Pre-Analysis Plan
Impact Evaluation of Shelter Diversion

Daniel Tannenbaum
University of Nebraska-Lincoln

Bill Evans
University of Notre Dame

Stephen Billings
University of Colorado Boulder

August 2024

I. Introduction

On a given night, homeless services providers in the United States give shelter to nearly 350,000 people (National Alliance to End Homelessness, 2023). Emergency shelter stays are meant as transitional options, but are considered potentially harmful to individuals compared with other short-term housing arrangements, such as staying with friends and family, or longer-term placement in rental housing. Entering emergency shelters without referral to active housing support often leads to lengthy shelter stays for families. In one study, about one quarter of families who were randomly assigned to receive "usual care" in an emergency shelter spent over six months in emergency shelters during the follow-up period (Gubits et al., 2017). Most individuals who enter homeless shelters are facing acute, rather than chronic, housing issues, suggesting that light-touch shelter diversion programs may help families avoid initial shelter entry and avert lengthy future shelter stays (Evans et al., 2021). Diversion programs aim to help families and individuals experiencing housing crises find a place to stay through referrals and family connection, preserving limited shelter space for more difficult homeless cases and preventing any negative impacts of shelter stays. Service providers in many communities attempt diversion with their clients experiencing or at risk of homelessness. However, there is limited rigorous evaluation of shelter diversion programs and of what combination of services allows them to most effectively improve outcomes. For this reason, this study aims to contribute new knowledge to the literature by generating rigorous evidence on whether supplementing diversion conversations with flexible financial assistance and proactive case management effectively helps individuals experiencing housing crises to stay out of shelter and achieve a higher degree of long-term housing stability .

Family Promise of West Michigan, Family Promise of Spokane, and the Lord's Place (located in West Palm Beach) are three nonprofit organizations dedicated to fighting homelessness in their local communities. As part of this mission, these organizations participate in diversion conversations with clients who are experiencing housing crises, searching for solutions that could keep them out of homeless shelters. These organizations and the Wilson-Sheehan Lab for Economic Opportunities (LEO) at the University of Notre Dame want to examine the impact of supplementing these conversations with additional support and resources. They plan to launch a randomized controlled trial in which eligible clients will be randomly assigned to receive either a diversion conversation alone, a diversion conversation with flexible financial assistance available, or a diversion conversation, flexible financial assistance, and proactive follow-up case management. Researchers will track the outcomes of those in each treatment group who receive additional support and those in the control group who receive only the standard diversion conversation in order to make conclusions about the effectiveness of these new support services.

The purpose of this project is to determine the effectiveness of offering additional support along with a diversion conversation for clients experiencing housing crises. The results of this study will help LEO's partner organizations decide where they should allocate their resources and what

types of support will make the largest impact for their clients. Should flexible financial assistance alone or in combination with proactive follow-up case management be effective in increasing diversions from homeless shelters and future housing stability, it will provide each organization the opportunity to scale up these support services. The results from this study will also contribute to the general scientific knowledge on which interventions effectively prevent homelessness.

II. Evaluation Design

Research Questions

What is the impact of offering flexible financial assistance during diversion conversations, with and without proactive follow-up case management, on whether individuals are diverted from shelter or unsheltered homelessness, short- and long-term housing stability, and long-term outcomes such as benefits usage, income, and employment?

Eligibility

Diversion programs are designed to prevent homelessness for individuals who are at an immediate risk of becoming homeless, rather than to house people experiencing chronic homelessness or to prevent this risk from developing in the first place. Therefore, clients are eligible for the program if they are experiencing an immediate housing crisis and their situation is not deemed a better fit for another program. For example, someone who has been issued an eviction notice and must leave their home within 72 hours is eligible for the program, as it is designed to help them find housing to stay out of shelter. On the other hand, someone experiencing chronic homelessness would not be eligible because their situation may call for a more intensive intervention. Likewise, someone who cannot pay their rent that is due soon is also not eligible for the program as they are not imminently losing their housing and may be a better fit for a rental assistance program. All three social service partners have many clients who contact our partners for assistance and already receive diversion conversations.

Intervention

When a staff member from a partner organization first interacts with a client who is experiencing an impending housing crisis, they will speak with them to assess their situation and needs. If the staff member determines that the client could potentially be diverted and that financial assistance could have a positive impact on the likelihood of their being diverted, they will inform them about the study and possible availability of resources. If these eligible clients consent, they will be enrolled into the study. Participation in the study is completely voluntary and will not affect a client's access to standard care. Whether or not these clients give their consent to participate in the study, partner staff will use a randomizer provided by the research team to assign them to either 1) receive the standard diversion conversation with no additional resources, 2) a diversion conversation with flexible financial resources available, or 3) a diversion conversation with both flexible financial resources and follow-up case management. Clients are randomized regardless of their consent so that all eligible clients have the same chances of receiving additional resources, but only those who consent will be enrolled in the study for researchers to track their outcomes. Consent, enrollment, and randomization will take place either during the initial intake conversation or during another conversation shortly following it. After randomization, the diversion conversation will continue as usual, with the only change being that clients in the treatment groups will be offered flexible financial assistance. This assistance will initially be capped at $500, although more will be offered in

some circumstances if the additional resources would provide a more permanent solution to their housing crisis. For example, a family facing eviction may have the ability to pay a monthly rent but they may not have the resources for a security deposit. Case managers could go over the $500 cap to pay the security deposit in this case as the family would have a leased apartment as a result. If clients are assigned to receive proactive case management, partner staff will reach out to them once a week for up to 60 days after they have been diverted from shelter. Staff will not reach out to clients who are not assigned to proactive case management, but these clients may reach out to the providers for additional resources and will receive the support they request.

Randomization

Randomization odds are 50:25:25 for control and each of the treatment arms, respectively. All clients will have the same likelihood of receiving the treatments, whether or not they consent, but they will only be included in the study sample if they consent to participate. Randomization will take place within STATA, and a pre-randomized list will be called upon in a Qualtrics tool to avoid having partners handle long strings of exclusively one treatment arm which may exceed or underutilize their capacity.

Power Calculations

The target sample size is 3,200, and we estimate it will take us 2.5 years of enrollment to obtain this sample. We plan to track interim outcomes while the study is still enrolling.

We primarily plan to compare negative diversion outcomes, such as entering a shelter or hotel or unsheltered homelessness, between clients who had access to flexible financial assistance and those who did not. In data acquired from partner organizations, we observe 48% of clients receiving only diversion conversations experience a negative outcome, as defined above, within 60 days of an initial diversion conversation. With an expected take-up rate of at least 90% for financial assistance and 3,200 participants in the RCT, we will be able to detect a 5.2 percentage point decrease in the proportion of study participants who experience such a negative outcome.

We will also compare outcomes between clients who received organization-initiated case management and flexible financial assistance to clients who received only client-initiated case management to determine the impact of organization-oriented case management in conjunction with flexible financial assistance. 2.5 years of enrollment will give us a sample size of 1,600 for this comparison. Using data from partner organizations, we observe that 41% of diversion clients offered flexible financial assistance experience a negative outcome within 60 days of an initial diversion conversation. After 2.5 years of enrollment, with our expected take-up rate of 60% for organization-initiated case management, where take-up is defined as at least one extra call with a case manager after the initial diversion conversation, in addition to the flexible financial assistance, we will be able to detect a 10.89 percentage point decrease in the proportion of study participants who experience a negative outcome within 60 days of an initial diversion conversation.

III. Key Data Sources

The following section summarizes the planned primary data sources for this project. Notably, securing access to these data sources is partially complete. Given this, any outcomes for which we do not already have data secured may ultimately be excluded if there are barriers to gathering the needed information.

A. Family Promise of West Michigan, Family Promise of Spokane, The Lord's Place

We plan to use baseline data from partner organizations that is collected during the diversion conversations with clients. These data include HMIS/CMIS number, client name, date of birth, Social Security Number, race, ethnicity, gender, income, other demographic and household information, and information on the client's background including history of housing and homelessness. We will also receive program engagement data including financial assistance dates and amounts, as well as dates of proactive case management sessions. LEO has data sharing agreements with the partner organizations.

B. Regional Continuums of Care in Michigan, Washington, and Florida

We plan to use data from regional continuums of care (CoC) and their HMIS administrative data to measure the effect of the intervention on prior and subsequent engagement with any homeless service providers publicly contracted with the regional CoCs. LEO has data sharing agreements with Grand Rapids area Coalition to End Homelessness Continuum of Care, Spokane regional Continuum of Care Board, and Palm Beach County Continuum of Care to collect this data on study participants.

C. Infutor Data Solutions

We plan to measure housing stability using data from Infutor Data Solutions. Infutor collects data on consumers' living addresses in the United States. We will use this data to track individuals' address histories, allowing us to gauge housing stability based on how frequently an individual's address changes over time. LEO has an existing relationship with Infutor which will allow us to collect this data on study participants.

D. Experian

We plan to measure the impact of the intervention on participants' credit score, use of credit, and total balance in delinquent accounts using data from Experian. LEO has an existing relationship with Experian which we will use to link records in this study with Experian's credit data.

E. Secondary Outcomes

In addition to our primary outcomes of interest of successful diversion and shelter entry, housing stability, and credit outcomes, we are interested in pursuing data on potential secondary outcomes. These include outcomes such as usage of government benefits, income, employment, and criminal justice system involvement. We are still in the process of codifying these administrative sources in formal data sharing agreements.

IV. Hypotheses: Analysis by Outcome Domains

A. *Homelessness*

1. Primary Measure: Negative diversion outcome index
    a) Indicator for diversion exit into shelter, hotel stay, or unsheltered homelessness within the 60, 120, and 365 days following randomization, constructed from HMIS service records. This index

will equally weight each of the following standardized elements. We will also report average treatment effects for each component of this index separately.

b) Hypothesis: The intervention is designed to decrease negative diversion outcomes. We will test the hypothesis that there is no effect of the intervention on shelter or hotel admittance and unsheltered homelessness, and we can conclude the intervention worked if we can reject the null and there is a statistically significant decrease in negative diversion outcomes for treatment group participants.

2. Complimentary primary measure: short-term positive diversion outcome index
    a) Indicator for diversion exit to temporarily or permanently staying with family/friend, renting housing unit, or owning housing unit within 60, 120, and 365 days following randomization, constructed from partner organization diversion service records. This index will equally weight each of the following standardized elements. We will also report average treatment effects for each component of this index separately.
    b) Hypothesis: The intervention is designed to increase positive diversion outcomes. We will test the hypothesis that there is no effect of the intervention on exits to staying with family/friends or renting/owning, and we can conclude the intervention worked if we can reject the null and there is a statistically significant increase in positive diversion outcomes for treatment group participants.
    c) For many, a positive outcome will be just one minus a negative outcome. However, some client's outcomes are not known so the compliment is not always known, which is why we focus on both outcomes.

B. *Housing Stability*
1. Primary Measure: long-term housing stability
    a) Constructed using address histories in the Infutor data
    b) Hypothesis: The intervention is designed to increase housing stability. We will test the hypothesis that there is no effect of the intervention on having a permanent address, and we can conclude the intervention worked if we can reject the null and there is a statistically significant increase in permanent addresses one year following randomization for treatment group participants.
    c) As the Infutor data is constructed from such things as utility bills, magazine subscriptions, driver's licenses, etc., showing up in the

Infutor data is a sign that the person has established some degree of permanence.

2. Secondary Measure: self-reported housing stability and status
    a) Since unsheltered homelessness and some positive housing outcomes are often not measurable through HMIS/CMIS records, we are exploring the possibility of doing 12-, 18-, or 24-month follow-up surveys to more accurately measure longer term housing stability outcomes. The research team piloted a phone-based long-term follow-up survey with LEO's summer intern cohort. They conducted approximately 400 phone interviews of clients that connected with Family Promise of West Michigan 12, 18, or 24 months ago outside of the study sample. The survey asked about changes to the household's housing situation since they sought out services and evaluated their current housing stability. The goal of the pilot was to ascertain whether collecting long-term outcomes of shelter diversion via a phone survey is viable. More than half of client phone numbers were still valid, making a phone survey for long-term outcomes a distinct possibility.
    b) While the study-specific follow-up survey has yet to be designed, key outcomes will include categorical variables representing self-reported housing stability in likert scales and categorical variables of housing status over the specified time period (categories reflect standard HMIS entry/exit destinations). We will also report these categories in terms of the number of nights a participant spent in a given housing status.
    c) Hypothesis: The intervention is designed to increase housing stability. We will test the hypothesis that there is no effect of the intervention on self-reported positive housing stability, and we can conclude the intervention worked if we can reject the null and there is a statistically significant increase in self-reported positive housing stability one year following randomization for treatment group participants.

C. Credit

1. Primary Measure: total balance in delinquent accounts
    a) Constructed with Experian data listing this value over time. When analyzing the effect of the intervention on Experian outcomes, we may restrict the sample to the set of individuals who have a credit record prior to random assignment. We expect roughly 50 percent of individuals to meet this criterion.
    b) Hypothesis: The research team will test the hypothesis that there is no effect of the intervention on the total balance in delinquent accounts for study participants. We can reject the null if there is a

statistically significant decrease in the total balance in delinquent accounts for treatment group participants.

D. Criminal Justice Contact

1. Primary Measure: Record of any arrest, incarceration or court case
    a) We will do a full name and date-of-birth match between our sample of diversion participants and local administrative records for arrest, incarceration or court case (depending on data availability).
    b) Hypothesis: The research team will test the hypothesis that there is no effect of the intervention on contact with the criminal justice system. We can reject the null if there is a statistically significant decrease in any arrest, incarcerations or court cases.

V. Balance Checks

A. Head of household demographics

1. Race
2. Ethnicity
3. Gender
4. Age
5. Marital status
6. Veteran status
7. Disability status
8. Employment status

B. Household characteristics

1. Household income, where available
2. Number in household

C. Prior Use of Housing Services

1. Number of contacts with homeless service providers in two years leading up to randomization

VI. Treatment Effect Heterogeneity and Sub-group Analysis

Given that this study will recruit a broad range of households, the intervention will likely have different effects within different sub-groups. Understanding whether the intervention works broadly, for some sub-population of policy interest, or is most beneficial for some surprising sub-sample provides crucial information to governments on how they might scale this program in the event of a positive finding.

The study will estimate the impact of flexible financial assistance with and without proactive case management across several outcome categories and subgroups. The research team is interested in determining whether the intervention is more effective for certain populations relative to others. Data from partner organizations describing their client base tell us that the sample will be majority black and female clients, and we need to cut the sample into larger, more equally sized groups. This descriptive data also tell us that approximately half of eligible clients have zero monthly household income. Household size and prior HMIS contact similarly vary.

A. Household income

1. Households with zero reported monthly income vs. households with some income

B. Prior use of homeless services

1. Households with some amount of prior contact with CoC-affiliated organizations vs. households without/with less prior contact with CoC-affiliated organizations
2. Households with a prior shelter enrollment at CoC-affiliated

C. Household constitution

1. Households with children vs. households without children
2. Households 2 and fewer vs 3 and above

VII. Data Analysis

A. Main Specification

We will estimate intent-to-treat (ITT) treatment effects by OLS using the following regression:

$$(1) \qquad Y_i = T_{1i}\theta_1 + T_{2i}\theta_2 + X_{ist}\theta_3 + \mu_{1s} + \lambda_{1t} + \epsilon_{1ist}$$

where Y_i is the outcome. T_{1i} is an intent-to-treat dummy indicating the random assignment of person *i* to the flexible financial assistance only group, and T_{2i} is an intent-to-treat dummy indicating the random assignment of person *i* to the flexible financial assistance and case management arm of the experiment. The vector X_{ist} includes a set of person-level characteristics collected at baseline. We will also control for provider fixed effects (μ_{1s}) and month/year fixed effects (λ_{1t}), and ϵ_{1ist} is an error term. The coefficient on the treatment dummies, θ_1 and θ_2 will give us the difference in means between the treatment and comparison groups for each treatment group, or the estimated impact of the program. We will also test the null hypothesis that $\theta_1 = \theta_2$ or that the two treatment arms have the same impact.

B. Treatment on Treated Specifications

In addition to the reduced-form estimates obtained in the equations above, we are also interested in estimating the causal impact of diversion plus intervention participation, also known as the *treatment-on-treated* (TOT) effect. In this case, not all clients assigned to financial assistance will be able to use the resources. Likewise, not all people will engage with case management. To this end, we will estimate the TOT by instrumenting for intervention participation with treatment assignment.

There are two possible models. Using only those assigned to treatment group 1 (flexible financial assistance only) and the control group, we can examine the impact of the receipt of income on outcomes. In this case, let F_i be a dummy that equals 1 if the person was assigned to treatment group 1 and received some flexible financial assistance. The equation of interest in this case can be described by the equation

$$(2) \qquad Y_i = \beta_0 + F_i\beta_1 + X_{ist}\beta_3 + \mu_{2s} + \lambda_{2t} + \epsilon_{2ist}$$

As F_i is endogenous, we would need to use the assignment to treatment group 1 where the first-stage regression is then

$$(3) \qquad F_i = \Phi_0 + T_{1i}\Phi_1 + X_{ist}\Phi_3 + \mu_{3s} + \lambda_{3t} + \epsilon_{3ist}$$

We can so examine whether the size of the flexible financial assistance matters, where F_i can be replaced with the size of the assistance in dollars.

In a second model, we can use data from both experiments and consider the joint impact of financial assistance and case management on outcomes. In this case, let C_i be a dummy that equals 1 for clients that engage with case management where engagement is defined as at least one extra call with a case manager after the initial diversion conversation. In this case, the equation of interest is a variant of equation (2) where we add C to the model:

$$(4) \qquad Y_i = \beta_0 + F_i\beta_1 + C_i\beta_2 + X_{ist}\beta_3 + \mu_{2s} + \lambda_{2t} + \epsilon_{2ist}$$

And the first-stage regressions include a variable of (3) that now adds the dummy for T_{2i}

$$(5a) \qquad F_i = \Phi_0 + T_{1i}\Phi_1 + T_{2i}\Phi_2 + X_{ist}\Phi_3 + \mu_{3s} + \lambda_{3t} + \epsilon_{3ist}$$

And the second stage has an identical form with

$$(5b) \qquad C_i = \pi_0 + T_{1i}\pi_1 + T_{2i}\pi_2 + X_{ist}\pi_3 + \mu_{4s} + \lambda_{4t} + \epsilon_{4ist}$$

As with above, we also plan to replace F and C with actual values to see whether the intensity of interaction produces different outcomes. In all models, we will use heteroskedasticity-robust standard errors. There is some within-year variation in call volume, shelter capacity, and participant preferences for unsheltered homelessness, so standard errors will be clustered at the levels of the organization by month throughout the study time period.

C. Covariates

We plan to include the following covariates in our regressions:

1. *Head of household demographics*
    a) Race and ethnicity (set of mutually exclusive variables)
    b) Gender (1 = female, 0 otherwise)
    c) Age and age-squared

d) Marital status
e) Veteran status
f) Disability status
g) Employment status

2. *Household characteristics*
    a) Household income, where available
    b) Number in household
3. *Prior Use of Housing Services*
    a) Number of contacts with homeless service providers in two years leading up to randomization

D. Multiple Hypothesis Testing

The research team has limited their primary outcomes to those described above, which each fall under distinct domains. Classic p-values will be reported for all outcomes, which will provide a reader with full information that they can use to make multiple hypothesis testing corrections if they desire. To the extent we further explore secondary outcomes beyond those listed above, we will report traditional standard errors and p-values, as well as p-values that adjust for a family-wise error rate through a standard approach, such as the Westfall and Young step-down procedure. We will treat each data source (e.g., HMIS data, Infutor data, Experian data, etc.) as its own family when making these adjustments.

Testing multiple hypotheses raises the likelihood that any one hypothesis is found to be statistically significant purely by chance. We will report summary indices that aggregate multiple outcome variables within a common outcome domain. Aggregation not only improves the statistical power within a given domain but also vastly reduces the number of hypotheses examined. This plan pre-specifies what data will be collected, primary and secondary outcomes, the main specification, and subgroups of interest. By committing to a set of analyses in advance, we avoid concerns about data-mining and specification searching, and credibly commit to a few hypotheses that, together, comprise the central test of the diversion interventions. Classic p-values will be reported for all outcomes, which will provide a reader with full information that they can use to make multiple hypothesis testing corrections if they desire. We will also conduct non-parametric permutation tests and report permuted p-values for the main sets of analyses following Chetty et al. (2016).[1]

[1] This approach entails randomly re-assigning treatment status to households in the main sample and running the main specification thousands of times to simulate a counterfactual distribution of T-statistics. Relative to this counterfactual distribution, we can then compute permuted p-values as likelihood of observing our realized T-statistic. The same approach can be applied to sets of hypotheses to calculate the likelihood of observing by chance the magnitudes of treatment effects observed in the study.

VIII. References

Chetty, Raj et al. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment." *The American economic review* vol. 106,4 (2016): 855-902. doi:10.1257/aer.20150572

Evans, William N., et al. "Policies to Reduce and Prevent Homelessness: What We Know and Gaps in the Research." *Journal of Policy Analysis and Management*, vol. 40, no. 3, 2021, pp. 914–963., https://doi.org/10.1002/pam.22283.

Gubits, Daniel, et al. "Family Options Study: Short-Term Impacts of Housing and Service Interventions for Homeless Families." *SSRN Electronic Journal*, 2017, https://doi.org/10.2139/ssrn.3055272.

"State of Homelessness: 2023 Edition." *National Alliance to End Homelessness*, 23 May 2023, endhomelessness.org/homelessness-in-america/homelessness-statistics/state-of-homelessness/.