Pre-Analysis Plan for

Elevating Families: Randomized Evidence on Goal-oriented Case Management for Low-income Parents and their Children

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PRELIMINARY DRAFT

Contents

I Overview

This is a pre-analysis plan for the randomized evaluation of the Elevating Families program administered by Catholic Charities Northern Nevada in Washoe County, Nevada. We begin with background and motivation for the study in Section II. Section III describes the research questions, study population, eligibility criteria, and enrollment and randomization processes. Section IV describes our planned data sources, and Section V defines primary and secondary outcomes. In Section VI, we present and discuss our power calculations, and we conclude with a discussion of our empirical strategy in Section VII.

II Background

A Policy Relevance

Children born into poverty in the U.S. often struggle to escape it. One third of children born into the bottom income quintile will still be there when they are thirty; over half will still be in the second lowest quintile or below (Chetty et al., 2014). Poverty disrupts child wellbeing and human capital development through a number of channels including excess stress, unstable home environments, and exposure to violence (Engle and Black, 2008; Banovcinova et al., 2014). Unfortunately, the barriers to elevating households with children out of poverty are often complex and multi-faceted: "losing one's home to crippling medical debt, being a single parent, recovering from an addiction, having a criminal record, being an undocumented worker, not having adequate transportation to reliably get to a job" are common examples of impediments to upward mobility (Evans et al., 2023).

The complexity of poverty suggests that programs targeted at a single cause, like job training, might fail to move the needle when more holistic approaches are needed. In addition, poverty-induced stress has been shown to impede the cognitive functioning needed to make and stick to long-term plans, making economic mobility even more difficult in the presence of complex barriers (Mullainathan and Shafir, 2013). Thus, anti-poverty programs designed flexibly enough to help families with a multitude of challenges and train parents in goal-oriented problem solving in the face of stress could reduce intergenerational poverty.

This study will evaluate a form of programming that has emerged in response to this view of poverty: comprehensive goal-oriented case management. The program is entitled *Elevating Families* and is provided by the social service provider Catholic Charities of Northern Nevada (CCNN). *Elevating Families* combines intensive coaching, goal-setting, and financial incentives to help low-income parents achieve economic stability for them and their children. Over the past 7 months, CCNN have successfully enrolled 99 participants across 90 households, achieving a take-up rate of 90%. Survey data collected from these households reveal numerous and significant barriers to economic mobility. The typical parent is Hispanic or Black, has a high school diploma, and earns $16,391 per year. A large majority (80%) of these parents report not being able to meet essential household expenses in the past year, about half (45%) feel they never or rarely get the social and emotional support they need, and over 30% screen positive for clinical symptoms of depression or generalized anxiety.

Elevating Families is an implementation of the Mobility Mentoring model for goal-oriented case management. Mobility Mentoring was developed by Economic Mobility Pathways (EMPath) in Boston and has been adopted by over 395 non-profit organizations across the U.S. (Engle et al., 2022). In this model, each participant is assigned a professionally trained mentor. Mentors first help participants set goals across five domains key to economic success: family stability, health and well-being, financial management, education and training, and employment and career. Goals are structured sequentially and are meant to function as stepping stones toward economic independence. These goals are then linked to financial incentives which, unlike previous workforce programs, are quite varied and highly individualized to the participant's mobility goals. Mentors give participants practical assistance in achieving their goals; e.g., referring them to education or job training programs, finding alternative approaches to attaining goals when original plans fail, helping them persist when they feel overwhelmed, and tracking progress. After a year of piloting, CCNN received accreditation from EMPath to call the *Elevating Families* program an implementation of the Mobility Mentoring model.

Family life is one of the five focal areas of the Mobility Mentoring intervention and is often a key rationale for policymakers in providing these supports to low-income families. There are two main channels through which the program could benefit children. First, children could benefit from improvements in household resources, such as higher parental wages or reduced parental stress. A large literature on both cash and in-kind safety net programs documents positive effects of material resources on student achievement and wellbeing (Aizer et al., 2022; Hoynes and Schanzenbach, 2018).

Second, participants are encouraged to participate in one of three parenting classes with trained specialists. These programs teach positive parenting behaviors, such as how to utilize routines and rules to promote emotional well-being, and help parents navigate difficult barriers, such as a criminal history, parenting as a teenager or young adult, substance use, or a history of child neglect. In addition, mentors help parents monitor their children's academic performance and provide strategies on how to improve student effort. Prior studies have found that equipping parents with better information about their child's academic performance can substantially boost educational outcomes (Bergman, 2021; Bergman and Chan, 2021; Barrera-Osorio et al., 2020).

Incorporating effects on children may fundamentally change a cost-benefit assessment of the program, or its marginal value of public funds (Hendren and Sprung-Keyser, 2020). For example, the re-evaluation of Moving to Opportunity revealed that children who moved to lower-poverty neighborhoods when they were young experienced large earnings gains in their twenties, even though initial studies had found null effects on earnings for parents and older children. If this program were to meaningfully change the trajectory of children's schooling outcomes and earnings, the compounding nature of children's future outcomes could even lead program benefits to exceed program costs.

B Academic Relevance

The key differentiator of this study from the other randomized evaluations of comprehensive case management is its unique position to examine the intergenerational effects of Mobility Mentoring on children of program participants. All 600 households enrolled in this study will have at least one child under the age of 14, and the average household has 2.2 children living in the household. The research team has already received IRB approval to passively track outcomes of children in administrative records. The team has also secured a data sharing agreement with the Washoe County Human Services Agency (WCHSA) to collect data on parental interactions with child protective services (CPS) and have a verbal agreement (and are finalizing a formal one) with Washoe County Schools and the Nevada Department of Education, Training, and Rehabilitation (DETR) to obtain parental data on employment, earnings, and education (including certificates).

This study is poised to provide an important contribution to the literature on child maltreatment, home environment, and child educational achievement, as well as their link to parental labor market outcomes. Much of the existing causal work is identified by negative shocks to the home environment like placement into foster care (Doyle Jr, 2007; Gross and Baron, 2022) or incarceration-induced removal of a parent/sibling (Arteaga, 2023; Dobbie et al., 2018; Norris et al., 2021). Findings have been mixed in both of these contexts, and there is even less evidence on how a positive shock like enrollment in family-focused case management might affect child outcomes. Our ability to jointly observe CPS and schooling outcomes will allow us to see whether reductions in maltreatment are associated with increased child educational achievement.

This paper would also contribute to scientific knowledge on the efficacy of comprehensive case management for low-income adults. Tebes is a co-author on the Amp Up evaluation and LEO is involved with three of the other four aforementioned randomized evaluations (Amp Up Boston, Padua, and Bridges to Success). All four research teams have already discussed a future research collaboration that would share data across the various implementations of the Mobility Mentoring model to estimate more precise causal effects on labor market outcomes. This collaboration would also allow for a more detailed and coordinated exploration of treatment effect heterogeneity, program intensity and implementation fidelity, and external validity across a mix of different labor markets, and geographic and demographic sub-populations.

III Study Design

We are implementing a randomized controlled trial (RCT) evaluation to estimate the impact of Mobility Mentoring on families three years after study enrollment. In conjunction with EMPath and CCNN, we have designed the study's systems to mirror those of the Amp Up evaluation: the intake survey, programmatic data systems, and incentives are all modified versions of the ones employed in Amp Up. This similarity will allow us to compare and contrast the effects of Elevating Families with those of Amp Up and future programs as part of a larger evaluation effort.

A Eligibility Criteria

Applicants are eligible for the *Elevating Families* program only if they are eligible for the study and consent to participate in research. While we do not collect or document assent for children, we do collect parents' consent on their behalf. In order to be eligible for the study, an applicant must meet the following criteria:

- Be between the ages of 18 and 55

- Live in Washoe County, Nevada
- Have at least one child below the age of 13
- Be willing and able to work:
  - Have a Social Security Number (SSN) or Individual Taxpayer Identification Number (ITIN)
  - Not prevented from work by a disability
- Not be convicted of a violent or sexual crime
- Earn at most $65,000[1]
- Not share a household with separate applicant to the *Elevating Families* program[2]

The 18-55 age cutoff captures parents who are working age for both the duration of the intervention and several years after the intervention concludes. The child age criteria exists because CCNN wants to focus their limited resources on families with young children. This criterion also enables the research team to track child academic engagement and success over a longer time horizon.

B Recruitment & Enrollment Procedures

Recruitment and enrollment into the study will proceed according to the following steps:

1. CCNN advertises using online ads, flyers, and in-person outreach at community events.
2. Potential applicants fill out an initial interest form and eligibility screening on the CCNN website or on tablets during in-person community outreach events.
3. CCNN's enrollment specialist then contacts the applicant by phone to confirm their eligibility for the program. During these calls, the applicant is informed of the existence of the study and that the opportunity to receive the *Elevating Families* service is randomly assigned. Contingent on their continued interest in the study, the applicants are then offered the option of undergoing the consent and enrollment processes virtually or at an in-person meeting:
   - Virtual Enrollment: the enrollment specialist provides additional information about the applicant's rights under the study and the data collection process. If the participant verbally affirms their interest in the program, the enrollment specialist will then email them a link to a Qualtrics survey that houses the consent form, documents consent, and asks a number of baseline questions. The questions

[1] This translate to 65% of the living wage for Washoe County as calculated by MIT's Living Wage Calculator.

[2] This prevents treatment-control group contamination. Members of the same household wishing to enroll in the study must apply jointly in the same randomization cohort. In this case, one adult is listed as the primary recipient and is expected to engage fully with the program, though the other adult will receive the same randomization group assignment as the primary recipient.

cover topics such as household demographics, education, employment, finances, health, and parenting. As they fill out the survey, CCNN's intake staff remain on the call to assist with any technical difficulties and answer questions about the program, study, or survey.

- In-Person Enrollment: the enrollment specialist will meet with them at a predetermined location, such as the CCNN campus in downtown Reno. They will provide the applicant with more information about the study, answer any questions they may have, and conduct the informed consent process for the applicant and their children. After consent is recorded by a signed consent form and statement, applicants will fill out the same baseline survey as in the remote case. CCNN staff are present throughout this process to clarify any questions that arise.

4. At the end of the survey, enrollees are compensated for their time with a $25 gift card.

5. At the end of each month, the research team randomly assigns families who consented to the study and completed a baseline survey into treatment or control groups:

   - Treatment Group: the treatment group is offered the *Elevating Families* service.
   - Control Group: a resource coordinator from CCNN reaches out to the control group via email or phone to provide information on other supports that are available to all applicants. These supports primarily include access to a food pantry, information on available public services, and (when funding is available) small amounts of rental assistance.

C Randomization Procedures

We implement a matched-pair randomization design that ensures study group balance on reported earnings. Specifically, we rank applicants by employment status, annual individual earnings of the primary household applicant, the number of months the primary applicant was employed in the past 12 months, and household income. Then, we sequentially form "matched pairs" of households and randomize one household within each matched pair into the treatment group and one into the control group. In the event of an odd number of applicants in a given month, we randomly select either one-third or two-thirds of the final triple to be randomized into treatment. This way, on average, half of all households are assigned to treatment. Such stratification can substantially improve precision (Bai, 2022).

IV Data

Our primary research questions require three data sources: earnings and employment records provided by the Nevada Department of Education, Training, and Rehabilitation; academic and disciplinary records from Washoe County Schools; and referral, investigation, substantiation, and removal records from the Washoe County Human Services Agency. Measures of financial health will come from Experian Credit Bureau data, while most other secondary outcomes, such as labor market experiences, engagement in alternative jobs programs, gig economy work, and mental and physical health measures will come from a follow-up survey (if funding is available). We also hope to secure administrative records on public benefits record utilization. CCNN's Apricot database will be used to document participant enrollment, engagement and services utilizes. They will also capture standard CCNN services used by the control group unrelated to the program. Below we provide additional information on each data source.

A Nevada Department of Education, Training, and Rehabilitation (DETR)

Quarterly data on employment and earnings from DETR will allow us to gauge the effect of *Elevating Families* on participants' labor market outcomes. We will use these data to construct a labor market index and then track its development for the treatment and control groups in the post-randomization period.

Ideally, these data will reach back eight quarters prior to a participant's date of randomization. Pre-assignment wage data will show participant's employment trajectory leading up to random assignment, allowing us to assess whether participants enter the program following a negative shock from which they naturally recover during the post-period (i.e. mean reversion). These data also improve precision by allowing us to control for earnings at baseline. If long-term funding permits, we will collect wage data for as long as ten years after random assignment, allowing for estimation of program impacts seven years after program completion.

Additionally, the DETR data may contain information on enrollment in state-funded job training programs or benefits. Contingent on the details of the data use agreement, we will receive these data twice a year. To collect these data, we will send files of study participant's SSNs to DETR and arrange for them to extract data from their centralized data.

B Washoe County Schools (WCS)

We would receive both student information and academic outcomes on children of study participants from WCS. Based on datasets that other organizations have received from this organization, the student information could include date of birth, place of birth, gender, ethnicity and race, and language information. Academic outcomes might include cumulative grade point average, standardized test scores for math and English language arts, WCS risk index scores (attendance, transiency, retention, suspension), disciplinary records, and attendance records. Importantly, we could also access parent's engagement with their child's academic performance by accessing use statistics for Infinite Campus, the district's grading portal, which both parents and students are able to monitor.

C Washoe County Human Services Agency (WCHSA)

We have executed a data sharing agreement with WCHSA and will receive data relating to parent participants' interactions with that organization. These will include the date of occurrence and category (neglect, mistreatment, etc.) for information-only calls, information and referral calls, investigations, substantiations, and case transference into permanency.

D Experian Credit Bureau

To analyze program impacts on debt, credit, and general financial health, we hope to link the study sample to private consumer credit agency data maintained by Experian. These data provide comprehensive consumer credit and borrowing information gleaned from public records, collection agencies, and trade lines (such as credit card balances, auto loans, and mortgages). These data could provide quarterly measures of credit balances, credit scores, unpaid bills, delinquencies, and bankruptcy. We are exploring the possibility of securely linking these data to our study sample.

E Follow-Up Survey

Access to a follow-up survey would allow use to collect novel outcome data. In particular, we would be interested in the following: more detailed labor market outcomes (hours and wage), involvement in gig-work, participation in alternative jobs programs, mental and physical health, and questions assessing child well-being. Parental questions on the survey will closely mirror those asked in the evaluation of EMPath's Amp Up Program in the Boston area.

F CCNN Apricot Database

CCNN stores data on their interactions with study participants (treatment and control). For the treatment group, these include detailed case notes, goals set, bridges completed, assistance and incentives received, and other CCNN services utilized. These data will be key in generating the take-up measures outlined in section VII. For the control group, we will be able to access their records for the use of non-Elevating Families services at CCNN. This will allow use to better clarify the nature of comparison we are making when estimating program effects.

G Baseline Survey

Prior to randomization, all potential participants complete a baseline survey. This survey contains detailed demographic questions about the applicant, their children, and their household. These data include demographic information (race, age, gender, household characteristics, information on children for linkage purposes), educational attainment, employment, months worked, material deprivation, individual and household income, and personal identifiers for data linkage purposes, such as date of birth and SSN/ITIN.

V Outcomes

A Primary Outcomes

The goal of the *Elevating Families* program is twofold: to assist parents in becoming economically self-sufficient and to build a supportive home environment where their children can thrive. As such, our primary outcomes will also be two-fold, measured three years after random assignment. In addition to individual outcomes, these include indices designed to increase statistical power and capture the more holistic outcomes specified in our research questions. The exact variables that enter these indices may also change as we learn more about data availability and quality.

Following Kling et al. (2007), each component of the index will be standardized based on the control mean and standard deviation in the year prior to receipt of financial assistance. Each component will then be summed, assigning directions to components such that they appropriately point in a common logical direction, and then will be re-standardized using the control group mean and standard deviation of the aggregate index. All other standardized indices described in Section B will be constructed in a similar manner. Unless otherwise specified, we will aggregate outcomes to the annual level in *the third year post-assignment*. When data are available quarterly, we will complement these analyses with quarterly event-study figures.

1. Parental labor market outcomes:
   (a) Annual Earnings: we will report annual earnings for the primary recipient
   (b) Labor Market Index: we will construct an index a la Kling et al. (2007) that combines annual earnings, indicators for any earnings, earnings over $20,000 , and earnings over $50,000
2. Child well-being index that consists of two standardized indices a la Kling et al. (2007):
   (a) Schooling Index: this index will contain information on standardized test scores (math, ELA), GPA, attendance rates, on-time grade progression, and disciplinary incidents (count)
   (b) Child Protective Services Index: this will include the following outcomes: any CPS investigation, any CPS investigation for abuse, any substantiated CPS investigation, any CPS removal, months removed from parents (over the 3 years post randomization)

B Secondary Outcomes

Additionally, we will report disaggregated schooling and CPS outcomes for children of parents enrolled in the study.

1. Additional Labor Market Outcomes: we will report each component of the labor market index as well as other available outcomes (if data are available through an endline survey):
   - Household income
   - Individual hourly wage, defined as earnings / hours worked per week
   - Hours worked
   - Any earnings in gig-economy work not captured in UI records
   - Annual earnings in gig-economy work not captured in UI records
   - Indicator for being employed full-time

- Indicator for receiving additional certification or accreditation post-assignment
- Indicator for being employed in a higher-paying occupation relative to baseline
- Indicator for being employed in a high-growth sector (e.g. health care, IT, etc.) relative to baseline

2. Financial Health and Credit Index: we will adapt the indices used by Miller and Soo (2021) to measure credit access and delinquency. Our index will be composed of two distinct indices, a credit index and a delinquency index, that will also be reported separately in supplemental tables. The exact variables that enter these indices may change as we learn more about the available data from Experian.

   (a) **Credit Index**:
      - Credit score estimated using VantageScore[3]
      - Total amount of credit available on all accounts (Miller and Soo, 2021)
      - Total balance on all accounts in last 3 months

   (b) **Delinquency Index**:
      - Amount past due on trades presently 30 days delinquent reported in the last 6 months
      - Amount of debt past due held by third-party collection agencies
      - Total number of public record bankruptcies and tax liens
      - Total number of trades presently satisfactory that were ever 30 or more days delinquent or derogatory excluding collections

3. Health & Well-being Index: provided sufficient funding, the follow-up survey will collect the following health and well-being measures, which were included on the baseline survey. These will be combined into a singular standardized health and well-being index.

   - Participant displays major depression symptoms (i.e. has a score of 10 or higher on the PHQ-8 instrument).
   - Participant displays symptoms of generalized anxiety disorder (i.e. scores a 3 or higher on the GAD-2 instrument).
   - Participant reports being generally not too happy.
   - Participant reports having fair or poor health when asked about their general health.

4. Housing Stability: to the extent data are available, we will leverage homeless management information system (HMIS) records to explore utilization of homeless support services. If participants are unstably housed, mentors often focus on helping them secure housing first, making this is a key outcome for this subpopulation (Evans et al., 2023).

[3] A consumer's credit score uses their payment history, delinquencies, number of accounts, and credit applications to provide a numeric assessment of their "likelihood to be over 90 days delinquent on loans" (Miller and Soo, 2021).

5. Public Benefit Utilization, Enrollment in Alternative Jobs Programs, Utilization of Other CCNN Services: to the extent data are available, we will explore how the program offer affects receipt of government benefits, like SNAP, TANF, and Medicaid, engagement in other job search programs, and utilization of other CCNN services, like food pantry and rental assistance.

VI Power Calculations

The intended sample size is 600 households to be enrolled over two years, with 300 households assigned to treatment (offered a spot in Elevating Families) and 300 assigned to the control group (offered standard CCNN services). Much of our power calculations are based on the Amp Up Boston evaluation design. We currently observe a take-up rate of 90% among the 45 individuals who have been randomized into treatment. Given initial engagement, we assume a take-up rate of 85% in our power calculations, which is 15-20% higher than the take-up rate observed in the Amp Up study. We also assume 80% statistical power, 5% statistical significance, and 25% of the outcome variance is soaked up by the inclusion of a lagged dependent variable (and other control variables). Control group means for parental labor market outcomes come from baseline survey data collected for the first 90 primary participants enrolled in the study. Baseline rates of involvement with child protective services comes from historical estimates from CCNN.

We are powered to detect a 10.0 percentage point (18%) increase in employment (ITT) and a \$3,666 (22%) increase in annual earnings. Evans et al. (2023) find that the Padua program increased employment by 25% (ITT), which this study would be powered to detect. Furthermore, Evans et al. (2023) find that those who were stably housed saw employment gains of 37% (ITT), while those who were unstably housed experienced improvements in their housing situation but not employment. Given that this program works primarily with participants who are already stably housed and aims to provide a higher level of support, existing evidence points to employment effects above 30%. For example, detectable TOT effect sizes are about one-third of the gains observed for graduates of EMPath's flagship program in Boston. As discussed in the description of the Amp Up evaluation in Engle et al. (2022), the Mobility Mentoring model provides more comprehensive support than sectoral employment programs, and thus previously documented effect sizes for these programs serve as "a plausible lower bound on the range of policy-relevant treatment effects" (Engle et al., 2022).

As for child outcomes, we are powered to detect a 0.148 SD increase in our child well-being index (ITT). While prior estimates of Mobility Mentoring effects on children do not exist, comparisons to other work suggest this to be a plausible effect size. Work on the individual channels through which Mobility Mentoring might affect children, when combined, suggests reasonably large effects on our child well-being index. Estimates identified by EITC expansions suggest that a \$3,666 increase in household income (our MDE on parental earnings) could improve test scores by 0.22 SD (Dahl and Lochner, 2012). Likewise, a similarly-sized increase in unearned income among low-income households is associated with an approximate 36% decrease in referrals to CPS (Rittenhouse, 2023), 14% higher than our MDE for CPS referrals. On top of the financial resource channel, Mobility Mentoring aims to help parents stay up to date with child performance in school, and such information alone has been shown to significantly improve child schooling outcomes (Bergman, 2021; Bergman and Chan, 2021; Kraft and Rogers, 2015; De Walque and Valente, 2023). For example, providing parents with school achievement information for students with low baseline scores improves outcomes by 0.28 SD (Barrera-Osorio et al., 2020). These channels could potentially compensate for reductions in parental time spent with children resulting from increased parental labor supply (Bastian and Lochner, 2022).

VII Empirical Strategy

A Main Specification

Our primary specification estimates the *intention-to-treat* (ITT) effects of the offer to enroll in the *Elevating Families* program on outcomes. Our base specification is

$$y_i = \beta Treatment_i + \gamma y_i^0 + \alpha_{p(i)} + \epsilon_i \tag{1}$$

where y_i is an outcome for enrolled participant i, $Treatment_i$ indicates whether participant i was randomly assigned to the treatment group, y_i^0 is the outcome of participant i at baseline, and $\alpha_{p(i)}$ are matched pair fixed effects.[4] The coefficient of interest – β – estimates the average difference in outcomes between treatment and control groups, controlling for pre-randomization outcome levels. The standard errors will be clustered at the household level. We also plan to graph the event-study equivalent of Equation 1 that will allow for visual inspection of pre-trends and differential evolution of outcomes between study groups in the post-period. Earnings and employment outcomes will be explored at the quarterly level while other outcomes will be reported at the annual level.

To improve precision and account for chance differences in characteristics at baseline, we will report results when including a vector of controls captured at baseline (X_i'):

$$y_i = \beta Treatment_i + \gamma y_i^0 + X_i'\delta + \alpha_{s(i)} + \epsilon_i \tag{2}$$

The inclusion of X_i' will account for any variation in the composition of treatment and control groups. For parental outcomes, X_i may include gender, educational attainment, age, race, home language, marital status, household size, and other control variables captured at baseline. For child outcomes, it may include the child's age, gender, school (if applicable), and an indicator for enrollment in Head Start or similar programs. Additionally, we will include baseline household income, household size, parents' race, and parents' education level in the child controls. We will select baseline covariates based on two criteria: (A) treatment-control differences observed at baseline, and (B) how much of the residual outcome variance they are able to explain.

B Treatment on Treated Specifications

In addition to the reduced form estimates obtained from Equations 1 and 2, we are also interested in estimating the causal impact of Elevating Families on those who took up the

[4]Note that matched pairs are constructed within a given randomization block, such that matched pair fixed effects restricts comparisons to occur between pairs of applicants within the same randomization block.

service, or *treatment-on-treated* (TOT) effects. To this end, we will instrument for program enrollment or engagement with the study group assignment. Using a two-stage least squares regression, we will estimate the following first and second stage regressions:

$$D_i = \pi Treatment_i + \eta y_i^0 + X_i'\nu + \zeta_{s(i)} + \upsilon_i \tag{3}$$

$$y_i = \beta_D \widehat{D}_i + \gamma y_i^0 + X_i'\delta + \alpha_{s(i)} + \epsilon_i \tag{4}$$

where D_i is some measure of program engagement and $\widehat{D}_i$ is engagement as predicted by Equation 3. We plan to examine three measures of program engagement. The first is an indicator for whether the individual received *any* mobility mentoring services as part of the program; specifically, whether the participant attended at least one meeting with a mentor after completing the baseline survey. Second, we will explore an indicator for having any meaningful engagement with a mentor as measured by whether the participant completes EMPath's bridge assessment and sets at least one goal with a mentor. Third, we will measure a participant's cumulative treatment exposure or "dosage" as the fraction of months they were marked as "actively engaged" throughout the three-year program period.[5] Under reasonable assumptions, β_D captures the causal impact of enrollment/engagement in the program on outcome y_i.[6].

C Treatment Effect Heterogeneity

This study will recruit from a diverse population of low-income families and the program will likely have different effects within different sub-groups. Understanding which populations the program is most effective for will allow the research team to validate the program's theory of change and inform policymakers' decisions to target and scale mobility mentoring.

C.1 Parental Outcomes

Following Evans et al. (2023), we will explore how labor market effects vary by parental attachment to the labor market and housing stability prior to randomization. Evans et al. (2023) show that a similar program improved labor market outcomes for unemployed adults who had stable housing, and improved housing (but not labor market) outcomes for those who lacked stable housing. The main heterogeneity analysis of parental outcomes will explore differences in treatment effects by individual and household outcomes prior to randomization, as well as household demographic characteristics. In particular, we will examine outcomes for the following sub-groups:

1. *Employed at baseline*: using data from DETR, we will examine outcomes for study participants who were unemployed in the quarter prior to application for Elevating Families. According to our baseline survey, about half of all participants are employed at baseline.

[5] Participants are removed from "active engagement" status if they miss three consecutive meetings with program mentors.

[6] This approach relies on the assumption that there was no average effect of being offered program enrollment on those who did not take up the program and that the control group was not affected by losing the lottery.

Given our limited sample size, we will also present suggestive evidence on how labor market effects vary by unemployment spells, earnings, and household income during the year prior to randomization. In particular, since we sequentially match pairs on employment status, individual earnings, months worked in the past 12 months, and household income, we can non-parametrically graph pairwise labor market effects by pre-randomization labor market scores of matched pairs (or clusters of matched pairs).

2. *Stably housed at baseline*: using data from the baseline survey, we will split the sample based on their housing status. We will then separately analyze outcomes for the stably and unstably housed groups.

3. *Stably housed X Employed/Labor market scores at baseline*: Interacting the two variables above will allow us to test for differential treatment effects as found in (Evans et al., 2023). Labor market matching variables may prove more precise than an employment indicator and may allow for more evenly-sized (and thus powered) interacted categories.

C.2 Child Outcomes

Among the children of study participants, the main sub-group analysis will explore heterogeneity of treatment effects by demographic characteristics. We will split the sample based on the following sub-groups:

1. *Parental employment at baseline*: to investigate the role of parental earnings impacts on children's outcomes, we will explore the same parental sub-groups discussed above. If we observe earnings effects are mediated by an interaction of employment and housing stability or another parental characteristic at baseline, we will report effects on children by those categories as well.

2. *Above/below median academic outcomes*: using data from WCS, we estimate effects separately for children with above versus below median baseline academic outcomes as measured by a joint index of academic outcomes.

3. *Past CPS involvement*: using data from WCHSA, we will examine heterogeneity by whether there is any documented previous CPS involvement with the household

4. Child Demographics

   - *Gender*: using data from Washoe County Schools, we will split the sample of children based on whether they are identified as male or female in school records. We will then analyze outcomes for these groups separately.
   - *Above/below median age*: using data from Washoe County Schools, we will split the sample of children based on whether they fall above or below the median age of children at the time of their parents' application to Elevating Families.[7] We will also explore age effects non-parametrically, noting that program effects may non-linearly benefit children at younger ages.

[7] If data on students' ages are unavailable, we will use the child's age as reported in the baseline survey.

C.3 Exploring Heterogeneity using Machine Learning

To learn in a disciplined fashion which sub-populations benefit most or least from the program, we may draw on an emerging literature that leverages machine-learning methods to explore heterogeneity of causal effects (Chernozhukov et al., 2018; Athey and Imbens, 2015, 2019; Davis and Heller, 2017).

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