Article

Transitions through homelessness and factors that predict them: Three-year treatment outcomes

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Abstract

The course of homelessness was examined among adults entering treatment in the Chicago Target Cities sample, which was aimed at improving the service delivery system in large metropolitan areas across the U.S. The objectives of the present study were: (1) Examine transitions in and out of homelessness over 3 years post entry into treatment; and (2) Determine the treatment and non-treatment factors that predict achieving and sustaining residential stability. Sixty-one percent of initially homeless participants were stably housed at 36 months. By contrast, only 14% of initially housed participants were homeless at 36 months. Sample-wide, homelessness was reduced by 43% over 3 years. In conditional logistic regression models, the most consistent and persistent predictors were crack as the primary problem substance, which appears to be a risk factor for becoming and remaining homeless, and whether or not others were dependent on the participant for food/shelter, which appears to be a protective factor for achieving housing and preventing homelessness. In general, specific treatment factors did not predict outcomes. Limitations and implications for treatment are discussed. © 2005 Elsevier Inc. All rights reserved.

Keywords: Substance abuse; Addiction; Homelessness; Treatment; Predictors

1. Introduction

Epidemiological research consistently demonstrates, and service providers can confirm, the high prevalence of substance abuse in homeless populations. An examination of numerous studies to date (Koegel, Burnam, & Baumohl, 1996) concluded that about half of homeless people studied have had a diagnosable substance abuse disorder at some point in their lives, with a history of alcohol abuse occurring in almost half of single adults who are homeless, and a history of drug abuse in approximately one third. Called a "socioeconomic leveler" by Devine and Wright (1997), substance abuse/dependence is a documented pathway to job loss, family dissolution, social isolation, and for many, homelessness. At the same time, many persons who become homeless via other pathways turn to alcohol or other drugs to cope with the fear, deprivation, loss of dignity, and depression associated with their situation. This bi-directional relationship between substance abuse and homelessness creates a vicious cycle, further aggravated by exposure to the elements, infectious diseases, and crime (Rosenheck, Bassuk, & Salomon, 1999). Work by Sosin and Bruni (1997, 2000) suggests that resource problems may determine homelessness regardless of substance abuse, but substance-related problems raise the resource threshold for vulnerability to homelessness, and reduce the protection afforded by social networks against homelessness. The lack of social networks in turn appears to predict rejection of treatment by homeless persons who need it.

The existence of a treatment "gap" in the U.S. is well-established; it has been estimated that fewer than one fourth of those in need of treatment actually receive it (Horgan, 2001). Much of this gap is explained by structural barriers (lack of available slots or limited funding) as well as by users who may not want or acknowledge a need for treatment. These barriers are even more imposing for homeless

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persons, who often receive lower priority from the treatment community than higher functioning non-homeless clients, who are seen as more desirable by providers (Stahler, Cohen, Shipley, & Bartelt, 1995). Nonetheless, evidence suggests that the cycle of substance abuse and homelessness can be broken. With intensive treatment, in tandem with access to housing or housing supports, homeless clients can achieve and maintain stable housing as well as substantially reduce their substance abuse (Stahler & Stimmel, 1995). Evidence also suggests that treatment is cost-effective in reducing utilization rates of jails, emergency rooms, and other costly resources (Olfson, 1980; Willenbring & Whelan, 1991).

The present study analyzes homelessness among substance abusers seeking or mandated to treatment in the Chicago Target Cities sample (described below). A recent U.S. Conference of Mayors study estimated that 49% of Chicago's homeless population are substance abusers, ranking Chicago sixth among the 27 cities surveyed (U.S. Conference of Mayors, 2001).

The research objectives are: (1) examine participant movement in and out of homelessness over a 3-year period from entry into the Target Cities index treatment episode to 3 years post entry; and (2) determine the treatment and non-treatment factors that predict achieving and sustaining residential stability among initially homeless participants, as well as factors that *prevent* homelessness among initially housed participants.

2. Methods

2.1. Study participants

Participants in this study were originally enrolled in the Center for Substance Abuse Treatment (CSAT) Target Cities Program in Chicago, which was aimed at improving the service delivery system in large metropolitan areas across the U.S. (Scott, Muck, & Foss, 2000; Scott, Foss, & Sherman, 2003a, 2003b). Specifically, the Chicago Target Cities model was designed to (a) improve access to treatment by decreasing the amount of time participants waited to begin the assessment process and enter treatment, (b) maintain satisfaction with the intake process, and (c) improve the extent to which co-occurring or ancillary problems were identified and addressed. A cornerstone of the model was the establishment of a Central Intake Unit (CIU). Planners predicted that adherence to, and management of a centralized wait-list would improve the efficiency with which participants gained access to treatment and moved through the system.

From 1996–1998, a sample of 1,326 participants (541 men and 785 women) was recruited from 22 service delivery units in 12 substance abuse treatment facilities on Chicago's West Side. All programs in the region participated. Clients were enrolled across six levels of care: (1) outpatient drug-free programs; (2) intensive outpatient drug-free programs; (3) methadone maintenance programs; (4) short-term inpatient programs; (5) long-term inpatient program; and (6) halfway house. Approximately half the participants were sampled before the implementation of the CIU (Pre-CIU cohort) and the other half after the implementation of the CIU (CIU cohort).[1] Cluster sampling was used to ensure sufficient numbers in each modality for later comparisons. Clients were predominantly African-American (87.4%), unemployed (85.6%), and unmarried (90.4%). Approximately one third self-presented as homeless at admission.

To be eligible, clients had to be (a) 18 years or older; (b) residents of Chicago or those who declare themselves homeless; (c) users of alcohol or drugs in the past 6 months;[2] and (d) publicly-funded participants, excluding any participants whose treatment would be paid for, at least in part, by private insurance/HMO. Participants in the evaluation had to (a) present for treatment at one of the providers in the network or its CIU and (b) not be seeking treatment as a result of a DUI Level 2 or higher conviction.[3] Participants were interviewed shortly after intake and subsequently at 6, 18 (CIU cohort only), 24, and 36 months post-intake. Follow-up rates were extremely high: 98%, 95%, 94%, and 94% at 6, 18, 24, and 36 months, respectively (adjusted for deaths).

To be included in the present analysis, participants had to be measured at baseline, 6, 24, and 36 months (given that economic constraints limited the 18-month follow up to half of the sample, 18-month data was not used). In addition, information on residential status had to be non-missing at each wave. Of the original 1,326, 1,143 (86.2%) met these criteria.

2.2. Measures

The interview battery included the Addiction Severity Index, Fifth Edition (ASI; McLellan et al., 1992), a semi-structured interview designed to assess lifetime and past month problem severity in several domains. Internal consistency averages from .63 to .93 and test-retest reliability ranges from .51 to .92 (Hendricks, Kaplan, Van Limbeek, & Geerlings, 1989; Kosten, Rounsaville, & Kleber, 1985; McCusker, Bigelow, Serigon, & Zorn, 1994; McLellan, Luborsky, Cacciola, & Griffith, 1985; McLellan et al., 1992;

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[1] We were originally looking at the differences in outcomes between clients who accessed treatment directly at the treatment agency and clients who accessed treatment via the CIU.

[2] Those who spent the past 6 months in a controlled environment were included even though they had not used alcohol or drugs in the past 6 months as long as their abstinence was involuntary. This was done so as to not exclude a significant proportion of Halfway House residents who enter treatment after release from prison/jail and whose abstinence was forced.

[3] Those with DUI Level 2 or higher convictions and mandated to treatment were excluded because their assignment to modality and length of stay are determined, in part, by a court assessment that was outside the influence of the system changes implemented in Chicago Target Cities.

Rogalski, 1987). To test the generalizability of the ASI to homeless persons, Drake, McHugo, & Biesanz (1995) assessed its reliability in a sample of 184 homeless substance abusers across nine cities. Intraclass correlation coefficients (ICC) for the composite scores ranged from 0.64 for the medical and family/social scales to 0.86 for alcohol problems. The ICCs for the other composites were 0.82 for employment, 0.83 for drug, 0.78 for legal, and 0.71 for psychiatric problems. These were comparable to the published reliabilities for non-homeless samples of substance abusing patients in treatment for alcohol or other drug problems. A separate study by Joyner, Wright, & Devine (1996) also supported the reliability and validity of the ASI for assessing homeless substance misusers seeking treatment.

For the Chicago Target Cities Evaluation, the standard ASI was augmented by (a) spelling out all questions/response sets, (b) adding questions to expand measurement time frames, (c) adding questions to address state and federal reporting requirements, (d) adding measures of services received in each of the ASI areas (e.g., substance abuse treatment, mental health treatment, health treatment), and (e) adding more detailed questions in every area of the ASI, including the participant's living arrangements. Specifically, it asked about time participants spent in nine residential status categories over the prior 6 months, 1 month, and 1 week.

The follow-up augmented ASI (A-ASI) contained the intake ASI questions less the lifetime and state reporting questions (Scott, Dennis, & Foss, 1996). In addition, the follow-up A-ASI asked about treatment services to which a participant was mandated, referred, or admitted in the 6 months since the previous follow-up period. Involvement in 12-step groups (nature of group and number of sessions attended) was also included in these questions.

2.2.1. Measurement of homelessness

The A-ASI included nine residential status categories: (1) In your own apartment or house (includes living with a parent or guardian); (2) In someone else's apartment or house; (3) In a room in a hotel or motel, in a rooming or boarding house or single-room occupancy (SRO); (4) In a homeless shelter; (5) Vacant buildings, public or commercial facilities, parks, cars, or on the street because you didn't have a place to stay; (6) In medical treatment, such as a hospital; (7) In psychiatric treatment, such as a psychiatric unit in a hospital; (8) In jail, prison, or detention, including being on work release and electronic monitoring; and (9) In a group home or residence, such as a residential treatment center, or halfway house. For the present analysis, these were reduced to four composites: stably housed, marginally homeless, literally homeless, and institutionalized. This was done to maintain consistency and comparability with prior treatment studies of homeless substance abusers in which residential stability was assessed (Drake, McHugo, & Biesanz, 1995; Orwin, Garrison-Mogren, Jacobs, & Sonnefeld, 1999). The residential status categories in the Chicago interview (with

recall periods of 6 months, 1 month, and 1 week) are a derivation of like items from the housing section of the Personal History Form (Barrow et al., 1985), in which respondents are asked the number of days spent in various residential settings during the 60 days prior to the interview (or if appropriate, a reference date preceding the interview). The assessment by Drake et al. (1995) of the ASI on homeless substance abusers (described above) also included a test-retest reliability study of the 60-day composite items. It yielded intraclass correlations of 0.89, 0.75, 0.78, and 0.83 for the literal homelessness, marginal homelessness, stable housing, and institutional housing composites, respectively.

The composites are:

- Stable Housing: (a) own house/apartment, (b) own SRO room, (c) group home, boarding house, or board-and-care facility, or (d) parent/guardian's house/apartment.
- Institutional Housing: (a) residential treatment or recovery program, (b) hospital (including psychiatric facility), (c) nursing home, (d) jail or prison, or (e) corrections halfway house.
- Marginal Homelessness: (a) in a hotel/motel, (b) in someone else's SRO room, (c) in someone else's apartment or house (not parent/guardian), or (d) transitional housing.
- Literal Homelessness: (a) on the street or other outdoor place, (b) in a vehicle, abandoned building, or indoor public place, or (c) in an emergency shelter.

To measure movement across the composite categories over time, we created a variable called "Predominant residential status," defined as the composite category in which the respondent lived the most time over the prior 6 months in any given measurement wave. We chose to use 6 months rather than 30 days (or less) because 30 days provides only a snapshot, prone to regression toward the mean, and with far more sampling error and possibly bias as well (e.g., because the time interval sampled by the snapshot is linked by design to the beginning of the index treatment episode).

The 6-month responses were recorded as ordinal intervals (0, <1 month, 1–2 months, 3–4 months, 5–6 months). So, for example, if the participant was marginally homeless for 3–4 months, and no more than 1–2 months in any other category, then marginally homeless would be the predominant residential status for that measurement wave. For ties (e.g., the participant reports 1–2 months in three different composite categories), the 30-day version of the same composites served as the first-level tiebreaker. Additional tiebreaking rules were imposed for cases that remain tied (e.g., the same participant reports 12 days on two of the three 30-day composites), but less than 1% of cases were affected.

For participants for whom the 6-month responses are missing, all 0, or a combination thereof, predominant residential status was set to missing as well. Using these rules, the 1,326 baseline cases break down as: 51 literal homelessness, 293 marginal homelessness, 201 institutionally

housed, 758 stably housed, and 23 missing. While the number of participants whose predominant residential status at baseline is literal homelessness is relatively small, the combination of literal and marginal homelessness is substantial (27% of non-missing cases). Consistent with the inclusive definition of homelessness used by CSAT in its cooperative agreement programs for homeless adults (Homelessness Prevention, Homeless Families, and Addiction Treatment for Homeless), as well as the "McKinney definition" used by NIAAA and NIMH in the late 80s and early 90s, the present study defined homelessness to include both categories. This provided sufficient sample size to address the research questions posed above.

2.3. Analysis

The analysis had two phases: a descriptive phase which examined the direction and rate of movement across housing categories over time, and a modeling phase which determined what treatment and non-treatment factors predicted said movement. For the modeling phase, a series of conditional logistic regressions were conducted. The analyses are conditional in that separate analyses are conducted at each wave among participants with the same residential status during prior wave. As shown in Table 1, the models attempt to determine the factors that predict residential stability at four time points: baseline, 6 months post-baseline, 24 months post-baseline, and 36 months post-baseline. SAS PROC LOGISTIC was used to perform the logistic regressions.

The four 24-month logistic regressions address the twice-conditioned subgroups created by the 6-month outcomes. The first set of predictors is identical to that of the baseline and 6-month analysis. In the second set, baseline status predictors (e.g., problem severities) were updated with 6-month status. In the third set, however, treatment experience variables were aggregated across the full 24-month period, in recognition of recent literature suggesting that treatment effects are cumulative (e.g., Hser, Grella, Chou, & Anglin, 1998). For the two analyses nested within the initially homeless baseline class, baseline homeless status (literal v. marginal) was again included. The 36-month models were the same as the 24-month models, except that 6-month status predictors were replaced by 24-month status predictors, and the 24-month aggregate treatment experience was updated to 36 months.

Selection bias is a concern in any observational (non-experimental) study that attempts to estimate effects of treatment. In the 6-month and later models (the baseline models had no treatment variables), the background variables were given causal priority over the treatment variables, providing some measure of protection against the misinterpretation of selection bias as treatment effects (at the cost of potentially underestimating those effects). Specifically, the background variables were retained in the models regardless of statistical significance; the treatment experience variables, on the other hand, were only retained if they contributed statistically to the model over and above the backgroun; otherwise they were removed by backward elimination. This was done for several reasons: (a) to limit the total number of included variables in the final model (particularly important in the 24- and 36-month models where the sample sizes are much smaller), (b) to prevent model convergence problems due to multicollinearity, and (c) to recover cases lost due to listwise deletion of cases that were *only* missing in the nonsignificant variables from the treatment set. These cases were recovered by re-running the model after omitting the nonsignificant variables from that set.

3. Findings

3.1. Movement across categories over time

Fig. 1 displays, for each of the four composite categories, all movement across categories over the 3-year period. The 6-month column details the probabilities of transitioning

Table 1
Summary of contrasts, conditional status, and predictor sets for logistic regression models

Contrast	Conditional status	Predictor sets
Stably housed vs. homeless at intake	None	Background characteristics Baseline status variables
Stably housed vs. not stably housed at 6 months	Homeless at intake Housed at intake	Background characteristics Baseline status variables Initial treatment experience (intake to 6 months)
Stably housed vs. not stably housed at 24 months	Homeless at intake, not stably housed at 6 months Homeless at intake, stably housed at 6 months Housed at intake, not stably housed at 6 months Housed at intake, stably housed at 6 months	Background characteristics 6-month status variables Cumulative treatment experience to date (intake to 24 months)
Stably housed vs. not stably housed at 36 months	Homeless at intake, not stably housed at 6 months Homeless at intake, stably housed at 6 months Housed at intake, not stably housed at 6 months Housed at intake, stably housed at 6 months	Background characteristics 24-month status variables Cumulative treatment experience to date (intake to 36 months)

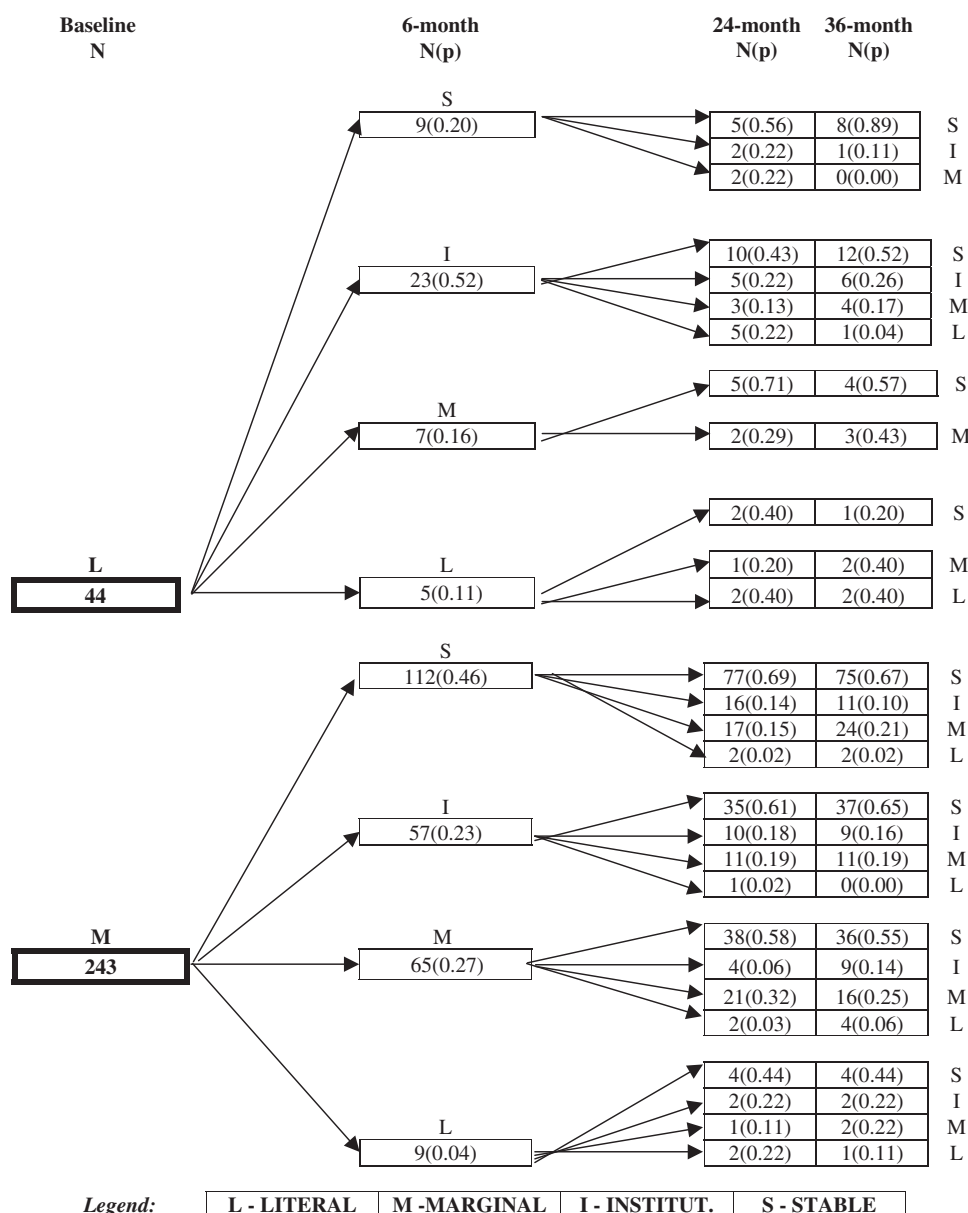Stably housed is the index event in all models.

Fig. 1. Conditional probabilities of predominant residential status at 6, 24, and 36 months, given baseline status.

from each initial category to each 6-month category. The 24-month/36-month column details the conditional probabilities of transitioning from each 6-month category to each 24-month and 36-month category, given the initial category.[4]

Combining and recalculating across selected combinations of transition rates in Fig. 1 permits more general statements about the likelihood of achieving and sustaining residential stability, taking into account initial and interim status.

The transitions experienced by initially institutionally housed participants are presented for illustrative purposes, but will not be analyzed further. The rationale is that participants in this category are very heterogeneous, mixing incarcerated persons with those institutionalized for mental illness and those in residential substance abuse treatment. Moreover, their follow-up residential status is so heavily—yet differentially—influenced by the circumstances of their initial institutionalization that it would be difficult to interpret other treatment and non-treatment variables with the same confidence as the other three groups. The trade-off is that the results of the analysis will not generalize to those whose initial residential status is institutionalized.

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[4] While it is theoretically possible to also examine triply conditioned transition rates (36-month status given 24-month status given 6-month status given initial status), the small cell sizes preclude this option.
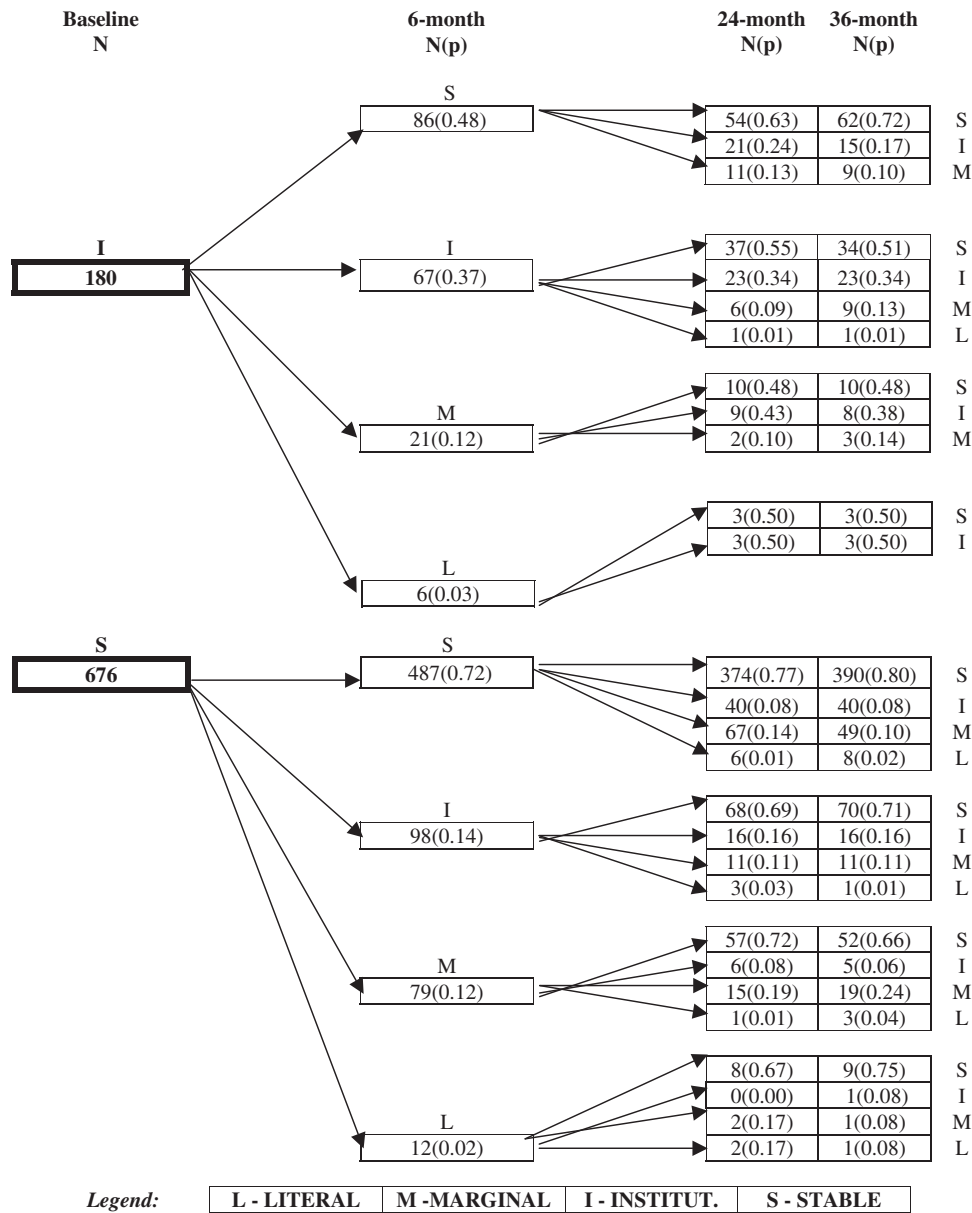
Fig. 1. (*continued*)

3.1.1. All homeless (N = 287)

Pooling across literal and marginal participants (while maintaining their distinctness in assessing improvement), 72% had improved their status at 6 months. Of these, 82% maintained at least partial improvement at 36 months, 49% maintained the level they had reached at 6 months, and 25% improved still further. Altogether, 63% were stably housed at 24 months, and 65% were stably housed at 36 months.

Of the 28% of pooled participants who had *not* improved at 6 months, 68% had improved by 36 months, and 52% were stably housed. Altogether, 61% of participants whose predominant residential status prior to intake was homeless (literal or marginal) were residentially stable at 36 months. This is particularly notable because prior research suggests that homeless substance abusers that are helped into housing

by treatment programs often fail to keep it (Braucht et al., 1995; Burnam et al., 1995; Conrad et al., 1998), in part due to high rates of relapse and its consequences.

3.1.2. Literally homeless (N = 44)

Disaggregating homeless participants back into the literal and marginal categories, we find that 89% of literally homeless participants had improved their status (i.e., were no longer literally homeless as defined above) at 6 months post-intake. Of these, almost all (97%) maintained at least partial improvement at 36 months, 44% maintained the level they had reached at 6 months, and 41% improved still further. Of those literally homeless prior to intake, 51% were residentially stable at 24 months, improving to 57% by 36 months.

Of the 11% of literally homeless participants who had *not* improved at 6 months, 60% were improved by 36 months, and 20% were stably housed. Altogether, over half (56%) of participants whose predominant residential status prior to intake was literally homeless were residentially stable at 36 months. Given the relatively small number of literally homeless participants, however, *SE*s for all these percentages are large.

3.1.3. Marginally homeless (N = 243)

Sixty-nine percent of marginally homeless participants had improved their residential status at 6 months post-intake. Of these, 78% maintained at least partial improvement at 36 months, 50% maintained the level they had reached at 6 months, and 22% improved still further. Sixty-six percent were residentially stable at both 24 and 36 months.

Of the 31% of marginally homeless participants who had *not* improved at 6 months, 69% had improved by 36 months, and 54% were residentially stable. Altogether, 63% of participants whose predominant residential status prior to intake was marginally homeless were residentially stable at 36 months.

3.1.4. Stably housed (N = 676)

Of the participants who were stably housed prior to intake, 72% were still stably housed at 6 months. Of these, 80% remained stably housed at 36 months. However, of the 28% that were *not* in stable housing at 6 months, the majority (121 of 189) can in part be accounted for by entry into residential treatment. Sixty-nine percent of participants that had lost stable housing status at 6 months had regained that status at 36 months. Altogether, 74% of participants whose predominant residential status prior to intake was stable were still (or again) residentially stable at 24 months, 77% at 36 months. An additional 9% were institutionally housed. The remaining 14% were homeless (12% marginal and 2% literal).

The contrast between the proportion of initially homeless participants who were stably housed after 3 years (61%) and that of initially housed participants who were homeless after 3 years (14%) is striking. By all appearances, residential status of homeless participants was far more likely to improve than residential status of housed participants was to worsen. (Alternative explanations are examined in section 4.1.1.). We now turn to examining the treatment and non-treatment factors that *predict* successful transition to, and maintenance of, residential stability.

3.2. Predictors of movement across categories over time

3.2.1. Baseline

The baseline logistic regression addresses the research question, "How do homeless participants differ from non-homeless participants in the Chicago Target Cities sample?" Background characteristics included age, race (black

v. other),[5] gender, education, primary problem substance, treatment history (number of admissions prior to the study index treatment), pregnant or postpartum, and number of persons dependent on participant for food/shelter. Current status variables included problem severity in eight domains (alcohol, drug, employment, legal, social, family, medical, and psychiatric), plus current monthly income from all sources.[6] Note that some variables (e.g., pregnant or postpartum) are gender-specific, and therefore are only interpretable if gender is also in the model.

Results are in Table 2. As shown, there were no significant differences in age, race, education, or prior treatment experience of homeless and housed participants at intake.[7] However, housed participants at baseline were significantly less likely to be female (OR = 0.66) and more likely to report heroin as their primary problem substance (OR = 1.69), controlling for other factors. Put another way, the heroin finding means that heroin as primary problem substance is less associated with homelessness than alcohol or crack cocaine, given a problem with at least one of the three. Crack and alcohol both had negative odds ratios, but were nonsignificant individually due to widespread cross-use.[8]

Finally, participants scoring high on drug, legal, employment, or family severity were significantly more likely to be homeless. This too is consistent with prior literature (e.g., Stahler & Stimmel, 1995). Despite a number of significant predictors, as a whole the model explained only 10% of outcome variance relative to a fully saturated model, as assessed by the generalized pseudo-R^2 statistic (Hosmer & Lemeshow, 1989).

3.2.2. Interim and long-term outcomes

The 6-month, 24-month, and 36-month logistic regressions outlined in Table 1 address the research questions, "Among initially homeless participants, what treatment and non-treatment factors predict achieving and sustaining residential stability?" and "Among initially housed participants, what treatment and non-treatment factors prevent

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[5] Race was operationalized as black v. other because 88% of the participants were black. Consequently, there were too few members of other ethnicities to separate out.

[6] We alternately tested clinician severity ratings and composite scores, and elected to use the severity ratings because they generally served as stronger predictors of outcomes and consistently yielded better model fit. The two measures were highly correlated in all domains except employment (possibly reflecting the lack of relevance of some employment composite score items to homeless participants, e.g., car ownership).

[7] Variances were not adjusted for any intraclass correlations that may have resulted from the cluster sampling. Consequently, Type 1 error rates may be slightly higher than listed in the tables.

[8] The cross-use would cause correlation between the two parameter estimates, decreasing the chance that either one alone will be statistically significant. We note that crack nonetheless *was* significant in the complete-cases file up to the 24-month wave (OR = 0.78, *p* < .05), but became nonsignificant in the 36-month complete-cases file (though still <1) in part due to power loss from attrition at the 36-month wave.

Table 2
Significant odds ratios and summary statistics from baseline logistic regression model: stably housed versus homeless at intake

Effect	All clients (N = 935)[1]
Age	
Black	
Female	0.662*
Education	
Number of treatments pre-intake	
Primary problem substance	
Alcohol	
Crack	
Heroin	1.690**
Pregnant/Postpartum	
Dependents (y/n)	2.202**
Total monthly income at intake	
Problem severity at intake	
Alcohol	
Drugs	0.873**
Legal	0.913*
Employment	0.853**
Medical	
Psychiatric	
Family	0.913*
Social	
R^2 (final)	0.1040
Max-rescaled R^2	0.1475
Gamma	0.395

* $p < .05$.
** $p < .01$.
[1] Twenty-nine observations (3%) were deleted due to missing values in predictors.

falling into homelessness and chronic homelessness?" The first two sets of predictors are identical to those of the baseline logistic regression. A third set was then added: treatment experience between intake and 6 months. These included cohort, referral source (criminal justice or not), days in treatment between intake and 6 months, and days attending outpatient self-help meetings in the 30 days prior to the interview.[9] They also included whether the participant believed that treatment staff addressed problems and needs associated with: (a) living arrangements, (b) medical needs, (c) employment status, (d) substance abuse needs, (e) family/social situation, and (f) psychological/emotional status.[10] For the initially homeless

[9] Days in treatment for the index episode was obtained from provider MIS records, while days in treatment for subsequent admissions (applicable for approximately 40% of participants) was obtained from self-report. The two quantities were summed to yield our best estimate of total days in treatment between intake and 6 months.

[10] No treatment components were excluded a priori; rather, components were excluded if (1) they showed virtually no variance or (2) their missing value rate was prohibitively high. For example, participants were asked whether treatment staff referred them to HUD, an item with the potential to be highly predictive of subsequent residential status. However, only 1% of participants responded yes. Similarly, we would have preferred to use accumulated self-help days over the 6-month period, but the high missing value rate left the variable unusable for our purposes.

baseline class only, baseline homeless status (literal v. marginal) was also included.

Table 3 displays the results from the 6-month models for initially homeless and initially housed participants,

Table 3
Odds ratios and summary statistics from conditional logistic regression models: stably housed versus not stably housed at 6 months

Effect	Condition	
	Homeless at intake (n = 295)[1]	Housed at intake (n = 671)[2]
Age		1.029*
Black	0.345*	
Female		
Education		
Number of treatments pre-intake		0.799**
Primary problem substance		
Alcohol		
Crack		0.686*
Heroin		
Pregnant/Postpartum		
Dependents (y/n)	2.977**	1.942**
Total monthly income at intake		
Problem severity at intake		
Alcohol		
Drugs		
Legal		
Employment		
Medical		
Psychiatric		
Family		
Social		
Marginally homeless	3.381*	N/A
R^2 (non-Tx factors)	0.1678	0.0862
Max-rescaled R^2	0.2251	0.1242
Cohort		
Mandated to index Tx	3.277*	
Total days in Tx, intake to 6 months		
Tx staff:		
Helped w/living arrangements	0.074**	0.222**
Addressed medical needs		
Provided services to address employment		
Addressed substance abuse needs		
Addressed family/social situation		
Addressed psychological/emotional status		
Days attending OP self-help, last 30		
R^2 (all factors)	0.2588	0.1106
Max-rescaled R^2	0.3472	0.1594
Gamma	0.609	0.406

Abbreviations: Tx, treatment; OP, outpatient.
* $p < .05$.
** $p < .01$.
[1] Ten observations (3%) were deleted due to missing values in predictors.
[2] Thirty-seven observations (5%) were deleted due to missing values in predictors.

respectively. Tables 4 and 5 display comparable results from the four 24- and 36-month models, respectively, conditioned on the 6-month outcomes. The remainder of this section synthesizes the findings, broken out by non-treatment vs. treatment factors.

3.2.3. Non-treatment factors

As shown in Table 3, participants who were predominantly homeless in the 6 months prior to intake were significantly less likely to be stably housed between intake and 6 months if they were black (OR = 0.35), and more likely to be housed if they had one or more economic dependents (OR = 2.98) or were marginally homeless (rather than literally) prior to intake (OR = 3.38). Participants who were marginally homeless (rather than literally) prior to intake *and* stably housed at 6 months (Table 4) were far more likely to still be housed at 24 months (OR = 10.84).

On the other hand, primary problem substance and the problem severity variables, which were highly predictive of baseline homelessness, did not discriminate 6-month status among initially homeless participants at all. In part, this

Table 4

Odds ratios and summary statistics from conditional logistic regression models: stably housed versus not stably housed at 24 months

Effect	Condition			
	Homeless at intake, not stably housed at 6 months (n = 151)[1]	Homeless at intake, stably housed at 6 months (n = 108)[2]	Housed at intake, not stably housed at 6 months (n = 166)[3]	Housed at intake, stably housed at 6 months (n = 452)[4]
Age				1.086**
Black				
Female	5.829**			
Education				
Number of treatments pre-intake				
Primary problem substance				
Alcohol				
Crack				
Heroin				
Pregnant/Postpartum				
Dependents (y/n)				1.848*
Total monthly income at 6 months				
Problem severity at 6 months				
Alcohol	1.376*			
Drugs	0.705*	0.587*		
Legal				
Employment		0.613*		
Medical	0.700**			
Psychiatric	0.776*			
Family	1.275*			
Social			0.753*	
Marginally homeless		10.837*	N/A	N/A
R^2 (non-Tx factors)	0.2175	0.2226	0.0679	0.0979
Max-rescaled R^2	0.2928	0.3108	0.0966	0.1475
Cohort				
Mandated to 1 or more Tx		0.045**		0.517*
Total days in Tx, intake to 24 months				
Tx staff:				
Helped w/living arrangements				0.279*
Addressed medical needs				
Provided services to address employment				
Addressed substance abuse needs		6.154*		
Addressed family/social situation				
Addressed psychological/emotional status				
Days attending OP self-help, last 30				
R^2 (all factors)	0.2175	0.3397	0.1051	0.1226
Max-rescaled R^2	0.2928	0.4743	0.1495	0.1847
Gamma	0.538	0.732	0.413	0.495

Abbreviations: Tx, treatment; OP, outpatient.

  * $p < .05$.

  ** $p < .01$.

[1] Fifteen observations (8%) were deleted due to missing values in predictors.

[2] Thirteen observations (10%) were deleted due to missing values in predictors.

[3] Twenty-three observations (11%) were deleted due to missing values in predictors.

[4] Thirty-six observations (7%) were deleted due to missing values in predictors.

Table 5
Odds ratios and summary statistics from conditional logistic regression models: stably housed versus not stably housed at 36 months

Effect	Condition			
	Homeless at intake, not stably housed at 6 months (n = 151)[1]	Homeless at intake, stably housed at 6 months (n = 114)[2]	Housed at intake, not stably housed at 6 months (n = 173)[3]	Housed at intake, stably housed at 6 months (n = 439)[4]
Age	0.920*			
Black		0.065*		
Female				
Education				
Number of treatments pre-intake				
Primary problem substance				
Alcohol				3.384*
Crack				0.778*
Heroin				
Pregnant/Postpartum	0.277*			
Dependents (y/n)				
Total monthly income at 24 months	1.001*	1.001*		1.001*
Problem severity at 24 months				
Alcohol	0.718*			
Drugs	0.655**			
Legal	0.511**	0.621**		0.761**
Employment				
Medical			1.317**	
Psychiatric		0.661*		
Family	1.373*			
Social		2.215*		
Marginally homeless			NA	NA
R^2 (non-Tx factors)	0.2820	0.2350	0.1089	0.0939
Max-rescaled R^2	0.3780	0.3298	0.1527	0.1484
Cohort		0.070**	0.277**	
Mandated to 1 or more Tx				0.387**
Total days in Tx, intake to 36 months				
Tx staff:				
Helped w/living arrangements				
Addressed medical needs				
Provided services to address employment				
Addressed substance abuse needs				
Addressed family/social situation				
Addressed psychological/emotional status	0.308*			
Days attending OP self-help, last 30				
R^2 (all factors)	0.3236	0.3119	0.1672	0.1098
Max-rescaled R^2	0.4338	0.4376	0.2343	0.1736
Gamma	0.645	0.681	0.505	0.471

Abbreviations: Tx, treatment; OP, outpatient.

  * $p < .05$.
  ** $p < .01$.
[1] Fifteen observations (8%) were deleted due to missing values in predictors.
[2] Thirteen observations (10%) were deleted due to missing values in predictors.
[3] Twenty-three observations (11%) were deleted due to missing values in predictors.
[4] Thirty-six observations (7%) were deleted due to missing values in predictors.

could be explained by reduced sample size; however, for all variables but one (primary substance crack), the absolute distance of the odds ratios from 1.0 is also smaller, and a sample size of 295 is still reasonably large to detect effects of clinical interest. A more likely explanation is reduced heterogeneity on the primary problem substance and problem severity variables. Restricting the sample to initially homeless participants concentrates those most likely to be crack addicts and those with high severity scores. Despite showing fewer significant predictors than the baseline model, non-treatment factors in the initially homeless 6-month model explained about 17% of outcome variance, about half again as much as the baseline model.[11] Inspection of the R^2s in Tables 2 through 5 shows that models conditioned on initial homelessness outperform the unconditioned (baseline) model on this criteria, as well as models conditioned on initial housing. This holds for full models and those with non-treatment factors only.

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[11] Variance accounted for is estimated relative to a fully saturated model, as assessed by the generalized pseudo-R^2 statistic (Hosemer & Lemeshow). Like true R^2 in linear models, pseudo-R^2 ranges from 0 to 1.

Among initially housed participants, those who were older and had fewer prior treatment experiences were more likely to maintain residential stability through 6 months post-intake. Unlike the homeless group, primary problem substance *did* significantly discriminate 6-month status in the initially housed group, with crack again predicting no stable housing (OR = 0.69, $p < .05$). The capacity for crack addiction to discriminate in the unconditioned baseline model, and again in the 6-month model conditioned on baseline housing, may be demonstrating its persistence in driving people into homelessness. That is, crack predicts greater homelessness at baseline, but for crack users who are not homeless at baseline, crack predicts greater homelessness at 6 months. Similarly, for users who are stably housed at baseline and stably housed at 6 months, crack as the primary problem substance significantly predicts greater homelessness at 36 months (OR = 0.78, $p < .05$). However, the single most persistent non-treatment predictor through the first 2 years was whether or not the participant reported that persons were dependent on him/her for food/shelter. Controlling for all other factors, a "yes" to this question at baseline significantly increased the odds of being stably housed at baseline, at 6 months under both conditions (initially housed and initially homeless), and at 24 months if stably housed at both baseline and 6 months.

With one exception, higher problem severity at baseline did not predict 6-month residential status among initially housed or initially homeless participants. In contrast, later problem severity (at 6 months) predicted 24- and 36-month residential status in several conditions, and the direction was typically negative—higher scores on all eight severity scores predicted greater homelessness in one or more of the 24- or 36-month models. Occasionally, the association was positive, and we investigated these anomalies where possible. For example, the sign of the alcohol severity parameter in predicting 24-month outcomes among initially homeless who were not yet housed at 6 months (OR = 1.38, $p < .05$) appears to have resulted from a high correlation between alcohol and drug severity ($r = 0.42$, $p < .0001$), reflecting the high levels of poly-abuse in this subgroup. When an interaction term was added (alcohol severity × drug severity), the alcohol parameter reversed sign, making the alcohol severity leaned toward predicting non-stable housing (though the term was now nonsignificant).

Finally, total monthly income reported at 24 months predicted a positive housing outcome at 36 months in all four conditional groups (statistically significant in the two groups that had been stably housed at 6 months, tending toward significance ($p < .10$) in the two groups that had not been stably housed at 6 months). That this predictor would appear "across the board" at 36 months was surprising because prior to then it had not appeared at all.

3.2.4. Treatment factors

As noted in section 2.3., Analysis, a hierarchical "set" approach was employed, offering a measure of protection against the misinterpretation of selection bias as treatment effects. However, the possibility of effect estimates being biased due to omitted covariates cannot be ruled out. Despite the richness of the Chicago Target Cities data, we were constrained in the number of predictors we could put in the models, particularly the 24- and 36-month models in which the baseline sample size was split four ways. Predictors not in these data have also demonstrated importance in predicting housing outcomes in other studies of this population (e.g., Erickson, Stevens, McKnight, & Figueredo, 1995).

Homeless participants who were legally mandated to the index treatment were significantly more likely to achieve stable housing by 6 months (OR = 3.28, $p < .05$). On the other hand, participants who were stably housed at 6 months were less likely to remain stably housed at 24 months if they were mandated to treatment at any point in the 2 years (OR = 0.05 and 0.52 for initially homeless and initially housed, respectively). For those who were initially housed, this effect persisted at 36 months as well (OR = 0.39). One possible explanation is a higher incidence of incarceration prior to the 24-month interview, which precluded the possibility of stable housing. In fact, 18% of participants stably housed at 6 months *were* in jail, prison, or detention at some point in the 18–24-month interval, though half of these were there for less than one month. Note that the negative relationship between legal mandate and housing outcome held regardless of initial housing status, but *not* for participants who were homeless at 6 months.

Controlling for background variables and initial problem status, amount of treatment—as measured by total days in treatment between intake and the specific follow-up interview being conducted—was nonsignificant in all models. This was somewhat unexpected given the frequency with which retention in treatment has predicted outcomes in prior research (Baekeland & Lundwall, 1975; De Leon, 1991; Hser, Anglin, & Liu, 1991; McCusker, Stoddard, Frost, & Zorn, 1996; Stahler et al., 1993). One possible explanation is that treatment as measured by quantity alone is divorced from the modality in which it is offered (30 days of methadone maintenance is not 30 days of short-term residential), as well as other criterion-relevant information embedded in that choice. The inclusion of modality was complicated by the fact that clients entered multiple modalities over the 3-year period. We attempted to include *index* modality in the models, both with and without days in treatment. For a variety of reasons the results were problematic, so modality was dropped from the model. Self-help group attendance also failed to predict housing status after controlling for other factors.

Finally, the effects of specific treatment components were weak and inconsistent. Substance abuse counseling and discharge referrals were associated with improved housing outcomes in only one of the four groups, and only at 24 months. Receiving help with living arrangements was a strong predictor of housing status in the two 6-month outcome models but in the wrong direction; i.e., participants

reporting getting the service did worse. It is unlikely that getting help with housing "caused" homelessness. It more likely resulted from a combination of two related phenomena: (1) the relatively small proportion of participants (11%) who reported receiving the service at any time over the 3-year period, and (2) the failure of the covariates to "balance" across the two groups being contrasted. Consequently, need for the service was not controlled for, so it went to the individuals most predisposed to remain homeless (in the initially homeless condition) or become homeless (in the initially housed condition). The relationship persisted in only one of the four 24-month models, and in none of the 36-month models.

4. Discussion

4.1. Movement across categories

As noted previously, 72% of initially homeless participants improved their residential status after entering treatment. Among initially homeless participants who became stably housed between intake and 6 months, three fourths maintained that status at 24 months. The contrast between the proportion of initially homeless participants who were stably housed after 3 years (61%) and that of initially housed participants who were homeless after 3 years (14%) is also striking. By all appearances, residential status of homeless participants was far more likely to improve than residential status of housed participants was to worsen. The *net* effect on the entire analysis sample (excluding those institutionalized at baseline) was a 43% reduction in homelessness from the pre-baseline period to the 36-month follow-up.

4.1.1. Alternative explanations

While plausible that these impressive findings are due to treatment, some examination of alternative explanations is warranted before drawing that conclusion, particularly in light of the weakness of specific treatment factors in predicting outcomes. Some of the contextual and methodological factors that may have contributed to this pattern include: attrition bias, incarceration masquerading as improvement and related problems with the institutionally housed category, regression to the mean, and socioenvironmental factors.

4.1.2. Attrition bias

The analysis sample consisted of participants whose (1) predominant residential status at baseline was not institutional, and (2) residential status was non-missing at all three waves. Those who failed the second requirement included those who were not followed up at 6 or 24 months as well as those who were followed up but did not answer the questions on residential status. There were 139 such participants, meaning the effective follow-up rate for the anal-

ysis sample was 87% (963/(963+139)). While a follow-up rate this high substantially reduces the threat of attrition bias, we nonetheless ran some comparisons between the followed-up and non-followed up samples.

Even with a high follow-up rate, attrition bias remains a threat if the rate is differential across groups. Under the theory that initially homeless participants who are not followed up are more likely to be homeless at follow-up than their initially housed counterparts, a lower follow-up rate among the initially homeless group would lead to an inflated estimate of the percent stably housed at follow-up, and therefore, an inflated estimate of the percent reduction in homelessness. Follow-up rates *were* in fact slightly higher among initially housed participants than initially homeless participants (89% vs. 83%, respectively). With 1,102 persons in the sample, this difference achieved statistical significance ($\chi^2_{adj} = 6.54$, $p < .05$), but its effect size conversion was only 0.17 *SD* units, a relatively trivial difference by most standard benchmarks. In practical terms, it is not large enough to substantially bias the estimate of percent reduction in homelessness under most reasonable attrition scenarios.

With respect to the regression models, odds ratio estimates could be biased if followed-up and non-followed up participants differed on factors that affected the criterion outcome. Both initially homeless and initially housed participants were significantly more likely to be followed up if they were black and lower in drug severity. In addition, homeless participants were more likely to be followed up if they were female or scored lower on family problems, while housed participants were more likely to be followed up if they had more prior treatment episodes and scored lower in employment problems. However, neither group showed differences between followed and non-followed participants on the majority of predictors tested, including education, primary substance, pregnant/postpartum status, existence of dependents, whether treatment was prompted by the criminal justice system, cohort (CIU v. pre-CIU), or severity of medical, alcohol, legal, social, or psychiatric problems. There also was no difference in the homeless group on initial level of homelessness (literal v. marginal). Given the low levels of attrition in both groups, and only a handful of predictors on which the followed-up and non-followed samples differed, substantial bias from attrition is unlikely.

4.1.3. Incarceration masquerading as improvement and related problems with the institutionally housed category

As noted above, 89% of literally homeless participants had improved their status (i.e., were no longer literally homeless as defined above) at 6 months post-intake. A caveat is warranted regarding those who "improved" to institutional status (as defined above) because this included those who became incarcerated (defined as in jail, prison, or detention). However, further examination of these cases revealed that only five of the 23 literally homeless

participants that transitioned to institutionally housed status at 6 months were incarcerated for any period of time in that interval, with two of the five for less than 1 month. Results were much the same for the 70% of marginally homeless participants that had improved their residential status at 6 months post-intake. Of those participants that had primarily resided in institutional settings in this interval ($N = 57$), 12 had been incarcerated, six for less than 1 month. Moreover, because our concept of predominant residential status represented the composite category occupied through the majority of the 6-month interval, participants incarcerated for less than 1 month would have still been classified in that category from time spent in other institutional settings (e.g., residential treatment). In sum, the data provide no support that incarceration substantially affected the transition rates cited above.

We also examined participants who were institutionally housed at 24 and 36 months to determine what was driving their institutional status. The concern was that the contrast between initially homeless participants who achieved stable housing and initially housed participants who lost it might be distorted by differences between the two groups in the way that clients who became institutionalized were distributed *among* institutional settings. If, for the sake of comparison, we reclassified residential treatment as housed and incarcerated as homeless, it would increase the proportion of initially homeless participants who were stably housed from 60% to 67% at 2 years, and from 61% to 67% at 3 years. Meanwhile, it would increase the proportion of initially housed participants who were homeless from 16% to 22% at 2 years, and from 14% to 20% at 3 years. The original conclusion would be unchanged: Residential status of homeless participants was far more likely to improve than residential status of housed participants was to worsen. The 43% reduction in homelessness would be reduced to 37%, still a substantial amount from a policy standpoint.

4.1.4. Regression to the mean

Regression to the mean (RTTM) must be considered in any evaluation design lacking a comparison group. In treatment research, RTTM has both a statistical and a clinical interpretation. Statistically, it refers to the tendency for individuals who score at the extreme ends of a distribution on fallible measures to exhibit less extreme observations upon subsequent measurement. Clinically, RTTM *does* connote real improvement over time, but still unrelated to the treatment. There are a number of reasons why this might happen, but the most obvious is that clients do not enroll in treatment (or get referred) at random points in their lives. Rather, they tend to enroll after "hitting bottom," not only in terms of their substance abuse problem, but psychologically, economically, socially, legally, etc. Moreover, by agreeing to enroll in treatment, they show at least minimal motivation to change. Alternatively, if they are mandated to enroll, motivation is provided for them (to avoid jail, regain custodial rights, etc.).

At least four factors reduce the likelihood that RTTM is primarily responsible for the observed change in the present study. First, statistical RTTM is only possible when dependent measures are unreliable. As described in section 2, Methods, we constructed the Drake et al. (1995) composite scores in part because they had previously demonstrated acceptable levels of reliability among homeless treatment-seeking substance abusers. Second, the decision to extend the original 60-day composites to "predominant residential status" over 6 months was intended to further increase reliability as well as temporal stability. This in turn decreases vulnerability to within-subject sampling error in general and regression to the mean in particular. Third, with respect to clinical RTTM, there is nothing in the literature to suggest that homeless substance abusers can achieve and maintain stable housing at the rates observed here without outside intervention. Fourth, the fact that a nontrivial percentage of housed participants did lose and fail to regain their housing—while not negating the RTTM argument—does erode its parsimony by introducing additional interactions and assumptions.

4.1.5. Socioenvironmental factors

Because the study design lacked a comparison group, socioenvironmental factors independent of participant characteristics and treatment could also be responsible for the observed positive effects. Specific explanations might include an increase in HUD Section 8 housing vouchers, an upturn in the low-income housing market, or an improving employment environment. The data collection period coincided with positive strong economic growth nationally, which also extended to Chicago. Therefore, an argument that socioenvironmental factors were *in part* responsible for the observed trends cannot be readily dismissed. Examining these factors was outside the scope of the present analysis, but could in principle be done.

In sum, the high percentage of homeless substance abusers that achieved and maintained stable housing is consistent with a conclusion that treatment reduced homelessness in the Chicago Target Cities sample. Alternative explanations cannot be entirely ruled out, but are insufficient to nullify the overall conclusion.

4.2. Predictors of movement across categories

As noted above, factors discriminating homeless and housed participants at baseline were consistent with prior literature. Several non-treatment factors predicted housing outcomes in the conditional logistic regression models in multiple groups and at multiple time points. The most consistent non-treatment negative factor was crack as the primary substance, which appears to be a persistent risk factor for becoming and remaining homeless. This is consistent with prior observations that a high proportion of the homeless participants in the Chicago Target Cities were female crack addicts who were operating as commercial

sex workers or were forced into "turning tricks" to support their habit. In fact, of the 640 women in our analysis sample, 160 reported recently trading sex for drugs or money. Of this subset, 48% of the 160 were homeless at baseline, almost twice the rate of women who reported *not* trading sex for drugs or money (28%). Among commercial sex workers, the nature of street prostitution—most notably the need to turn so many tricks per day—impedes the ability to maintain stable housing. In contrast, the 370 participants whose primary problem substance was heroin were significantly *more* likely to be housed, possibly because many (approximately one third) were already in methadone maintenance prior to intake. Consequently, some of what appears to be a background discriminator may in part be a positive effect of pre-index treatment. This pattern is consistent with the study by Royse et al. (2000) of 5,225 crack users and drug injectors drawn from five different geographic areas. The authors found that (a) substance abusers who were female and persons of color were more likely to be homeless when other variables were controlled, and (b) trading sex for money was positively associated with homelessness, while participating in methadone detoxification and methadone maintenance programs seemed to offer some protection from homelessness.

The most consistent non-treatment positive factor was whether or not the participant reported that persons were dependent on him/her for food/shelter, which appears to be a persistent protective factor for achieving housing and preventing homelessness. This might be reflecting that (1) economic responsibility for others (particularly children) provides motivation to become and stay stably housed, (2) participants with dependents have more opportunities for direct housing subsidies (e.g., HUD vouchers) or public assistance to put toward housing (e.g., Temporary Assistance to Needy Families), (3) reverse causation in selected models (e.g., regaining custodial rights was contingent on finding stable housing), or (4) the variable is simply a close surrogate for something else that might predict the same way, such as being in a family. The finding disappeared entirely at 36 months, which may simply reflect shifting in the sample over time; those with dependents at 24 and 36 months were a different subset than those with dependents at baseline.

It was somewhat surprising that psychiatric severity did not significantly discriminate between the housed and non-housed conditions in the baseline or either 6-month model, given the well-documented role that mental illness plays in causing and sustaining homelessness, particularly those with a co-occurring substance use disorder (Drake, Osher, & Wallach, 1991). The recruiting process did not exclude clients with mental health problems. Psychiatric severity did discriminate at 24 months among initially homeless participants who were still not stably housed at 6 months, and at 36 months among initially homeless participants who were stably housed at 6 months, in each case with greater severity predicting homelessness. In addition, pre-intake treatment history was significant in one model only—all else

equal, initially housed participants with fewer prior treatment experiences were more likely to remain stably housed through 6 months post-intake. Stahler and Stimmel (1995) also found that fewer prior treatment episodes predicted successful outcomes among homeless substance abusing men in Philadelphia. Most likely, this reflects the natural association between severity and treatment seeking, analogous to individuals seeing a doctor because they are ill. In our sample, it may also reflect that housed participants with higher needs were more likely to present to a residential facility, which classified them as institutionally housed on our measure.

The most consistent treatment factor was whether the participant was legally mandated to the index treatment, though the direction of the effect changed over time; at 6 months it was positively associated with stable housing, while at 24 and 36 months this association was negative. Variables comprising the treatment itself such as amount of treatment and receipt of specific treatment components did not predict housing outcomes; nor did attendance at self-help meetings. Both treatment and non-treatment factors explained more outcome variance among initially homeless participants than among housed participants.

Drawing clear treatment implications from these findings is not simple. There is reasonably good evidence that satisfactorily addressing the participant's core substance abuse needs (as perceived by the participant) resulted in better housing outcomes at 24 months in the homeless at intake, stably housed at 6 months group, but this finding was isolated to that group at that time point. In addition, reported receipt of ancillary service components contributed little to discriminating housing outcomes. One possible reason is that the service components were inherently ineffective. As or more likely, however, was that (1) most participants either didn't get them or don't remember getting them (we can't disentangle this since it's all self-report) and (2) when they were provided, they were insufficient for this population. We know the former to be true from the distributions; the proportion of participants reporting receiving a given service at any time in the 36-month interval ranged from 5% to 20%. Of course, these percentages reflect participant dropout and non-participation even when the provider made the service available. They also reflect the demand-based nature of some ancillary services (some participants do not receive them because they do not want or need them), which also complicates estimating their effectiveness, as demonstrated by the negative effects of "receiving help with living arrangements" described earlier. Still, it seems telling that while 32% of the sample self-reported as homeless at intake, only 11% reported receiving help with living arrangements at any time over the next 3 years. Moreover, this 11% was distributed over the entire sample, not just those self-reporting as homeless.

The latter argument—that services may have been insufficient even when provided—is plausible as well. The

Chicago Target Cities programs were not specifically tailored to clients who were homeless, and may not have included the level of intensity and comprehensiveness recommended for such programs. While the development of services for homeless addicts has lagged behind the development of primary health care services and services for treating mental illness (McMurray-Avila, Gelberg, & Breakey, 1999), some guidelines have emerged, largely spurred by two rounds of McKinney community demonstration projects sponsored by the National Institute on Alcohol Abuse and Alcoholism (Argeriou & McCarty, 1990; Conrad, Hultman, & Lyons, 1993; Orwin et al., 1994, 1999; Stahler & Stimmel, 1995). Stahler and Stimmel (1995) drew the following general conclusions regarding treatment programming needs for homeless addicts: (1) Develop treatment programs that focus not only on the addiction, but also address the tangible needs of people without stable living arrangements; (2) Develop flexible, low-demand interventions that can accommodate clients who are not initially willing to commit to more extended care; (3) Provide longer-term, continuous interventions for this population (aftercare needs to address not only the maintenance of sobriety, but also the tangible needs and social isolation of clients); and (4) Match clients to appropriate treatment services based on characteristics such as educational attainment, cultural background, severity of substance use, criminal involvement, and level of social isolation.

More recently, the Center for Substance Abuse Treatment (2001) summarized the lessons learned from the McKinney demonstrations as well as more recent federal initiatives sponsored by SAMHSA. They concluded that positive outcomes are most likely to occur when: (1) basic needs for food, shelter, and safety are met, substance abuse treatment is linked with housing, and clients are permanently housed; (2) barriers to receiving services are acknowledged and reduced; (3) a range of services—e.g., housing, substance abuse treatment, mental health, primary health care, education, employment, substance abuse prevention—are provided in an integrated, seamless manner; and (4) clients are never discharged from one treatment system element (e.g., detoxification) without a firm link to another appropriate form of service.

On the surface, this analysis seems to present conflicting results. On the one hand, there was substantial net movement out of homelessness, with no truly viable explanation other than treatment. On the other hand, the predictors in the models failed to identify specific treatment factors responsible for this movement. In this light it is useful to consider some limitations of the modeling itself.

4.2.1. Limitations to the interpretation of predictors

Limitations to the interpretation of treatment and non-treatment predictors included how homelessness was operationalized, selection bias, and limits on generalizability. These limitations are discussed next. Some of the alternative ex-

planations to treatment as the cause of outcomes discussed above (in particular, socioenvironmental factors) could also have affected the predictor estimates.

4.2.2. Operationalization of homelessness

The operationalization of homelessness has long been recognized as problematic, and was one of the reasons for the wide variation in national point-prevalence estimates of homelessness produced in the 1980s (Rossi, 1989). Whether to include precariously housed along with literally homeless, what constitutes precariously housed, and how long someone must be undomiciled to be classified as homeless are some of the issues that researchers and advocates have debated (Link et al., 1994). The relevance to the homelessness measure used in the present study is that defining 6-month status based on the modal housing category over that period of time blurs some of the dynamism that may be present in the housing status of individuals within that 6-month period. For instance, there is a classic pattern exhibited by homeless individuals who rely on general assistance or other fixed but meager monthly income flows: the individual is housed for the first few weeks of the month in an SRO but is in shelters or on the streets for the last week of the month. Others are simply precariously housed (e.g., doubled up) for the entire period. Both types would be depicted in our measure as marginal homelessness, when their actual housing experiences are quite different. We recognized that our choice of a single value to represent a 6-month period blurs some of the dynamism of homelessness. We accepted this tradeoff for the practical reason that trying to capture the dynamism would have greatly complicated both the independent and dependent variables of interest. Still, an alternate operationalization of homelessness that was more sensitive to the dynamism may well have yielded a different picture of which treatment and non- treatment factors predicted housing outcomes, and to what degree.

4.2.3. Selection bias

The 6-month and 24-month models bring in treatment factors. Inferences about treatment effects from non-experimental data are inherently vulnerable to selection bias. In such situations, it is customary to model background variables—e.g., demographics—as a proxy for the selection process (case-mix adjustment). However, this approach has technical drawbacks (Orwin & Goldman, 2004). Ideally, the problem is addressed by constructing selection models and measurement models and analyzing the data with a structural equation modeling program such as LISREL or EQS, or attempting to "balance" covariates across natural variation in dosages, e.g., propensity scoring. However, the requirements of these approaches were not met in these data. As a simpler alternative, a hierarchical "set" approach was employed (Cohen & Cohen, 1975), with the background variables set entered prior to the treatment variables set. The hierarchical approach produces

a conservative estimate of the treatment effects, because outcome variance common to both the background variables and the treatment variables are "credited" to the background variables. Therefore, it provides some measure of protection against the misinterpretation of selection bias as treatment effects, at the cost of potentially underestimating those effects.

4.2.4. Limits on generalizability

The Chicago Target Cities sampling frame was designed to achieve a representative sample of persons entering publicly funded treatment facilities in the Target Cities catchment area (Chicago's West Side). It was *not* designed to achieve a representative sample of homeless substance abusers in Chicago. The characteristics of initially homeless participants suggested they were over represented by one subgroup of the homeless substance abusing population: young, African-American, crack-addicted females, many of whom were operating as commercial sex workers or were forced into "turning tricks" to support their habit. Consequently, the treatment and non-treatment factors that predicted housing outcomes in the present sample cannot be generalized to other homeless subgroups, e.g., older single white males whose primary substance was alcohol. The same treatment factors that did not predict well in this sample—amount of treatment, specific treatment services, and self-help attendance—might have performed differently within other subgroups.

Acknowledgments

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