Analysis Plan: Evaluating the Impact of Training for In-Home Supportive Services (IHSS) Workers

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Abstract

This trial investigates whether high-quality training for consumer-directed home care workers impacts health outcomes for care consumers and employment outcomes for care workers. We are conducting this study in the context of the In-Home Supportive Services (IHSS) program, a consumer-directed, Medicaid-funded home care program in California serving older adults and people living with disabilities. Our research team will partner with the Center for Caregiver Advancement (CCA), a training provider based in California, to conduct a randomized evaluation of the impact of training for IHSS workers on labor and health care outcomes. The evaluation will enroll IHSS workers in San Bernardino County, where CCA will be expanding its program.

Introduction

Home care is a rapidly growing industry, but the quality of these jobs is often low. The home care workforce, which disproportionately employs women and people of color, faces low wages and high levels of poverty, with one in five home care workers living in a household below the federal poverty line. Due to the aging of the United States population and changes in insurance reimbursement policies, the Bureau of Labor Statistics projects that the home care workforce

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will grow substantially, adding approximately 1.2 million workers by 2029 (twice as many as any other occupation). Although poorly remunerated, evidence suggests that home care workers can have a substantial impact on health outcomes. This combination of low pay and high potential efficacy suggests that boosting the skills of home care workers through training could both raise wages and improve care.

This research project will test whether equipping home care workers with greater expertise will benefit home care clients, home care workers, or, ideally, both. Specifically, we will evaluate the impact of a state-of-the-art, comprehensive home care training program in the country's largest, deregulated home care program: the In-Home Supportive Services (IHSS) program in California. California's IHSS is a Medicaid-funded, consumer-directed program—meaning that individuals are free to hire whomever they wish—for older adults and people living with disabilities who are enrolled in Medicaid and unable to live at home safely without additional support. The program includes no training requirements and limited restrictions on tasks performed. As of November 2024 (the most recent data available), 721,792 IHSS providers currently serve over 816,880 authorized recipients statewide; the average number of beneficiaries has grown by 32% in the past 6 years (California Health Care Foundation, 2023).

This uniquely deregulated environment provides a rare opportunity to rigorously evaluate the efficacy of high quality training for boosting the quality of care while simultaneously raising the earnings power of care workers. Utilizing an experimental design, this research will shed light on the relative merits of the various policy approaches to home care currently in use across the nation. If, when permitted and properly trained, home care workers can improve the health of those they care for and use their skills to take on more consumers, work more hours, and potentially command a higher wage, then perhaps providing training and easing regulatory restrictions on the tasks that home care workers can perform (as permitted by the California IHSS environment) can improve outcomes for both workers and consumers. Alternatively, if this upskill intervention has no impact, this may suggest that training requirements in other states are creating inefficient barriers to entry that, if relaxed, could increase employment for workers and access to care for consumers. This research aims to address these unanswered questions.

Study design

Intervention

The Center for Caregiver Advancement is the largest provider of training for caregivers in California and has trained over 35,000 nursing home workers and IHSS providers. CCA provides educational opportunities including free classes for in-home caregivers and nursing home workers to help workers build better lives for themselves and the people they serve.

We will evaluate the IHSS Essentials course (formerly known as the Basics course) developed by CCA. This course is designed to equip caregivers with skills, such as infection control, nutrition, body mechanics, medication adherence, and home safety.[6] Each class is 2.5 hours, and the course takes place online for a total of 12 weeks, resulting in 30 total training hours. In addition to course content, participants receive a substantial stipend for completion, equivalent to their hourly rate as IHSS workers and a $1,000 completion bonus, for a total of $1,495.[7] To complete the training, participants must attend all classes, missing no more than three regularly scheduled classes, which must be made up at alternative times. The content of the course was created by CCA without input from the researchers.

The program will be delivered and evaluated in San Bernardino County. Through a High Road Training Partnership grant with the California Workforce Development Board, CCA expanded its training offerings to San Bernardino County beginning in 2024. CCA has funding to train approximately 1,200 people in its IHSS Essentials course in 2024 and 2025 in San Bernardino County. Because there are 35,000 IHSS providers in the county, CCA expects to be oversubscribed and has chosen to use a random lottery as a fair and transparent method to allocate spots in training.

Inclusion criteria and study enrollment

IHSS providers

Participants are eligible to enroll in the study and receive training from CCA if they:

- Are an IHSS provider[8]
- Serve an IHSS consumer in San Bernardino County
- Are proficient in English or Spanish (the two languages in which training is offered)
- Are over 18 years old.

Enrollment into the trial will be conducted by CCA in a process similar to the one used to enroll IHSS providers in training outside of the trial. Online registration for training opens approximately 24 weeks prior to the start of training. Potentially eligible participants navigate to CCA's website to complete a screening course to prove eligibility and, if directed to the IHSS Essentials course, complete enrollment and consent forms, a baseline survey, and indicate scheduling preferences. CCA staff review eligibility information from the screening course and determine final eligibility status. CCA staff are also available to help potential participants with the enrollment process.

[6] For an outline of topics covered in the IHSS Essentials, refer to CCA's website.
[7] At the time of the intervention, the hourly rate in San Bernardino County was $16.00. See wages by county.
[8] In cohorts 1 and 2, the IHSS provider had to reside in San Bernardino County. This will be relaxed for cohorts 3 and 4.

In addition to the IHSS Essentials course, CCA is running two other training programs in the county concurrently: one for workers caring for consumers with Alzheimer's disease and related dementia (ADRD) and one to equip workers for emergencies and disasters titled Caregiver Resiliency Teams (CRT). The screening course and staggered enrollment schedules serve to direct IHSS providers to the appropriate course. Providers who serve consumers with ADRD will generally be directed toward that course and thus may make up a smaller portion of our evaluation sample. Only those who select the IHSS Essentials course are randomized. Providers who apply for the IHSS Essentials course are not eligible to participate in the ADRD or CRT courses.

IHSS consumers

Only IHSS providers participate in training; however, many of the trial outcomes relate to IHSS consumers who receive care from providers enrolled in the study. IHSS operates as an entitlement, but consumers must first apply and be assessed and approved to receive IHSS, therefore not all eligible participants are enrolled in IHSS. To be eligible for IHSS as a consumer, you must be:

- Aged 65+, disabled, or blind, and
- Living in a home, not an institution, and
- Meet the financial eligibility criteria for Medicaid, and
- Unable to live at home safely without IHSS services

We will define study consumers as those who ever receive care from a provider in the study from the start of training to the end of the study period. We will analogously define treatment group consumers as any consumer who receives care from a provider randomized to treatment from the start of training to the end of the study period. These relationships are recorded in CMIPS data. Most providers have one consumer and most consumers have one provider, but these relationships need not be one-to-one. As discussed below, we will address different degrees of exposure to the intervention for consumers by conducting both individual-level and weighted analyses.

Randomization

Eligible IHSS providers will be randomized to either a group that is offered CCA's training or a control group that does not receive training. Participants are randomized at the individual level using simple randomization in batches throughout each registration period using R. We aim to randomize at an overall 1:1 ratio into the treatment and control groups. However, this ratio may vary across cohorts depending on the number of participants to ensure that CCA meets its programmatic goals for training. To date in Cohorts 1 and 2, 67% of participants were randomized to the treatment group.

- **The treatment group will be invited to participate in the IHSS Essentials training and receive the $1495 stipend upon completion**. Participants who are randomly assigned an offer of training but who cannot complete the training at the originally designated time may re-enroll in training in a later cohort during the study period, as our goal is to maximize the chance that treatment participants complete training. CCA also runs make-up classes for individual sessions during a course run. Participants may not miss more than three classes for CCA to consider them to have completed the training and receive the stipend. This is a CCA requirement that pre-dates this evaluation. We expect at least 80% take-up of the offer of training.

- **The control group is not offered training and receives a stipend of $75**.[9] For programmatic reasons, the control group will also function as a waitlist. Those not offered training will be informed that they have not been offered training but have been placed on a waitlist and may be contacted to fill spots as needed. CCA may pull from the randomly ordered control group as needed to fill program spots in order to run sufficiently large classes. Use of the waitlist is at the discretion of CCA, but we expect use to be limited. To date in Cohorts 1 and 2, the waitlist has not been used. Participants will be analyzed based on their assigned group after any use of the waitlist.

Study participants randomized into the control group will not be able to enroll in training for two years after their treatment assignment. Control group participants may seek other training opportunities in the community, for example through California's IHSS Career Pathways Program, in which CCA also participated until the program's end in August 2024. CCA delivered the same training content as the Essentials course as part of the Career Pathways Program, but in single class modules, with a less generous stipend. We expect few participants will complete equivalent training if assigned to the control group.

Anyone who begins the application but does not complete enrollment and is either not randomized or is not notified of their randomization status is eligible to reapply and join the study in a later cohort.

Timeline

CCA will offer IHSS Essentials several times throughout the two-year enrollment period. We label each scheduled offering a cohort. In some cases, CCA may delay the start of some classes to allow more time for recruitment and enrollment. In those cases, we have separated cohorts into A and B sections (four cohorts were initially planned). As of this writing, the enrollment timeline is as follows:

- January 2024: Cohort 1A
- February 2024: Cohort 1B

[9] The decision to compensate the control group was made after Cohort 1A enrolled, so was applied retroactively to that cohort.

- July 2024: Cohort 2A
- August 2024: Cohort 2B
- April 2025: Cohort 3 (expected)
- October 2025: Cohort 4 (expected)

Data collection

Administrative data

This project will link data from at least three administrative sources in California:

1. Case Management, Information and Payrolling System (CMIPS) dataset from the IHSS program, provided by the California Department of Social Services (CDSS).

2. Department of Health Care Access and Information (HCAI) data including:
    a. Patient Discharge Data (PDD)
    b. Emergency Department Data (EDD)
    c. Ambulatory Surgery Data (ASD)

3. Medi-Cal claims data for IHSS consumer, provided by the California Department of Health Care Services (DHCS)

4. Wage and unemployment data from the Employment Development Department (EDD), if we are able to successfully negotiate a data use agreement.

Together these data allow the ability to observe health care utilization for consumers and career outcomes for providers, within IHSS and overall (if EDD data can be used). These data will be linked to study participation through identifiers collected by CCA during enrollment. CMIPS data allows for linkages between participating providers and the IHSS consumers they care for. As of this writing, data use agreements with these data providers are still being developed, so the exact variables and years of data that are available to us may change. We expect to observe participants for at least two years following randomization.

Survey data

Provider participants complete a baseline survey before randomization as part of study enrollment. The baseline data measures provider demographic characteristics, employment history, characteristics of provider roles, provider attitudes towards caregiving, and provider knowledge of caregiver training content. The baseline survey will be used to examine balance between treatment and control groups, differences between study participants and the broader IHSS provider population, and will be used as control variables in analysis.

Participants will be sent an endline survey after training concludes. The survey is conducted online via Qualtrics. Participants are invited to complete the survey by email, text, and postcard, and participants are offered a $25 Tango gift card upon completion. We intend to send the

survey within 3 months of the end of training (to date surveys have been launched approximately one month after training ends). Participants assigned to treatment who elect to complete the training in a later cohort from the one initially assigned will be surveyed after they actually complete the training.

Data from the endline survey will measure caregiving knowledge, career outlook and satisfaction, and personal well being, outcomes we are unable to observe in administrative data. Where similar outcomes are reported in administrative data and survey data, we will use administrative data to measure impact, with survey outcomes used as a robustness check. The exception to this is demographic information, such as race, ethnicity, and gender, which we will take from the baseline survey when available.

Training monitoring data

We will also receive internal training data from CCA, including attendance and graduation records and weekly quiz scores. These data will be used to estimate take up of the program and can be used to monitor the extent to which the training was implemented successfully.

IRB approval

This study was approved by the Committee on the Use of Humans as Experimental Subjects (COUHES) at MIT (Protocol 2308001081). The University of Chicago ceded review to MIT. Use of administrative data from the California Health and Human Services Agency was approved by the agency's Committee for the Protection of Human Subjects (CPHS) (Protocol 2023-158).

Analysis

Outcomes

This study will measure health and health care utilization outcomes of IHSS consumers whose providers participated in training, and knowledge gains (Guerrero et al. 2019) and job quality outcomes for IHSS providers. All administrative data outcomes will be measured 1 year and 2 years after training begins. Survey outcomes will be measured at the time of the survey, which is delivered to study participants within 3 months of the end of training for their cohort. We assess the overall impact of the program within several domains by presenting standardized treatment effects across outcomes in the domain, following Finkelstein et al. (2012). One notable weakness of this approach is that it weights each outcome within a domain equally, which may not be the correct weighting. We also present the treatment effects for specific outcomes, given their ease of interpretation and the interest in each outcome, with both per-comparison and adjusted p-values.

IHSS consumer outcomes

Utilization

1. Any / **number of emergency department visits*** (primary outcome)
2. Any / number of / length of stay in hospital admission (excluding admissions for childbirth)
3. Any / number of / length of stay in a skilled nursing facility, rehab, or long-term care facility
4. Any / number of outpatient visits
5. Estimated annual health care spending.
    - Following Baicker et al. (2013), we calculate spending by multiplying the number of inpatient admissions, emergency department visits, outpatient office visits, long term care stays, and prescription drugs by estimated average costs for those services among low income publicly insured adults. This outcome also serves as an alternative standardized, summary effect measure for health care utilization.

Caregiver-sensitive health outcomes

6. Any / number of falls resulting in a health care visit (measured using diagnosis codes from medical claims)
7. Any / number of urinary tract infections resulting in a health care visit (measured using diagnosis codes from medical claims)
8. Any / number of pressure ulcers resulting in a health care visit (measured using diagnosis codes from medical claims)

We also plan to conduct exploratory analysis attempting to distinguish avoidable from unavoidable utilization, such as by examining Ambulatory Care Sensitive Conditions (Agency for Healthcare Research and Quality 2001) and by examining utilization excluding utilization relating to oncologic care, surgical procedures for acute health problems, pregnancy, and progressive chronic diseases with limited treatment options (Finkelstein et al. 2020).

IHSS provider outcomes

Knowledge gains and tasks performed

1. Caregiving knowledge, measured as the proportion of correct answers out of 18 true false knowledge questions in the endline survey. Questions were developed by CCA to reflect training content but are not specific to content learned only in CCA courses.
2. Whether providers perform paramedical services (self-reported; defined as giving injections, blood/urine testing, wound care, catheter or ostomy care; note that these are not taught in CCA courses.)
3. How often providers communicate with their consumer's care team (self-reported on a scale from "Not at all" to "Four or more times" in the last month)

Labor market

1. IHSS job retention (measured by whether a provider is still employed in IHSS according to CMIPS records at the end of the 1 and 2 year follow up periods)
2. Mean weekly IHSS hours (measured in CMIPS data)
3. Mean weekly IHSS earnings (measured in CMIPS data)
4. Mean weekly total earnings (measured in EDD data)
5. Mean number of consumers served per provider (measured in CMIPS data)
6. Number of jobs outside of IHSS (measured in EDD data)

Career outlook

1. Job satisfaction (self-reported 5-point Likert scale from very dissatisfied to very satisfied)
2. Job confidence (self-reported 5-point Likert scale from strongly disagree to strongly agree for the statements "I feel I have the skills I need to perform my job well" and "I know I can meet all the needs of the IHSS consumer"
3. Whether being an IHSS provider is viewed as "a short term job" or "a long term career" (self-reported)
4. Willingness to care for non-family (self reported 5-point Likert scale from definitely not to definitely yes)
    - Approximately 60% of IHSS caregivers provide care to family, so caring for non-family serves as a proxy for career aspirations in care work beyond addressing a current family need.
5. Likelihood of leaving IHSS job in next year (self reported 3-point scale: very, somewhat, or not at all likely)

Personal well being[10]

1. Stress (self-reported 5-point low to high scale)
2. Health (self-reported response to "In general, would you say your health is: excellent, very good, good, fair, poor")
3. Ability to ask for help (self-reported 5-point Likert scale from strongly disagree to strongly agree for the statement "I have people in my life I feel comfortable asking for help when I need it")

Empirical model

Our primary analysis will utilize an intention-to-treat (ITT) approach. The analysis will compare outcomes for those randomized to participate in the IHSS Essentials course compared to those

[10] Our originally proposed plans to measure wellbeing included depression (PHQ-2), anxiety (GAD-2), and the Caregiver Self-Assessment Questionnaire, but these were dropped from the endline survey after baseline data from Cohort 1A suggested high levels of baseline wellbeing and therefore insufficient statistical power to demonstrate improvements.

who were randomized to the comparison group. Consider an outcome, Y, such as the rate of emergency department visits one year post-training. The estimating equation is:

$$Y_{ihj} = \beta_0 + \beta_1 1(Treatment_h) + \beta_2 X_{ih} + \epsilon_{ihj}$$

where *i* denotes a consumer, *h* denotes a provider (since treatment is randomized at the provider level), and $j \in J$ denotes a domain of related outcomes. $1(Treatment_h)$ is an indicator variable equal to one if the provider was randomized to the treatment group and zero if the subject was randomized to the control group. β_1 is therefore the parameter of interest, the causal impact of being offered training. X_i is a vector of control variables, which includes cohort (since this is correlated with treatment probability) and covariates (described below) that are uncorrelated with treatment but may be predictors of the outcome and therefore improve precision.

We will also estimate the direct effect of training, which differs from the ITT estimate in cases where we observe non-compliance with treatment assignment and either treatment group participants do not complete training or control group participants are able to complete training. We will therefore estimate the impact of the intervention on compliers using a local average treatment effect (LATE) estimated using Two-Stage Least Squares (2SLS). For subject *i*:

$$Y_{ihj} = \Pi_0 + \Pi_1 1(Training)_h + \Pi_2 X_{ih} + \vartheta_{ihj}$$

The first stage is:

$$1(Training)_h = \alpha_0 + \alpha_1 1(Treatment)_h + \alpha X_{ih} + \omega_{ihj}$$

The estimate of Π_1 is the local average treatment effect, estimating the average treatment effect for participants who completed training if randomly assigned to the treatment group. From past program data, we expect take-up to be approximately 85%. We will estimate local average treatment effects using different measures of take-up. These measures include participants attending at least one class, completing the training, and the number of classes completed.

Consumers may have different degrees of exposure to the intervention, resulting from differences in the number of authorized IHSS hours, providers caring for multiple consumers, and changes in consumer-provider relationships (such as ending a care arrangement or taking on a new consumer) during the study period. To account for these differences, we will also perform weighted least squares (WLS) analysis for both the ITT and LATE estimates of consumer outcomes, weighting each participant by the mean weekly IHSS hours worked during the study period. We cluster standard errors at the provider level because treatment is at the provider level.

Controls

Variables that are strong predictors of outcomes will be used as controls. Pre-study or control group data will be used to determine which variables to control for. Potential variables include: language, ethnicity, number of authorized IHSS hours, age of consumer, gender, education level, and socioeconomic status as well as pre-treatment values of the outcomes of interest. We will also control for cohort to account for any time trends, changes to the intervention, or changes to the randomization procedure that occur between cohorts.

Treatment effect heterogeneity

This study is unlikely to be powered to distinguish effects within or between subgroups. Subgroup analysis will be exploratory to inform future areas of work and will be dependent on sufficient variation and subgroup sample sizes in the underlying data. We will consider investigating treatment effect heterogeneity by:

- Type of provider-consumer relationship (family vs. non-family; living in same household vs. not)
- Prior consumer health care utilization (0, 1, or 2+ total ED visits, inpatient admissions, or SNF admissions in the 12 months prior to randomization)
- Consumer IHSS eligibility categorization (aged, blind, or disabled) and number of hours assessed, if available
- Provider's number of consumers
- Language of instruction (English, Spanish) if the study recruits a sufficient number of Spanish participants.

Survey attrition

We will evaluate response rates for differential attrition between treatment and control groups. If treatment group assignment predicts whether a participant responds to the survey we will employ a correction procedure.

Multiple hypothesis testing

We have a multitude of outcomes across several distinct domains (consumer health care utilization; consumer health; provider knowledge and tasks; provider labor market outcomes; provider caregiving career outlook; and provider personal well being). To account for the multiple inference problem, for each outcome we report both per-comparison p-values and family-wise p-values adjusted for the multiple outcomes in that domain. The family-wise p-value corresponds to the probability of rejecting the null hypothesis of no effect on a given outcome under the null family of hypotheses of no effect on any outcomes in this domain. We do not adjust for multiple inferences across different domains that are conceptually distinct, as the program may have impacts on caregiver knowledge but not health care utilization for example. We also do not adjust for multiple inferences for robustness checks.

Statistical power

Table 1 lists minimum detectable effects for select outcomes in two scenarios: a sample size of 1000, based on status quo level of recruitment through cohorts one and two, and a sample size of 1470, which is the current target sample size that we are hoping to be able to recruit based on planned changes to recruitment to be implemented by CCA. We assume an even sample split, 80% power, and alpha = 0.05 and use publicly available data to estimate control group means and standard deviations. We report the minimum detectable intent-to-treat effect as well as the effect on compliers, assuming 80% take up, to better assess the plausibility of these effects. These calculations are conservative in that they do not include control variables, while in practice we will add controls from the baseline survey and administrative data that we expect to be strong predictors of the outcomes.

Currently, there is no experimental literature exploring the relationship between care workers' job quality, quality of care provided, and health care outcomes, though non-experimental literature exists in the nursing home setting (Ruffini 2022). Because of a lack of similar studies, setting power benchmarks is difficult. As a rough guide, based on conversations with CCA and a review of the literature, we have targeted minimum detectable effects for compliers of ten percentage points or less on the dichotomous outcomes shown and $1000 in monthly Medicaid spending to consider ourselves well-powered for this evaluation. While we are able to detect meaningful impacts if recruitment stays steady; power improves significantly, with MDEs consistently below our targets, if the sample size can be increased to our current object of 1470. We present both sets of estimates given the uncertainty over future recruitment.

Other research on wage increases (either similar in magnitude or smaller than CCA's training stipend alone, albeit over a more sustained period) have found impacts on job retention after a year ranging from three (Ruffini 2022) to 17 percentage points (Howes 2005). Sectoral training programs likewise have demonstrated sustained gains in employment in the target field. Project QUEST, for example, increased the share working in health care by 12 percentage points 6 years after the start of the program (Roder and Elliott 2018). For ER utilization, caregiver involvement and education have been identified as components of successful ER reduction interventions (Jehloh et al. 2022). A systematic review found successful educational interventions reduced ER use from 21% to 80%, though not all studies were RCTs and several that were found null results (Morgan et al. 2013). We are powered to detect effects comparable to those found in some ER diversion programs, which offer in-home or clinic based services when patients request EMS transport to the ER (Schaeffer et al. 2009).

Transparency and reproducibility

This study was pre-registered in the AEA RCT Registry (AEARCTR-0012289) and at ClinicalTrials.gov (NCT06205433) (Autor et al. 2023). This analysis plan is being archived after study enrollment and data collection have begun but before analyzing any outcome data. Delaying creation of the analysis plan until after launch allowed us to better understand the data that could be used in the study and specify our plans in greater detail. The analysis plan will be posted to AEA RCT Registry. We will make de-identified data (to the extent allowed by data use agreements) and the code used in analyses publicly available so that other researchers can reproduce, extend, and adapt our analyses.

Table 1: Minimum detectable effects depend on recruitment

		N = 1000				N = 1470			
		ITT		TOT		ITT		TOT	
	Control group mean (SD)	MDE	% of control group mean	MDE	% of control group mean	MDE	% of control group mean	MDE	% of control group mean
IHSS job retention rate	0.7	7.8 pp	11 %	9.7 pp	14 %	6.5 pp	9 %	8.1 pp	12 %
ER utilization rate	0.47	8.9 pp	19 %	11.1 pp	24 %	7.3 pp	16 %	9.1 pp	19 %
12-month fall rate	0.3	8.4 pp	28 %	10.5 pp	35 %	6.9 pp	23 %	8.6 pp	29 %
Monthly Medicaid spending	1405 (4872)	$864	62 %	$1,080	77 %	$712	51 %	$891	63 %

Note: MDEs are reported for 80% power and alpha = 0.05, assuming an even sample split. Take-up for TOT results is 80%. Control group means come from Howes (2005) (job retention), California Long Term Care Education Center (2021) (ER utilization), CMS (2016) (fall rate), and HHS (2008) (spending).

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