Using Behavioral Insights to Increase Youth Use of Workforce Services in Virtual Contexts: Appendices

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Appendix A: Resources for Learning More About Behavioral Insights and How to Use Them for Continuous Improvement

Behavioral Insights and DOL

Behavioral insights involve the application of behavioral sciences—or understanding how people make and act on decisions—to improve the design of public policies and programs in ways that reflect a deeper understanding of human nature. The Behavioral Interventions (BI) Program team in the Chief Evaluation Office (CEO) works with Department of Labor (DOL) offices and agencies to use behavioral insights to test strategies aimed at improving the effectiveness of Departmental programs and strategies.

Trials offer new tools for improving outcomes by combining what we know from psychology and economics, which suggest that a deeper understanding of decision-making and behavior could improve program design and operation. For example, small changes in the environment, program operations, or default rules can reduce barriers to engagement or facilitate desired behaviors.

Learn more about CEO's Behavioral Interventions Program, including: current and completed trials, the Practitioner's Playbook, and other resources for practitioners to get started with behavioral insights.

Behavioral Interventions Design Process

Visit DOL's Chief Evaluation Office to learn more about:

Using Behavioral Interventions in DOL programs to:

- Help employers resolve citations around unsafe work conditions
- Improve participation in reemployment services
- Increase work search among unemployed workers
- Understand customer experiences at American Job Centers

Brainstorm future applications to labor contexts:

- Practitioner's Playbook for Applying Behavioral Insights to Labor Programs
- A Literature Scan and Synthesis of Research on Labor-Related Behavioral Science (dol.gov)

Apply behavioral insights to your own communications:

- Behavioral Insights Communications Checklist (dol.gov)
- Watch The Science of Corresponding with Busy People with Todd Rogers

Learn even more about behavioral science using these free resources:

- EAST: Four Simple Ways to Apply Behavioural Insights | The Behavioural Insights Team (bi.team)
- Behavior Change Strategy Cards
- Practitioner's Guide to Nudging
- World Development Report 2015: Mind, Society, and Behavior

Appendix B: Supplementary Details on Study Context and Design

This appendix provides additional technical details that supplement the information provided in the body of the report.

1. Context

Exhibit B.1 provides a logic model for the Comprehensive Case Management and Employment Program (CCMEP).

Exhibit B.1. CCMEP logic model

Note: Logic model content is based on reviews of CCMEP program websites and documentation (https://ohio.gov/wps/portal/gov/site/residents/resources/ccmep).

* Based on report reviews and discussions with program officials, we have determined that high-quality data on these outcomes are not available for enough participants to merit inclusion in our study.

Exhibit B.2 summarizes the characteristics of CCMEP participants statewide.

Exhibit B.2. Characteristics of CCMEP participants statewide

	Count	Percentage
Gender		
Female	12,384	66%
Male	6,249	33%
Did not disclose	146	1%
Age		
18 and younger	6,558	35%
19–20 years	3,676	20%
21–22 years	2,814	15%
23–24 years	2,845	15%
25 years and older	2,886	15%
Ethnicity and race		
Black/African American	9,630	51%
White	7,444	40%
Hispanic	1,158	6%
More than one race	644	3%
Asian	132	1%
American Indian/Alaskan Native	50	< 1%
Native Hawaiian/Pacific Islander	23	< 1%
Program eligibility		
WIOA youth eligible	16,464	88%
Ohio Works First (OWF) work eligible	1,644	9%
OWF/Prevention, Retention, and Contingency volunteer	671	4%
Education status		
In school	9,127	49%
Not in school	9,652	51%
Educational attainment		
9th grade or less	5,932	32%
Grade 10	2,725	15%
Grade 11	3,318	18%
Grade 12	483	3%
High school diploma or equivalency	5,618	30%
Some college or postsecondary credential	703	4%
Barriers to employment		
Basic skills deficient	8,410	45%
Pregnant or parenting	4,740	25%
Single parent	4,670	25%
Ex-offender	2,497	13%
Disabled	2,362	13%
Foster youth	1,363	7%
Homeless	1,153	6%
Temporary Assistance for Needy Families benefit recipient	115	1%

Source: CCMEP Program Year 2021 Unadjusted Performance Report (https://jfs.ohio.gov/owd/WIOA/Performance/CCMEP-PY-2021-Annual-Unadjusted-Performance-Report.stm)

Notes: Within a category, percentages may not sum to 100 percent due to rounding. Counts represent the number of participants in Program Year 2021, which ran from July 1, 2021, through June 30, 2022. Percentages reported are based on a total count of 18,779 participants. A participant is defined as an individual who "(1) Is a mandatory or voluntary CCMEP participant; (2) Has signed an individual opportunity plan (IOP) that includes one or more assignments to a CCMEP activity and (3) Has been exited in the current program year or has not yet been exited from participation in CCMEP in accordance with OAC Rule 5101:14-1-06."

Exhibit B.2 summarizes several features of the young adults served by CCMEP:

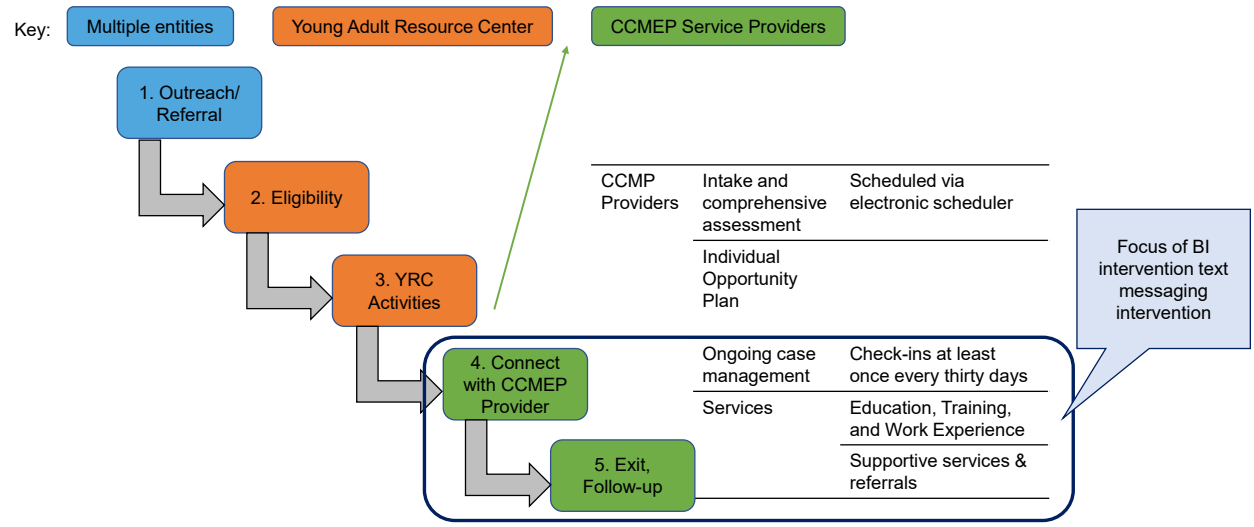
- Two out of every three are young women
- Half are younger than 20 years old; over one-third are younger than 18
- Just over half are African American and two out of every five are White
- A large majority (nearly 90 percent) are eligible for WIOA Youth services
- About half are in school when they enroll, and half are not
- Around one-third have completed grade 9 or less schooling, another one-third have completed grades 10 or 11, and another one-third have completed high school or its equivalent
- They face a range of barriers to employment—almost half are identified as basic skills deficient, one out of every four has a child, about one in eight is disabled, and about one in eight is an ex-offender.

Ohio's transition to a new case management system. During the study period, Ohio transitioned from its previous case management system (the Ohio Workforce Case Management System, OWCMS) to a new system beginning in April 2022. Unfortunately, due to several unanticipated issues related to the transition (including unintended changes to response options for program service milestones, and disrupted access to the new system among case managers), randomization was forced to end earlier than anticipated and the outcomes data available for our analyses cover only up to April 2022.

2. Intervention design

Exhibit B.3 presents the initial customer journey map we developed in partnership with Cuyahoga County.

Exhibit B.3. CCMEP Customer Journey Map



3. Sample intake

Exhibit B.4 summarizes our sample intake over the 22-week randomization period.

Exhibit B.4. Sample intake by week

Week	Randomization date	Number of participants randomized	Week	Randomization date	Number of participants randomized
W1	November 24, 2021	26	W14	February 23, 2022	31
W2	December 1, 2021	16	W15	March 2, 2022	30
W3	December 8, 2021	35	W16	March 9, 2022	39
W4	December 15, 2021	32	W17	March 16, 2022	29
W5	December 22, 2021	16	W18	March 23, 2022	30
W6	December 29, 2021	8	W19	March 30, 2022	16
W7	January 5, 2022	11	W20	April 6, 2022	29
W8	January 12, 2022	14	W21	April 13, 2022	12
W9	January 19, 2022	22	W22	April 20, 2022	33
W10	January 26, 2022	21			
W11	February 2, 2022	24	**All**		**530**
W12	February 9, 2022	37			
W13	February 16, 2022	19			

Source: OWCMS data provided by Ohio Department of Job and Family Services (ODJFS).

In total, we randomized 530 new CCMEP enrollees between November 2021 and April 2022. On average, we randomized 24 new participants per week. Our weekly sample intake ranged from a low of 8 during the winter 2021 holiday season (week 6, which coincided with a rise in COVID-19 cases due to the spread of the Omicron variant) to a high of 39 in early March 2022.

Characteristics of participants who were randomized. Exhibit B.5 summarizes the characteristics of CCMEP participants who were randomized into one of the two study groups (exhibits summarizing the baseline equivalence between treatment and control groups are presented in section B.5).

Exhibit B.5. Characteristics of participants who were randomized

	Count	Percentage
Gender		
Female	356	69%
Male	153	30%
Not declared	5	1%
Age		
18 and younger	165	32%
19–20 years	115	22%
21–23 years	169	33%
24 years and older	65	13%
Race		
Black/African American	393	77%
White	92	18%
Other race	29	6%
Ethnicity		
Hispanic or Latino	26	5%
Not Hispanic or Latino	462	90%
Did not declare	26	5%
Education status		
In school	148	29%
Not in school	366	71%
Educational attainment		
Less than 9th grade	53	10%
Grade 9	33	6%
Grade 10	42	8%
Grades 11 or 12 (no GED)	127	25%
High school	228	44%
One year of postsecondary schooling	15	3%
Two or more years of postsecondary schooling	16	3%
Barriers to employment		
Has disabilities	12	20%
Pregnant	82	16%
Parenting	208	41%

	Count	Percentage
Single parent	187	36%
Basic skills deficient	96	19%
Lacks transportation	482	94%
Funding source		
WIOA	183	36%
TANF	200	39%
Both	131	25%

Source: OWCMS data provided by ODJFS.

Notes: Within a category, percentages may not sum to 100 percent due to rounding. Barriers to employment are not mutually exclusive.

Exhibit B.6 summarizes the percentage of randomized participants served by each of the 11 participating counties.

Exhibit B.6. Randomized participants served by each county

County	Count	Percentage
Hamilton	146	28%
Cuyahoga	132	25%
Lucas	111	21%
Franklin	69	13%
Richland	29	5%
Huron	21	4%
Clark	11	2%
Perry	4	1%
Summit	3	1%
Trumbull	3	1%
Crawford	1	< 1%
Total	**530**	**100%**

Source: OWCMS data provided by ODJFS.

Notes: Percentages may not sum to 100 percent due to rounding.

Over half of the participants randomized into the two study groups were served by Hamilton and Cuyahoga counties. Another 21 percent were served by Lucas county and 13 percent by Franklin county. The other seven counties each served 5 percent or less of the participants who were randomized. These patterns are expected—Franklin, Cuyahoga, and Hamilton counties are the three most populous in the state and Lucas county is the sixth-most populous.[1]

[1] See 2021 Census county population estimates for Ohio (https://www2.census.gov/programs-surveys/popest/tables/2020-2021/counties/totals/co-est2021-pop-39.xlsx).

4. Data sources

Our quantitative analyses rely on two data sources: (1) administrative data from OWCMS, and (2) data from Twilio, the system the state used to deliver the intervention messages.

- **OWCMS** was the administrative data system of record for workforce programs in Ohio from the beginning of the study until April 2022, when the state transitioned to a new system. During the intake period, we used OWCMS data on weekly CCMEP enrollments to identify participants for randomization. The state also provided us with data from OWCMS on both participant characteristics collected at the time of enrollment and on program services received by participants.
  - The data on **participant characteristics** include the following: County office where enrolled; age, gender, race, ethnicity, educational attainment, educational status at enrollment (i.e., in school, not in school); selected barriers to employment (i.e., indicators for whether basic skills deficient, had disability, lacked transportation; whether pregnant or parenting; whether a single parent); and source of funding (i.e., WIOA, TANF, or both).
  - The data on **service receipt** include the following: the date when each service began, the date when each service was completed (if completed), and the type of each service.

  The OWCMS data include a unique participant identifier assigned by the state to each CCMEP participant. We used the identifier to link records across data files provided to us by the state and to link the OWCMS data to our randomization records, allowing us to identify the treatment and control groups.
- **Twilio** is a cloud communication system that allows for engagement with customers through various channels, including text messaging and phone calls. The state used Twilio to send the intervention messages to the treatment group, using an automated process. The state has provided us with message disposition data that allow us to observe whether each intervention message was delivered. The state has also provided us with data on inbound messages sent in response to intervention messages. Each inbound message received an automated reply that the number was not monitored and directing the participant to contact their coach with any questions.

Exhibit B.7 provides a list of all the variables we used in our quantitative analysis and their definitions.

Exhibit B.7. List of quantitative analysis variables

Name	Type	Definition
Gender	Categorical	A measure of the participant's gender, which we coded into three values: female, male, and did not disclose.
Race	Categorical	A measure of the participant's race, which we coded into three values: black, white, and other.
Ethnicity	Categorical	A measure of the participant's ethnicity, which we coded into three values: Hispanic or Latino, not Hispanic or Latino, and did not disclose.
Under 18	Binary	An indicator equal to 1 if the participant is younger than 18 years old.
Parenting	Binary	An indicator equal to 1 if the participant was a parent, and zero otherwise.
Pregnant	Binary	An indicator equal to 1 if the participant was pregnant, and zero otherwise.
Single parent	Binary	An indicator equal to 1 if the participant was a single parent, and zero otherwise.
Basic skills deficient	Binary	An indicator equal to 1 if the participant was basic skills deficient, and zero otherwise.
Disabled	Binary	An indicator equal to 1 if the participant was disabled, and zero otherwise.
Lacks transportation	Binary	An indicator equal to 1 if the participant lacked transportation, and zero otherwise.
In school	Binary	An indicator equal to 1 if the participant was in school, and zero otherwise.
County	Categorical	A measure indicating which of the 11 participating counties served the participant.
Funding source	Categorical	A measure indicating the program that funded the participant's CCMEP experience, taking on three values: WIOA, TANF, or both.
Age	Categorical	A measure of the participant's age, grouped into four categories: 18 or younger, 19–20, 21–23, and 24 or older.
Educational attainment	Categorical	A measure of the participant's educational attainment, grouped into seven categories: less than 9th grade, 9th grade, 10th grade, 11th or 12th grade (but no high school diploma), a high school diploma, 1 year of postsecondary education, or more than 1 year of postsecondary education.
Cohort	Categorical	A measure indicating the week that the participant was randomized.
Days between enrollment and randomization	Continuous	A measure of the number of days between the day the participant enrolled in CCMEP and the date the participant was randomized.
Number of services started within 60 days	Continuous	A measure equal to the total number of services with start dates no earlier than the participant's enrollment start date and no later than 60 days after the participant's enrollment start date.
Number of services started within 90 days	Continuous	A measure equal to the total number of services with start dates no earlier than the participant's enrollment start date and no later than 90 days after the participant's enrollment start date.

Name	Type	Definition
Whether at least one service was successfully completed within 60 days	Binary	An indicator equal to 1 if at least one service with a start date no earlier than the participant's enrollment start date was marked as successfully completed no later than 60 days after the participant's enrollment start date, and zero otherwise.
Whether at least one service was successfully completed within 90 days	Binary	An indicator equal to 1 if at least one service with a start date no earlier than the participant's enrollment start date was marked as successfully completed no later than 90 days after the participant's enrollment start date, and zero otherwise.

Source: OWCMS data provided by ODJFS.

With the exceptions of the cohort variable and the days between enrollment and randomization variable, all the variables we used in our quantitative analyses are from OWCMS. Data on participant characteristics were all captured at the time of the participant's enrollment into CCMEP. Data on service receipt were entered into OWCMS as services were delivered.

We created the days between enrollment and randomization variable to account for a feature of our random assignment design. Because we randomized participants on a weekly basis and because CCMEP providers sometimes enter participant enrollments with some delay, participants in our sample were randomized between 3 and 12 business days after their enrollment start dates.[2] Creating a measure of that delay allows us to incorporate it as a control variable in our quantitative analyses, as described in section F below.

5. Sample attrition and baseline equivalence

Although 530 CCMEP participants were randomized, our effective sample sizes are smaller, and they differ across outcomes measured for different follow-up periods. This is for two reasons:

- **Data are unavailable for eight participants, for unknown reasons**. Data provided by the state on baseline characteristics and services received are missing all information for eight participants in the study sample. The state investigated these cases but was unable to determine why the participants no longer appear in the data. This leaves us with data for 522 participants who were randomized. The result of a t-test indicates that the incidence of attrition is balanced between treatment and control groups.
- **The outcomes data coverage period ends in April 2022**. Because the outcomes data from the state cover only through April 2022, we cannot use them in our analyses records for participants who entered the sample late in the intake period. This is because not enough time had passed for those participants to allow their outcomes to be reflected in the data. For example, we cannot observe 60- or 90-day outcomes for a participant who entered the sample on March 20, 2022, because the data from the state would only reflect outcomes for approximately 30 days post-enrollment. This constraint also means that our effective sample size is smaller for outcomes measured 90 days after enrollment compared to outcomes measured 60 days after enrollment.

[2] We performed the randomization on Wednesday each week. We randomized all participants with an enrollment start dates that fell in either of the preceding two weeks who had not already been randomized.

Attrition. Sample attrition can lead to biased estimates of program impacts because it introduces the possibility that the treatment and control groups used for analysis are no longer balanced (Shadish et al. 2002). In each of our two analysis samples (i.e., the one we use for 60-day outcomes and the one we use for 90-day outcomes), there are two types of attrition: (1) overall attrition and (2) differential attrition. The overall attrition rate for each analysis sample is equal to the proportion of the sample for which outcomes data are not recorded in OWCMS (i.e., the participants who are missing from OWCMS for unknown reasons). Differential attrition refers to the difference in attrition rates between the treatment and control groups. Exhibit B.8 summarizes both the overall and differential attrition rates for both the 60-day and 90-day analysis samples.

Exhibit B.8. Attrition by analysis sample

	Full sample	Treatment group	Control group	Difference†
60-day analysis sample				
Number of participants randomized	315	164	151	13
Number of complete records	304	159	145	14
Number of missing records	11	5	6	-1
Attrition rate	3.5%	3.1%	4.0%	-0.9 pp
90-day analysis sample				
Number of participants randomized	207	109	98	11
Number of complete records	196	104	92	12
Number of missing records	11	5	6	-1
Attrition rate	5.3%	4.6%	6.1%	-1.5 pp

Source: OWCMS data provided by ODJFS.

Notes: Overall attrition rates are provided in the second column. Differences in the last column are equal to the treatment group value minus the control group value.

† Statistical significance based on a X^2 test.

***/**/* Statistically significant at the 1/5/10 percent level. Neither of the differences in attrition rates between treatment and control groups were statistically significant (the p-values for the test statistic were 0.655 for the 60-day sample and 0.623 for the 90-day sample).

Exhibit B.8 shows that for each sample, the level of overall attrition was no higher than 5 percent and differential attrition was less than 2 percent. These overall and differential attrition rates are well below the thresholds of acceptable combinations of overall and differential attrition identified by criterion RCT.2 in the causal evidence guidelines of DOL's Clearinghouse for Labor Evaluation and Research.[3] This means that we can be confident that there is no meaningful risk that our impact estimates are biased due to attrition.

Baseline equivalence. Another potential source of bias is an imbalance between treatment and control groups due to a failure of random assignment. If random assignment was successful, the two groups should reflect baseline equivalence—in other words, there should be no systematic differences between

[3] Version 2.2 of the causal evidence guidelines (https://clear.dol.gov/sites/default/files/CLEAR%20Causal%20Evidence%20Guidelines_v.2.2_1.pdf).

the two groups other than their study assignment (Shadish et al. 2002). To test for baseline equivalence, we used statistical procedures to test for differences between treatment and control groups that are larger than what would be expected to occur just due to chance. We performed the tests for each of the participant characteristics in our data.

Exhibit B.9 summarizes the results of the baseline equivalence tests for participant demographic characteristics, for both the 60-day and 90-day analysis samples.

Exhibit B.9. Baseline equivalence, participant demographics

	Total	Treatment group	Control group	Difference†
60-day analysis sample				
Female	70%	71%	69%	-2
Age				
18 and younger	31%	33%	28%	5
19–20	22%	21%	24%	-3
21–23	36%	35%	38%	-3
24 or older	11%	11%	10%	1
Race				
Black	75%	75%	76%	-1
White	19%	21%	17%	4
Other race	5%	4%	7%	-3
Ethnicity				
Hispanic or Latino	5%	5%	6%	-1
Education status				
In school	27%	31%	22%	9*
Educational attainment				
Less than 9th grade	9%	9%	10%	-1
Grade 9	7%	7%	6%	1
Grade 10	9%	9%	9%	0
Grade 11 or 12	28%	30%	26%	4
High school	41%	40%	42%	-2
One year of postsecondary	3%	4%	2%	2
Two or more years of postsecondary schooling	3%	2%	5%	-3
90-day analysis sample				
Female	73%	77%	69%	8
Age				
18 and younger	35%	37%	34%	3
19–20	23%	21%	25%	-4
21–23	31%	31%	30%	1
24 or older	11%	12%	11%	1

	Total	Treatment group	Control group	Difference†
Race				
Black	74%	74%	74%	0
White	20%	22%	19%	3
Other race	6%	4%	8%	-4
Ethnicity				
Hispanic or Latino	6%	7%	5%	2
Education status				
In school	28%	29%	27%	2
Educational attainment				
Less than 9th grade	10%	8%	13%	-5
Grade 9	8%	10%	7%	3
Grade 10	7%	5%	10%	-5
Grade 11 or 12	30%	32%	27%	5
High school	38%	40%	36%	4
One year of postsecondary	3%	3%	3%	0
Two or more years of postsecondary schooling	4%	3%	4%	-1

Source: OWCMS data provided by ODJFS.

Notes: Table entries represent means. Within a category, percentages may not sum to 100 percent due to rounding. Because all variables listed are binary indicators, differences represent percentage points.

For the 60-day sample, N = 304. For the 90-day sample, N = 196.

† Statistical significance based on a X^2 test.

***/**/* Statistically significant at the 1/5/10 percent level.

Only one of the differences in participant demographics between the treatment and control groups were statistically significant for either sample. For the 60-day analysis sample, the difference in the proportions of treatment and control group participants who were in school is statistically significant, but at only the 10 percent level. These results mean that participant demographics are balanced between the two groups for each of our two analysis samples.

Exhibit B.10 summarizes the results of the baseline equivalence tests for participant barriers to employment and relevant program characteristics.

Exhibit B.10. Baseline equivalence, participant barriers to employment, and relevant program characteristics

	Total	Treatment group	Control group	Difference†
60-day analysis sample				
Barriers to employment				
Parenting	44%	41%	48%	-7
Pregnant	17%	19%	15%	4
Single parent	40%	40%	41%	-1
Basic skills deficient	21%	21%	21%	0
Disabled	20%	21%	19%	2
Lacks transportation	95%	94%	96%	-2
Funding source				
WIOA	35%	36%	35%	1
TANF	38%	38%	37%	1
Both	27%	26%	29%	-3
Number of days between enrollment and randomization	8.95 (3.13)	8.72 (2.99)	9.20 (3.26)	-0.48
90-day analysis sample				
Barriers to employment				
Parenting	44%	44%	45%	-1
Pregnant	19%	23%	15%	8
Single parent	40%	43%	36%	7
Basic skills deficient	26%	26%	26%	0
Disabled	16%	16%	16%	0
Lacks transportation	96%	96%	96%	0
Funding source				
WIOA	34%	30%	39%	-9
TANF	42%	44%	39%	5
Both	24%	26%	21%	5
Number of days between enrollment and randomization	8.48 (2.61)	8.31 (2.60)	8.67 (2.63)	-0.36

Source: OWCMS data provided by ODJFS.

Notes: Table entries represent means. Within a category, percentages may not sum to 100 percent due to rounding. For binary indicators, differences represent percentage points.

For the 60-day sample, N = 304. For the 90-day sample, N = 196.

† Statistical significance based on a X^2 test.

***/**/* Statistically significant at the 1/5/10 percent level.

None of the differences in participant barriers to employment or relevant program characteristics between the treatment and control groups were statistically significant for either sample. As for participant demographics, the results mean that barriers to employment and relevant program characteristics are balanced between the treatment and control groups for each of our two analysis samples.

Exhibit B.11 summarizes the results of the baseline equivalence tests for the county serving each participant.

Exhibit B.11. Baseline equivalence, county

	Total	Treatment group	Control group	Difference†
60-day analysis sample				
Clark	3%	3%	4%	-1
Crawford	1%	1%	0%	1
Cuyahoga	24%	23%	26%	-3
Franklin	14%	14%	14%	0
Hamilton	27%	28%	27%	1
Huron	4%	4%	3%	1
Lucas	19%	18%	20%	-2
Perry	1%	1%	0%	1
Richland	6%	6%	6%	0
Summit	1%	1%	1%	0
Trumbull	1%	1%	0%	1
90-day analysis sample				
Clark	5%	3%	7%	-4
Crawford	1%	2%	0%	2
Cuyahoga	16%	14%	17%	-3
Franklin	18%	18%	18%	0
Hamilton	31%	31%	30%	1
Huron	2%	2%	2%	0
Lucas	18%	18%	17%	1
Perry	1%	1%	0%	1
Richland	7%	7%	7%	0
Summit	2%	2%	1%	1
Trumbull	1%	2%	0%	2

Source: OWCMS data provided by ODJFS.

Notes: Table entries represent means. Within a category, percentages may not sum to 100 percent due to rounding. For binary indicators, differences represent percentage points.

For the 60-day sample, N = 304. For the 90-day sample, N = 196.

† Statistical significance based on a X^2 test.

***/**/* Statistically significant at the 1/5/10 percent level.

As was the case for both barriers to employment and program characteristics, there are no statistically significant differences in the counties serving participants assigned to the two study groups for either of our analysis samples.

Exhibit B.12 summarizes the results of the baseline equivalence tests for the participant randomization cohorts.

Exhibit B.12. Baseline equivalence, randomization cohorts

	Total	Treatment group	Control group	Difference†
60-day analysis sample				
Week 1	9%	8%	9%	-1
Week 2	5%	6%	5%	1
Week 3	11%	11%	10%	1
Week 4	10%	10%	9%	1
Week 5	5%	6%	4%	2
Week 6	2%	3%	2%	1
Week 7	3%	3%	3%	0
Week 8	5%	4%	5%	-1
Week 9	7%	7%	8%	-1
Week 10	7%	6%	7%	-1
Week 11	7%	6%	8%	-2
Week 12	12%	12%	12%	0
Week 13	6%	6%	6%	0
Week 14	10%	10%	10%	0
Week 15	2%	1%	3%	-2
90-day analysis sample				
Week 1	13%	13%	14%	-1
Week 2	8%	9%	8%	1
Week 3	17%	17%	16%	1
Week 4	15%	15%	14%	1
Week 5	8%	9%	7%	2
Week 6	4%	4%	3%	1
Week 7	5%	5%	4%	1
Week 8	7%	7%	8%	-1
Week 9	11%	11%	12%	-1
Week 10	10%	9%	11%	-2
Week 11	3%	3%	3%	0

Source: OWCMS data provided by ODJFS.

Notes: Table entries represent means. Within a category, percentages may not sum to 100 percent due to rounding. For binary indicators, differences represent percentage points.

For the 60-day sample, N = 304. For the 90-day sample, N = 196.

† Statistical significance based on a X^2 test.

***/**/* Statistically significant at the 1/5/10 percent level.

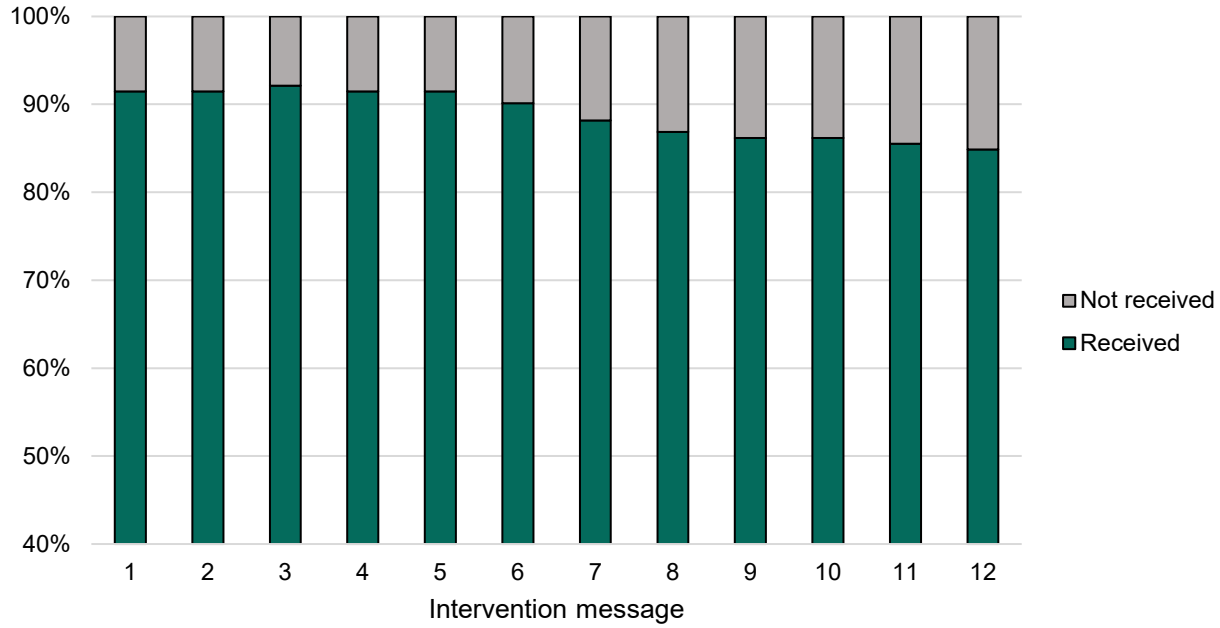
The results for randomization week are similar to the results of the other baseline equivalence tests, showing no statistically significant differences between treatment and control groups for either of the two analysis samples.

Collectively, the results in Exhibits B.9–B.12 show that the random assignment process was successful in producing balanced treatment and control groups, for both of our two analysis samples.

6. Receipt of intervention messages

Exhibits B.13 and B.14 show the proportion of the 60- and 90-day samples that received each of the 12 individual text messages.

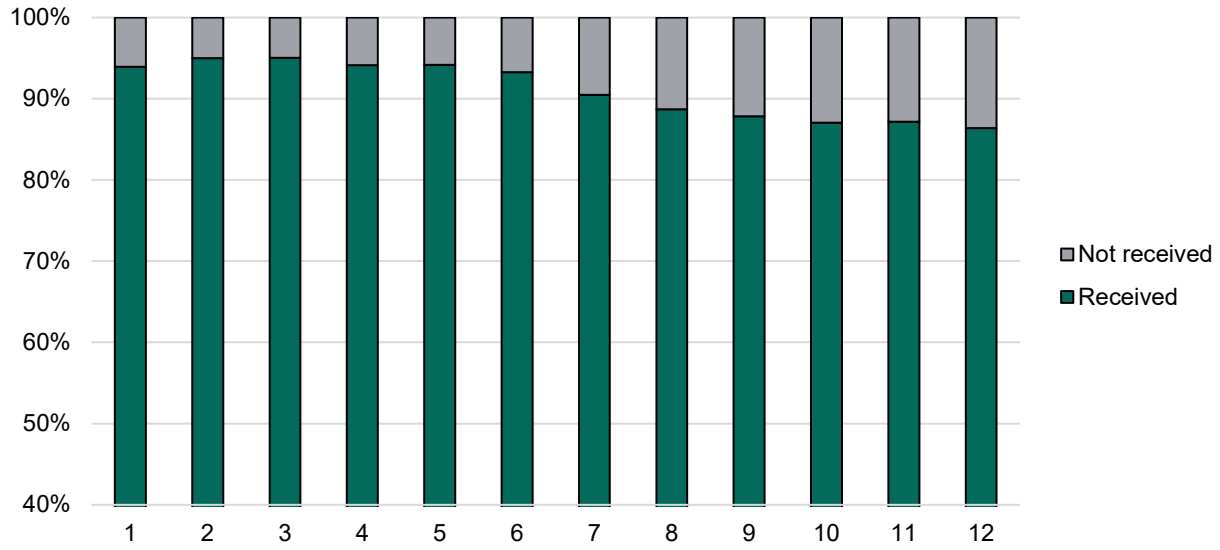
Exhibit B.13. Receipt of individual text messages, 60-day sample



Source: Twilio data provided by ODJFS.

Notes: Proportions for messages received represent for each message the proportion of all intervention messages attempted that were classified in Twilio as either sent or delivered. Proportions for messages not received represent for each message the proportion of all intervention messages attempted that were classified in Twilio as either not delivered or failed.

Exhibit B.14. Receipt of individual text messages, 90-day sample



Source: Twilio data provided by ODJFS.

Notes: Proportions for messages received represent for each message the proportion of all intervention messages attempted that were classified in Twilio as either sent or delivered. Proportions for messages not received represent for each message the proportion of all intervention messages attempted that were classified in Twilio as either not delivered or failed.

Across both study samples, the first 6–7 messages were typically more likely to be received by participants than later messages.[4] Moreover, participants in the treatment group in our 60-day sample would have received no more than 8 of the messages during the 60-day follow-up period over which we measured the outcomes of interest. Recalling Exhibit II.1, the behavioral strategies applied in the first 8 messages included the following:

- Promoting a fresh start, self-efficacy, and positive self-image
- Making program benefits more salient
- Leveraging loss aversion and endowment effects
- Leveraging social proof and peer groups (including via a link to video content)
- Providing planning prompts and feedback loops

Our results, particularly for the 60-day sample, suggest that the application of these behavioral strategies to a sequence of text messages can lead to improved engagement among CCMEP participants.

[4] All of our quantitative analyses are based on the full samples of participants assigned to the treatment and control groups. For participants assigned to the treatment group, this includes all participants regardless of the number of intervention messages they received.

Appendix C: Technical Details on Impact Estimates

1. Impact estimation and results

Mean comparisons. Because of the experimental design of the evaluation, the simple difference in average outcomes between the treatment and control groups gives us an unbiased estimate of the impact of the text messages on each of our outcomes of interest.[5] Exhibit C.1 summarizes the unadjusted averages of our primary and secondary outcomes and the estimated impacts of the text messages (i.e., the difference in means between the two groups) for both of our analysis samples.

Exhibit C.1. Outcome averages and impact estimates by analysis sample

	Treatment group	Control group	Difference†	P-value
60-day analysis sample				
Number of services started (60 days)	3.99 [2.44]	3.68 [2.36]	0.31 (0.28)	0.26
Whether at least one service is successfully completed (60 days)	0.31	0.23	0.09* (0.05)	0.09
90-day analysis sample				
Number of services started (90 days)	4.31 [2.85]	3.90 [2.70]	0.41 (0.40)	0.31
Whether at least one service is successfully completed (90 days)	0.35	0.27	0.07 (0.07)	0.26

Source: OWCMS data provided by ODJFS.

Notes: Table entries represent averages, with standard deviations for continuous measures provided in brackets. Differences in the last column are equal to the treatment group value minus the control group value, with standard errors in parentheses. For the binary indicator of whether at least one service was successfully completed, differences represent percentage points.

For the 60-day sample, N = 304. For the 90-day sample, N = 196.

† Statistical significance based on a t-test.

***/**/* Statistically significant at the 1/5/10 percent level.

Exhibit C.1 shows that all the estimated treatment effects are positive, but only one estimate—the one for the impact of the messages on whether at least one service is successfully completed in 60 days—is statistically significant (and only at the 10 percent level).

Regression adjustment. To complement the mean comparisons, we also estimated impacts using regression adjustment. We report the regression-adjusted impact estimates in the body of the report for our main findings.

[5] All our impact estimates are intent-to-treat estimates and are calculated using data for all participants assigned to the treatment and control groups for whom data are available. This includes participants who may have been assigned to the treatment group but did not receive all of the intervention messages. Participants in the treatment group may not have received all the intervention messages because they asked to opt out (participants making such a request did not receive any subsequent messages) or for other reasons (e.g., their phone number no longer being in service).

To implement regression adjustment, for each outcome we estimated the following regression model:

$$Y_i = \alpha + \beta X_i + \gamma T_i + \varepsilon_i \quad (1)$$

In the regression equation, the subscript *i* indexes individuals, Y_i represents the outcome, and the right-hand side of the equation includes a constant term, α, a set of participant characteristics and other control variables, X_i, an indicator T_i equal to 1 for individuals assigned to the treatment group and zero otherwise, and ε_i, a mean-zero error term.

The variables included in X_i are all the participant characteristics and relevant program characteristics described in section B.4 of this appendix (i.e., those for which we assessed baseline equivalence, as reported in Exhibits B.9–B.12).[6]

The parameter of interest is γ, which represents the causal impact of being in the treatment group on the outcome of interest. We estimated equation (1) separately for each outcome using ordinary least squares regression using Stata.

Subgroup analyses. Our exploratory analyses focused on whether the estimated impacts of the text messages differed for participants in and not in four different subgroups. To estimate separate regression-adjusted impacts for each subgroup, we modified regression equation (1) to include two additional explanatory variables:

$$Y_i = \alpha + \beta_1 X_i + \beta_2 G_i + \gamma T_i + \delta(T_i \bullet G_i) + \varepsilon_i \quad (2)$$

The additional explanatory variables are G_i, an indicator equal to 1 if participant *i* is in the subgroup of interest and zero otherwise, and $(T_i \bullet G_i)$, an interaction term between the treatment group indicator and the subgroup indicator. The parameter δ represents the marginal impact of the intervention among participants in the subgroup of interest. We will estimate equation (2) separately for subgroup for each outcome of interest using ordinary least squares regression. We will report the estimates of both γ (the impact of the intervention on those not in the subgroup of interest) and $\gamma + \delta$ (the impact on those in the subgroup of interest) and will use t-tests of the null hypothesis that δ is equal to zero, reporting whether the marginal impact is statistically significant at the 5% level. Although we will report whether the estimates are statistically significant, it is worth noting that these analyses are purely exploratory because we do not have a priori hypotheses about the directions or magnitudes of the impacts.

[6] Explanatory variables included in our regression models include a continuous measure of the number of days between enrollment and randomization and binary indicators for the following: male; gender not declared; white race; non-white, non-black race; Hispanic ethnicity; non-Hispanic ethnicity; parenting; pregnant; single parent; skills deficient; disabled; lacking transportation; each of ten of the 11 participating counties; being funded by WIOA; being funded by both WIOA and TANF; three of four age group categories (19–20, 21–23, and older than 23); six of seven educational attainment categories (9th grade, 10th grade, 11th or 12th grade [but no high school diploma]; high school diploma; 1 year of postsecondary education, and more than 1 year of postsecondary education); in school at enrollment; and all but one weekly randomization cohort.

Full regression-adjusted impact results. Exhibit C.2 summarizes the results of our regression-adjusted impact analyses for primary and secondary outcomes. These are the basis for the related findings we describe in the main body of the report.

Exhibit C.2. Regression-adjusted impact estimates, primary and secondary outcomes

	Treatment group	Control group	Difference†	Standard error
Primary outcomes				
Number of services started (60 days)	4.19	3.68	0.51**	0.24
Number of services started (90 days)	4.56	3.90	0.65	0.35
Secondary outcomes				
Successfully completed at least one service (60 days)	0.33	0.23	0.10**	0.05
Successfully completed at least one service (90 days)	0.35	0.27	0.08	0.07

Source: OWCMS data provided by ODJFS.

Notes: The control group values represent unadjusted control group means. The treatment group values represent the control group mean plus the estimated impact of the program. Differences are equal to the estimated regression coefficient for the treatment group indicator variable in regression equation (1). For the binary indicators for successful service completion, differences represent percentage points.

For 60-day outcomes, N = 304. For 90-day outcomes, N = 196.

† Statistical significance based on a t-test.

***/**/* Statistically significant at the 1/5/10 percent level.

Exhibits C.3–C.6 summarize the results of our regression-adjusted impact analyses for exploratory outcomes (i.e., our subgroup analyses). These are the basis for the related findings we describe in the main body of the report.

Exhibit C.3. Regression-adjusted impact estimates, exploratory outcomes, younger than 18 versus 18 or older

	Treatment group	Control group	Difference†	Standard error
Younger than 18				
Number of services started (60 days)	3.79	3.08	0.71	0.57
Number of services started (90 days)	4.12	3.58	0.54	0.79
Successfully completed at least one service (60 days)	0.43	0.04	0.39***	0.11
Successfully completed at least one service (90 days)	0.43	0.21	0.22	0.15
18 or older				
Number of services started (60 days)	4.34	3.82	0.52*	0.27
Number of services started (90 days)	4.69	3.98	0.71*	0.40
Successfully completed at least one service (60 days)	0.31	0.27	0.05	0.05
Successfully completed at least one service (90 days)	0.33	0.29	0.04	0.08

Source: OWCMS data provided by ODJFS.

Notes: The control group values represent unadjusted control group means. The treatment group values represent the control group mean plus the estimated impact of the program. Differences are equal to the estimated treatment effect in regression equation (2). For participants younger than 18, the estimated treatment effect

is equal to the sum of the estimated regression coefficients for the treatment group indicator and the under 18 indicator. For participants 18 or older, the estimated treatment effect is equal to the estimated regression coefficient for the treatment group indicator. For the binary indicators for successful service completion, differences represent percentage points.

For 60-day outcomes, there were 57 participants who were younger than 18 and 247 participants who were 18 or older. For 90-day outcomes, there were 42 participants who were younger than 18 and 154 participants who were 18 or older.

† Statistical significance based on a t-test.

***/**/* Statistically significant at the 1/5/10 percent level.

Exhibit C.4. Regression-adjusted impact estimates, exploratory outcomes, basic skills deficient versus not basic skills deficient

	Treatment group	Control group	Difference†	Standard error
Basic skills deficient				
Number of services started (60 days)	4.23	3.30	0.93*	0.55
Number of services started (90 days)	4.75	3.58	1.17	0.74
Successfully completed at least one service (60 days)	0.40	0.10	0.30**	0.11
Successfully completed at least one service (90 days)	0.38	0.21	0.17	0.14
Not basic skills deficient				
Number of services started (60 days)	4.17	3.78	0.39	0.27
Number of services started (90 days)	4.51	4.01	0.50	0.40
Successfully completed at least one service (60 days)	0.31	0.26	0.05	0.06
Successfully completed at least one service (90 days)	0.34	0.29	0.05	0.08

Source: OWCMS data provided by ODJFS.

Notes: The control group values represent unadjusted control group means. The treatment group values represent the control group mean plus the estimated impact of the program. Differences are equal to the estimated treatment effect in regression equation (2). For participants that are basic skills deficient, the estimated treatment effect is equal to the sum of the estimated regression coefficients for the treatment group indicator and the basic skills deficient indicator. For participants not basic skills deficient, the estimated treatment effect is equal to the estimated regression coefficient for the treatment group indicator. For the binary indicators for successful service completion, differences represent percentage points.

For 60-day outcomes, there were 63 participants who were basic skills deficient and 241 participants who were not. For 90-day outcomes, there were 51 participants who were basic skills deficient and 145 participants who were not.

† Statistical significance based on a t-test.

***/**/* Statistically significant at the 1/5/10 percent level.

Exhibit C.5. Regression-adjusted impact estimates, exploratory outcomes, in school versus not in school

	Treatment group	Control group	Difference†	Standard error
In school				
Number of services started (60 days)	4.06	2.94	1.12**	0.49
Number of services started (90 days)	4.31	3.36	0.95	0.69
Successfully completed at least one service (60 days)	0.38	0.13	0.26*	0.10
Successfully completed at least one service (90 days)	0.45	0.24	0.21	0.13
Not in school				
Number of services started (60 days)	4.20	3.89	0.31	0.28
Number of services started (90 days)	4.60	4.10	0.54	0.41
Successfully completed at least one service (60 days)	0.31	0.26	0.05	0.06
Successfully completed at least one service (90 days)	0.31	0.28	0.03	0.08

Source: OWCMS data provided by ODJFS.

Notes: The control group values represent unadjusted control group means. The treatment group values represent the control group mean plus the estimated impact of the program. Differences are equal to the estimated treatment effect in regression equation (2). For participants in school, the estimated treatment effect is equal to the sum of the estimated regression coefficients for the treatment group indicator and the in school indicator. For participants not in school, the estimated treatment effect is equal to the estimated regression coefficient for the treatment group indicator. For the binary indicators for successful service completion, differences represent percentage points.

For 60-day outcomes, there were 81 participants who were in school and 223 participants who were not. For 90-day outcomes, there were 55 participants who were in school and 141 participants who were not.

† Statistical significance based on a t-test.

***/**/* Statistically significant at the 1/5/10 percent level.

Exhibit C.6. Regression-adjusted impact estimates, exploratory outcomes, parenting versus not parenting

	Treatment group	Control group	Difference†	Standard error
Parenting				
Number of services started (60 days)	4.18	3.87	0.31	0.36
Number of services started (90 days)	4.20	3.85	0.35	0.51
Successfully completed at least one service (60 days)	0.23	0.30	-0.07	0.07
Successfully completed at least one service (90 days)	0.30	0.29	0.01	0.10
Not parenting				
Number of services started (60 days)	4.18	3.51	0.67**	0.33
Number of services started (90 days)	4.90	3.94	0.92*	0.48
Successfully completed at least one service (60 days)	0.40	0.16	0.24***	0.06
Successfully completed at least one service (90 days)	0.39	0.25	0.14	0.09

Source: OWCMS data provided by ODJFS.

Notes: The control group values represent unadjusted control group means. The treatment group values represent the control group mean plus the estimated impact of the program. Differences are equal to the estimated treatment effect in regression equation (2). For participants who were parenting, the estimated treatment effect is equal to the sum of the estimated regression coefficients for the treatment group indicator and the parenting indicator. For participants 18 or older, the estimated treatment effect is equal to the estimated regression coefficient for the treatment group indicator. For the binary indicators for successful service completion, differences represent percentage points.

For 60-day outcomes, there were 135 participants who were parenting and 169 participants who were not. For 90-day outcomes, there were 87 participants who were parenting and 109 participants who were not.

† Statistical significance based on a t-test.

***/**/* Statistically significant at the 1/5/10 percent level.

Correlations between shorter- and longer-term outcomes. Although data limitations precluded the analysis of longer-term outcomes (as we had initially planned), we used the data to explore what the potential impacts on 120-day outcomes might have been, were we able to observe them. To do this, we used the 127 observations in the control group for which we are able to observe 120-day outcomes to construct those outcomes and then calculate statistical correlations between 60-, 90-, and 120-day outcomes. Exhibit C.7 summarizes those correlations for the first of our two outcome measures.

Exhibit C.7. Correlations between number of services started within 60, 90, and 120 days

	Number of services started in…		
	60 days	90 days	120 days
Number of services started in …	-	-	-
60 days	-	0.957	0.918
90 days	-	-	0.973

Source: OWCMS data provided by ODJFS.

Notes: Table entries represent correlation coefficients calculated among outcomes measured for 127 participants for whom at least 120 days had passed before data entry into OWCMS was stopped and who were assigned to the control group.

The results show that the number of service started within 120 days is strongly correlated with the number of services started within either the first 60 or the first 90 days, with correlation coefficients close to 1 for both.

Exhibit C.8 summarizes those correlations for the second of our two outcome measures.

Exhibit C.8. Correlations between successfully completing at least one service within 60, 90, and 120 days

	Successfully completing a service in …		
	60 days	90 days	120 days
Successfully completing a service in …	-	-	-
60 days	-	0.726	0.666
90 days	-	-	0.917

Source: OWCMS data provided by ODJFS.

Notes: Table entries represent correlation coefficients calculated among outcomes measured for 127 participants for whom at least 120 days had passed before data entry into OWCMS was stopped and who were assigned to the control group.

The results are similar for the second outcome measure—successfully completing at least one service within 120 days is strongly correlated with successfully completing at least one service in 60 or 90 days. Clearly all participants who successfully completed a service in 60 days have also done so within 90 or 120 days. However, the correlations are not precisely equal to 1 because some of the participants who did not successfully complete a service within 60 or 90 days do so within 120 days.

Testing for compositional differences between the 60- and 90-day samples. To understand whether differences in the magnitudes of the impacts we estimate (when measured at 60 days compared to when measured at 90 days) could be due to differences in sample composition, we conducted two analyses. First, we re-estimated regression equation (2)—the model we use for subgroup analyses—for the two 60-day outcome measures, with the subgroup of interest defined as those participants who are also in the 90-day sample. If the estimated regression coefficient on the interaction term is statistically significant, the interpretation would be that the impact of the text messages on the 60-day outcomes differed for those participants who were and who were not also in the 90-day sample. This would suggest that the differences between 60-day and 90-day impact estimates may be in part due to compositional differences between the two samples. On the other hand, if the estimated coefficient on the interaction term is not statistically significant, the interpretation would be that there is no evidence that the impact on the text messages on the 60-day outcomes was any different between participants who were also in the 90-day sample and those who were not. In that case, we could have greater confidence that the differences between 60- and 90-day impact estimates are not affected by compositional differences between the two samples.

Exhibit C.9 shows the estimates of the interaction terms of interest.

Exhibit C.9. Estimated differential impacts on 60-day outcomes for participants in both the 60-day and 90-day samples

	Interaction Term Coefficient Estimate†	Standard Error
Number of services started (60 days)	0.319	0.513
Successfully completed at least one service (60 days)	0.022	0.103

Source: OWCMS data provided by ODJFS.

Notes: Coefficient estimates represent the estimated values of the parameter δ in regression equation (2), with the subgroup indicator G_i equal to 1 if the participant is in both the 60- and 90-day analysis samples and zero otherwise.

† Statistical significance based on a t-test.

***/**/* Statistically significant at the 1/5/10 percent level.

Neither of the regression coefficients associated with the interaction term of interest is statistically significant. This result indicates that there is no evidence that the impact of the text messages on outcomes measured at 60 days is different for the group of participants in our 60-day sample who were also in the 90-day sample, compared to those who were not also in the 90-day sample.

Reweighting the subgroup analyses. To assess the robustness of our findings for the exploratory subgroup analyses, we reproduced all of our results incorporating a weighting procedure described in Hock et al. (2018). The procedure involved reweighting the data so that each weekly randomization cohort would equally affect the impact estimate for each subgroup. Hock et al. (2018) explain that this can avoid confounding differences across (in our context) randomization weeks with differences across subgroups. The impact estimates generated using this reweighting procedure (not reported) were substantively no different than the results we report, giving us confidence that the subgroup findings are not due in part to systematic differences among subgroups across randomization weeks.

2. Very late enrollees and external validity

To supplement our main results, we also considered an issue affecting the external validity of our findings. The issue is related to how random assignment was implemented.

During the sample intake period, each Wednesday morning the state would retrieve from OWCMS records for all CCMEP participants in the database who had enrollment start dates that fell either (1) during the immediately preceding Monday–Sunday period or (2) during the Monday–Sunday period before that. We used this process because CCMEP participant data can be entered into OWCMS with some delay. For example, a participant who enrolls on Monday during week 1 may not have their data entered into OWCMS until Thursday of week 2 (though, assuming no data entry errors, the participant's record would show an enrollment start date of Monday of week 1). Having the state deliver participant data with enrollment start dates that fall in the preceding 2 weeks allowed us to include in the sample new enrollees whose data was entered into OWCMS with up to 12 days of delay.

Although our process allowed us to include in the sample new enrollees whose data were entered into OWCMS with a brief delay, in some participating counties a substantial proportion of enrollment data may be entered with a delay of greater than 12 days. We refer to these participants as very late enrollees.[7]

Because very late enrollees would not be captured by the weekly data for random assignment, the composition of our study sample may differ from the population of all participants who enrolled in CCMEP during our intake period. To the degree that this is the case, it would suggest that our results may have limited generalizability.

Comparing our analysis samples to very late enrollees, by county. Exhibit C.10 summarizes the number of CCMEP participants who enrolled during the study period, showing the number in our analysis sample and the number of very late enrollees by county.

Exhibit C.10 summarizes those correlations for the second of our two outcome measures.

Exhibit C.10. Analysis samples and very late enrollees, by county

	Total (60 day)	Analysis sample (60 day)	Very late enrollees (60 day)	Total (90 day)	Analysis sample (90 day)	Very late enrollees (90 day)
Clark	15	10 [67%]	5 [33%]	14	9 [64%]	5 [36%]
Crawford	4	2 [50%]	2 [50%]	4	2 [50%]	2 [50%]
Cuyahoga	162	73 [45%]	89 [55%]	88	31 [35%]	57 [65%]
Franklin	58	43 [74%]	15 [26%]	48	36 [75%]	12 [25%]
Hamilton	101	83 [82%]	18 [18%]	78	60 [77%]	18 [23%]
Huron	20	11 [55%]	9 [45%]	10	4 [40%]	6 [60%]
Lucas	315	58 [18%]	257 [82%]	236	35 [15%]	201 [85%]
Perry	5	2 [40%]	3 [60%]	2	1 [50%]	1 [50%]
Richland	18	17 [94%]	1 [6%]	14	13 [93%]	1 [7%]
Summit	5	3 [60%]	2 [40%]	5	3 [60%]	2 [40%]
Trumbull	8	2 [25%]	6 [75%]	7	2 [29%]	5 [71%]
All counties	**711**	**304 [43%]**	**407 [57%]**	**506**	**196 [39%]**	**310 [61%]**

Source: OWCMS data provided by ODJFS.

Notes: Total counts represent the number of CCMEP participants with enrollment start dates that fall within our sample intake period for each analysis sample. Sample counts represent the number of CCMEP participants with enrollment start dates that fall within our sample intake period and who were randomized into the study treatment and control groups. Counts of very late enrollees represent the number of CCMEP participants with enrollment start dates that fall within our sample intake period and who were not randomized.

For the 60-day sample, we randomized 43 percent of all participants with enrollment start dates recorded in OWCMS that fell in our sample intake period for that sample. For the 90-day sample, we randomized slightly fewer, 39 percent. These figures suggest there is some potential that our results may not be

[7] The term "very late enrollees" is not used by the state. Rather, it is a term the study team used to refer to CCMEP participants who enrolled in the program during our sample intake period but who were not randomized because the records of their enrollment were not entered into OWCMS until after we had completed randomization for the corresponding enrollment cohort.

generalizable to the larger population of CCMEP enrollees among participating counties. The reason is that there may be systematic differences between our analysis samples and the very late enrollees.

Regression analysis. To assess whether such systematic differences between our analysis samples and the groups of very late enrollees, we estimated the following regression model:

$$Y_i = \alpha + \beta X_i + \varepsilon_i \quad (3)$$

In the regression equation, the subscript *i* indexes individuals, Y_i represents the dependent variable, and the right-hand side of the equation includes a constant term, α, a set of control variables, X_i, and ε_i, a mean-zero error term. The dependent variable is an indicator equal to 1 if the participant was randomized and zero otherwise. The control variables in X_i include all the same variables as in equation (1), with the exception of the measure of the number of days between enrollment and random assignment (because the measure has no meaning for very late enrollees, who were not randomized).

We estimated equation (3) separately for the 60- and 90-day samples using ordinary least squares using Stata.[8] For each sample, we estimated the model using records for all participants with enrollment start dates that fall in the relevant sample intake period. This analysis is intuitively similar to the assessment of potential non-response bias when analyzing survey data—coefficient estimates that are statistically significant indicate that the corresponding control variable is associated with the probability of being randomized. Each such result therefore represents a meaningful difference between the set of CCMEP participants who are in our sample and those who are not.

Exhibit C.11 presents the estimated regression coefficients.

Exhibit C.11. Regression results, likelihood of being randomized

	60-day sample	90-day sample
Gender		
Female [a]	-	-
Male	0.023	0.007
Did not declare	-0.190	-0.156
Age		
18 and younger [a]	-	-
19–20	-0.019	-0.053
21–23	-0.012	-0.089
24 or older	-0.090	-0.195

[8] Because the outcome in each regression model is binary, using ordinary least squares regression means that we used a linear probability model (LPM), rather than an approach that explicitly accounts for the binary nature of the outcome (e.g., logistic regression). We used the LPM approach because LPM results are easier to interpret; our primary interest is in the statistical significance of the coefficient estimates, not in the point estimates; and often there is no substantive difference in the results produced by the two approaches. We also estimated both models using logistic regression and there were no differences in the results.

	60-day sample	90-day sample
Race		
Black [a]	-	-
White	-0.022	-0.022
Other race	-0.063	-0.040
Ethnicity		
Hispanic or Latino	-0.067	-0.131
Educational attainment		
Less than 9th grade [a]	-	-
Grade 9	-0.004	0.034
Grade 10	-0.008	0.020
Grade 11 or 12	0.037	0.069
High school	-0.053	0.043
One year of postsecondary	0.080	0.228*
More than one year of postsecondary education	0.106	0.203
Barriers to employment		
In school	-0.006	0.012
Parenting	-0.012	0.086
Pregnant	0.016	0.007
Single parent	-0.042	-0.143*
Basic skills deficient	-0.137***	-0.107**
Disabled	0.011	0.015
Lacks transportation	0.078	0.139
Funding source		
WIOA	0.075	0.054
TANF [a]	-	-
Both	0.130***	0.124**
County		
Clark [a]	-	-
Crawford	-0.043	-0.036
Cuyahoga	-0.310***	-0.347***
Franklin	0.048	0.074
Hamilton	0.232**	0.216*
Huron	-0.237	-0.327*
Lucas	-0.433***	-0.470***
Perry	-0.194	-0.043
Richland	0.294*	0.269
Summit	0.079	0.052
Trumbull	-0.254	-0.189
Cohort		
Week 1 [a]	-	-
Week 2	0.052	0.042
Week 3	0.097	0.086
Week 4	0.083	0.088
Week 5	-0.009	-0.014

	60-day sample	90-day sample
Week 6	-0.075	-0.069
Week 7	-0.101	-0.080
Week 8	0.089	0.073
Week 9	-0.030	-0.049
Week 10	0.089	0.089
Week 11	0.132	-0.018
Week 12	0.319***	0.000
Week 13	0.093	0.042
Week 14	0.169**	0.086
Week 15	-0.089	0.088
Constant	0.569***	0.642***
Number of observations	711	506

Source: OWCMS data provided by ODJFS.

Notes: Table entries are coefficient estimates from an ordinary least squares regression with a binary dependent variable equal to 1 if the participant was randomized and zero otherwise. Separate regression models were estimated for each analysis sample using data for participants who had enrollment start dates within the study sample intake period for that sample.

[a] Omitted condition.

***/**/* Statistically significant at the 1/5/10 percent level.

The regression results show that broadly speaking there are few systematic differences between the participants who were randomized and participants who enrolled in CCMEP during our sample intake period but were not randomized. Among the factors included in our regression models, only a small number were statistically significant at the 5 percent level. Patterns that hold for both of our analysis samples are:

- Participants who were skills deficient were less likely to be randomized than those who were not.
- Participants in two counties—Cuyahoga and Lucas—were less likely to be randomized than participants from other counties.
- Participants who were dual-funded were more likely to be randomized than participants funded only by either WIOA or TANF.

It is unclear what may be driving these patterns. Our interpretation of these results is that although there are few substantive differences between the CCMEP participants who were randomized as part of our study and the group of very late enrollees, some caution may be warranted when generalizing our results.

References

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