RESEARCH REPORT

Denver Supportive Housing Social Impact Bond Initiative

Evaluation and Research Design

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Denver Supportive Housing Social Impact Bond Initiative

Background and Context

The Denver Supportive Housing Social Impact Bond (SH-SIB) Initiative will provide supportive housing for individuals who are frequent users of both criminal justice and emergency medical services in the city of Denver, Colorado. In addition to experiencing homelessness and struggling with substance use and mental health problems, the target population commits frequent low-level offenses such as public nuisance violations, alcohol and drug use, panhandling, and trespassing. As a result, individuals in this population are frequently arrested and cycle in and out of jail, detox, and emergency services, effectively increasing costs across systems. Because they often do not receive follow-up services when they are released from jail, this population returns to the same risks and falls into a recurring cycle of negative outcomes. This cycle continuously results in high costs across city agencies and service providers.

The SH-SIB initiative will provide supportive housing to interrupt the status quo. Supportive housing is an evidence-based intervention that provides housing plus intensive case management and connects clients with community services, including primary health care (National Alliance to End Homelessness 2007).[1] Past research indicates that the joint provision of housing and services increases housing stability, improves mental and physical health, and decreases the number of low-level offenses. Together, those improvements lead to several desired outcomes for the city–decreases in the number of arrests, detox visits, and use of emergency medical services (Aidala et al. 2014; Larimer et al. 2009). Overwhelming evidence shows (1) that supportive housing is effective for chronically homeless adults who are frequent and costly users of public systems, and (2) that the cost of the program can be offset by its benefits (Culhane, Metraux, and Hadley 2002; Perlman and Parvensky 2006).

The city of Denver has identified "front-end" or frequent users who drive up the cost of public services, it has highlighted some of the gaps in service delivery for that population, and has identified an evidence-based solution—namely, supportive housing—to fill those gaps. Denver's SH-SIB initiative also gives researchers and other municipalities opportunities to understand how to efficiently target supportive housing to those users, to measure impacts, and to weigh the costs and benefits of the

program. The Denver SH-SIB will be one of the first supportive housing programs funded through a social impact bond financing mechanism. The program's structure, the investors the program attracts, the key performance measures for payment structures and thresholds, and the associated development of data tracking mechanisms will all contribute knowledge to the field and could lead to an expansion of supportive housing that uses this financing mechanism.

The SH-SIB initiative includes the following core partners (table 1).

TABLE 1

Initiative Partners for SIB Implementation

Role	Partner	Responsibilities
Local government	City of Denver	Repay investors if performance benchmarks are met
PFS (Pay for Success) lenders	To be determined	Provide capital to fund services Receive principal and interest when performance benchmarks are met
Intermediary	Denver PFS LLC (jointly owned by CSH and Enterprise Community Partners)	Manage service providers and facilitate PFS lender agreements and payments from the city to PFS lenders
Supportive Housing Providers	Colorado Coalition for the Homeless Mental Health Center of Denver	Provide housing through new tax credits and existing vouchers Provide supportive housing services
Evaluation	Urban Institute with local partners: the Evaluation Center at the University of Colorado Denver and the Burnes Institute	Establish research design Verify that performance benchmarks are met Measure other outcomes of interest

Program Structure

Target Population

The city's target population for the SH-SIB initiative includes frequent users of public services who increase the costs of such services by cycling in and out of jail and detox centers and by using emergency medical services. Eligibility criteria for the program must result in a group of such individuals that is large enough to fill the available housing units and to make up a separate control group. To establish a target group, the team used arrest data from 2012 through 2014 to identify individuals who had at least eight arrests over three years and who identified as transient (having no address or providing the address of a shelter) at the time of their arrest. The result was a sample size of approximately 1,456

individuals. Table 2 shows the target population's use of public services during the three-year period for homelessness, jail stays, and detox and other health services.

TABLE 2

System Use among Target Population with Eight or More Arrests over Three Years

Public service	Use
HMIS Shelter stays	
At least one stay in shelters over three years	62 percent
Two or more recorded stays over three years	33 percent
Jail days	
First year after eligibility	77 jail days
Second year after eligibility	45 jail days
Detox	
Per year after eligibility	3–8 detox visits

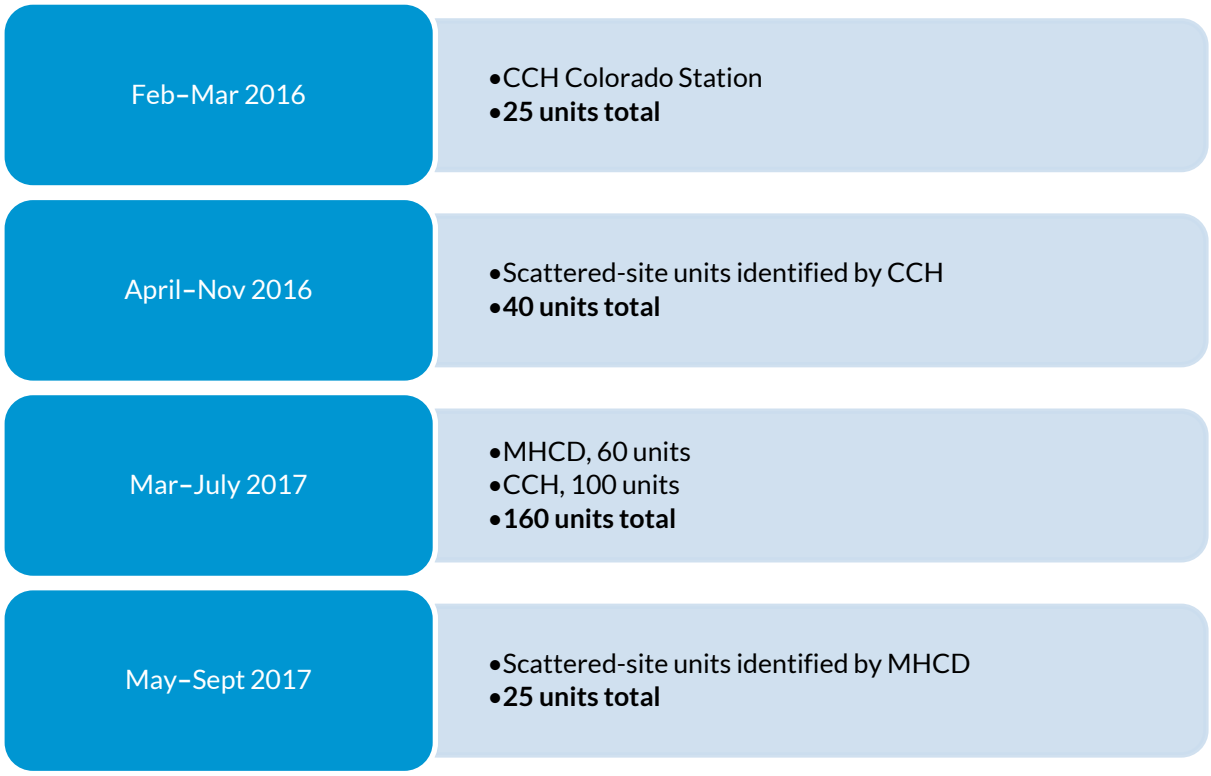
The data match to the Homeless Management Information System (HMIS) shows that 899 out of the 1,456 individuals had at least one shelter stay recorded in the HMIS over three years, and about a third had two or more recorded stays. Although we know that this population is also likely to experience types of homelessness not captured in HMIS data, the data confirm that our targeting criteria will indeed reach a homeless population. Similarly, data from a random sample of the target population show that individuals spent, on average, 77 days in jail in the year following their eligibility and an average of 45 days in jail in the second year following their eligibility. The target population also averaged three to eight detox visits per year over the three years following their eligibility (a range is used because of an incomplete data match for this system).

Housing Types

To meet its goal of providing 250 individuals with supportive housing units, the city will provide a combination of housing options. The units will include single-site homes in two new buildings built with low-income housing tax credits and also scatter-site units. The latter are existing rental housing units in the community that will be paired with a housing subsidy and services to convert them to supportive housing. The subsidies will come from the Colorado Division of Housing, the Denver Housing Authority, flexible subsidy dollars from the SIB transaction, and the Denver Continuum of Care. Housing is expected to become available according to the timeline in figure 1.

FIGURE 1

Housing Flow Timeline



Note: CCH = Colorado Coalition for the Homeless; MHCD = Mental Health Center of Denver.

Program Services

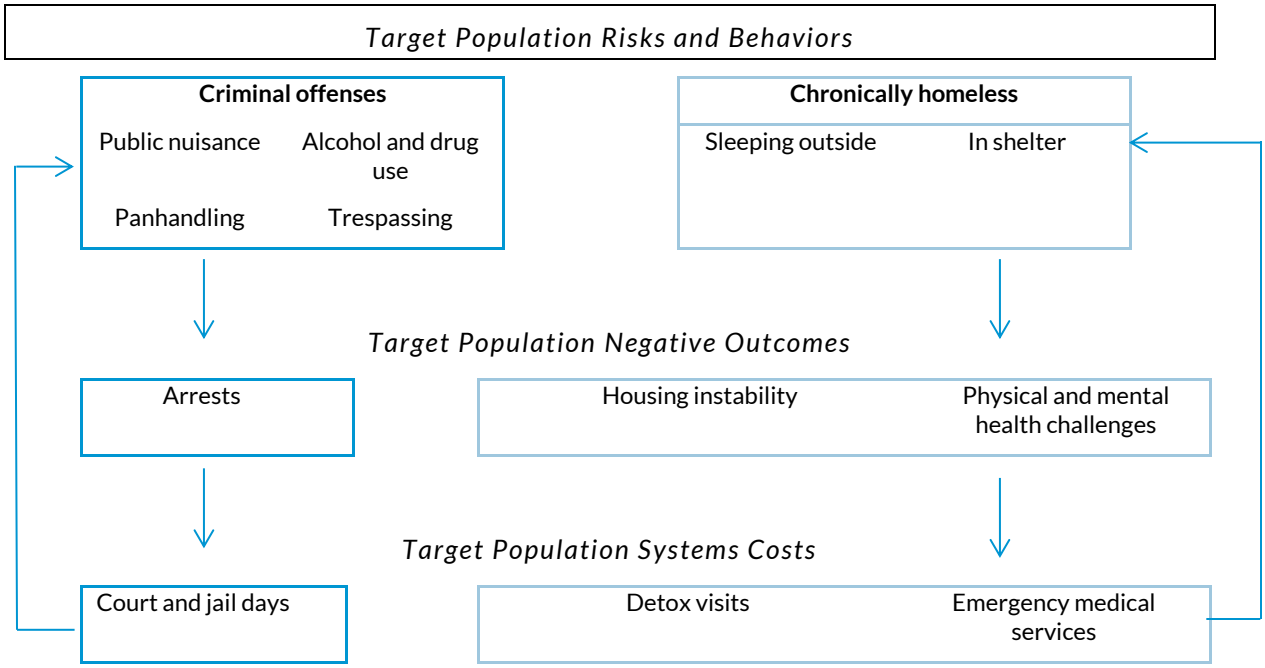
Supportive services such as case management will be provided by the Colorado Coalition for the Homeless (CCH) and the Mental Health Center of Denver (MHCD). Both organizations use modified models based on an assertive community treatment (ACT) model for supportive services, which is a highly integrated and intensive approach for delivering community mental health services. Other programs have demonstrated that variations on the ACT model can be implemented with great success. The SH-SIB initiative's partners (shown in table 1) will define the adaptation of ACT used for the initiative. In addition to receiving case management services, the target population will be enrolled in Medicaid through Colorado Access, the current managed-care network in Colorado.

Usual Care

When frequent users exit the multiple systems in which they are involved without receiving follow-up services, they return to the same risks and behaviors and experience a recurring cycle of negative outcomes, adding to the high system costs. Figure 2 depicts the cycle of usual care for this population.

FIGURE 2

Status Quo: Cycle of Target Population Outcomes and Costs



Overview of Evaluation

Theory of Change

The SH-SIB initiative provides supportive housing to a target population to break the cycle of jail, detox, and emergency medical services experienced by many front-end users. Supportive housing will integrate the services of multiple systems to provide care that results in increased housing stability and improved physical and mental health as well as fewer arrests. Along with supportive services, the intervention will provide a housing unit that is safe, sustainable, functional, and suitable for tenant

stability. An important difference between supportive housing and other models is its "housing first" tenet, not sobriety first.

The theory of change behind supportive housing is that once individuals in the target population are housed, they are not living on the streets, openly drinking in public spaces, panhandling, trespassing, or engaging in similar sorts of nuisances or crimes. Instead, they have a place to live and sleep. They may continue with substance use, though research shows modest reductions in substance use over time (Collins 2011).

As depicted in figure 3, the intermediate outcomes of this intervention include increased housing stability; reductions in homelessness, drug and alcohol use, and instances of public nuisance; and improvements in mental and physical health. These intermediate outcomes will result in several intended program outcomes, including decreases in arrests, jail days, detox visits, and the use of emergency medical services. These ultimate outcomes are of particular interest to investors and to the city.

Figure 3

Theory of How Supportive Housing Leads to a Reduction in Front-End User Costs

Goal: To reduce jail days, detox visits, and use of emergency medical services

Target population: Chronically homeless, frequent users of jail, detox, and emergency medical services

Providers:
Mental Health Center of Denver (MHCD) Colorado Coalition for the Homeless (CCH)

Entry points:
Police contact Noncustodial arrest Custodial arrest

Supportive housing seeks to integrate the services of multiple systems to provide care that results in increases in housing stability and physical and mental health and decreases in arrests.

Intervention—supportive housing	*Intermediate outcomes*	*Long-term outcomes*
Housing subsidy ■ Provide rent assistance in a housing unit that is safe, sustainable, functional, and conducive to tenant stability *Case management services* ■ Develop a case plan ■ Facilitate access to benefits ■ Provide referrals ■ Coordinate care *Health care services* ■ Enroll in Medicaid ■ Provide mental and physical health care	*Increase housing stability* ■ Reduce homelessness ■ Provide a safe, healthy, stable housing unit *Decrease public nuisance* ■ Decrease alcohol and drug use, trespassing, and panhandling *Improve health* ■ Improve mental health ■ Improve physical health	*Decrease arrests* *Decrease jail days* *Decrease detox visits* *Decrease use of emergency medical services*

Research Questions

Two sets of research questions drive the evaluation of the Denver SH-SIB. The questions will be answered through two primary components of the evaluation, including a process study and an outcomes and impact study.

1. Questions in the process study include the following: How is the program implemented? How are eligible individuals located and engaged? How do participants take up housing and services? Does it align with the CSH guidebook, "Dimensions of Quality Supportive Housing"? Is there fidelity to the service model? How does this look different from usual care? What types of systems change and services integration were achieved? What are the key facilitators and challenges?
2. The following are questions for the outcomes and impact study: Do housed participants retain housing? Does supportive housing increase housing stability and decrease the use of high-cost public services (e.g., jails, courts, detox, homeless shelters, and hospitals)? Do outcomes differ for participants housed in scatter-site housing versus single-site housing? Were performance goals met so that investors should be paid?

Major Components of the Evaluation

PROCESS STUDY

Key process-related information, including the housing and referral pipeline, is necessary to manage implementation and to make midcourse corrections to keep the initiative on track to achieve long-term outcomes. Process information will also help us interpret the results of the impact evaluation based on documentation of the program model and participant engagement. To collect information about these different domains, we will manage an engagement dashboard as well as a housing enrollment pipeline. We will conduct annual site visits and informant interviews with service providers and other important stakeholders. We will also review program-related documents such as training manuals, standard operating procedures, or other descriptions of program components.

OUTCOMES AND IMPACT STUDY

To validate the data needed to support interim investor payments, which will be based on housing retention among housed participants, we will (1) track participant exits from housing and measure days spent in housing; (2) validate the data needed to support final investor payments, which will be based on

the impact that supportive housing has on the target population's jail days; and (3)explore impacts on a broader set of outcomes and consider whether those outcomes differ for participants housed in scatter-site versus single-site units. As described in the next section, we will use a randomized controlled trial (RCT) as part of the research design. Eligible individuals will be randomly assigned to one of two groups—one that receives supportive housing as part of the initiative or one that receives usual care services. We will measure differences in key system outcomes between the groups (i.e., their use of services) using administrative data from the primary systems of interest, such as jails, courts, detox units, homeless shelters, and hospitals.

TABLE 3

Primary Evaluation Components

Evaluation component	Research questions	Data sources
Process study	How is the program implemented? How are eligible individuals located and engaged? How do participants take up housing and services? Does it align with the CSH guidebook, "Dimensions of Quality Supportive Housing"? Is there fidelity to the service model? How does this look different from usual care? What types of systems change and services integration were achieved? What are the key facilitators and challenges?	Engagement dashboard, housing enrollment pipeline, annual site visits and key informant interviews, review of program-related documents
Outcomes and impact study	Do housed participants retain housing? Does supportive housing increase housing stability and decrease the use of high-cost public services (e.g., jails, courts, detox, homeless shelters, and hospitals)? Do outcomes differ for participants housed in scatter-site housing versus single-site housing? Were performance goals met so that investors should be paid?	Program housing retention data, administrative data from systems of interest

RCT Design

Randomized controlled trials are widely considered to be the gold standard in measuring the effectiveness of a policy or intervention. RCTs are useful for establishing the counterfactual, or what would have occurred in the absence of the intervention. In the case of this initiative, the RCT design will be able to compare the trajectories of front-end users who receive priority placement in supportive housing and those who receive usual care. The target population for the Denver SH-SIB initiative includes many more individuals who are in need of and are eligible for the intervention than can be accommodated by the city's limited housing slots. The initiative will therefore allocate the limited housing slots by lottery, which is a fair way to allocate the scarce housing resources, and it also enables random assignment.

The evaluation will track outcomes for both the supportive housing and the usual care groups and attribute any differences to the supportive housing intervention. The selected eligibility criteria will allow for a sample of at least 500 participants, including 250 in the treatment group and 250 in the control group. This sample size allows the evaluation to detect effects of at least 25 percent, which the literature suggests is reasonable to expect for reduced jail time (Aidala et al. 2014). For example, if the control group experiences an average of 50 days in jail, we can attribute effects to the program if the treatment group experiences 25 percent fewer days, or an average of 37.5 days in jail.

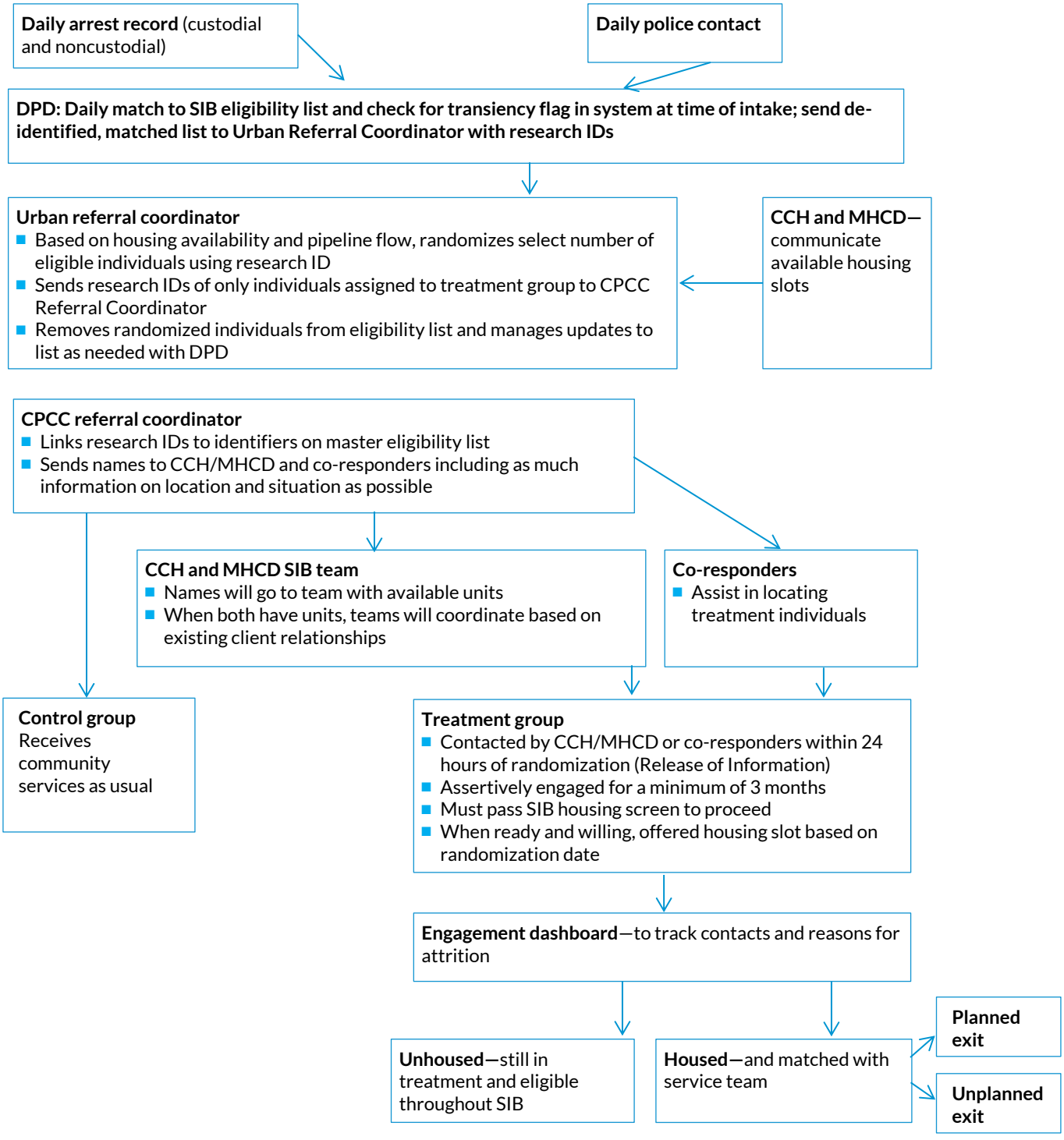
Referral and Randomization Strategy

Using the eligibility criteria, the Denver Police Department (DPD) will identify eligible individuals through a data pull and create a deduplicated, deidentified eligibility list for the initiative, assigning a unique research ID to each individual on the eligibility list (see figure 4). Program enrollment begins when individuals are identified from the eligibility list as they enter a designated intake point. Intake points include police contact and arrest, both custodial and noncustodial arrests. DPD will serve as a coordinated intake point and will electronically maintain the SIB eligibility list (including periodic updates) and match the eligibility list with daily arrest and contact lists to identify eligible individuals. DPD will then send a deidentified list of matched SIB-eligible individuals to the Urban Institute staff members, who will ensure that eligible individuals are randomly assigned only once and who will stratify arrest and police contact intakes equally.

The Urban Institute will generate a deidentified list of individuals assigned to the treatment group and send it to the referral coordinator at the Denver Crime Prevention and Control Commission (CPCC). The CPCC referral coordinator will link the unique research IDs back to the individual identifiers (i.e., names and as much information as is available from the intake points to support location and outreach) on the master eligibility list. The coordinator will then send those individuals' information to the service providers that have available housing slots. If necessary, staff within DPD will help with locating eligible individuals and connecting them with service providers.

FIGURE 4

Referral and Randomization Flowchart



Note: CCH = Colorado Coalition for the Homeless; CPCC = Crime Prevention and Control Commission; DPD = Denver Police Department; MHCD = Mental Health Center of Denver; SIB = Social Impact Bond.

If both CCH and MHCD have supportive housing slots available, the two service providers will work together to assign individuals based on any existing client relationships. Outreach workers will attempt to locate each referred individual within 24 hours of referral to minimize location challenges. When outreach workers locate individuals in the treatment group, they will first have them sign a Release of Information. They then can immediately begin program engagement, working with other service providers and co-responders to engage the individual. Service providers will engage participants in the treatment group for a minimum of three months before stepping down engagement and requesting a new referral.

After they are located, individuals must also pass the SIB housing screen (appendix E) to confirm homelessness and continue engagement toward housing placement. The SIB housing screen will screen *out* only individuals who are not considered homeless according to the SIB screening requirements. However, it will also screen for chronic homelessness (appendix F), which will help determine the most appropriate housing subsidy for the individual. Urban, working with DPD, will update the list to ensure that individuals are randomized only once, will manage any updates as the list is refreshed or expanded, and will coordinate with service providers to turn randomization on and off as necessary.

Randomization Stratification

Because eligible individuals can be randomly assigned from three different intake points—police contact, noncustodial arrest, and custodial arrest—it is important that the treatment and control groups be equivalent in terms of the number of individuals randomized from each intake point. To ensure this type of equivalency, we use randomization stratification. Each day, each eligible individual from all three intake points will be given a number generated by a random number generator from a uniform distribution using Stata software. The sample will be stratified across the three entry types; that is, the number of treatment individuals in each entry type will equal the number of control individuals in the same entry type.

The treatment and control groups will be created based on their random number and the number of individuals in that entry type to be matched. The treatment group will be composed of the individuals with the lowest random numbers that day, up to the number of open slots, conditional upon having at least one possible match within that individual's entry type. The control group will be identified as the next lowest random numbers in the entry type group.

For example (shown in table 4), we might have two open housing slots to fill on a given day. We want to randomize two individuals into treatment and two into control. We will take the two individuals with the lowest random numbers—in this example, they would be PIN 3 and PIN 4, with 102 and 122.

However, there is no comparable control (of the same entry type) for PIN 3, so PIN 3 cannot be a treatment case. We would then pick the next lowest random number, which is 138, for PIN 1; there is another individual in that entry type that can be a control, so PIN 1 would be a treatment case. We then pick the controls as the next lowest random numbers within each entry type; PIN 2 for custodial arrests is the match for PIN 1, and PIN 5 is the match for PIN 4 in the contact group. No other cases are randomly assigned, and unassigned individuals will be eligible for a new random assignment if they come back in through one of the entry points on another day.

TABLE 4

Example of Random Assignment

PIN	Random number	Group	Assignment
1	138	Custodial arrest	Treatment
2	476	Custodial arrest	Control
3	102	Noncustodial arrest	None
4	122	Contact	Treatment
5	180	Contact	Control
6	367	Contact	None
7	757	Contact	None

SIB HOUSING SCREEN

The SIB housing screen that will be completed for each individual randomized to the treatment group is based on the strict US Department of Housing and Urban Development (HUD) definition of homelessness as outlined in the federal HEARTH Act. That definition includes the following categories: the core definition (living in a shelter, on the street, or exiting an institution after previously being homeless); imminently losing primary nighttime residence; experiencing persistent housing instability; and fleeing domestic violence.

The referral strategy will begin with using the screen to eliminate from consideration any individuals who do not meet the strict HUD definition of homelessness. Individuals who are screened out will not be eligible for supportive housing at that time, but they will remain in the treatment group and can be rescreened if their situation changes in ways that would make them eligible for supportive housing. The screen includes additional questions that will help us understand whether participants who are screened out would be eligible under a slightly modified definition of homelessness.

If the evaluation risks screening out too many participants from the treatment group, thus creating an equivalency problem between the treatment and control groups, then we will modify the housing screen to reflect a modified definition of homelessness and allow the service providers to continue engaging any treatment individuals who would then be eligible. This strategy still allows the evaluation

to provide a clear description of the homelessness characteristics of the full treatment group. The service providers will be trained on how to use the housing screen and Urban will closely monitor the screening process.

MINIMUM TREATMENT RANDOMIZATION TIMELINE

The minimum treatment randomization timeline shown in table 5 ensures that a sufficient number of individuals are randomized to the treatment group to meet available housing slots. Urban will ensure that individuals are randomized at least one month before housing slots become available to allow for engagement before lease-up. Should the lease-up schedule be amended at any time, Urban can also quickly amend the randomization timeline.

TABLE 5

Minimum Treatment Randomization Timeline

Month	Total monthly projected placements	Cumulative projected placements	Minimum monthly treatment assignments	Minimum cumulative treatment assignments
November 2015	0	0	0	0
December 2015	0	0	0	0
January 2016	0	0	10	10
February 2016	10	10	15	25
March 2016	15	25	10	35
April 2016	10	35	9	44
May 2016	9	44	4	48
June 2016	4	48	4	52
July 2016	4	52	4	56
August 2016	4	56	3	59
September 2016	3	59	3	62
October 2016	3	62	3	65
November 2016	3	65	20	85
December 2016	0	65	20	105
January 2017	0	65	20	125
February 2017	0	65	20	145
March 2017	20	85	20	165
April 2017	20	105	25	190
May 2017	45	150	25	215
June 2017	45	195	25	240
July 2017	45	240	10	250
August 2017	5	245	0	250
September 2017	5	250	0	250

Data Sharing and Consent

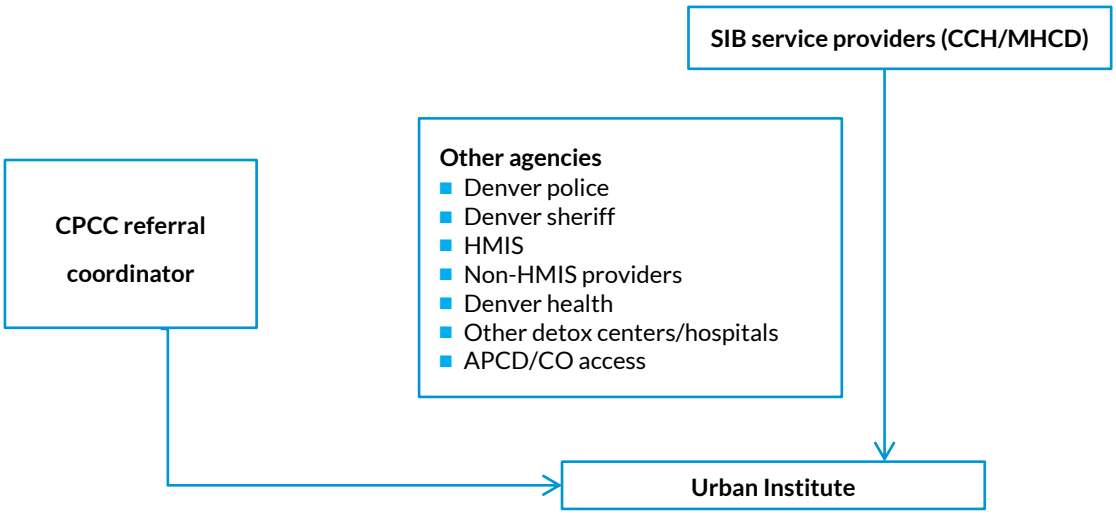
The Urban Institute will collect only deidentified administrative data that it then links through a project-specific ID that one central agency will share with other administrative data agencies. To make this work, the Denver CPCC will assign a staff person to fulfill the role of the CPCC referral coordinator, who will have access to the master eligibility list. That list will include personal identifiers as well as a project-specific ID for each individual in the treatment or control group (Urban will have only the deidentified eligibility list).

The CPCC referral coordinator will share the personal identifiers and the project-specific ID of the individuals in the study with each of the other agencies (see figure 5). The Urban Institute will collect administrative data based on data-sharing agreements with each of those agencies. The other agencies will pull the requested data for each individual in the study using the personal identifiers, attach the unique research identifier to their dataset, and strip the personal identifiers from the dataset. Each of the agencies will send their data, including the project-specific ID, directly to the Urban Institute. This will allow the Urban Institute to generate a single deidentified dataset with data from each agency.

Under this plan, the Urban Institute will never have access to any personal identifiers for any of the participants in the study. This method of data collection and data sharing ensures that no single agency or entity has access to more than one dataset with identifiers. Furthermore, the Urban Institute will be in control of the linking process and ensure its quality.

FIGURE 5

Data Access Plan



Note: APCD = All Payer Claims Database; CCH = Colorado Coalition for the Homeless; CPCC = Crime Prevention and Control Commission; HMIS = Homeless Management Information System; MHCD = Mental Health Center of Denver; SIB = Social Impact Bond.

Evaluation Components

PROCESS STUDY

Key process-related information, including the housing and referral pipeline, is necessary to manage implementation and to make midcourse corrections to keep the initiative on track to achieve long-term outcomes. Process information will also help us interpret the results of the impact evaluation, which is based on documentation of the program model and participant engagement. To collect information about these different domains, we will manage an engagement dashboard as well as a housing enrollment pipeline. We will conduct annual site visits and informant interviews with key service providers and other important stakeholders. We will also review all program-related documents such as training manuals, standard operating procedures, or other descriptions of program components.

RESEARCH QUESTIONS

- How are eligible individuals located and engaged?
- How often and how quickly do participants take up housing and services? What prevents take-up?

- How is the program implemented? Does it align with CSH's guidebook, "Dimensions of Quality Supportive Housing"?
- Is there fidelity to the initiative's service model?
- Does the provision of supportive services look different for participants in a single-site versus scattered-site housing placement?
- How does the intervention look different from usual care?
- What changes to the system and integration of services were achieved?
- What are the key facilitators and challenges to successful program implementation?

DATA COLLECTION, SOURCES, AND ANALYTIC METHODS

We will conduct the process study over the course of the evaluation by collecting and analyzing data at regular intervals. Early data collection, especially, will inform research design and evaluability. The process study will begin at enrollment and determine program flow—that is, the number of eligible individuals flowing through the initiative's intake points on any given day, week, or month. The process study also will collect data on how service providers locate and engage individuals in the treatment group. Because the target population historically is known to resist treatment, many other similar studies have experienced challenges in engaging eligible individuals, which results in low take-up rates within the treatment group. To understand how service providers locate and engage individuals, and how those individuals take up (or don't take up) the housing and services offered through the intervention, the process study will use tools such as an engagement dashboard and referral pipeline. These tools will be maintained in real time to inform both the research design and program model.

Answering research questions regarding program implementation and challenges will help identify important midcourse corrections. Identifying and evaluating the different program components is also critical to describing the entirety of the program model and interpreting the results obtained by the impact study.

To help guide the identification and analysis of program components, structures, and processes, we will assess the key components of the initiative and how they compare to CSH's "Dimensions of Quality Supportive Housing" (Corporation for Supportive Housing 2014), which assess whether supportive housing projects are tenant-centered, accessible, coordinated, integrated, and sustainable. The key components we will examine include the following:

- **Participants**. Partners, roles, service contributions, levels of staff involved (from frontline to leadership), types and modes of interaction, and changes over time.
- **Program goals**. For individuals and for agencies, and changes over time.
- **Referral and intake process.** How individuals get to the program, how intake decisions are made, what tools are used, how the information collected by assessment tools is used, and changes over time.
- **Program components and requirements.** For individuals, including program duration and intensity, program features, rules, restrictions, how program components compare with usual care services, and how components change over time.
- **Data and client tracking systems**. How service providers assess individuals over time, the nature and frequency of assessments and data monitoring by program, how data are used to influence program performance, and changes over time.
- **Housing subsidy type and duration**. Housing type, subsidy type (Section 8, Shelter+Care, local rent subsidy program, other), direct to permanent housing or some interim situations, rehousing if participants lose housing, and changes over time.
- **Supportive services**. What types of services are offered, how the services are staffed and run, how providers design and implement services and how they differ from usual care, and how supportive services change over time for individuals.

In addition to describing these key program components and how they align with the CSH guidebook on quality supportive housing, we will collect information on the larger environment in which the program operates. The supportive housing SIB initiative will operate within the criminal justice and other public systems that will have shifting processes for responding to the target population. We also will document the local housing market, which can create both opportunities and challenges for the program. Provider capacity may also differ. Some providers may be establishing new program models, while others are launching enhanced versions of existing activities; thus, each provider will have different capacities and experience. We will examine how all of these factors affect program design and implementation.

Because systems change is critical to the success of this program and to serving the target population, we will document the strengths of the partnerships within the SIB and the level of service integration they achieve. We will document changes in the numbers and types of agencies involved; the

levels of staff involvement within the various agencies; and the flow of information, clients, and money. We will look at specific structures developed for the project at the line worker, manager, and agency director levels to establish procedures, unblock bottlenecks (and develop strategies to eliminate them in the future), and deal with challenges to project implementation as they arise. Ideally we would also be able to look at the relationship of increased integration of services and participant outcomes, on the hypothesis that greater integration leads to better participant outcomes.

Finally, we will document what constitutes usual care in the Denver community as the program is implemented over time. In doing so, we will rely on the same components as we do in describing the program model, including the absence of components (e.g., housing subsidies and certain types of supportive services). Understanding the counterfactual—what housing and services the individuals in the target populations are likely to receive in the absence of the program—is critical to interpreting the results of the evaluation.

We anticipate using the following qualitative data collection mechanisms:

- **Document review**. We will request program policy manuals, training tools, and other relevant documents generated by the service providers about their activities.
- **Observation.** We will observe select program components and partner coordination; for example, we will attend management meetings and program meetings.
- **In-person staff interviews and provider or partner focus groups**. We will conduct annual in-person interviews with program staff and other appropriate staff respondents.
- **Phone interviews and conference calls.** We will conduct regular calls to get program and evaluation updates and encourage coordination among all partners.

The semistructured interview and observation protocols we use during site visits to conduct interviews and focus groups with key informants and stakeholders will include discussion topics and questions that reflect key research areas, as will the tools used for extracting information from program documents. We will use a qualitative analysis software package, such as NVivo, to organize and categorize key themes and issues. Results will be presented qualitatively and also converted into a few key quantitative measures to be included in the impact analysis. We will develop an effective way to share timely findings from the process study.

Outcomes and Impact Study

The outcomes and impact study will validate both the interim and final payment triggers for the SIB project and contribute to the broader field of supportive housing for frequent user populations. Our outcomes and impact study will have two components: an analysis for validating outcomes tied to payments made to investors and a broader impact analysis. To validate the payment triggers, we will measure housing retention, days in housing, and the impact of the program on jail days. To analyze the payment trigger outcomes, we will use a straightforward method of analysis for estimating the outcomes for the sake of clarity and transparency. We will base the broader outcome analysis on a more technical analytic method to estimate the impacts on a host of outcomes, including homelessness, arrests, detox visits, Medicaid use, and the use of emergency medical services.

In this section we describe the measures, data, and analytic methods that will be used for each of the components of the outcomes and impact study. We include a provision for calculating payment outcomes should the program be terminated early, as well as an alternative analytic method, should there be inadequate take-up of housing or too many violations of the control condition (i.e., if controls obtain housing specifically through the SIB program).

RESEARCH QUESTIONS

The following are the research questions for evaluation of outcomes and impacts:

- Do housed participants retain housing?
- Were performance goals met so that investors should be paid?
- Does supportive housing increase housing stability and decrease the use of high-cost public services (e.g., jails, courts, detox centers, homeless shelters, and hospitals)?
- Do outcomes differ for participants housed in scatter-site housing versus single-site housing?

MEASURES, DATA COLLECTION, SOURCES, AND ANALYTIC METHODS

We first describe the data and methodology to be used to estimate the payment triggers of housing retention, days in housing, and the effect of the program on jail days. We then describe the data and estimation technique and the data to be used for the broader outcome evaluation.

PAYMENT TRIGGERS

The primary payment triggers will be based on measures of housing stability and reductions in jail days. Housing retention and days in housing among the housed treatment group will be used as an interim payment trigger because housing retention is a strong predictor of longer-term outcomes of interest. The final payment trigger for the SIB will be the effect on jail stays, measured by the difference in average jail days between the treatment and control groups.

Housing Stability

Housing stability will be tracked through program and administrative data and will be measured only for the individuals in the treatment group who enter program housing. The threshold, payment points, and other information on how housing stability will be measured are outlined in table 6.

TABLE 6

Measurement of Housing Stability and Payment Points

Threshold	Payment Points	Limitations
■ Individual must maintain a lease for one year from lease-up date before eligible for payments, as defined in the contract. ■ The client has a lease, sublease, or occupancy agreement in their name, as defined in the contract.	■ After threshold is met, the city makes payments annually starting on October 30, 2017, based on days in housing before and after threshold, according to payment schedule, as defined in the contract.	■ Days spent in jail since lease-up date will be subtracted from days eligible for payments, as defined in the contract.
Exits		
Unplanned: If a client meets the condition below before achieving the one-year threshold, success payments will not be made for that client: ■ Loss of voucher/lease for any reason other than those specified under planned exit reasons (voucher loss can occur after 90 days away from unit; e.g., incarceration or returns to homelessness, or after eviction).	*Planned:* If a client meets any of the conditions below prior to or after achieving the one-year threshold, success payments will be made for the total number of days that the client was stably housed before exit at the per diem rate: ■ Death. ■ Exit to other permanent stable housing where the client is named on a lease, sublease, or occupancy agreement OR has a letter stating that they are allowed to reside with the leaseholder or owner in the unit on a permanent basis. ■ Tenant entered long-term residential treatment or other level of care (e.g., assisted living) that exceeds 90 days in order to address a physical or behavioral health issue. ■ Tenant was incarcerated for actions solely occurring before SIB randomization.	

The data sources and measures that will be used to calculate housing stability are outlined in table 7. Program data from MHCD and CCH will be collected approximately biweekly through the engagement dashboard as specified in the Urban Institute–Mental Health Center of Denver data sharing agreement and the Urban Institute–Colorado Coalition for the Homeless data sharing agreement. Data

from the Denver Sheriff Department will be collected at least every six months as specified in the Denver Sheriff Department data sharing agreement within the Urban Institute's contract with the City of Denver. Data will be linked by unique research ID to calculate housing stability outcomes.

TABLE 7

Data Sources and Measures for Calculating Housing Stability

Data source	Measures
MHCD and CCH Program Data	■ Unique research ID ■ Lease-up date ■ Housing exit date ■ Housing exit reason
Denver Sheriff Department	■ Unique research ID ■ Jail entry date ■ Jail exit date ■ Facility

Jail Day Reductions

Final payment will be based on the program's impact on reducing jail days. Jail day reductions will be measured as the average difference of jail days between the treatment and control groups, over a period of three years from randomization date, and estimated using a treatment-on-the treated (TOT) approach described in the analysis plan below. The payment for jail day outcomes will be made at the end of the evaluation period.

Estimation methods. To understand the calculation of how treatment impacts using the treatment on the treated (TOT) approach, we first explain how treatment impacts are calculated using the intent to treat (ITT) approach. The ITT estimate is defined as the difference between the average outcomes for those referred to the SH-SIB (the treatment group) and those not referred to the SH-SIB (the control group), adjusting for prerandomization covariates.

All eligible individuals randomized to the treatment population will be counted in the treatment population, regardless of whether they actually engage with the service provider, pass the SIB housing screen, or obtain housing. All eligible individuals randomized to the control population will be counted in the control population, even if they enroll with the service provider or obtain housing.

Calculation: The ITT estimate is measured as the average individual outcomes for the treatment population minus the average individual outcomes for the control population. We control for prerandomization covariates using a regression framework. Specifically, the ITT estimate, π_Y, would be measured using the regression equation below:

$$Y_i = \alpha + \beta^T T_i + \sum_{n=1}^{N} \beta^n X_i^n + \varepsilon_i$$

Where Y_i is the number of jail days for each individual, *i*, that was randomly assigned. T_i is an indicator equal to 1 for individuals who were assigned to the treatment group and 0 for individuals assigned to the control group. β^T is the parameter of the ITT effect on the outcome (Y_i), the number of population members assigned to the treatment population and control population, respectively. X^n is a vector of prerandomization covariates and β^n is the vector of coefficients on the covariate, X^n. ε is the regression error term. The inclusion of the prerandomization covariates is intended to improve the precision of the estimates. The initial proposed list of covariates to control for in the model is $X_i^1 \ldots X_i^{Nn}$: race, gender, age, number of prior custodial arrests (8/1/2012–7/31/2015), number of prior transient arrests (8/1/2012–7/31/2015), number of prior noncustodial arrests (8/1/2012–7/31/2015), and entry type (contact, noncustodial arrest, custodial arrest).

We will finalize the exact covariates after we review the historical data for data quality and completeness. In addition, the sample will be evaluated for equivalence between the treatment and control groups on observable prerandomization variables. Although random assignment is intended to create two equivalent groups, small samples can result in some differences between the groups by chance. Variables that show differences between the two groups at $p = .05$ (i.e., with at least 95 percent confidence that they are different) will be included as covariates in the regressions. The Urban Institute will provide the final regression specification no later than June 1, 2018, approximately six months after the latest date at which the evaluation could be fully enrolled.

The TOT estimate will be calculated using an instrumental variables (IV) estimate (Angrist, Imbens, and Rubins 1996). The IV estimate is per person served, among those who comply with their referral assignment, which accounts for the fact that some people referred to SH-SIB may not enroll and that some people in the control group may end up receiving services from the SH-SIB. For example, all study participants can be divided into three types of individuals: (1) those who will always enroll in SH-SIB regardless of whether they are referred to it or not; (2) those who will never enroll in SH-SIB even if they are referred to it; and (3) those who comply with whatever referral assignment they are given, whether it is to enroll in SH-SIB or to remain in the control group. The IV estimate represents the effect of SH-SIB enrollment on study outcomes among this third group, the compliers. In the special circumstance where decisions to comply or not are independent of the study outcomes, the IV estimate also represents the average treatment effect.

Calculation: The IV estimate scales up the ITT estimate by the difference between the treatment group's and the control group's fractions enrolled in SH-SIB. Conceptually, the Urban Institute will estimate the effect of referring an individual to SH-SIB on enrollment in SH-SIB in exactly the same

manner as calculating the ITT above, except that the dependent variable in the model will be enrollment:

$$P_i = \alpha + \delta^T T_i + \sum_{n=1}^{N} \delta^n X_i^n + \varepsilon_i$$

Where P_i is 1 if the individual, *i*, actually enrolled in the program, regardless of whether they were in the treatment group or the control group. Enrollment will be defined as the participant having an initial housing lease-up (enrollment) date in SIB housing. T_i is an indicator equal to 1 for individuals assigned to the treatment group and 0 for individuals assigned to the control group. δ^T is the parameter of the effect of getting randomly assigned into treatment on actual enrollment (P_i). X^n is a vector of prerandomization covariates, and β^n is the vector of coefficients on the covariates, X^n. ε is the regression error term. The IV estimate is the ratio of the two estimates:

$$\text{TOT estimate} = \frac{\beta^T}{\delta^T}$$

In practice, the two equations will be estimated simultaneously using a two-stage least squares estimation procedure. In the first stage, the dependent variable (enrolling in the program) is regressed on the exogenous covariates plus the instrument (randomization into treatment). In the second stage, fitted values from the first-stage regression are plugged directly into the structural equation in place of the endogenous regressor (enrolling in the program). We will include the same covariates as used in the ITT regression.

Because the payment schedule specifies the payment amount in per-person-served units, the IV estimate will be the basis for the performance-based outcome payments. The IV estimate also represents the per-participant-served difference in mean jail days between the treatment and control group, among those who comply with referral assignments.

Determination of individuals included in jail day reduction analyses. All individuals who have been randomly assigned to the treatment or control group for at least three years before the last day of the observation period will be included for the ITT estimate of jail days. For the TOT estimate we will define the treatment group as all individuals who had an initial lease-up date in SIB housing at least three years before the last day of the observation period. If an individual has been in the defined treatment group for longer than three years, we will look at the first 3 years they were in the treatment group as defined for the analyses. Therefore, any individuals enrolled after January 1, 2018, will not be included in the final verification of jail day outcomes.

However, referrals will continue past this point (if and when housing slots are open), since individuals enrolled in the treatment group after that point are still potentially eligible to generate housing stability payments. Given the housing stability payment threshold of one year after initial lease-up, any individuals enrolled after January 1, 2020, will not be eligible to generate housing stability payments. At this point, the city will determine whether the referral process should continue (if and when housing slots are open), even though any individuals enrolled after January 1, 2020, will not be included in the evaluation outcomes.

The data sources and measures that will be used to calculate reduction in jail days are outlined in table 8. Jail days will be collected from the Denver Sheriff Department at least every six months as specified in the Urban Institute–Denver Sheriff Department data sharing agreement.

TABLE 8

Data Sources and Measures for Calculating Reduction in Jail Days

Data source	Measures
Denver Sheriff Department	■ Unique research ID ■ Jail entry date ■ Jail exit date ■ Facility

JAIL DAY REDUCTIONS EARLY ANALYSIS CHECKPOINT

Although jail day reductions for payment purposes will not be analyzed until the final windup period in 2021, the Urban Institute will provide an early analysis of jail day reduction outcomes at an interim checkpoint during the SH-SIB project period.

An early analysis of jail day reduction outcomes will require a minimum sample of 150 randomized individuals, which we assume will be 75 individuals in treatment and 75 in the control group. Further, we will conduct our early analysis after this first group of 75 individuals assigned to the treatment group has been assigned for at least two years, because other similar studies have measured jail impacts over at least two years. If the current projected housing timeline is maintained, the SH-SIB will have at least 75 individuals assigned to the treatment group by March 2017, so two years later, the conditions for the early analysis checkpoint would be met in March 2019, about three years into the study. If the housing timeline is adjusted, we will conduct the early analysis on jail day reduction outcomes whenever the conditions for the checkpoint are met, and we will share the early analysis with the city, Pay for Success (PFS) lenders, and SPV within six months of the project's meeting the conditions for the checkpoint.

For this early analysis and checkpoint, because of its relatively small sample size, we will not be looking for any specific effect size or statistical significance. Rather, we will look for evidence that the difference between the treatment and control groups is as expected.

EARLY OUTCOMES TERMINATION PROCESS

If the agreement is terminated early, the outcome measurements for payment purposes, if appropriate as specified in the SH-SIB contract, will be calculated in the following ways:

Housing stability outcomes will be measured for all participants meeting the payment requirement before the early termination quarter as outlined in the research design and contract.

> If (i) this Contract is terminated prior to the end of the Project Term due to a Termination Event, ii) at least seventy-five (75) Participants were included as part of the Treatment Group for a period of at least one (1) year, and (iii) at least seventy-five (75) Eligible Referrals were included as part of the Control Group for a period of at least one (1) year, then jail day reduction outcomes will be measured for these individuals' first years following random assignment and analysis will be conducted as described in the research design to determine both an ITT and TOT estimate of the difference in jail days for one year. In this scenario, individuals who have been randomly assigned for less than one year will not be included in the analysis.
> If the minimum sample size as described above for a Termination Event is reached and the individuals in the minimum sample have been randomly assigned for at least two years prior to the date of early termination, then jail day reduction outcomes will be measured for these individuals' first two years following random assignment and analysis will be conducted as described in the research design to determine both an ITT and TOT estimate of the difference in jail days for two years. In this scenario, individuals who have been randomly assigned for less than two years will not be included in the analysis.
> If the minimum sample size as described above for a Termination Event is reached and the individuals in the minimum sample have been randomly assigned for at least three years prior to the date of early termination, then jail day reduction outcomes will be measured for these individuals' first three years following random assignment and analysis will be conducted as described in the research design to determine both an ITT and TOT estimate of the difference in jail days for three years. In this scenario, individuals who have been randomly assigned for less than three years will not be included in the analysis.

ALTERNATE ANALYSIS PLAN FOR THE TRIGGER PAYMENTS

An alternative analysis plan for trigger payments will apply if the difference between the percentage of treatment population members that enroll with the service provider and the percentage of the control population members that enroll is greater than or equal to 0.3. Should the enrollment difference be less than 0.3, then it is considered an insufficient enrollment difference, and the Urban Institute will use an alternative approach. That alternative replaces the control population with a historical comparison group using a matching procedure, called propensity score matching, to determine the alternative analysis of jail days reduction.

Propensity Score Approach: The propensity score approach will create a comparison group that is as similar as possible to those enrolled in the program in their distribution of observable characteristics. The comparison sample will be pulled from administrative data and meet the targeting criteria for our eligible sample at the time they are pulled. The propensity score is the estimated probability that an individual randomized into treatment is enrolled into the program based on individual characteristics. In the analysis procedure, the individual will be weighted as a function of his or her propensity score. The Urban Institute will estimate the propensity score using the treatment sample through the following logistic regression:

$$E_i = g(\alpha + \sum_{k=0}^{K} \beta_k \, X_{ik})$$

where E_i is a binary indicator for whether individual *i* is enrolled in the program; α is the overall intercept; X_{ik} is the *k*th covariate for the individual *i*, with associated coefficient β_k; and *g*() is the logistic function. As a part of finalizing this methodology, Urban will define the covariates. The propensity scores will be checked for balance and overlap. If the propensity scores generate extreme weights, these weights will be trimmed.

Urban will estimate the weights using the following methodology. The weight for each individual enrolled in the program will be 1. The weight for each individual, *j*, in the comparison samples will be

$$W_j = \frac{1 - \widehat{PS}_j}{\widehat{PS}_j}$$

where $\widehat{PS}_i$ is the estimated propensity score for each individual *i*.

The propensity score weighted effect will be estimated as

$$ITT_{PS} = \widehat{\Upsilon}_E - \widehat{\Upsilon}_{CS}$$

where $\widehat{\Upsilon}_E$ and $\widehat{\Upsilon}_{CS}$ are estimated by applying the weights to the observed outcomes, *Y*:

$$\widehat{\Upsilon}_E = \frac{\sum_{j=1}^{N_E} Y_i^E}{N^E}$$

$$\widehat{\Upsilon}_{CS} = \frac{\sum_{j=1}^{N_{CS}} Y_j^{CS} W_j^{CS}}{\sum_{j=1}^{N_{CS}} W_j^{CS}}$$

N_E and N_{CS} are the number of individuals enrolled in the program and the comparison group, respectively; Y_i^E is the outcome (number of days in jail) for each individual, *i*, enrolled in the program, and Y_j^{CS} is the outcome (number of days in jail) for each individual, *j*, enrolled in the comparison group; and W_j^{CS} is the weight for each individual in the comparison group.

BROADER IMPACT STUDY

The broader impact study will go beyond the measures used for payment triggers. Annual administrative data (detailed in the Data Access Plan, figure 5) will be used to measure the impact of the intervention on jail stays, homelessness, arrests, use of detox and other health services, and Medicaid use. These measures directly relate to the intermediate outcomes and final outcomes outlined in the theory of change (see figure 3). The broader study will also examine whether outcomes differ for participants housed in scatter-site versus single-site housing. Table 9 outlines the data sources and measures of interest for each outcome by study component. Deidentified individual-level data will be linked by a unique research ID to facilitate analysis while maintaining confidentiality.

Similar to the payment triggers estimation, the analytic methods for the broader impact study will use both ITT and TOT methods to estimate the impacts of the program. For the ITT estimate, we will both calculate the straightforward difference in means described above and use a regression-based method that controls for measured characteristics. In that way, we can control for sampling variation, which can lead to differences in the characteristics of members in each group, particularly in smaller samples.

We will use an instrumental variables (IV) approach to calculate the TOT for the broader impact study (Angrist, Imbens, and Rubins 1996). In this approach, as described previously for jail day reduction estimates, randomization into the treatment group is used as an instrument for actual treatment to remove some of the bias caused by selection into take-up. We will include the same covariates as used in the ITT regression. We will evaluate this model using multiple different definitions of treatment, including lease-up, engaged in services and leased up for 6 months, and engaged in services and leased up for 12 months.

TABLE 9

Data Sources and Measures for Other Impacts

Study	Outcome	Data source	Measure
Process study	Housing stability	CCH and MHCD program data	Unique research ID Random assignment date Client location and date Number of client contacts and dates Client housing screen outcome and date Client agreement to housing and date Voucher application outcome and date Housing orientation and date Voucher issuance date Voucher denial date Voucher denial reason Lease-up date Voucher loss reason and date Rent HAP Services used (date, type, dosage, duration)
Outcomes and impact study	Jail days	Denver sheriff	Unique research ID Charges Jail entry date Jail exit date Facility Exit reason
	Homelessness	Denver HMIS Non-HMIS service providers (Rescue Mission, St. Francis)	Unique research ID Shelter entry date Shelter exit date Shelter type Living situation before homelessness Destination Services received
	Arrests	Denver police	Unique research ID Demographics Contact date Contact reason Arrest date Arrest reason Indicator of transient arrest Indicator of custodial arrest
	Detox visits	Denver Health (Denver Cares)	Unique research ID Detox entry date Detox exit date Detox admission reason Detox exit destination Services administered
	Use of emergency medical services	Denver Health APCD databases	Unique research ID Emergency room entry date Emergency room exit date Emergency room admission reason Emergency room services administered Emergency room exit status
	Medicaid utilization	APCD databases	Unique research ID Medicaid enrollment Claim data for services: date of visit, type of visit

Note: CCH = Colorado Coalition for the Homeless; HMIS = Homeless Management Information System; MHCD = Mental Health Center of Denver; APCD = All Payers Claims Database.

In table 10, we show minimum detectable effect sizes for possible outcomes of a binomial variable with 80 percent power in a two-tail test at the traditional 0.05 significance level. The effect size puts differences in outcomes in percentage terms. From the earlier equations, the percentage difference ITT estimate will be calculated as $\frac{\pi_Y}{Y^C}$. As can be seen, the current design can be expected to allow us to detect effect sizes of 25 percent at the 0.05 significance level, which the literature suggests is reasonable to expect for reduced jail time. Should program take-up be an issue, as we expect it may be, the effect size needed among the treated group in program housing will increase, since we assume the effect for those in the treatment group who do not take up housing will be zero. The effect sizes listed for the TOT in the last column of table 9 come from a Bloom (1984) adjustment to the ITT estimate, which is a conservative approximation of the IV estimates of the TOT, as described earlier. The estimates in table 9 are conservative for both the ITT and TOT because they do not reflect regression-based estimates. Regressions in the ITT and in the IV equation should improve the precision of our estimates, allowing us to identify smaller effects.

TABLE 10

Minimum Detectable Effect Sizes

Control group	Treatment group	Number treated	Take–up (%)	Effect size for ITT	Effect size for TOT
250	250	250	100	0.25	0.25
333	333	250	75	0.22	0.29
417	417	250	60	0.19	0.32
500	500	250	50	0.18	0.36
581	581	250	43	0.16	0.37
676	676	250	37	0.15	0.41

Note: ITT = intent to treat; TOT = treatment on the treated.

HOUSING TYPE ANALYSIS

In addition to conducting an impact analysis on the broader outcomes, we will explore how outcomes differ by housing type (scatter-site or single-site housing). Because the evaluation will not randomly assign individuals within the treatment group to one of the two types of housing, the results of this comparison will not be causal. Without randomization, certain types of individuals may be more likely to end up in one housing type than the other. We will not be able to determine whether the difference in the outcomes across the two types of housing reflects differential effects by housing type or it reflects differences in the individuals placed in each type.

We will, however, be able to control for some of the observable differences in types of individuals placed in each housing type. We believe these observable differences will be driven largely by consumer

preference, eligibility for the housing type, and the timing of randomization and housing availability. We will use regression analysis to estimate the difference in outcomes between the two types of housing, controlling for these factors as much as possible. To conduct this analysis, we will collect information in the service provider engagement dashboard on whether participants exercise choice in housing type; whether they are placed in a housing type based on individual characteristics that affect housing type eligibility; such as chronic homelessness or sex offender status; and the type of housing available at each participant's time of randomization. This analysis will be conducted during the final windup period and reported along with final outcomes.

Data Security and Ownership

Data Security

Data will be provided via Secure File Transfer Protocol (SFTP) with password protection. This is the *only* acceptable method of providing data. The following methods are unacceptable: plain text e-mail, US Postal Service with unencrypted CD-ROM, unsecure File Transfer Protocol (FTP), and all other methods that are not mentioned above.

Urban staff members will use PGP software to encrypt the administrative data file and password-protect the hard drive. If we need to make backup copies of restricted data files, we will encrypt the files before the backup takes place. All restricted data and extracts will be encrypted. All backups of data onto CDs or DVDs will be stored in a locked file cabinet in the researcher's office. Only research staff members who have signed confidentiality pledges will be allowed to access the data.

We will treat all data derived from restricted data in the same manner as the original restricted data. Data derived from restricted data include, but are not limited to, subsets of cases or variables from the original restricted data, numerical or other transformations of one or more variables from the original restricted data, and new variables constructed from the original data.

Data Ownership

Urban will have full ownership of all data we collect for this study. We are bound by Urban Institute institutional review board (IRB)-approved standards of confidentiality and will not be able to turn over raw data to the city of Denver, SPV, investors, or any other stakeholders. In the event any of these entities requests an audit of the data to verify the outcomes reported by Urban, the requesting entity may select and fully pay for a qualified independent researcher to travel to Urban and conduct an audit of the data needed to verify the outcomes tied to the SIB payment triggers. The qualified independent researcher must sign the confidentiality pledge signed by all members of the research team and would operate under the same IRB standards of confidentiality as the research team. The qualified independent researcher would have access to only the data outlined in table 11 for verifying the outcomes tied to the SIB payment triggers.

TABLE 11

Data for Outcome Verification for SIB Payment Triggers

Data source	Measures
MHCD and CCH program data	■ Unique research ID ■ Random assignment date ■ Client housing screen outcome and date ■ Client agreement to housing and date ■ Voucher application outcome and date ■ Voucher issuance date ■ Voucher denial date ■ Voucher denial reason ■ Lease-up date ■ Voucher loss reason and date
Denver Sheriff Department	■ Unique research ID ■ Jail entry date ■ Jail exit date ■ Facility

In the event that Urban's role as the independent evaluator is terminated and a new independent evaluator is selected, new data-sharing agreements must be negotiated between the new independent evaluator and each of the agencies from which data were collected before Urban can turn over any data to the new independent evaluator. It will be incumbent on the new independent evaluator to ensure that any necessary confidentiality and data security protocols are in place such that new data-sharing agreements can be signed with each administrative data agency to allow Urban to turn over any data already collected to the new independent evaluator.

Reports and Findings

Final reports and findings will be presented in aggregate form only. No data will be presented in such a way that individuals could be identified. Frequencies and cross-tabulations will be sufficiently aggregated to protect individuals from identification through unique combinations of sensitive information and geographic identifiers. We may impose other restrictions based on our assessment of the data.

Destruction of Data

All data maintained online in the randomization tool database will be cleared within a month of completing random assignment. All data will be destroyed by June 2022, or two years after the final

project windup. The Urban Institute will use PGP data encryption software to permanently destroy all datasets in a way that renders them unreadable.

Project Monitoring and Outcome Reports

Project Monitoring

For project monitoring purposes, the Urban Institute will maintain a biweekly engagement dashboard (appendix A) and monthly pipeline dashboard (appendix B). Data for these dashboards will be collected at least biweekly from CCH and MHCD as specified in the data sharing agreements with each service provider. The biweekly engagement dashboard will track individual-level data on participant engagement and on enrollment in the program. Those data will be used by the service providers and Urban Institute to manage the randomization timeline and address any implementation challenges. Data from the engagement dashboard will be aggregated into a monthly pipeline dashboard that Urban will share with the city and SPV. The process for project monitoring will follow the schedule outlined in table 12.

TABLE 12

Project Monitoring Reports

Report name	Frequency and distribution	Description	Source
Engagement Dashboard	Biweekly—data dashboard due to Urban on the 15th and 30th of each month	Individual-level data of client engagement and enrollment	CCH, MHCD
Pipeline Dashboard	Monthly—data dashboard due to the City on the 15th of each month	Aggregate number of referrals, assignments, and housing outcomes	Urban

Note. CCH = Colorado Coalition for the Homeless; MHCD = Mental Health Center of Denver.

Outcome Reports

Urban will submit outcome reports on housing stability starting in quarter 7 and continuing every six months (although payments will only be made annually) thereafter, as indicated in table 12, through the evaluation project windup in quarter 22. Urban will conduct the outcome measurements on jail days for final payment purposes in the evaluation project windup in quarter 22. Outcome reports (appendix C) will be delivered to the city and SPV by the 15th of the last month of the quarter, as outlined in table 13.

TABLE 13

Outcome Reports

Project and payment quarter	Outcome report delivered	Quarter ending	Period of project under evaluation, housing stability	Housing stability outcomes observed through	Period of project under evaluation, jail days	Jail days outcomes observed through
7	9/15/17	9/30/17	Q1–6	6/30/17		
9	3/15/18	3/31/18	Q1–8	12/31/17		
11	9/15/18	9/30/18	Q1–10	6/30/18		
13	3/15/19	3/31/19	Q1–12	12/31/18	Q1–12[a]	12/31/18[a]
15	9/15/19	9/30/19	Q1–14	6/30/19		
17	3/15/20	3/31/20	Q1–16	12/31/19		
19	9/15/20	9/30/20	Q1–18	6/30/20		
22	5/15/21	6/30/21	Q1–20	12/31/20	Q1–20	12/31/20

Notes Urban's ability to produce the report on time is dependent upon receiving proper information from providers and the Sheriff's Department. To the extent there are delays, Urban may request reasonable extensions. Payment dates will be adjusted accordingly.

[a] This report will be an initial analysis of jail day outcomes for an early cohort of participants and will not be used for payment purposes. These are approximate dates for the report, but analysis will be conducted only when conditions for the checkpoint are met, as described on page 26.

Appendix A. Biweekly Engagement Dashboard

ID	Random assignment date	Located	Date first located	Number of contacts	Date of last contact	Date of last attempt to engage
Unique research identifier	Random assignment date	Client was located (Y/N)?	Date first contact with CCH/MHCD	Number of contacts with the client before agreement to housing	Date of last contact before agreement to housing	Date of last attempt to contact before agreement to housing

Passed housing screen	Date of housing screen	Agreed to housing	Date agreed to housing	Packet approved	Date of packet approval	Case manager
Client passed SIB eligibility housing screen (Y-Chronic, Y-SIB definition, No)?	Date client passed housing screen	Agreed to housing (Y/N)?	Date client agreed to housing	Voucher application approved (Y/N)?	Date of voucher application approval	Name of case manager

Housing orientation	Date of orientation	Housing lease up	Housing subsidy source	Date of lease up	Housing type assignment	Housing type reason
Housing orientation completed (Y/N)?	Date housing orientation completed	Housing lease up outcome: Yes, No-Still Looking, No-Voucher Expire, No-Lost Voucher, No-Other?	Housing subsidy source: SIB subsidy, COC voucher, DHA voucher, CDOH voucher	Date of housing lease up	Single-site or scatter-site?	Is there any specific reason the individual was placed in the housing type (choice, sex offender status, other eligibility issue, etc.?)

Clinical intake	Date of intake	Planned housing exit	Date of planned housing exit	Unplanned housing exit	Date of unplanned housing exit
Clinical intake completed (Y/N)?	Date clinical intake completed	Exited housing for: other permanent housing, residential treatment, prior offense incarceration, death? Leave blank if no exit.	Date of planned housing exit	Exited housing for: voluntary voucher loss, lease violation voucher loss, other voucher loss? Leave blank if no exit.	Date of unplanned housing exit

Appendix B. Monthly Pipeline Dashboard

	Total	Nov-15	Dec-15	Jan-16	Feb-16	Mar-16	Apr-16	May-16
Referrals								
Total on eligibility list								
Eligible individuals identified								
Arrest								
Police contact								
Jail								
Eligible individuals randomized								
Control								
Treatment								
# Not found								
# Found								
Failed housing screen								
Passed housing screen								
Agreed to housing								
Refused program								
Found ineligible for voucher								
Housing								
# Available slots								
# Issued voucher								
# Not leased up								
Still looking for housing								
Voucher expired								
Lost voucher								
Other								
# Leased up								
# Exited housing								
Planned exit event								
Other permanent housing								
Residential treatment/other care								
Prior offense incarceration								
death								
Unplanned exit event								
Lost voucher—voluntary								
Lost voucher—lease violation								
Lost voucher—incarceration								
Lost voucher—other								

Appendix C. Quarterly Housing Stability Outcomes Report

	Period under evaluation: Q1–7 (outcomes observed through 9/30/17)							Period under evaluation: Q1–9 (outcomes observed through 3/31/18)							Period under evaluation: Q1–11 (outcomes observed through 9/30/18)						
		Housing type		Race					Housing type		Race					Housing type		Race				
	All	Scattered site	Single site	B	W	H	O	All	Scattered site	Single site	B	W	H	O	All	Scattered site	Single site	B	W	H	O
Number of participants meeting payment requirement																					
Number of participants maintaining voucher for 365 days																					
Number of participants with planned exit event																					
Total days in housing for participants meeting payment requirement																					
Total days in jail for participants meeting payment requirements																					
Total adjusted days in housing for participants meeting payment requirement																					
Total new adjusted days in housing for participants meeting payment requirement																					

Appendix D. Early Analysis Checkpoint and Final Windup Jail Days Outcomes Report

	Period under evaluation: Project Quarters 1–20 (outcomes observed through 12/31/20)						
		Housing type		Race			
	All	Scattered site	Single site	Black	White	Hispanic	Other
Number of participants assigned to treatment group for at least 3 years (2 years for early analysis checkpoint)							
Total days in jail							
Average days in jail							
Number of participants assigned to control group for at least 3 years (2 years for early analysis checkpoint)							
Total days in jail							
Average days in jail							
Difference in total jail days							
Difference in average jail days							

Appendix E. SIB Housing Screen

Client SIB ID:

Date of Screen:

Instructions: Start with Question 1 and follow the instructions. If you reach a question where the answer is identified as **ELIGIBLE**, circle the eligible answer. The participant is eligible and the screen is complete.

If you reach a question where the answer is identified as **NOT ELIGIBLE**, circle the not eligible question. The participant is not eligible. **Continue** answering the next question and follow the instructions until you reach another question where the answer is ELIGIBLE or NOT ELIGIBLE. Circle this answer; the screen is complete.

If the individual meets the HUD definition of chronically homeless, also complete the **Chronic Homelessness Qualification Checklist.**

1. Where are you currently living?

- ☐ Apartment/House/Room where the individual has a lease, occupancy agreement, or owns -- **GO TO QUESTION 2**
- ☐ With Friend/Family -- **GO TO QUESTION 3**
- ☐ Motel/Hotel – **GO TO QUESTION 3**
- ☐ Hospital, Rehabilitation Center, Drug Treatment Center, Jail, Other Temporary Institution – **GO TO QUESTION 8**
- ☐ Transitional housing – **ELIGIBLE (CORE DEFINITION)**
- ☐ Emergency Shelter --**ELIGIBLE (CORE DEFINITION)**
- ☐ Anywhere outside (e.g., street, vehicle, abandoned building) – **ELIGIBLE (CORE DEFINITION)**

2. Are you trying to leave a domestic violence situation?

- ☐ No -- **NOT ELIGIBLE**
- ☐ Yes -- **GO TO QUESTION 4 (FLEEING DOMESTIC VIOLENCE)**

3. Will you be able to stay here or somewhere else for the next 2 weeks?

- ☐ No -- **GO TO QUESTION 4**
- ☐ Yes -- **GO TO QUESTION 5**

4. Do you know where you will stay when you leave your current situation?

- ☐ No -- **ELIGIBLE (IMMINENTLY LOSING PRIMARY NIGHTTIME RESIDENCE)**
- ☐ Yes -- **GO TO QUESTION 5**

5. Are you 24 years of age or younger or a family with children and/or youth?

- ☐ No -- **NOT ELIGIBLE**
- ☐ Yes -- **GO TO QUESTION 6**

6. Have you had your own lease, occupancy agreement, or owned a home in the last 2 months?

- ☐ No -- **GO TO QUESTION 7**
- ☐ Yes -- **NOT ELIGIBLE**

7. How many times have you moved in the last 2 months?

- ☐ Less than two times -- **NOT ELIGIBLE**
- ☐ Two or more times -- **ELIGIBLE (PERSISTENT HOUSING INSTABILITY)**

8. How long have you been in the hospital/rehabilitation center/drug treatment center/jail/other temporary institution? (Note: If individual was in multiple institutional settings in a row, add total time)

- ☐ 3 Months or Less -- **GO TO QUESTION 9**
- ☐ More than 3 Months -- **NOT ELIGIBLE**

9. Where were you staying right before you went to the hospital/rehabilitation center/drug treatment center/jail/other temporary institution? (Note: If individual was in multiple institutional settings in a row, determine situation prior to first institutional setting)

- ☐ Apartment/House/Room where the individual has a lease, occupancy agreement, or owned -- **NOT ELIGIBLE**
- ☐ With Friend/Family -- **NOT ELIGIBLE**
- ☐ Motel/Hotel – **NOT ELIGIBLE**
- ☐ Transitional housing – **NOT ELIGIBLE**
- ☐ Emergency Shelter -- **ELIGIBLE (CORE DEFINITION)**
- ☐ Anywhere outside (e.g., street, vehicle, abandoned building) – **ELIGIBLE (CORE DEFINITION)**

****Complete Questions 10–13 for any individual who answered Question 9, even if not eligible****

10. Will you be able to stay there or somewhere else for the next 2 weeks?

- ☐ No -- **GO TO QUESTION 11**
- ☐ Yes -- **GO TO QUESTION 12**

11. Do you know where you will stay when you leave your current situation?

- ☐ No -- **ELIGIBLE (IMMINENTLY LOSING PRIMARY NIGHTTIME RESIDENCE)**
- ☐ Yes -- **GO TO QUESTION 12**

12. Have you had your own lease, occupancy agreement, or owned a home in the last 2 months?

- ☐ No -- **GO TO QUESTION 13**
- ☐ Yes -- **NOT ELIGIBLE**

13. How many times have you moved in the last 2 months?

- ☐ Less than two times -- **NOT ELIGIBLE**
- ☐ Two or more times -- **ELIGIBLE (PERSISTENT HOUSING INSTABILITY)**

Appendix F. Chronic Homelessness Screen

Client Name: _____

HUD defines a Chronically Homeless person as: an unaccompanied homeless person (a single homeless person who is alone and is not part of a homeless family and not accompanied by children).

Part I. Disabling Condition (*Check appropriate box(es)):*

- ❑ A diagnosable substance abuse disorder
- ❑ A serious mental illness
- ❑ A developmental disability
- ❑ A chronic physical illness or disability, including the co-occurrence of two or more of these conditions.

Acceptable forms for documenting a person's disability status are as follows and must be completed by a knowledgeable professional: *(One of the following must be obtained)*

- ❑ Med-9
- ❑ SSDI/SSI/TPQY Statement (within 45 Days of paperwork submitted)
- ❑ Signed Disability Verification Form
- ❑ Signed Letter (on Letterhead) from social service agency confirming disability
- ❑ Hospital Record stating disability or mental health diagnosis

Part II. Literally Homeless Status (*Check ONE):*

- ___ is living in a place not meant for human habitation, such as cars, parks, sidewalks, abandoned buildings (on the street).

  VERIFICATION: Statement of situation and signature of current service provider.

- ___ is staying at an emergency shelter for homeless persons or safe haven.

  VERIFICATION: Statement of situation and signature of shelter staff.

- ___ is in rapid re-housing or supportive housing for homeless persons who was originally chronically homeless and came from the streets or emergency shelters; and/or in any of the above places but is spending a short time (up to 90 consecutive days) in a hospital or other institution.

  VERIFICATION: Statement of situation and signature of rapid re-housing/ supportive housing staff.

- ___ is exiting an institution where they resided for 90 days or less AND were residing in emergency shelter or place not meant for human habitation immediately before entering institution.

- ___ is an individual fleeing or attempting to flee domestic violence, dating violence, sexual assault, stalking, or other dangerous or life threatening conditions related to violence, who have no identified subsequent residence; AND lack the resources and support networks needed to obtain other permanent housing.

Part III. *Chronically Homeless Status (Check ONE):*

❑ **The individual has been continuously homeless for a year or more.**

❑ **The individual has had four (4) episodes of homelessness in the last three (3) years that total at least 12 months (3 months self-report; 9 months 3rd Party Verification)**

Part II or III is supported by Third Party Certification, which includes dates and locations of homelessness, from one or more of the following: *Check ALL that apply.* This third party or narrative verification should include dates and locations of episodes of homelessness. Verification Levels should be attempted in order from 1 through 4. Narrative should include date(s) attempted for third party verification and date(s) completed as appropriate.

First Level of Verification

- ❑ Signed Third Party letter (s) on agency letterhead from a shelter worker, homeless service provider, outreach worker, other healthcare or human service provider attesting to homelessness. Print outs from HMIS database documenting episode(s) of homelessness can be used with written narrative explaining such.

Second Level of Verification

- ❑ Signed written documentation on agency letterhead by Intake Worker of phone/in person/email conversations with a shelter worker, homeless service provider, outreach worker, other healthcare or human service provider attesting to homelessness. Print outs from HMIS database documenting episode(s) of homelessness can be used with written narrative explaining such.

Third Level of Verification

- ❑ Signed written documentation on agency letterhead by Intake Worker of their observations of the client's housing history attesting to homelessness. Housing history should include length of stay at each place during the past 4 years if possible. Print outs from HMIS database documenting episode(s) of homelessness can be used with written narrative explaining such.

Fourth Level of Verification

- ❑ Signed & notarized written documentation by client of their homelessness status along with a housing history showing episode(s) of homelessness during the past 4 years.

Staff Name: _____ Staff Title: _____

Organization: _____

Signature: _____ Date: _____

Instructions: This Homelessness History Summary provides a suggested timeline to be used by individuals who receive funds for programs targeted to chronically homeless persons. It may be used to analyze whether or not the chronology of a homeless person's history meets the time frame for the definition of chronic homelessness.

Client Name:

Time period	Whereabouts	Documented?

Note

1. “Evidence and Research,” Corporation for Supportive Housing, accessed January 13, 2016, http://www.csh.org/supportive-housing-facts/evidence/.

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