, 9.5)	10.5	778	354
Program completion (%) ^a	42.6	37.3	5.3	14.3	4.6	(-2.2, 12.8)	11.4	778	354
Employment (%) ^b	75.1	75.6	-0.4	-0.6	3.2	(-5.7, 4.8)	7.9	931	458
Employment in healthcare (%) ^a	53.3	44.1	9.2**	20.9	4.1	(2.6, 15.9)	10.1	778	354
Job quality (%) ^c	54.8	52.1	2.6	5.1	4.0	(-3.9, 9.1)	9.9	775	353
Earnings (\$) ^b	4,410	4,325	85	2.0	248	(-322, 491)	617	931	458
TANF (%) ^c	10.4	11.5	-1.1	-9.2	2.2	(-4.7, 2.6)	5.6	775	353
None: 23 programs									
Educational progress (%)^a	71.1	62.1	9.0***	14.6	1.5	(6.6, 11.4)	3.6	3,744	2,291
Program completion (%) ^a	54.3	43.7	10.7***	24.4	1.5	(8.2, 13.1)	3.7	3,744	2,291
Employment (%) ^b	67.6	67.2	0.4	0.5	1.3	(-1.8, 2.5)	3.3	4,868	3,171
Employment in healthcare (%) ^a	54.3	42.9	11.5***	26.7	1.5	(9.0, 14.0)	3.8	3,744	2,291
Job quality (%) ^c	62.6	60.5	2.1	3.5	1.7	(-0.6, 4.9)	4.2	2,533	1,165
Earnings (\$) ^b	3,251	3,154	97	3.1	104	(-73, 267)	258	4,868	3,171
TANF (%) ^c	7.3	8.5	-1.3	-14.8	1.0	(-2.9, 0.3)	2.4	2,533	1,165

Notes: Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Employment and earnings are measured in the fifth quarter after random assignment.

Sources:

^a HPOG and PACE follow-up surveys.

^b National Directory of New Hires.

^c HPOG follow-up survey.

Appendix G: Analytic Details and Expanded Results for Chapter 7

This appendix describes the approach to selecting program characteristics for inclusion in the Chapter 7 model relating program characteristics to impacts. It also presents the model used to estimate the influence of program characteristics on program impacts, provides expanded results for Exhibit 7.3, and results from alternative model specifications.

G.1 Approach to Selecting Program Characteristics to Analyze

In response to the limited number of variables we can include at the division-, program-, and local context-level, we used a combination of theory and an empirical approach to select which program components, implementation features, participant composition measures, and local context measures were included in the model relating those measures to impact magnitude. As described in Harvill et al. (2017), we first identified lists of candidate variables based on our expectations regarding their relationship to the effectiveness of the program. We then prioritized all candidate measures based on their theorized ability to produce policy relevant findings (based on both the literature as documented in Harvill et al. (2015) as well as the expertise of Abt staff); the amount of variation in the candidate measures across divisions and programs; and missing data rates.²⁴ To ensure that the analytic model produced findings that are policy relevant, we automatically included those measures deemed to have the highest priority based on these criteria, which we refer to as “*priority 1*” measures in the model specification used to relate program characteristics to impact magnitude.²⁵

We used a combination of theory and empirically-based approaches to define a number of candidate versions of the model specification as follows:²⁶

- Model “0” includes the five *priority 1* measures but no additional candidate measures.
- We defined six candidate models based on theory, labeled 1t through 6t, where the suffix “t” indicates that these models are theoretically-based. For these theory-based models, the model number (1 through 6) is simply used to differentiate between the different theory-based candidate models, and does not necessarily correspond to the number of candidate measures included in the model.
- We defined six candidate models using an empirical approach that is constrained such that no more than two participant composition measures are selected and no more than one local context measure is selected for inclusion in each candidate model. These models are

²⁴ Harvill et al. (2017), Exhibit 3 lists the candidate measures and their corresponding priority level.

²⁵ *Priority 1* measures include three program components and two implementation features, for a total of five *priority 1* measures. *Priority 1* program components include: (1) extent to which available offerings and program content is based on principles of the career pathways framework; (2) number of services that case managers and counselors deliver; (3) access to and delivery of tuition and other financial services. *Priority 1* implementation features include: (4) percent of management and staff that indicate education is the primary goal of the program; and (5) percent of management and staff that indicate employment is the primary goal of the program. For these two implementation features, management and staff who reported that education and employment are equally important goals of their HPOG program do not fall into either of these two categories.

²⁶ Harvill et al. (2017), Exhibit 4 provides more detail on the candidate model specifications.

Appendix G: Expanded Results for Chapter 7

labeled 1c through 6c, where the suffix “c” indicates that these models are empirically based and constrained. The model number (1 through 6) indicates the number of candidate measures included in the model (in addition to the five *priority 1* measures).

We defined six candidate models using an “unconstrained” empirical approach, labeled 1u through 6u, where the suffix “u” indicates that these models are empirically based and unconstrained (i.e., they are not subject to the constraint that we placed on the constrained models described above). The model number (1 through 6) indicates the number of candidate measures included in the model (in addition to the five *priority 1* measures).²⁷

We then used Akaike Information Criterion (AIC) to determine which of the candidate models to use as the specification of the model used to report findings. Among the candidate models, the one with the smallest AIC is considered the best (although the AIC value itself is not meaningful). AIC rewards goodness of fit, but it also includes a penalty that is an increasing function of the number of estimated parameters, which discourages overfitting. For this exercise, we computed the AIC value for each candidate model using educational progress as the outcome. Exhibit G.1 presents the results of this model-building strategy. The model that achieved the best (lowest) AIC value was chosen as the specification used to report findings presented in the main text of chapter 7. In addition to the five *priority 1* measures automatically included in the model specification, this model includes the following two empirically selected factors (the same two factors were selected when empirical selection was constrained or unconstrained): access to childcare and transportation and the division-level percent of study participants with a GED.

Exhibit G.1: Results of Specification Exercise, Specifications Ordered from Most to Least Preferred

Model	AIC—Min AIC	Candidate Measures Included (in Addition to <i>Priority 1</i> Measures) ^a
2 (2u=2c)	0	<ul style="list-style-type: none"> Access to childcare and transportation The division-level percent of study participants with a GED
3 (3u=3c)	0.5	<ul style="list-style-type: none"> Access to childcare and transportation The division-level percent of study participants with a GED The division-level percent of study participants with a high school degree
1 (1u=1c)	3.5	<ul style="list-style-type: none"> Access to childcare and transportation
4u	3.9	<ul style="list-style-type: none"> Access to childcare and transportation The division-level percent of study participants with a GED The division-level percent of study participants with a high school degree Average weekly hours working at time of enrollment
0	5.1	None
4c	8.4	<ul style="list-style-type: none"> Access to childcare and transportation The division-level percent of study participants with a GED The division-level percent of study participants with a high school degree Unemployment rate
2t	8.4	<ul style="list-style-type: none"> Access to childcare and transportation Staff perception of autonomy

²⁷ To the extent that the empirical approach selects candidate models that have no more than two participant composition measures and no more than one local context measure, a subset of Candidate Models 1c through 6c will be identical to Candidate Models 1u through 6u.

Appendix G: Expanded Results for Chapter 7

Model	AIC—Min AIC	Candidate Measures Included (in Addition to <i>Priority 1</i> Measures) ^a
5u	9.3	<ul style="list-style-type: none"> • Access to childcare and transportation • The division-level percent of study participants with a GED • The division-level percent of study participants with a high school degree • Average weekly hours working at time of enrollment • Unemployment rate
5c	10.1	<ul style="list-style-type: none"> • Access to childcare and transportation • The division-level percent of study participants with a GED • The division-level percent of study participants with a high school degree • Employment supports • Unemployment rate
6u	15.0	<ul style="list-style-type: none"> • Access to childcare and transportation • The division-level percent of study participants with a GED • The division-level percent of study participants with a high school degree • Average weekly hours working at time of enrollment • Unemployment rate • Employment supports
6c	16.0	<ul style="list-style-type: none"> • Access to childcare and transportation • The division-level percent of study participants with a GED • The division-level percent of study participants with a high school degree • Access to peer support • Unemployment rate • Employment supports
3t	19.0	<ul style="list-style-type: none"> • Access to childcare and transportation • Staff perception of autonomy • Unemployment rate
5t	22.9	<ul style="list-style-type: none"> • Access to childcare and transportation • Staff perception of autonomy • Unemployment rate • Employment supports
6t	24.3	<ul style="list-style-type: none"> • Access to childcare and transportation • Staff perception of autonomy • Employment supports • Average caseload
1t	29.2	<ul style="list-style-type: none"> • Access to childcare and transportation • Staff perception of autonomy • Unemployment rate • Average caseload
4t	37.8	<ul style="list-style-type: none"> • Access to childcare and transportation • Staff perception of autonomy • Unemployment rate • Aggregate participant wages • Aggregate prior employment

Notes: Models labelled with the suffix “t” are theoretically-based, with the suffix “c” are empirically-based and constrained, and with the suffix “u” are empirically-based and unconstrained. We computed the AIC value for each candidate model using educational progress as the outcome.

^a *Priority 1* candidate measures are included in all candidate models and include: (1) extent to which available offerings and program content is based on principles of the career pathways framework; (2) number of services that case managers and counselors deliver; (3) access to and delivery of tuition and other financial services; (4) percent of management and staff that indicate education is the primary goal of the program; and (5) percent of management and staff that indicate employment is the primary goal of the program.

G.2 Estimating the Influence of Program Characteristics on Program Impacts

Chapter 7 exploits natural (i.e., non-experimental) cross-division and cross-program variation in program characteristics to produce non-experimental estimates of the relationship between these program characteristics and impact magnitude. To relate program characteristics to impact magnitude, we extend the multi-level model presented in Appendix B, section B.1 by interacting the treatment indicator with measures of program characteristics to produce the following three-level model:

$$Y_{kji} = \alpha_0 + \beta_0 TE_{kji} + \sum_c \delta_c IC_{ckji} + \sum_q \kappa_q LC_{qk} + \sum_m \pi_m P_{mk} TE_{kji} + \sum_q \zeta_q LC_{qk} TE_{kji} + \sum_g \varphi_g I_{gkj} TE_{kji} + \sum_d \tau_d PC_{dkj} TE_{kji} + \{\varepsilon_{kji} + v_k + v_{kj} + u_k TE_{kji} + u_{kj} TE_{kji}\} \quad (\text{eq. G-1})$$

Model terms are defined in Exhibit B.1. In this equation, the local context measures (LC_{qk}), program components (P_{mk}), implementation features (I_{gkj}), and participant composition measures (PC_{dkj}) are all interacted with the treatment indicator. These interaction terms capture the influence of these measures on impact magnitude.

G.3 Expanded Estimates of the Influence of Program Characteristics

Exhibit G.2 presents a more detailed version of Exhibit 7.3, and includes standard errors associated with each coefficient (reported in parentheses).

Exhibit G.2: Influence of Program Characteristics on HPOG's Impact (Model 2: Preferred Model)

Program Characteristics	Educational Progress (%)	Program Completion (%)	Employment (%)	Employment in Healthcare (%)	Job Quality (%)	Earnings (\$)	TANF (%)
Program Components							
Career pathways framework (range is 0-8)	-0.6 (0.5)	-1.1 (0.7)	-0.5 (0.4)	-1.1* (0.6)	-1.0 (0.6)	-34 (37)	-0.2 (0.3)
Case manager services provided (range is 0-7)	1.0 (0.6)	1.3 (0.8)	-0.8* (0.5)	0.4 (0.7)	0.5 (0.7)	-44 (42)	-0.1 (0.3)
Tuition and other financial services (range is 0-2)	5.1* (3.0)	4.0 (4.0)	-2.0 (2.5)	4.1 (3.6)	1.4 (3.5)	144 (210)	-1.1 (1.6)
Childcare and transportation (range is 0-8)	2.4*** (0.8)	1.6 (1.2)	-1.0 (0.7)	1.0 (1.0)	0.0 (1.0)	-57 (60)	-0.1 (0.4)
Implementation Features							
Education is the primary goal of the program (%)	-6.9 (4.2)	4.3 (5.9)	3.0 (3.6)	5.0 (5.3)	7.3 (5.1)	328 (296)	-3.5 (2.4)
Employment is the primary goal of the program (%)	-5.8 (4.7)	4.5 (6.4)	5.4 (3.9)	4.2 (5.7)	5.8 (5.4)	426 (328)	-3.0 (2.5)
Participant Composition Measures							
GED (%)	17.4** (7.0)	15.9 (9.8)	1.5 (5.7)	19.6** (8.4)	-5.6 (8.0)	-158 (501)	9.2** (3.6)
Sample Size							
Individuals (N)	10,318	10,318	13,252	10,318	7,959	13,237	7,928
Divisions (N)	87	87	87	87	81	87	81
Programs (N)	42	42	42	42	36	42	36

Notes: All program characteristics are grand mean centered. For program characteristics measured as a percentage, the reported effect corresponds to a 10 percentage point change in the program characteristic. . Employment and earnings are measured in the fifth quarter after random assignment. Standard errors appear in parentheses.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources: HPOG follow-up survey; PACE follow-up survey; National Directory of New Hires.

G.4 Alternative Analyses of the Influence of Program Characteristics

This section compares findings from the model chosen by our empirical selection approach to two alternative models. The purpose of this comparison is to demonstrate the robustness of the results to alternative model specifications. The preferred model, Model 2, achieved the best (lowest) AIC value and is the model specification used to report findings presented in the main text of Chapter 7. The two alternative models, Models 3 and 1 as defined in Exhibit G.1, achieved the second and third best (lowest) AIC values, and the estimates from these models are presented below for comparison.

The alternative models are very similar to the preferred model, differing only in the participant composition measures included. Model 3 adds the division-level percent of study participants with a high school degree to the preferred model. Model 1 removes the division-level percent of study participants with a GED from the preferred model. In comparing the Model 2 (the preferred model, reported in the text) estimates to the estimates from Models 3 and 1 we observe the following:

- Across all model specifications, HPOG programs that offer greater access to childcare and transportation show relatively larger impacts on educational progress.
- Model 2 indicates that HPOG programs that offer greater access to tuition and other financial services show relatively larger impacts on educational progress. The estimate associated with this finding is statistically significant in Model 1 and is not statistically significant in Model 3. The magnitudes of the estimates and the corresponding standard errors are similar across all model specifications.
- Model 2 indicates that HPOG programs where case managers provide a larger number of services experienced lower impacts on employment. The estimate associated with this finding is statistically significant in Model 1 and is not statistically significant in Model 3. The magnitudes of the estimates and the corresponding standard errors are similar across all model specifications.
- Model 2 indicates that greater alignment with the Career Pathways framework is associated with a lower impact on employment in healthcare professions. The estimate associated with this finding is statistically significant in Model 3 and is not statistically significant in Model 1. The magnitudes of the estimates and the corresponding standard errors are similar across all model specifications.
- Model 2 did not provide evidence that the program components and implementation features of interest influenced HPOG's impact on program completion, job quality, earnings, or public assistance receipt. The findings from Models 1 and 3 tell a similar story, with two out of 48 coefficients associated with the program components and implementation features being statistically different from zero (at the 10 percent level) across these two models, no more than we would expect due to random chance.

Taken as a whole, these analyses indicate that the findings reported in Chapter 7 are generally robust to alternative model specifications. In other words, the variables that arose as important in the model with the lowest AIC value remain important (or the magnitude of the coefficient remains similar) in the two models with the next lowest AIC values. The similar results give us confidence in the findings regarding which characteristics matter to HPOG's impacts.

Exhibit G.3: Influence of Program Characteristics on HPOG's Impact (Model 3)

Program Characteristics	Educational Progress (%)	Program Completion (%)	Employment (%)	Employment in Healthcare (%)	Job Quality (%)	Earnings (\$)	TANF (%)
Program Components							
Career pathways framework (range is 0-8)	-0.5 (0.5)	-1.0 (0.7)	-0.5 (0.4)	-1.1* (0.6)	-1.0 (0.6)	-32 (37)	-0.2 (0.3)
Case manager services provided (range is 0-7)	0.9 (0.6)	1.2 (0.8)	-0.8 (0.5)	0.4 (0.7)	0.3 (0.7)	-45 (42)	-0.1 (0.3)
Tuition and other financial services (range is 0-2)	4.4 (3.0)	3.6 (4.1)	-1.5 (2.5)	4.0 (3.7)	0.7 (3.6)	126 (214)	-1.5 (1.6)
Childcare and transportation (range is 0-8)	2.6*** (0.8)	1.7 (1.2)	-1.1 (0.7)	1.0 (1.0)	0.1 (1.0)	-53 (60)	0.0 (0.4)
Implementation Features							
Education is the primary goal of the program (%)	-8.0* (4.3)	3.6 (6.0)	3.5 (3.7)	4.9 (5.4)	6.2 (5.2)	301 (301)	-4.0 (2.5)
Employment is the primary goal of the program (%)	-6.3 (4.6)	4.0 (6.4)	5.6 (3.9)	4.1 (5.8)	5.0 (5.4)	410 (330)	-3.4 (2.5)
Participant Composition Measures							
GED (%)	16.3** (6.9)	15.0 (9.8)	2.3 (5.8)	19.4** (8.5)	-5.6 (8.0)	-191 (506)	9.0** (3.5)
High school degree (%)	-9.9 (6.8)	-6.3 (9.3)	6.2 (5.8)	-0.8 (8.5)	-8.8 (8.9)	-253 (479)	-5.0 (4.4)
Sample Size							
Individuals (N)	10,318	10,318	13,252	10,318	7,959	13,237	7,928
Divisions (N)	87	87	87	87	81	87	81
Programs (N)	42	42	42	42	36	42	36

Notes: All program characteristics are grand mean centered. For program characteristics measured as a percentage, the reported effect corresponds to a 10 percentage point change in the program characteristic. Employment and earnings are measured in the fifth quarter after random assignment. Standard errors appear in parentheses.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources: HPOG follow-up survey; PACE follow-up survey; National Directory of New Hires.

Exhibit G.4: Influence of Program Characteristics on HPOG's Impact (Model 1)

Program Characteristics	Educational Progress (%)	Program Completion (%)	Employment (%)	Employment in Healthcare (%)	Job Quality (%)	Earnings (\$)	TANF (%)
Program Components							
Career pathways framework (range is 0-8)	-0.5 (0.6)	-1.0 (0.7)	-0.5 (0.4)	-0.9 (0.6)	-1.1* (0.6)	-36 (36)	-0.2 (0.3)
Case manager services provided (range is 0-7)	0.9 (0.6)	1.2 (0.8)	-0.8* (0.5)	0.3 (0.7)	0.5 (0.7)	-43 (41)	-0.1 (0.3)
Tuition and other financial services (range is 0-2)	7.0** (3.1)	6.0 (3.9)	-1.8 (2.4)	6.0* (3.5)	0.8 (3.3)	124 (200)	0.0 (1.6)
Childcare and transportation (range is 0-8)	2.8*** (0.9)	1.9* (1.2)	-1.0 (0.7)	1.3 (1.0)	-0.2 (1.0)	-60 (58)	0.2 (0.5)
Implementation Features							
Education is the primary goal of the program (%)	-5.4 (4.3)	5.2 (6.1)	3.2 (3.6)	6.1 (5.4)	6.8 (5.0)	319 (292)	-2.4 (2.5)
Employment is the primary goal of the program (%)	-3.0 (4.7)	6.6 (6.3)	5.8 (3.7)	7.5 (5.6)	4.7 (5.1)	405 (312)	-1.2 (2.4)
Sample Size							
Individuals (N)	10,319	10,319	13,253	10,319	7,960	13,238	7,929
Divisions (N)	87	87	87	87	81	87	81
Programs (N)	42	42	42	42	36	42	36

Notes: All program characteristics are grand mean centered. For program characteristics measured as a percentage, the reported effect corresponds to a 10 percentage point change in the program characteristic. Employment and earnings are measured in the fifth quarter after random assignment. Standard errors appear in parentheses.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources: HPOG follow-up survey; PACE follow-up survey; National Directory of New Hires.

Appendix H: HPOG Impacts by Program Experiences and Milestones

The HPOG Program is intended to help individuals stay connected to the program and achieve desired program milestones, which then in turn are hypothesized to lead to more favorable labor market outcomes. The Interim Report Chapters 3 through 6 presented HPOG's impact on various elements of the logic model separately, reporting impacts on training and service receipt and on a variety of outcomes. This appendix links elements of the logic model by reporting variation in HPOG impacts for selected subgroups defined by individuals' predicted programmatic experiences and milestone achievement.

Specifically, this appendix analyzes five program experiences and two program milestones, all of which were selected for this analysis in advance as described in the study's Analysis Plan (Harvill, Moulton & Peck, 2015). Exhibit H.1 lists the program experiences and program milestones, as well as the outcomes that are the focus of this appendix. Three of the program experiences—emergency assistance, non-cash incentives and peer support—are described in detail in Chapter 6 of the Interim Report and Appendix F of this document. A fourth program experience, personal counseling, aims to assist with behavioral issues and other personal challenges, thereby promoting program retention and completion. The fifth program experience is defined as individual receipt of tutoring services. Tutoring is intended to promote students' success and decrease barriers to educational progress. With the exception of non-cash incentives, these program experiences were all designed to help HPOG participants overcome barriers to staying enrolled and completing training. As such, the most proximal outcome of interest is program completion.

Summary of Key Findings: Impacts by Program Experiences and Milestones

- HPOG's impact on educational progress is generally favorable for the subgroups predicted to have the program experience of interest. However, these impacts are not statistically different from corresponding impacts on those predicted not to receive these respective services.
- HPOG had a positive impact on employment and earnings for those least likely to obtain a license or credential as well as those least likely to complete a degree. However, we do not find any cross-subgroup differences in impacts on employment or earnings for subgroups defined by achievement of program milestones.
- This analysis involved noisy prediction, which may account for the lack of significant differences in impacts. Alternatively, the mediators analyzed may not have been key channels through which HPOG's impact operated.

Exhibit H.1: Program Experiences and Milestones and Outcomes Analyzed

Program Experiences and Milestones	Logic Model Component	Outcomes Examined
Received emergency assistance	Program experience	Educational Progress
Received a non-cash incentive	Program experience	Educational Progress
Participated in peer support	Program experience	Educational Progress
Used personal counseling services	Program experience	Educational Progress
Used tutoring services	Program experience	Educational Progress
Obtained a professional, state or industry license or credential	Program milestone	Employment and Earnings
Completed a degree (AA, BA or higher)	Program milestone	Employment and Earnings

In turn, the HPOG logic model hypothesizes that completing training leads to increased earnings and employment. The program milestones that we examine in this appendix

Appendix H: HPOG Impacts by Program Experiences and Milestones

operationalize “program completion” in two ways: obtaining a professional, state or industry license or credential, and completing a degree.

This appendix addresses the research question: How do impacts vary for individuals according to their program experiences or achievement of certain program milestones? This kind of analysis requires more complex techniques because it focuses on individuals’ experiences after random assignment; and, as such, those experiences are influenced by (or “endogenous to”) the treatment. For instance, to understand the impact of HPOG for individuals who received emergency assistance, we would want to compare outcomes for treatment group members who received emergency assistance to their control group counterparts who *would have* received emergency assistance had they been offered HPOG services.

The empirical challenge, therefore, is to identify individuals in the control group who would have had a given program experience had they been assigned to treatment. To make this comparison, we undertake an Analysis of Symmetrically-Predicted Endogenous Subgroups (ASPES). The ASPES method uses baseline observable characteristics to create subgroups with a high probability of having a given program experience and a low probability of having that experience. ASPES maintains the experimental design so we can (1) estimate the impact of HPOG on the subgroup of study participants “most likely to have a given program experience or achieve a certain program milestone”; (2) estimate the impact for study participants “least likely to have a given program experience or achieve a certain program milestone”; and then (3) compare these impacts.

This appendix proceeds as follows: Section H.1 describes the ASPES method. Section H.2 reports on the study participant profiles that are associated with having certain program experiences and achieving certain milestones. It also discusses the extent to which the subgroups that we estimate membership into are reflective of those who actually have the program experience or achieve the milestones of interest. Together, this information helps identify *for whom* the impacts we estimate are relevant. Section H.3 reports the impacts on mediators, and Section H.4 reports the subsequent impacts on outcomes of interest. Finally, Section H.5 discusses and concludes.

H.1 Methodological Approach

In its investigation of impact variation, the focus of the ASPES method is on the treatment group experience. To make this more concrete, consider the mediator receiving tutoring. When we refer to the impact for individuals who would receive tutoring, we conceptualize an impact that compares treatment and control outcomes for individuals who would receive tutoring if offered treatment. There are four possible combinations of treatment status and the mediator: the individual could be assigned to treatment and receive tutoring, assigned to treatment and not receive tutoring, assigned to control and receive tutoring, or assigned to control and not receive tutoring. Focusing on the individuals who would receive tutoring if offered treatment, the impact of interest compares the average outcome that would be realized if they were assigned to treatment to a combination of the two possible outcomes for the control group.

In the language of the potential outcomes framework, the impact for individuals who would receive tutoring if offered treatment combines impacts for people who would also receive tutoring under the control condition (always-takers) and people who would not receive tutoring

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under the control condition (compliers).²⁸ Similarly, the impact for individuals who would not receive tutoring if offered treatment combines impacts for those who would not receive tutoring in the control condition (never-takers) and those who would (defiers).²⁹

Because they are defined by a choice made after random assignment, we refer to those individuals who would and would not receive tutoring if offered treatment as “endogenous subgroups”. We cannot directly estimate the impact on the endogenous group as we do not know which control group members would use tutoring if offered treatment. The ASPES method is designed to address this empirical challenge by constructing subgroups using exogenous, baseline traits.

There are alternative channels that might yield differential impacts on endogenous subgroups. A first possibility is that the intervention increases the prevalence of the experience or milestone and the experience or milestone is beneficial to the program participant. A second possibility is that the quality of experiences differs between the treatment and control groups such that—even with no difference in prevalence of the experience—there still might be implications for subsequent impacts. An increase in the quality of the experience when combined with treatment would correspond to an increase in impacts on always-takers.

Applying the ASPES Method

We use ASPES to construct subgroups with high (and low) likelihoods of having a given program experience or achieving a given program milestone and then estimate HPOG’s impact on these groups. This section provides an overview of the ASPES method, and we refer readers to published documents to learn the details of this methodological approach.³⁰

The first stage of the ASPES method constructs subgroups defined by probability of having a post-randomization program experience or milestone. We predict treatment and control group members to subgroups based on their baseline characteristics. More specifically, the probability of having a given program experience or achieving a given program milestone is modeled as a function of the baseline characteristics of treatment group members. In this analysis, we use the same three-level model used to estimate HPOG’s overall impact described in Appendix B, section B.1, though we exclude the treatment group indicator.³¹ Predicted values from the model represent the estimated probability that a given study participant is a member of a specified subgroup, given their profile of baseline characteristics. Based on this continuous score, we

²⁸ Angrist, Imbens & Rubin (1996) introduced the terms always-taker, complier, never-taker and defier.

²⁹ Some individuals in this group do not participate in any components of HPOG. We do not explicitly model this variation in experiences because more than 96 percent of individuals who were offered access to the HPOG program experienced some exposure to the HPOG program.

³⁰ Peck (2003) first described the approach, and Peck (2013) revisits it in the context of the subsequent decade of evaluation research. Harvill, Peck, and Bell (2013) consider how to carry out subgroup identification without introducing bias. Bell and Peck (2013) further consider the method’s assumptions. Moulton, Peck, and Bell (2014) details the steps and analytic decisions required by the method and provides sample SAS and Stata code for executing ASPES. Applications useful for understanding how ASPES works in practice include Peck and Bell (2014), which considers the case of Head Start quality, and Moulton, Peck, and Dillman (2014), which considers the case of neighborhood quality in the Moving to Opportunity Experiment.

³¹ The specific baseline characteristics included in the prediction model are the same set reported in Exhibit H.2. In order to ensure the comparability of treatment and control group members within a subgroup without loss of sample, we used a cross-validation approach to predicting subgroup membership (Harvill et al. 2013). A cross-validation approach ensures that the subgroups are symmetrically-identified in the treatment and control groups. This ensures that neither subsample is any better identified than the other, thereby retaining the integrity of the experimental design. In this application, we randomly partitioned the sample into 10 cross-validation groups.

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divide the sample into two subgroups: those predicted to have the program experience of interest and those predicted to not have the program experience of interest.

To provide a concrete example, consider the case of participation in tutoring services. When *used tutoring services* is the outcome of interest in the prediction model, the continuous score represents the probability that a given study participant would use tutoring services if assigned to treatment. Based on this continuous score, we divided the sample into two subgroups: a subgroup of treatment and control group members predicted to use tutoring services and a subgroup of treatment and control group members predicted not to use tutoring services. The cutoff value used to divide the sample into subgroups based on this continuous score was set so that the proportion of study participants assigned to the *predicted to use tutoring services* subgroup was equal to the proportion of treatment group members who actually used tutoring services: 17 percent. The cutoff value was set so that the 17 percent of the sample with the highest predicted probabilities of using tutoring services was assigned to the *predicted to use tutoring services* subgroup. The remaining 83 percent of the sample (those with predicted probabilities below the cutoff value) was assigned to the subgroup of those *predicted to not use tutoring services*.

The second stage of the ASPES method is to estimate impacts on the subgroups defined by likelihood of having program experience (more likely and less likely).³² To estimate impacts within these subgroups, we used the same approach used to estimate impacts within subgroups defined by baseline characteristics described in Appendix B, section B.2. Because the subgroups defined by likelihood of having a particular program experience were constructed using only baseline characteristics, the integrity of the original randomized experiment remains intact. Therefore, the impacts for each of the subgroups are experimental impacts, unbiased by selection or other influences.

Because prediction to these subgroups is imperfect, the subgroups represent a blend of those who actually would and actually would not have that program experience or achieve that program milestone if assigned to treatment. The stronger the prediction to these groups, the stronger the evidence that differences in impacts between the predicted subgroups can be attributed to the experience or milestone.

For each program experience of interest, the sample is limited to the set of HPOG programs that offered the program experience based on responses to the grantee survey. For instance, the sample used to define subgroups by receipt of personal counseling services is restricted to HPOG programs that offered this service according to the grantee survey. For the three services that were offered as randomized enhancements—emergency assistance, non-cash incentives, and peer support—the sample is again restricted to the study participants from HPOG programs that offered the respective service according to grantee survey. However, for HPOG programs that randomized access to these services via an enhanced treatment group, study participants randomly assigned to the standard treatment group were excluded from the analysis (because the grantee indicated that these services were not offered to standard treatment group members).

³² In a full application of ASPES, as described in Peck (2003), these estimated impacts on the subgroups predicted to have a program experience would be converted to impacts on individuals who actually had those experiences, which requires “conversion assumptions.” We do not include the conversion in this application.

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Study participants with missing outcome data were dropped from this analysis. For study participants with missing baseline covariate data, we used the “dummy variable adjustment” approach. Following Puma et al. (2009), this method sets missing covariate values to a constant and includes a set of “missing data flags” in the impact model. Due to computational constraints, the approach to missing data differs for this analysis from the multiple imputation procedure in the main study results.

In the next section, we investigate whether prediction is strong enough that we would expect to see a difference in impacts between observed subgroups (i.e., the subgroups constructed from baseline characteristics using the prediction model coefficients) if such a difference existed between the unobserved, endogenous subgroups. Even with only modest prediction success, the impacts on the observed subgroups groups are relevant to policy for two reasons: (1) they can help identify the profile of individuals who have these experiences or reach these milestones, which can be helpful for program targeting; and (2) they can identify whether these mediators—as defined by their association with the “profile” of individuals in the subgroup—are important drivers of program impacts.

In the subsequent section, we present impacts on the mediator (i.e., the program experience or milestone) separately for the observed subgroups. These treatment-control differences in take-up inform our interpretation of impacts on educational progress, employment, and earnings estimated on the same subgroups. Smaller impacts on take-up of the mediator will produce smaller impacts on the outcomes.

H.2 Study Participant Profiles by Program Experiences and Milestones

Because the first stage prediction model does not perfectly predict program experiences or milestones, not everyone who is predicted to have a given program experience or milestone will actually realize that program experience or milestone. As such, the observed impacts in the predicted subgroup reflect the impact HPOG has on those with a profile of characteristics that makes them most likely have a given program experience or milestone or not. This section describes the participants in each subgroup, with a goal of addressing the following two questions:

- To what extent do subgroup members actually have the experience or achieve the milestone?
- What are the characteristics of the subgroups?

Program Experiences and Milestone Completion within Subgroups

Exhibits H.2 and H.3 summarize the prevalence of the mediator in the treatment group and success of the prediction model for program experiences and milestones, respectively. In the treatment group, participation in the program experiences we analyze in this appendix was relatively low, ranging from 12 to 20 percent of the sample (Exhibit H.2). Although almost half of the treatment group (47 percent) obtained a license or credential, only a fifth (20 percent) completed a degree (Exhibit H.3).

Placement into these subgroups captures the prediction of an individual’s participation in the experience if offered treatment. These predictions are made for both the treatment and control groups based on their baseline characteristics. We cannot assess whether prediction is correct for control group members because we do not observe what would have happened if they were

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offered treatment, and so we judge the quality of our prediction using the treatment group. We report the correct prediction rate, which is stated as the proportion of the treatment group who are placed in the correct subgroup. In the subgroup predicted to have the experience, the correct prediction rate is the proportion of the treatment group that had the experience. In the subgroup predicted not to have the experience, the correct prediction rate is the proportion of the treatment group that does not have the experience. Across the various mediators in Exhibit H.2, the correct prediction rate ranges from 17 to 31 percent for the subgroups predicted to have the experience and from 83 to 89 percent for the subgroups predicted not to have the experience. The correct prediction rates for the subgroup predicted to obtain a license or credential and the subgroup predicted to obtain a college degree, respectively, is 55 and 56 percent; the corresponding correct prediction rates for the subgroups predicted not to achieve these milestones are 59 and 90 percent (Exhibit H.3).

In Exhibits H.2 and H.3, we reference several rates, as follows:

- p refers to the proportion of the treatment group that had the program experience or achieved the program milestone
- α refers to the proportion of the subgroup predicted to have the program experience or achieve the program milestone that actually did
- β refers to the proportion of the subgroup predicted not to have the program experience or not to achieve the program milestone that actually did not.³³

As noted in the exhibits, we combine these values in certain ways to show how “good” the prediction was, in part by comparing the results to random selection. Random selection is a theoretical benchmark based on randomly dividing the sample into the subgroups with the proportions given by the percent of the treatment group that had the program experience. In the subgroup predicted to have the program experience, the expected success of random selection is simply the proportion of the treatment group that actually had the experience, p , because subgroup assignment is independent of actual program experience. Similarly, in the subgroup predicted not to have the program experience, the expected success of random prediction is the proportion of the treatment that did not have the experience ($1 - p$).

We compare the correct prediction rate from our model to the expected success of random prediction in two ways. First, we report the difference between the correct prediction rate and the expected success of random selection, which captures the absolute improvement of the

³³ One can show that α and β are mechanically related using the fact that p is both the proportion of the treatment group that had the experience and, by design, the proportion of the treatment group predicted to have the experience (this is imposed in the ASPES method). Therefore, focusing on the proportion of the treatment group that had the experience, p represents a blend of individuals correctly predicted to have the program experience and incorrectly predicted to not have the program experience. Focusing on the proportion of the treatment group predicted to have the experience, p also represents a blend of individuals correctly predicted to have the program experience and incorrectly predicted to have the program experience. These statements imply that individuals incorrectly predicted to have the program experience must be equivalent to individuals incorrectly predicted to not have the program experience. Algebraically, one can use this to demonstrate that

$$\alpha = 1 - \frac{(1 - \beta)(1 - p)}{p}$$

Despite this relationship, we refer to α and β in this section for ease of exposition and understanding.

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model over random selection. Then, we report the improvement relative to the maximum possible improvement over random selection.

The “Detecting differences in impacts” panel of Exhibits H.2 and H.3 provides information about how correct prediction rates affect our ability to detect an underlying difference in impacts. This calculation relates observed subgroup impacts to underlying impacts by noting that observed subgroups combine the impacts for people who were correctly predicted and those who were not. Suppose a is the impact for people who would have the experience or obtain the milestone if offered treatment and b is the impact for people who would not.³⁴ If baseline characteristics only influence impacts through the mediator, then the impact for the subgroup predicted to have the experience would be given by,

$$\Delta_1 = \alpha a + (1 - \alpha) b,$$

because α is the proportion of the subgroup that actually had the experience and $(1 - \alpha)$ is the proportion of the subgroup that did not. Similarly, the impact for the subgroup predicted to not have the experience is given by:

$$\Delta_0 = (1 - \beta) a + \beta b.$$

The difference in subgroup impacts is given by:

$$\Delta_1 - \Delta_0 = (\alpha a + (1 - \alpha) b) - ((1 - \beta) a + \beta b).$$

We can rearrange the difference in subgroup impacts as follows:

$$\Delta_1 - \Delta_0 = (\alpha + \beta - 1)(a - b).$$

The difference in observed subgroup impacts is therefore $(\alpha + \beta - 1)$ times the difference in actual impacts. The smaller this number, the larger a difference in impacts must be to be detectable. This term is reported in Exhibits H.2 and H.3 as the *proportion of actual difference*.

Exhibit H.2: Success Predicting Program Experiences

Mediator	Details	Received Emergency Assistance	Received a Non-Cash Incentive	Participated in Peer Support	Used Personal Counseling Services	Used Tutoring Services
Percent of Treatment Group that had Program Experience	p	19.4	17.6	12.0	12.9	17.0
Subgroup Predicted to Have Program Experience						
Correctly Predicted (%)	α	31.3	25.1	17.2	19.7	25.6
Expected Success of Random Selection (%)	p	19.4	17.6	12.0	12.9	17.0
Performance Relative to Random (pp)	$\alpha - p$	11.9	7.5	5.2	6.8	8.6
Share of Gap Closed (%)	$(\alpha - p)/(1 - p)$	14.8	9.1	5.9	7.8	10.4

³⁴ The impact a compares treatment and control outcomes for individuals who would have the program experience or achieve the milestone if offered treatment. Some of these individuals would also have the program experience or achieve the milestone if they were assigned to the control group, while others would not. Control group take-up of the mediator will affect the magnitude of the impact a . If impacts are larger for compliers than for always takers, lower rates of take-up of the mediator will reduce the impact a . This impact is fundamentally unobservable because we cannot identify the control group members who would take-up the mediator if offered treatment.

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Mediator	Details	Received Emergency Assistance	Received a Non-Cash Incentive	Participated in Peer Support	Used Personal Counseling Services	Used Tutoring Services
Subgroup Predicted not to Have Program Experience						
Correctly Predicted (%)	β	83.3	83.9	88.7	88.2	85.2
Expected Success of Random Selection (%)	$1 - p$	80.6	82.4	88.0	87.1	83.0
Performance Relative to Random (pp)	$\beta - 1 + p$	2.7	1.5	0.7	1.1	2.2
Share of Gap Closed (%)	$(\beta - 1 + p)/p$	13.9	8.5	5.8	8.5	12.9
Detecting Differences in Impact						
Proportion of Actual Difference	$\alpha + \beta - 1$	14.6	9.0	5.9	7.9	10.8
Sample Sizes						
Individuals (N)		4,349	1,861	4,559	7,825	3,388
Divisions (N)		47	30	44	83	36
Programs (N)		18	9	22	33	20

Notes: pp refers to percentage point

Sample Restrictions: For the three services that were offered as randomized enhancements—emergency assistance, non-cash incentives, and peer support—the sample is restricted to the study participants from HPOG programs that offered the service. For HPOG programs tested these services as enhancements, study participants randomly assigned to the standard treatment group were excluded from the analysis. The sample used to define subgroups by receipt of personal counseling or tutoring services is restricted to programs that offered this service according to the grantee survey.

Sources: HPOG follow-up survey; PACE follow-up survey.

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Exhibit H.3: Success Predicting Program Milestones

	Details	Obtained a License or Credential	Completed a Degree
Mean of Mediator			
Percent of Treatment Group that Achieved Milestone	p	47.2	19.8
Subgroup Predicted to Achieve Milestone			
Correctly Predicted (%)	α	55.0	55.9
Expected Success of Random Selection (%)	p	47.2	19.8
Performance Relative to Random (pp)	$\alpha - p$	7.8	36.1
Share of Gap Closed (%)	$(\alpha - p)/(1 - p)$	14.8	45.0
Subgroup Predicted not to Achieve Milestone			
Correctly Predicted (%)	β	58.5	89.5
Expected Success of Random Selection (%)	$1 - p$	52.8	80.2
Performance Relative to Random (pp)	$\beta - 1 + p$	5.7	9.3
Share of Gap Closed (%)	$(\beta - 1 + p)/p$	12.1	47.0
Detecting Differences in Impact			
Proportion of Actual Difference	$\alpha + \beta - 1$	13.5	45.4
Sample Sizes			
Individuals (N)		10,244	10,244
Divisions (N)		92	92
Programs (N)		42	42

Sources: HPOG follow-up survey; PACE follow-up survey.

Notes: pp refers to percentage point

Prediction Success

In all cases our prediction outperforms random selection. In the subgroup predicted to receive emergency assistance, the correct placement rate is 31.3 percent, which is 11.9 percentage points higher than we would expect given random selection. In the subgroup predicted to not receive emergency assistance, the correct placement rate is 83.3 percent, which is 2.7 percentage points higher than random selection. Although the absolute improvement over random selection is smaller for the subgroup predicted to not receive emergency assistance than for the subgroup predicted to receive emergency assistance, the improvement relative to the maximum possible improvement is similar for both groups. The share of the gap closed is 14.8 percent for the subgroup predicted to receive emergency assistance and is 13.9 for the subgroup predicted to not receive emergency assistance.

The pattern we observe for emergency assistance holds for the other mediators. While model-based prediction outperforms random selection for both subgroups, the absolute improvement over random selection is always larger for the subgroup predicted to have the experience or achieve the milestone. However, the share of the gap closed, which captures the improvement over random selection relative to the maximum possible improvement, is similar for both subgroups. This similarity is reasonable, given that the two subgroups are formed from a single model.

Implications of Prediction Success

When the correct prediction rate is high, impacts on the subgroups are more likely to reflect the impacts on individuals who would have the experience or achieved the milestone if offered treatment. However, for the subgroups predicted to participate in a particular program experience, the correct prediction rate is 31 percent or lower. This suggests that the impacts for the subgroup predicted to have the program experience must be more cautiously interpreted as reflecting the impact of the combination of an offer of access to HPOG and having the given program experience.

However, the difference in subgroup impacts could still reflect the difference in the impacts of interest. Exhibit H.3 shows a proportion of actual difference of 13.5 percentage points for the mediator measuring completion of a license or credential. That term means that, if the impact of HPOG on quarterly earnings is \$200 larger for individuals who actually obtain a license or credential than for those who do not, the impact on the subgroup predicted to obtain a license or credential would be \$27 larger (13.5 percent of \$200) than the impact on the subgroup predicted to not obtain a license or credential. For the mediator measuring completion of a college degree, the proportion of actual difference is 45.4 percent, which means that we would see a \$91 difference in observed impacts on quarterly earnings if the underlying difference in actual impacts was \$200. For reference, the minimum detectable effect for HPOG's impact on earnings in the fifth quarter after random assignment is \$169, and differences in impacts require much larger sample sizes to detect than the impacts themselves. Therefore, even a \$91 difference in observed impacts is very unlikely to be detected, even though the underlying difference in actual impacts is very large (\$200).

The story is similar for the program experience mediators. In Exhibit H.2, the proportion of actual difference ranges from 5.8 (participation in peer support) to 14.6 (received emergency assistance). We hypothesize that these program experiences affect HPOG's impact on educational progress. If HPOG increases educational progress for individuals who received emergency assistance by 10 percentage points more than it does for individuals who do not receive emergency assistance, the differences in impacts for observed subgroups would be 1.5 percentage points. For reference, the minimum detectable effect for the overall impact of HPOG on educational progress is 2.2 percentage points when estimated on the full sample of programs. Differences in impacts require much larger sample sizes to detect. Therefore, the minimum detectable difference in impacts would be larger than 2.2 percentage points for an analysis using the full sample. This analysis restricts the sample to programs that offer emergency assistance so differences in impacts will be even more difficult to detect. Therefore, a difference in impacts of 1.5 percentage points is unlikely to be detected, even though the underlying difference in impacts of 10 percentage points would be a very large difference in impacts. The other program experiences, which have smaller values for the proportion of actual difference, are less likely to have detectable differences in subgroup impacts than emergency assistance.

Participant Characteristics

To better understand the profile of study participants who have these program experiences or achieve these milestones, we present the baseline characteristics of study participants who are identified to be in each subgroup (Exhibits H.4 through H.10). For all mediators of interest, we generally find that study participants who are predicted to have the program experience or

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achieve the selected program milestone differ from those predicted not to have that same experience across a wide range of baseline characteristics, including those related to demographics, educational attainment, earnings and public assistance receipt, employment, and life challenges. This is perhaps not surprising because we used these baseline characteristics to sort study participants into the groups of those with and without the mediator experience.

In what follows, we highlight select cross-subgroup differences in baseline characteristics:³⁵

- Study participants were more likely to receive **emergency assistance** if they were female, separated or divorced, or had more barriers that interfered with school, work, job search, or family responsibilities (Exhibit H.4). We might expect that study participants with these characteristics have greater need for emergency assistance. Conversely, those born outside the U.S. and those with limited English proficiency were less likely to receive emergency assistance, perhaps because of difficulties completing the steps necessary to secure assistance.
- Study participants were likely not to receive **non-cash incentives** if they were born outside the U.S. or were receiving welfare at randomization (Exhibit H.5). Individuals who were separated or divorced were more likely to receive non-cash incentives.
- Study participants were more likely to participate in **peer support** if prior to randomization they had attended a course in how to succeed in school or a course in how to succeed at work (Exhibit H.6). Individuals who sought soft skills training prior to randomization may have also been more likely to seek out supports after randomization.
- Study participants were more likely to use **personal counseling services** if they were separated or divorced, or had more barriers that interfered with school, work, job search, or family responsibilities (Exhibit H.7). Study participants were less likely to use personal counseling services if they were married. Together, this suggests that those predicted to receive personal counseling face more personal challenges than those predicted to not receive personal counseling services. As such, those with this profile may be more likely to benefit from counseling services that can assist with navigating personal challenges.
- Study participants were more likely to use **tutoring services** if prior to randomization they had attended a course in English as a second language, adult basic education classes, or courses in how to succeed in work or school (Exhibit H.8). This indicates that individuals more likely to seek help with various forms of skill-building before randomization were also likely to use tutoring services after randomization.
- Study participants were more likely to **obtain a license or credential** if they had obtained an occupational skills license or attended vocational, technical, or trade school classes before randomization (Exhibit H.9). Study participants with these characteristics may be better positioned or more motivated to obtain a license or credential.
- Study participants were more likely to **complete a degree** (AA, BA or higher) if they had obtained a postsecondary degree prior to randomization (Exhibit H.10). These individuals may have more of the skills required and be more motivated to complete a degree.³⁶

³⁵ We highlight differences that are statistically significant and large relative to the scale of the measure.

³⁶ It is also possible that this relationship reflects measurement error, where a degree completed prior to random assignment is mistaken for a degree completed after random assignment.

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Exhibit H.4: Participant Characteristics by Predicted Receipt of Emergency Assistance

	Subgroup Predicted to Receive Emergency Assistance	Subgroup Predicted to Not Receive Emergency Assistance	Difference in Predicted Subgroup Means
Demographic Measures			
Male (%)	4.5	12.1	-7.6**
<i>Marital status</i>			
Never married (%)	36.8	70.8	-34.0**
Married (%)	19.9	13.9	6.1**
Separated or divorced (%)	40.9	14.8	26.1**
Parent to one or more dependent children (%)	90.1	55.9	34.2**
<i>Race/ethnicity</i>			
Non-Hispanic white (%)	49.2	43.4	5.7**
Hispanic/Latino (%)	8.0	16.5	-8.5**
Black/African-American (%)	35.9	35.0	0.9
Age (years)	35.2	31.6	3.5**
Born outside the U.S. (%)	4.6	14.2	-9.7**
Educational Background			
Attained postsecondary degree (%)	30.0	19.4	10.6**
Occupational skills license, or certification (%)	30.7	14.6	16.1**
Completed license, certification, or degree (%)	47.5	31.0	16.5**
Attended adult basic education classes (%)	16.8	21.9	-5.0**
Attended English as a second language classes (%)	3.8	6.4	-2.6**
Course attendance in how to succeed in school (%)	25.6	16.7	8.9**
Attended vocational, technical, or trade school classes (%)	48.5	27.7	20.8**
Course attendance in how to succeed at work (%)	13.5	18.7	-5.2**
Earnings and Public Assistance Receipt			
Average quarterly wage received in four quarters prior to the quarter of random assignment (\$)	2,416	2,374	42
Receipt of Welfare (%)	18.5	9.4	9.1**
Receipt of WIC/SNAP (%)	84.8	50.3	34.6**
Employment			
Proportion of quarter employed during the four quarters prior to the quarter of random assignment (ranges from 0-4)	2.4	2.5	-0.04
Ever employed in a healthcare job (%)	61.5	42.2	19.3**
Expect to be working for pay in the next few months (%)	52.4	81.9	-29.6**
Life Challenges			
Limited English proficiency (%)	0.0	1.3	-1.3**
Number of barriers that interfere with school, work, job search, or family responsibilities (ranges from 0-4)	0.9	0.3	0.6**
Sample Size			
Individuals (N)	844	3,505	-

Notes: Statistical significance levels for two-sided tests are indicated as follows: ** = 5 percent.

Sources: HPOG PRS; PACE Baseline Information Form; National Directory of New Hires.

Sample Restrictions: The sample is restricted to study participants from HPOG programs that offered emergency assistance according to the grantee survey. For HPOG programs that offered this service as randomized enhancement to the enhanced treatment group, study participants randomly assigned to the standard treatment group were excluded from the analysis.

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Exhibit H.5: Participant Characteristics by Predicted Receipt of Non-cash Incentives

	Subgroup Predicted to Receive a Non- Cash Incentive	Subgroup Predicted to Not Receive a Non- Cash Incentive	Difference in Predicted Subgroup Means
Demographic Measures			
Male (%)	5.2	12.8	-7.6**
<i>Marital status</i>			
Never married (%)	47.4	70.8	-23.4**
Married (%)	12.6	12.5	0.1
Separated or divorced (%)	39.7	15.4	24.3**
Parent to one or more dependent children (%)	68.4	60.6	7.9**
<i>Race/ethnicity</i>			
Non-Hispanic white (%)	62.6	29.5	33.1**
Hispanic/Latino (%)	16.8	14.8	2.0
Black/African-American (%)	19.9	48.1	-28.2**
Age (years)	32.0	30.5	1.5**
Born outside the U.S. (%)	2.6	12.3	-9.6**
Educational Background			
Attained postsecondary degree (%)	13.1	18.8	-5.7**
Occupational skills license, or certification (%)	31.1	24.0	7.1**
Completed license, certification, or degree (%)	39.3	36.1	3.3
Attended adult basic education classes (%)	11.9	25.2	-13.4**
Attended English as a second language classes (%)	2.4	5.1	-2.7**
Course attendance in how to succeed in school (%)	20.1	19.0	1.1
Attended vocational, technical, or trade school classes (%)	64.9	27.5	37.4**
Course attendance in how to succeed at work (%)	8.8	21.0	-12.2**
Earnings and Public Assistance Receipt			
Average quarterly wage received in four quarters prior to the quarter of random assignment (\$)	1,761	2,336	-575**
Receipt of Welfare (%)	1.5	12.3	-10.8**
Receipt of WIC/SNAP (%)	67.3	59.7	7.6**
Employment			
Proportion of quarter employed during the four quarters prior to the quarter of random assignment (ranges from 0-4)	2.3	2.4	-0.1
Ever employed in a healthcare job (%)	45.1	45.7	-0.5
Expect to be working for pay in the next few months (%)	67.5	76.1	-8.6**
Life Challenges			
Limited English proficiency (%)	0.0	0.6	-0.6
Number of barriers that interfere with school, work, job search, or family responsibilities (ranges from 0-4)	0.4	0.4	0.0
Sample Size			
Individuals (N)	328	1,533	-

Notes: Statistical significance levels for two-sided tests are indicated as follows: ** = 5 percent.

Sources: HPOG PRS; PACE Baseline Information Form; National Directory of New Hires.

Sample Restrictions: The sample is restricted to study participants from HPOG programs that offered non-cash incentives according to the grantee survey. For HPOG programs that offered this service as randomized enhancement to the enhanced treatment group, study participants randomly assigned to the standard treatment group were excluded from the analysis.

Appendix H: HPOG Impacts by Program Experiences and Milestones

Exhibit H.6: Participant Characteristics by Predicted Participation in Peer Support

	Subgroup Predicted to Participate in Peer Support	Subgroup Predicted to Not Participate in Peer Support	Difference in Predicted Subgroup Means
Demographic Measures			
Male (%)	20.4	8.7	11.7**
<i>Marital status</i>			
Never married (%)	53.9	63.3	-9.4**
Married (%)	21.7	15.3	6.4**
Separated or divorced (%)	24.4	19.9	4.6**
Parent to one or more dependent children (%)	72.2	59.1	13.1**
<i>Race/ethnicity</i>			
Non-Hispanic white (%)	41.9	35.3	6.5**
Hispanic/Latino (%)	26.4	18.5	7.9**
Black/African-American (%)	26.4	40.5	-14.0**
Age (years)	35.9	33.3	2.7**
Born outside the U.S. (%)	21.4	18.3	3.1
Educational Background			
Attained postsecondary degree (%)	20.2	22.2	-2.0
Occupational skills license, or certification (%)	24.3	20.3	4.0**
Completed license, certification, or degree (%)	30.9	37.2	-6.3**
Attended adult basic education classes (%)	23.0	18.1	4.9**
Attended English as a second language classes (%)	14.9	6.8	8.1**
Course attendance in how to succeed in school (%)	33.5	11.9	21.6**
Attended vocational, technical, or trade school classes (%)	41.4	30.4	10.9**
Course attendance in how to succeed at work (%)	47.1	11.1	35.9**
Earnings and Public Assistance Receipt			
Average quarterly wage received in four quarters prior to the quarter of random assignment (\$)	1,868	2,493	-626**
Receipt of Welfare (%)	18.4	11.1	7.3**
Receipt of WIC/SNAP (%)	76.4	51.3	25.2**
Employment			
Proportion of quarter employed during the four quarters prior to the quarter of random assignment (ranges from 0-4)	1.8	2.4	-0.6**
Ever employed in a healthcare job (%)	46.3	46.0	0.3
Expect to be working for pay in the next few months (%)	48.4	77.7	-29.3**
Life Challenges			
Limited English proficiency (%)	2.2	1.1	1.1
Number of barriers that interfere with school, work, job search, or family responsibilities (ranges from 0-4)	0.7	0.4	0.3**
Sample Size			
Individuals (N)	544	4,015	-

Notes: Statistical significance levels for two-sided tests are indicated as follows: ** = 5 percent.

Sources: HPOG PRS; PACE Baseline Information Form; National Directory of New Hires.

Sample Restrictions: The sample is restricted to study participants from HPOG programs that offered peer support according to the grantee survey. For HPOG programs that offered this service as randomized enhancement to the enhanced treatment group, study participants randomly assigned to the standard treatment group were excluded from the analysis.

Appendix H: HPOG Impacts by Program Experiences and Milestones

Exhibit H.7: Participant Characteristics by Predicted Receipt of Personal Counseling

	Subgroup Predicted to Receive Personal Counseling	Subgroup Predicted to Not Receive Personal Counseling	Difference in Predicted Subgroup Means
Demographic Measures			
Male (%)	8.2	10.0	-1.7
<i>Marital status</i>			
Never married (%)	34.1	67.8	-33.7**
Married (%)	6.6	17.0	-10.5**
Separated or divorced (%)	57.1	14.2	42.9**
Parent to one or more dependent children (%)	53.3	62.7	-9.4**
<i>Race/ethnicity</i>			
Non-Hispanic white (%)	57.2	37.2	20.0**
Hispanic/Latino (%)	19.8	15.4	4.4**
Black/African-American (%)	20.7	42.0	-21.3**
Age (years)	43.0	30.7	12.2**
Born outside the U.S. (%)	17.3	13.3	4.0**
Educational Background			
Attained postsecondary degree (%)	35.5	18.8	16.6**
Occupational skills license, or certification (%)	12.8	19.8	-7.0**
Completed license, certification, or degree (%)	41.5	33.4	8.1**
Attended adult basic education classes (%)	22.6	17.1	5.6**
Attended English as a second language classes (%)	7.8	5.8	2.0**
Course attendance in how to succeed in school (%)	21.0	15.9	5.1**
Attended vocational, technical, or trade school classes (%)	25.2	31.4	-6.2**
Course attendance in how to succeed at work (%)	32.4	13.7	18.7**
Earnings and Public Assistance Receipt			
Average quarterly wage received in four quarters prior to the quarter of random assignment (\$)	1,683	2,523	-840**
Receipt of Welfare (%)	15.7	9.6	6.1**
Receipt of WIC/SNAP (%)	57.9	56.1	1.8
Employment			
Proportion of quarter employed during the four quarters prior to the quarter of random assignment (ranges from 0-4)	2.0	2.51	-0.54**
Ever employed in a healthcare job (%)	55.4	44.5	10.9**
Expect to be working for pay in the next few months (%)	65.1	76.8	-11.7**
Life Challenges			
Limited English proficiency (%)	7.2	0.1	7.1**
Number of barriers that interfere with school, work, job search, or family responsibilities (ranges from 0-4)	0.9	0.4	0.6**
Sample Size			
Individuals (N)	1,007	6,818	-

Notes: Statistical significance levels for two-sided tests are indicated as follows: ** = 5 percent.

Sources: HPOG PRS; PACE Baseline Information Form; National Directory of New Hires.

Sample Restrictions: The sample is restricted to study participants from HPOG programs that offered personal counseling services according to the grantee survey.

Appendix H: HPOG Impacts by Program Experiences and Milestones

Exhibit H.8: Participant Characteristics by Predicted Receipt of Tutoring Services

	Subgroup Predicted to Receive Tutoring Services	Subgroup Predicted to Not Receive Tutoring Services	Difference in Predicted Subgroup Means
Demographic Measures			
Male (%)	7.3	12.4	-5.0**
<i>Marital status</i>			
Never married (%)	64.7	59.5	5.2**
Married (%)	15.5	18.0	-2.5
Separated or divorced (%)	19.3	21.1	-1.9
Parent to one or more dependent children (%)	55.9	61.9	-6.0**
<i>Race/ethnicity</i>			
Non-Hispanic white (%)	14.8	45.3	-30.5**
Hispanic/Latino (%)	26.8	22.2	4.6**
Black/African-American (%)	48.0	26.7	21.3**
Age (years)	29.0	34.2	-5.3**
Born outside the U.S. (%)	32.2	17.4	14.8**
Educational Background			
Attained postsecondary degree (%)	9.3	22.1	-12.8**
Occupational skills license, or certification (%)	27.5	19.1	8.4**
Completed license, certification, or degree (%)	33.3	35.5	-2.3
Attended adult basic education classes (%)	30.2	11.7	18.5**
Attended English as a second language classes (%)	27.5	4.4	23.2**
Course attendance in how to succeed in school (%)	55.3	7.1	48.2**
Attended vocational, technical, or trade school classes (%)	27.7	34.9	-7.2**
Course attendance in how to succeed at work (%)	37.9	10.3	27.6**
Earnings and Public Assistance Receipt			
Average quarterly wage received in four quarters prior to the quarter of random assignment (\$)	2,620	2,438	182
Receipt of Welfare (%)	8.2	11.6	-3.4**
Receipt of WIC/SNAP (%)	57.5	50.9	6.6**
Employment			
Proportion of quarter employed during the four quarters prior to the quarter of random assignment (ranges from 0-4)	2.7	2.3	0.4**
Ever employed in a healthcare job (%)	39.0	39.9	-0.9
Expect to be working for pay in the next few months (%)	80.0	69.9	10.2**
Life Challenges			
Limited English proficiency (%)	5.0	0.4	4.6**
Number of barriers that interfere with school, work, job search, or family responsibilities (ranges from 0-4)	0.4	0.5	-0.1**
Sample Size			
Individuals (N)	559	2,829	-

Notes: Statistical significance levels for two-sided tests are indicated as follows: ** = 5 percent.

Sources: HPOG PRS; PACE Baseline Information Form; National Directory of New Hires.

Sample Restrictions: The sample is restricted to study participants from HPOG programs that offered tutoring services according to the grantee survey.

Appendix H: HPOG Impacts by Program Experiences and Milestones

Exhibit H.9: Participant Characteristics by Predicted Obtainment of a License or Credential

	Subgroup Predicted to Obtain a License or Credential	Subgroup Predicted to Not Obtain a License or Credential	Difference in Predicted Subgroup Means
Demographic Measures			
Male (%)	11.7	10.6	1.1
Marital status			
Never married (%)	50.3	68.3	-18.1**
Married (%)	22.5	12.4	10.0**
Separated or divorced (%)	25.7	18.0	7.7**
Parent to one or more dependent children (%)	68.1	57.7	10.4**
Race/ethnicity			
Non-Hispanic white (%)	38.9	32.0	6.9**
Hispanic/Latino (%)	23.6	23.3	0.2
Black/African-American (%)	29.9	39.1	-9.3**
Age (years)	33.6	32.0	1.5**
Born outside the U.S. (%)	24.2	12.7	11.5**
Educational Background			
Attained postsecondary degree (%)	21.3	15.7	5.5**
Occupational skills license, or certification (%)	45.9	4.3	41.7**
Completed license, certification, or degree (%)	58.6	18.5	40.1**
Attended adult basic education classes (%)	15.9	17.4	-1.5**
Attended English as a second language classes (%)	8.3	7.4	0.9
Course attendance in how to succeed in school (%)	16.2	14.9	1.3
Attended vocational, technical, or trade school classes (%)	46.9	16.3	30.6**
Course attendance in how to succeed at work (%)	16.4	15.6	0.8
Earnings and Public Assistance Receipt			
Average quarterly wage received in four quarters prior to the quarter of random assignment (\$)	2,832	1,988	844**
Receipt of Welfare (%)	14.4	9.7	4.7**
Receipt of WIC/SNAP (%)	57.5	58.1	-0.6
Employment			
Proportion of quarter employed during the four quarters prior to the quarter of random assignment (ranges from 0-4)	2.5	2.2	0.3**
Ever employed in a healthcare job (%)	44.0	27.6	16.5**
Expect to be working for pay in the next few months (%)	82.1	67.9	14.2**
Life Challenges			
Limited English proficiency (%)	0.3	1.7	-1.5**
Number of barriers that interfere with school, work, job search, or family responsibilities (ranges from 0-4)	0.4	0.5	0.0**
Sample Size			
Individuals (N)	4,932	5,518	-

Notes: Statistical significance levels for two-sided tests are indicated as follows: ** = 5 percent.

Sources: HPOG PRS; PACE Baseline Information Form; National Directory of New Hires.

Appendix H: HPOG Impacts by Program Experiences and Milestones

Exhibit H.10: Participant Characteristics by Predicted Completion of a College Degree

	Subgroup Predicted to Complete a Degree	Subgroup Predicted to Not Complete a Degree	Difference in Predicted Subgroup Means
Demographic Measures			
Male (%)	14.7	10.3	4.5**
<i>Marital status</i>			
Never married (%)	51.8	62.0	-10.3**
Married (%)	21.6	16.0	5.6**
Separated or divorced (%)	25.0	20.7	4.2**
Parent to one or more dependent children (%)	56.4	63.8	-7.4**
<i>Race/ethnicity</i>			
Non-Hispanic white (%)	46.5	32.5	13.9**
Hispanic/Latino (%)	12.9	26.1	-13.2**
Black/African-American (%)	32.3	35.4	-3.1**
Age (years)	37.6	31.6	6.0**
Born outside the U.S. (%)	21.6	17.2	4.5**
Educational Background			
Attained postsecondary degree (%)	92.6	0.0	92.6**
Occupational skills license, or certification (%)	26.1	23.4	2.7**
Completed license, certification, or degree (%)	94.0	23.4	70.6**
Attended adult basic education classes (%)	14.0	17.4	-3.3**
Attended English as a second language classes (%)	9.3	7.5	1.8**
Course attendance in how to succeed in school (%)	20.6	14.3	6.3**
Attended vocational, technical, or trade school classes (%)	36.0	29.4	6.6**
Course attendance in how to succeed at work (%)	20.5	14.9	5.6**
Earnings and Public Assistance Receipt			
Average quarterly wage received in four quarters prior to the quarter of random assignment (\$)	2,792	2,229	563**
Receipt of Welfare (%)	8.8	12.7	-3.9**
Receipt of WIC/SNAP (%)	49.3	60.0	-10.6**
Employment			
Proportion of quarter employed during the four quarters prior to the quarter of random assignment (ranges from 0-4)	2.5	2.3	0.1**
Ever employed in a healthcare job (%)	50.8	31.5	19.2**
Expect to be working for pay in the next few months (%)	74.4	74.5	-0.2
Life Challenges			
Limited English proficiency (%)	1.6	0.9	0.7**
Number of barriers that interfere with school, work, job search, or family responsibilities (ranges from 0-4)	0.4	0.5	0.0**
Sample Size			
Individuals (N)	2,072	8,378	-

Notes: Statistical significance levels for two-sided tests are indicated as follows: ** = 5 percent.

Sources: HPOG PRS; PACE Baseline Information Form; National Directory of New Hires.

Appendix H: HPOG Impacts by Program Experiences and Milestones

H.3 Impacts on Mediators

To begin, Exhibit H.11 reports the estimated impacts on each of the program experiences. The impacts in the first panel refer to the subgroup predicted to have the program experience, the impacts in the second panel refer to the subgroup predicted not to have the experience, and the difference in these impacts appears in the third panel. Exhibit H.11 shows that there are favorable impacts on the mediator for both those predicted to have the program experience and for those predicted not to have the program experience. Finding a positive impact in the subgroup predicted not to have the experience is a result of imperfect prediction.

To support our argument that differences in impacts on outcomes are operating through these mediators, we would expect there to be a large difference in impacts on the mediators themselves in order for those mediators to influence subsequent impacts. Although the impact is always larger for the subgroup predicted to have the program experience, the difference is never statistically significantly different from zero.

Exhibit H.11: Impacts on Receipt of Services for Predicted Endogenous Subgroups Defined by Program Experiences

Mediator	Received Emergency Assistance	Received a Non-Cash Incentive	Participated in Peer Support	Used Personal Counseling Services	Used Tutoring Services
Subgroup Predicted to Have Program Experience					
Treatment Group Mean (%)	31.3	25.1	17.2	19.7	25.6
Control Group Mean (%)	18.1	3.8	7.8	14.8	19.8
Impact (pp)	13.2*** (3.5)	21.3*** (4.5)	9.4** (4.2)	4.9* (2.9)	5.8 (4.5)
Subgroup Predicted not to Have Program Experience					
Treatment Group Mean (%)	16.7	16.1	11.3	11.8	14.8
Control Group Mean (%)	9.6	1.9	5.8	9.0	11.7
Impact (pp)	7.1*** (1.2)	14.2*** (1.5)	5.5*** (0.9)	2.8*** (0.8)	3.1** (1.5)
Differential Impacts					
Difference in Impacts	6.1 (3.9)	7.0 (5.1)	3.9 (4.4)	2.1 (3.2)	2.7 (5.0)
Sample Sizes					
Individuals (N)	4,349	1,861	4,559	7,825	3,388
Divisions (N)	47	30	44	83	36
Programs (N)	18	9	22	33	20

Sources: HPOG follow-up survey; PACE follow-up survey.

Notes: Standard errors are in parentheses. "pp" refers to percentage point. Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sample Restrictions: For the three services that were offered as randomized enhancements—emergency assistance, non-cash incentives, and peer support—the sample is restricted to the study participants from HPOG programs that offered the respective service according to the grantee survey. For HPOG programs that offered these services as randomized enhancements to the enhanced treatment group, study participants randomly assigned to the standard treatment group were excluded from the analysis. The sample used to define subgroups by receipt of personal counseling services is restricted to HPOG programs that offered this service according to the grantee survey. The sample used to define subgroups by receipt of tutoring services is restricted to HPOG and PACE programs that offered this service according to the grantee survey.

Similarly, Exhibit H.12 shows that the subgroups of individuals predicted to complete a license or credential and those predicted not to complete a license or credential both have favorable

Appendix H: HPOG Impacts by Program Experiences and Milestones

impacts on license/credential completion; and that the difference between the two groups' impacts is not statistically significantly different from zero. There is also no statistically significant difference in the impacts on milestone achievement for those predicted to and predicted not to have completed a college degree. Note, however, that the difference between the two groups is negative for obtaining a license or credential; this unexpected finding means that the impact is larger for the subgroup predicted to not obtain a license or credential.

Exhibit H.12: Impact on Obtaining Milestones

	Obtained a License or Credential	Completed a Degree
Subgroup Predicted to Achieve Milestone		
Treatment Group Mean (%)	55.0	55.9
Control Group Mean (%)	45.3	53.3
Impact (pp)	9.7*** (1.9)	2.6 (2.7)
Subgroup Predicted not to Achieve Milestone		
Treatment Group Mean (%)	41.5	10.5
Control Group Mean (%)	29.5	12.1
Impact (pp)	12.0*** (1.6)	-1.6* (0.9)
Differential Impacts		
Difference in Impacts (pp)	-2.4 (2.6)	4.2 (2.7)
Sample Sizes		
Individuals (N)	10,244	10,244
Divisions (N)	92	92
Programs (N)	42	42

Sources: HPOG follow-up survey; PACE follow-up survey.

Notes: Standard errors are in parentheses. "pp" refers to percentage point. Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

The lack of statistically or practically significant large differential impacts in Exhibits H.11 and H.12 might indicate that any subsequent differences in impacts on outcomes are only weakly related to the mediators (program experiences and milestones). We suspect that this lack of precision is due in part to poor performance of our prediction model.

Despite finding no statistically or practically significant or difference in impact, we proceed and explore how the observed differences in impacts on the mediators might be related to impacts on outcomes. While we maintain that any observed differences in impacts can only be weakly attributed to receipt of the service, weak evidence of the impact of service receipt is better than no evidence.

As a hypothesis for how impacts on the mediators might relate to impacts on outcomes, we note that the difference in impacts is positive for all program experiences and one of the two milestones. If these mediators are positively related to impacts on outcomes, then we would expect to find a positive difference in impacts on outcomes. For obtaining a license or credential, the milestone with a negative difference in impacts, we would expect to find larger impacts on employment and earnings for the subgroup predicted not to achieve the milestone if it is positively related to employment and earnings.

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H.4 Impacts on Outcomes

HPOG's impact on educational progress is generally favorable and statistically significant for the subgroups predicted to have the program experiences of interest. However, these impacts are not statistically different from corresponding impacts on those predicted not to have those program experiences, implying that having the program experience does not necessarily improve program outcomes relative to not having the program experience. For instance, the estimated impact on educational progress is 11 percentage points for the subgroup of study participants predicted to receive emergency assistance, and the corresponding impact on those predicted to *not* receive emergency assistance is 7 percentage points. The interpretation of the findings for the subgroups defined by receipt of non-cash incentives, peer support, personal counseling services, and tutoring is similar. As discussed in the preceding section, the lack of differential impacts in Exhibit H.13 implies that (1) individuals with the profile of those most likely to have a particular experience fare no differently than their peers; and (2) we cannot draw any conclusion about the relative importance of the experiences as they relate to impact variation.

Exhibit H.13: Impacts by Predicted Program Experience

Mediator	Received Emergency Assistance	Received a Non-Cash Incentive	Participated in Peer Support	Used Personal Counseling Services	Used Tutoring Services
<i>Subgroup Impacts on Educational Progress (%)</i>					
Predicted to Have Experience	10.6*** (4.1)	3.6 (6.8)	7.3 (5.3)	9.9** (4.0)	16.1*** (5.5)
Predicted Not to Have Experience	7.1*** (1.4)	9.8*** (2.6)	8.3*** (1.9)	6.7*** (1.1)	11.3*** (2.1)
Difference in Impacts	3.5 (4.6)	-6.2 (7.5)	-0.9 (5.8)	3.2 (4.4)	4.9 (6.1)
<i>Sample Sizes</i>					
Individuals (N)	4,349	1,861	4,559	7,825	3,388
Divisions (N)	47	30	44	83	36
Programs (N)	18	9	22	33	20

Notes: None of the differences in impacts between subgroups are statistically significant.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sources: HPOG follow-up survey; PACE follow-up survey.

Sample Restrictions: For the three services that were offered as randomized enhancements—emergency assistance, non-cash incentives, and peer support—the sample is restricted to the study participants from HPOG programs that offered the respective service according to the grantee survey. For HPOG programs that offered these services as randomized enhancements to the enhanced treatment group, study participants randomly assigned to the standard treatment group were excluded from the analysis. The sample used to define subgroups by receipt of personal counseling services is restricted to HPOG programs that offered this service according to the grantee survey. The sample used to define subgroups by receipt of tutoring services is restricted to HPOG and PACE programs that offered this service according to the grantee survey.

The estimated impacts on the subgroups predicted to complete program milestones appear in Exhibit H.14. HPOG had a positive impact on employment and earnings for those least likely to obtain a license or credential as well as those least likely to complete a degree. For instance, treatment group members *least* likely to obtain a license or credential are 2 percentage points more likely to be employed and have earnings \$237 higher than their control group counterparts. These impacts are not statistically different from the corresponding impact on the subgroup of those who did not obtain a license or credential. The results are similar for the subgroup defined by completion of a degree.

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Exhibit H.14: Impacts by Predicted Program Milestones

Mediator	Obtained a License or Credential	Completed a Degree
Subgroup Impacts on Employment (%)		
Predicted to Achieve Milestone	0.1 (1.6)	-0.6 (2.0)
Predicted to Not Achieve Milestone	2.4* (1.3)	2.0* (1.1)
Difference in Impacts	-2.4 (2.4)	-2.6 (2.2)
Predicted Subgroup Impacts on Earnings (\$)		
Predicted to Achieve Milestone	23 (134)	189 (192)
Predicted to Not Achieve Milestone	260*** (94)	153* (84)
Difference in Impacts	-237 (176)	35 (207)
Sample Sizes		
Individuals (N)	10,244	10,244
Divisions (N)	92	92
Programs (N)	42	42

Notes: None of the differences in impacts between subgroups are statistically significant. Employment and earnings are measured in the fifth quarter after random assignment.

Statistical significance levels for two-sided tests are indicated as follows: *** = 1 percent; ** = 5 percent; * = 10 percent.

Employment and earnings are measured in the fifth follow-up quarter using NDNH data.

Sources: HPOG follow-up survey; PACE follow-up survey; National Directory of New Hires.

H.5 Discussion

To summarize, recall the intuition underlying this analysis: if the prediction to subgroups most likely to have program experiences and achieve program milestones is strong and we have difference in impacts on program experiences or milestones, then we can plausibly infer that the program experience or milestone may be a pathway that affects overall impacts. This is not what we found. As such, the findings presented here do not provide a strong policy recommendation regarding which program experiences and milestones are central to improving education- and employment-related outcomes.

In the analysis, we do not find any cross-subgroup differences in impacts on educational progress for subgroups defined by program experiences. Similarly, we do not find any cross-subgroup differences in impacts on employment or earnings for subgroups defined by achievement of program milestones.

HPOG had a positive impact on employment and earnings for those *least* likely to obtain a license or credential as well as those least likely to complete a degree. This finding was consistent with our predictions from the impacts on mediators for those who obtained a license or credential. However, this impact was not different from those who were most likely to obtain a license or credential or those most likely to complete a degree. As with program experiences, we are unable to draw strong conclusions related to the program milestones as mediators to overall program impacts.

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Although the analysis did not find evidence that the selected program experiences or milestones were important mediators of HPOG's impact, this should not be interpreted as evidence that the experiences are unimportant aspects of a training program or that the milestones are unrelated to impacts on earnings and employment. The nature of the HPOG program may make it particularly difficult to isolate the contribution of a single program experience or milestone to impact. The 42 HPOG programs are multicomponent interventions offering a range of training opportunities and supportive services. Even if each of the training opportunities and supportive services were an integral and effective part of the overall intervention, it is possible that no single service, experience, or training has a large enough impact to be detected separately from the overall intervention.

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