Published in final edited form as:

AIDS Behav. 2018 July ; 22(7): 2340–2359. doi:10.1007/s10461-017-1827-1.

Overlooked Threats to Respondent Driven Sampling Estimators: Peer Recruitment Reality, Degree Measures, and Random Selection Assumption

Jianghong Li^{1,*}, Thomas W. Valente², Hee-Sung Shin², Margaret Weeks¹, Alexei Zelenev⁴, Gayatri Moorthi¹, Heather Mosher¹, Robert Heimer³, Eduardo Robles¹, Greg Palmer¹, and Chinekwu Obidoa¹

¹Institute for Community Research, Hartford, CT

²University of Southern California, Keck School of Medicine, Los Angeles, CA

³Yale University, School of Public Health, New Haven, CT

⁴Yale University School of Medicine, Section of Infectious Diseases, New Haven, CT

Abstract

Intensive sociometric network data were collected from a typical Respondent Driven Sample (RDS) of 528 people who inject drugs residing in Hartford, Connecticut in 2012–2013. This rich dataset enabled us to analyze a large number of unobserved network nodes and ties for the purpose of assessing common assumptions underlying RDS estimators. Results show that several assumptions central to RDS estimators, such as random selection, enrollment probability proportional to degree, and recruitment occurring over recruiter's network ties, were violated. These problems stem from an overly simplistic conceptualization of peer recruitment processes and dynamics. We found nearly half of participants were recruited via coupon redistribution on the street. Non-uniform patterns occurred in multiple recruitment stages related to both recruiter behavior (choosing and reaching alters, passing coupons, etc.) and recruit behavior (accepting/rejecting coupons, failing to enter study, passing coupons to others). Some factors associated with these patterns were also associated with HIV risk.

Keywords

Respondent driven sampling; HIV/AIDS; people who inject drugs; hidden population; social networks; peer recruitment behavior

*Corresponding author: Jianghong Li, Institute for Community Research, 2 Hartford Square West, Suite 100, Hartford, CT 06106, Phone: (860) 278 2044 ext. 3297, Fax: (860) 278 2141, jianghong.li@icrweb.org.

COMPLIANCE WITH ETHICAL STANDARDS

Ethical approval: All procedures performed in studies involving human participants were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards. Informed consent was obtained from all individual participants included in the study. This article does not contain any studies with animals performed by any of the authors.

Conflict of Interest: All authors declare that they have no conflict of interest.

INTRODUCTION

Since its first development in the mid-1990's, Respondent Driven Sampling (RDS) (1, 2) has gained popularity in HIV research and surveillance worldwide for several reasons. First, the most acknowledged reason is its cost-effectiveness in reaching hidden populations at high risk of HIV transmission, such as people who inject drugs (PWIDs), sex workers, and men who have sex with men (3–8). Research staff typically recruit only a handful of participants as “seeds,” then the seeds will be given a small fixed number of peer referral coupons (usually 3) to pass on to their peers. When their eligible peers enter the study (referred to as “recruits” below), they are given the same number of coupons and become recruiters. This process continues until the desired sample size or time period is reached. Second, RDS also gained popularity in the context of several inference models that were claimed to produce unbiased population estimates (1, 2, 9–13). This is a very appealing claim due to the challenge of recruiting a probability sample of hidden populations and the lack of a satisfactory alternative sampling design.

With increasing application of RDS among a variety of populations in different contexts and countries, skepticism has arisen regarding the validity of RDS statistical inference models. HIV researchers and epidemiologists have reported challenges in meeting the underlying assumptions during field implementation and question the implications for the validity of population estimates (6, 14–18). In less than two decades, the inference estimators have gone through several versions of change by addressing the statistical models' unrealistic assumptions about network structure and peer recruitment process. The initial RDS model, referred to as the H1997 estimator hereafter (1), made a strong assumption about the sampling procedure, that is, sample proportions were representative of the population proportions (18). The H1997 estimator is very sensitive to differences in “subgroup homophily”, a term that describes the tendency for people with similar traits to be connected (1, 2, 19). Heckathorn attempted to address this problem in the H2002 estimator by using self-reported degree (i.e., network size) in addition to recruitment matrix data (2, 19). Subsequently, the SH2004 estimator (10) argued that the estimate is asymptotically unbiased and provided better network estimates under six listed conditions (18, 19). This approach was widely used before 2014, as it was built into RDS analysis software RDSAT (20) and was adopted by the US CDC for National HIV Behavior Surveillance (NHBS) (21). The first three estimators are quite similar and are called RDS I by Heckathorn's group (19, 22). A later estimator VH2008 (11), called RDS II (19, 22), permits analysis of continuous variables and included a version of data smoothed variance estimation. H2007 is a further refinement of VH2008, and was claimed by the developer that this estimator “provided a means for controlling for bias from differential recruitment,” the phenomenon in which some groups within the population systematically recruit more than others regardless of their proportion within the population. A simulation study by an independent research group demonstrated that the VH2008 estimator outperformed the SH2004 estimator (18), although others have doubt about the claimed improvement (23).

This doubt was related to the sampling with replacement assumption, on which all estimators described above were based. It means that at each step in the recruitment process, the new recruit is sampled from the entire pool of network members, regardless of whether

some of them have already been recruited. This is contradictory to RDS implementation practice that requires each person only be enrolled once. Gile addressed this problem by introducing a successive sampling design in the G2011 estimator (12). This estimator was built into another RDS data analysis software called RDS Analyst (24). But the G2011 estimator and all previous estimators were still based on an often difficult to achieve assumption that the initial seeds are randomly selected from the target population. The GH2015 estimator (13) used a model assist approach to address this problem, under the condition that the size of the population can be estimated or the sampling fraction is small.

The evolution of RDS estimators suggests that it is possible to improve the performance of these estimators if the underlying assumptions are close to the reality of peer recruitment patterns. It is important to conduct empirical research to better understand the real-world implementation processes and dynamics of peer recruitment. This paper will focus on four common assumptions underlying existing RDS estimators that have not been given sufficient attention. The first two assumptions are straightforward. First, all existing estimators were based on the assumption that *respondents' self-reported degree (network size) is accurate*, and therefore degree data are used as important information in all estimators. The second assumption was mostly stated as this: *a recruit's enrollment probability is proportional to his/her degree*. This assumption is critical in design-based estimators such as SH2004, VH2008, H2007, and G2011 (25). The third assumption, *random selection*, appears in various statements in the literature. Heckathorn's group sometimes described it as "when respondents recruit others, they recruit randomly from all edges [ties] that involve them," (10, 11) a statement emphasizing recruiters' behavior. But more than one separate social connection between individuals, for example coworkers and neighbors, is multiplex. It has also been stated that "peer recruitment is a random selection from a recruiter's network," a description that emphasizes recruitment results by comparing the sample and recruiter's eligible network members (9, 25). Though not previously stated clearly, the recruitment results can be affected by the behavior of recruiters, recruit candidates, and other eligible network members not chosen as recruitment targets. Gile and Handcock repeatedly pointed out the importance of modeling with real world recruitment behaviors, but they tend not to list all assumptions underlying their estimators and assessment approach explicitly and understandably for scholars from different disciplines (12, 13, 18). They did acknowledge that the G2011 and G2015 estimators were built upon earlier RDS estimators and in the family of Markov Chain Theory and random walk sampling methods. According to Volz and Heckathorn (11), "in mathematical terms, a chain-referral sample is analogous to a random walk." Given that the G2011 and GH2015 papers did not formally reject this assumption, it appears that some form of random selection is a universal assumption in all existing RDS estimators, and that the inconsistent language use is a reflection of the lack of clarity in conceptualizing the sources of non-uniform patterns in the RDS recruitment process (25–27). In this paper, we use random selection to mean that "recruits are a random sample uniformly selected from the recruiter's eligible network members," a statement that does not differentiate sources of non-uniform patterns. The fourth assumption in RDS implicitly holds that *recruitment occurs over the recruiter's (direct) network ties*. Since RDS is a link-tracing sampling method (a specific snowball method), this inherent assumption has never been questioned or assessed.

There is a critical need for empirical evaluation of peer recruitment behavior and processes, recruits' response patterns, the nature of social network linkages, and network changes during the period that RDS is implemented in real world settings. Such knowledge will have implications for future development of improved RDS estimators, RDS assumption violation diagnostic tools, and better designs of survey questions to estimate network degree. Due to the hidden nature of the populations studied using RDS methods, obtaining population network properties and observing peer recruitment processes are typically considered impractical. Previous evaluations of RDS performance have been mostly limited to comparing RDS sample composition with other forms of sampling or use of an analytical approach or simulation (18, 25, 28, 29). Our team conducted an intensive mixed methods social network study of people who inject drugs (PWID) with a typical RDS sample of 528 PWIDs in Hartford, Connecticut (2012–2013). Briefly named RDS-Net, this NIDA funded study focused on assessing common RDS assumptions built into most existing estimators. This paper will focus on assessing assumptions about random selection, recruitment occurring over recruiters' network ties, selection probability being proportional to degree, and an assessment of whether self-reported degree is reliable. A unique feature of our study is the acquisition of a list of each respondent's eligible network members' names and comprehensive sociometric network data associated with the full peer recruitment process. With these unique data, it is possible to assess the sample composition and peer recruitment patterns against an eligible pool of recruitment candidates, something typically believed not feasible given the usual resources of studies that rely on RDS for recruiting study participants.

THEORETICAL FRAMEWORK

The RDS-Net study takes a multiplexity perspective of the study population during multi-staged peer recruitment processes and dynamics. A single tie between individuals, such as shared workplace, is a uniplex relationship. Multiple social connections between individuals, for example co-workers and neighbors, is considered multiplex. (30–33). Model based RDS estimators such as H2002, SH2004, H2007, VH2008, and G2015 are all based on a reciprocity model (2, 9–11, 13, 25). The notion here is that when recruiter A knows recruit candidate B, B also knows A; therefore, the knowing tie is reciprocal. However, as HIV researchers familiar with the study population and RDS field implementation, we know that a reciprocal knowing relationship does not always translate into a reciprocal trust tie, intention to recruit tie, or actual recruitment action tie. Figure 1 offers a visual illustration of network multiplexity associated with RDS peer recruitment processes among PWIDs, one of the hidden populations to which RDS is most often applied. For a recruiter, his or her Known Injection Network (A in Figure 1), is the one researchers attempt to estimate with survey or other measures of degree. The Enrollment Network (E in Figure 1) is the network of eligible individuals successfully entered into the study with a recruiter's coupon. RDS developers assume that the Enrollment Network (E in Figure 1) is a random sample of the Known Injection Network (A in Figure 1), ignoring many possible sources of bias stemming from recruitment behaviors (steps 1–8 in Figure 1).

In step 1 of Figure 1, a recruiter may consider recruiting from a subset of his/her Known Injection Network (A in Figure 1), which is the Recruitment Intent Network (B1 in Figure

1). When on the street to recruit, PWIDs could encounter others that (i) they did not intend to recruit and are not in their network (B0 and step 7 in Figure 1), (ii) they did not intend to recruit but are in their network (B2 and step 5 in Figure 1), and (iii) those they did intend to recruit and are in their network (B1 and step 2 in Figure 1). Consequently, the Recruitment Encounter Network (C in Figure 1) includes a mixture of individuals within and outside of the recruiter's network, those initially targeted by recruiters and those who were not considered. In step 3, a portion of individuals in this mixed Recruitment Encounter Network will receive coupons and become the Coupon Pass Network (D in Figure 1), which is also mixed. Steps 6 and 8 describe factors that are rarely considered when designing estimators. Namely, a successful coupon passing event is not only affected by the recruiter's behavior, but also by behavior of the initial recruit candidate and the next person involved. A person being offered a coupon could choose to accept it and report to the study site, accept it but fail to report to the study site, refuse it, or accept the coupon and pass it to someone else for various reasons. Therefore, the composition of the Coupon Pass Network (D in Figure 1) is more complex than Recruitment Encounter Network (C in Fig. 1), involving interaction of recruiters, potential recruits, people they interact with, and researchers. In step 4, a portion of those who received coupons will successfully enroll in the study, becoming the Enrollment Network (E in Figure 1). This step is mostly out of the control of the initial recruiter. The presence of Steps 7 and 8 violates the assumption that peer recruitment occurs over a recruiter's direct network ties, the assumption on which all extant RDS estimators were based. Any non-uniform pattern associated with recruitment actions in steps 1–6 undermines the random selection assumption.

Regarding the model's sensitivity to violations of the random selection assumption, proponents of RDS previously indicated that when peer recruitment patterns are not uniformly at random, especially associated with recruiters or recruits' variable of interest (e.g., HIV risk), it could pose a threat to the validity of the population estimates (9, 34, 35). None of the early assessments of random selection was based on a concept of the multi-stage recruitment process. Selection of variables in previous assessments was limited to individual level demographic variables of sampled participants or their ego network members (6, 36–39). Recent social network study findings demonstrated that network composition, relationship type and tie strength are associated with HIV risk. For example, large and dense drug risk networks, or a high proportion of active drug users within one's ego network, have been found to be associated with elevated HIV risk behavior (40–43). By contrast, a large support network and a high proportion of supportive network members within the whole social network have been found to be protective (41, 44, 45). For the same PWID, injection behaviors with close network members with whom they share resources can produce greater risks for HIV than behaviors with those weaker ties (46–49). This paper will therefore assess whether and how HIV risk factors and relationship factors affect peer recruitment processes and network degree measures, which have been largely overlooked in previous RDS model assessment studies.

METHODS

RDS Survey Sample Recruitment and Data Collection

To maximize the practical value of research findings, the RDS-Net study sample recruitment protocol was designed to be comparable to the standard protocol of the National HIV Behavior Surveillance (NHBS) PWID cycles sponsored by the US Centers for Disease Control and Prevention (50). Although staff of the Institute for Community Research (ICR) had over 25 years of history conducting research among PWIDs in Hartford, we began the study with three months of formative outreach and ethnographic community observation. This systematic re-assessment of the community allowed field staff to become familiar with the current PWID population and for some members of the PWID population to become familiar and comfortable with the project field staff (51). This fieldwork also facilitated seed selection, follow-up of survey participants after two months, and sociometric network data construction.

Informed by formative research, we selected six seeds (the solid black nodes in Figure 2) with large network size and diverse in gender, ethnicity, neighborhood of residence, and type of drug injected. Each seed was given three peer recruitment coupons, and they all recruited others into the study. All subsequent study participants also received three recruitment coupons, except the last 17 at the end of recruitment. A total of 528 PWIDs completed the baseline survey. Of these, 511 received recruitment coupons. As shown in Figure 2, the size of the six chains were 4, 17, 25, 88, 110 and 286 participants, and the lengths of these chains (furthest number of links from the seed) were 2, 4, 11, 16, 26, and 29 waves, respectively. A small number of seeds and very long waves in most chains is a good indicator of sample convergence, minimal sample dependence on seed selection, minimal impact of differential recruitment (9), and excellent field implementation practice. We are confident that our assessment of RDS assumptions are minimally affected by field implementation of RDS recruitment in this study.

Each study participant received \$25 for completing the baseline and \$30 for completing a 2-month follow-up survey. Participants who recruited other eligible network members into the study additionally received \$10 for each successful recruit up to \$30. The incentive amount was determined by local standards for similar research with PWIDs and other at-risk populations. Written informed consent was obtained from all participants prior to any interviewing. The research design and procedures were fully reviewed and approved by the Institute for Community Research's Institutional Review Board.

The baseline survey lasted for about 1–1.5 hours to complete and included three main components. The first component was the typical RDS network size and composition questions (8, 52). The second component included typical individual-level survey questions, such as demographics, drug use and HIV risk behaviors, pro-social and prevention behaviors, perceived peer behavioral norms, and health status. The third component of the survey was a comprehensive ego-network assessment. The purpose of the baseline ego-network component was to generate ego's personal network member name list, to assess the nature of each of their relationships and tie strength with ego, and to assess ego's intention to recruit individuals from among the drug using network members. This ego-network

component of the survey was developed based on the social network literature (53–57) and updated by our research team during previous studies (48, 58–61). The network survey started with a few questions to solicit ego's (the participant's) social network member names/nicknames. Specific name-generating questions included who ego "knows who injected drugs in the last 6 months," "did drugs with," "had sex with," and "feels close to or received support from" in the prior 6 months. In order to maximize reporting of the Known Drug Using Network, we also asked ego to name all other PWIDs they know who also know them. Then, a set of questions about each alter (named network member) was asked, including alter characteristics, relationship type, tie strength, shared drug risk behaviors, shared sexual risk behaviors, perceived alter risks, recruitment intention, recruitment priority, and reasons to be considered when selecting a recruit.

To increase follow-up retention, we collected thorough locator information on each participant at baseline. We conducted routine outreach to locate participants whose follow-up survey due dates were upcoming. Reminder cards were sent out 2 weeks prior to the scheduled follow-up survey date. With these strategies, we reached 414 out of 511 invited recruiters, a follow-up rate of 81% for a PWID population. The main reasons for loss to follow-up were arrest, hospitalization, moved out of the area, and unknown.

Two months after the initial survey, we invited each participant who received coupons (N=511) to return for a follow-up survey. The purpose of the follow-up survey was to understand participants' actual experience recruiting their network members into the study. Each participant's baseline PWID network name list was pre-printed for the interviewer to use as part of the follow-up survey network name list. We asked each ego to list all those they considered giving a recruitment coupon, regardless of whether or not they were listed at baseline (B1+B2 in Figure 1). Then we asked a set of questions about ego's actual attempts to reach each of the intended alters (B1 in Figure 1). We asked about the availability of each alter the participant intended to reach and their refusal/acceptance of the coupon and reasons. If a baseline-listed PWID network member was not listed at the 2-month survey as a Recruitment Intent Network member, we first asked whether this baseline network member was considered for recruitment to minimize recall bias due to forgetting (62–64). If a PWID network member was not listed as a Recruitment Intent member, we then asked why. Consequently, the 2-month Recruitment Intent Network data were more complete than the baseline data, and were used in data analyses described below. To obtain their perspectives as a recruiter, we asked each participant the number of times he/she approached intended alters and their reasons for refusing or accepting invitations. To increase follow-up retention, we collected thorough locator information on each participant at baseline. We conducted routine outreach to locate participants whose follow-up survey due dates were upcoming. Reminder cards were sent out 2 weeks prior to the scheduled follow-up survey date. With these strategies, we reached 414 out of 511 invited recruiters, a follow-up rate of 81% for a PWID population. The main reasons for loss to follow-up were arrest, hospitalization, moved out of the area, and unknown.

Characteristics of the RDS participants are described in Table 1. Similar to other studies conducted in Hartford, CT (59, 60), this sample is mostly male, primarily Latino and African American, and includes a high proportion of homeless, low income, and

unemployed or disabled persons. The vast majority of the sample injected heroin by itself, while about half injected cocaine by itself or injected speedball (heroin and cocaine combined). The mean and mode of drug injection frequency in the last 30 days was 76.34, and 90 respectively, with a range of 1 – 470. About one-third of participants reported sharing syringes or other injection equipment (works) or rinse water in the last 30 days, and 13.3% self-reported being HIV positive. Of the 511 participants who were given coupons to recruit others, 325 recruited at least one person into the study.

Construction of Sociometric Network Data

The sum of all alter entries collected across all ego network surveys is not the number of total unique alters, because one alter's name can be reported by multiple egos. Construction of the sociometric network requires comparing all ego network data to identify and label the same alter (network member) reported by multiple egos (study participants), a process called identity resolution or duplicate removal by some scholars (65–68). When reported alters are among the 528 sampled individuals, identity resolution is much easier, as more data about them is available to make the judgement. We created a variable called Alter Status to identify whether a reported alter was in the sample.

To identify duplicate alter entries and indicate alter status, we developed a series of “connect the dots” working sheets, which contained several ID codes, including: 1) Ego ID, which identifies ego/recruiter as a unique person; 2) Alter Tie ID, which identifies each ego-alter as unique tie; and 3) Final Alter ID, which identifies each alter as a unique person. Other columns included Alter Status, Alter Full Name, Alter Nick/Street Name, key demographics, and other characteristics to assist the network tie verification process. Among these, the Final Alter ID and Alter Status columns needed to be processed and updated regularly. Beginning two months after starting recruitment, our team spent several days each month routinely reviewing collected data and updating the “connecting the dots” worksheet. Each alter's name was compared with all identical and similar names in the entire working sheet to identify potential matches. Data used to facilitate the connecting dots process included: (a) participant locator data, which included full name and street names, primary and additional address, frequent hangout and drug use location, phone numbers, and up to three contact persons' information; (b) ego network data, such as alter characteristics, relationship with ego, ego-alter most frequent meeting place, and their shared behaviors; (c) individual level survey data such as recruiter's name and frequent drug use locations; (d) recruitment tie data; and (e) outreach and field observations of network ties. A final alter ID was assigned to new alters only when the team members could confirm that people named by different egos were the same person. An important key to identity resolution success was staff familiarity with the study population. The two project outreach workers each had over 15 years of experience working with Hartford drug users. They knew many of them by name and had acquired a rich knowledge of observed network tie information within this population.

Although similar connecting the dots procedures had been used in our team's prior studies with good success (59, 60), the expanded breadth of network data collected in this study improved the success rate of alter identity resolution substantially. Among all 3392 alter

nomination entries (one person can be nominated multiple times), we were able to confirm that 1182 entries (34.8%) were sampled individuals, 2115 entries (62.4%) were not, and only 95 entries (2.8%) were unsure. After removing alters who did not inject drugs at baseline and 2-month survey measure, we obtained a sociometric network dataset of 2717 PWID nodes, that included individuals enrolled in the study ($n=528$) and their nominated drug injecting alters.

After completing the connecting dots process, we imported all original directional ties (who named whom as a drug-use tie, support tie, recruitment intention tie, recruitment attempt failure tie, etc.) and node attribute data (ego's individual level characteristics, i.e., demographic, injection frequency, etc.) into social network data analysis software UCINET (69) and graphic network analysis software NetDraw (70). We then created a Maximized PWID Network of all network ties to be used as a substitute for the Known PWID Network (See Figure 1) for each participant for data analyses.

Network Degree and HIV Risk Factor Measures

These sociometric network data provided measures for analyses at the individual, dyadic, and sociometric network levels. The Known Injection Network in Figure 1 is unknown but is traditionally estimated through survey measures. By comparing this traditional approach to alternative measures of recruiter degree, we hoped to develop a better understanding that would allow recommendation of a more reliable measure that is also feasible to collect in future RDS surveys. The types of network degree measures analyzed in this paper include the following:

- a. Total degree: number of outgoing and incoming PWID ties indicated at either baseline or 2-month survey. For each participant, any other ego-centric network of PWID will be a subset of his/her Total PWID Network. For the whole sample, the Total PWID Network matrix will also be the largest and most complete sociometric network matrix compared with matrices based on other ties (i.e., Recruitment Intent Network matrix, Injection Risk Network matrix, etc.). We believe this is the closest measure of Known PWID Network degree, although it is not feasible for most RDS studies to obtain. We use this measure as a proxy for the unknown A in Figure 1.
- b. Typical RDS degree: This is based on most RDS study survey questions, "Think about the people you know and met in the last 6 months, who also know you." "How many of these people use injection drugs?", "How many of those who injected drugs are men?" and so on (8, 21, 71). This is a rough estimate conceptually similar to self-reported (out-degree) known PWID ties (in 'a' above) in sociometric network data analysis. Used in the NHBS survey, this is a very common estimate of network A (21).
- c. Self-reported degree: number of outgoing ties that indicate a potential recruit is a PWID network member (out-degree of PWID ties) at either baseline or follow-up survey. This is one of our estimates of network $A = B1+B2$ in Figure 1.

- d. Peer reported degree: number of incoming ties from alters that indicate ego is a PWID (in-degree of PWID ties) at either baseline or follow-up survey. This is another estimates of network $A = B1+B2$ in Figure 1.
- e. Recruitment intent degree (B1 in Figure 1): number of PWID alters the ego would consider recruiting. This is based on answers to the question at baseline, “If you can give as many coupons as you want to someone who injects drugs on your list, who would it be?” plus additional ties mentioned at follow-up survey when asked, “Who did you think about giving coupons to before contacting anyone?”
- f. Recruitment encounter degree (C in Figure 1): number of PWID alters the ego actually made an effort to reach, based on answers to the question, “Who did you try to give the coupons to?”
- g. Coupon pass degree (D in Figure 1): number of alters who accepted the coupon, based on answers to the follow-up question, “Who actually took the coupons from you?”
- h. Coupon enrollment degree (E in Figure 1): number of successful recruits, based on returned coupon ID, suggesting a recruiter-recruit relationship. This measure assumes no coupon redistribution before it returns, which is the typical assumption of most RDS studies.
- i. Actual enrollment degree (E in Figure 1): number of alters successfully enrolled in the study by ego ID. The enrolled alters may or may not be recruited by the ego who named them.
- j. Matched coupon degree: number of ego-reported recruiter names matched with returned coupon ID confirming the recruiter. Additional network tie verification after connecting the dots was conducted to confirm a matched coupon. This is network E in Figure 1 minus those enrolled from step 7–8 who were not a subset of network A, also minus a subset of those indirectly enrolled from Step 5–6 (not labeled in Figure 1).

The network survey assessed egos’ perception of alters’ HIV risks. That includes egos’ perception of whether alters were homeless, unemployed, HIV positive, injected in a public venue, and shared injection works with others. Ego-alter shared risk measures included ego having injected drugs or shared injection works with alter. Tie strength measures included proximity (number of days in contact with alter in the last 30 days; hung out with alter in the last 6 months; lived with alter in the last 6 months), instrumental support (can go to alter for money if needed; can go to alter for a place to stay if needed), and level of trust (“how much do you trust alter?” [scale of 1–5]).

Conditions Underlying RDS Assumptions, Analytical Questions and Data Analysis Methods

The network research literature suggests that people with large networks tend to under report ties more than those with smaller networks due to forgetting (60, 62–64). In-degree (being reported as another’s network member) instead of out-degree measures (self-reported ties)

were sometimes recommended to address this reporting error (60, 62–64). Our own team also found that drug using peer health advocates with large networks tended to under report the number of individuals with whom they intervened (60), but it appeared easier for the peer intervention recipients to remember peer advocates who had an impact on their life. These findings suggest that typical RDS degree estimates (AS1 in Figure 1), similar to out-degree measures, may not be reliable indicators of true degree. Thus, the first analytical question (See Figure 3) is, how do the above network degree measures compare? Which of them is more representative of the total degree (A in Figure 1) and the enrollment network (E in Figure 1)? We ran descriptive statistics and bivariate correlation analysis to answer these questions.

The second set of analytical questions focused on recruiter-recruit-study site interactions. These bear on three assumptions (See Figure 3): recruitment occurs from recruiter's network ties (AS2 in Figure 3); random selection recruitment (AS3 in Figure 3); and recruits' probability of entering an RDS study is proportional to his/her degree (AS4 in Figure 3). For AS2 to be true, two scenarios must both be true. First, recruiters do not recruit outside of their social network (C1 in Figure 3). Second, an alter who accepts a coupon will not pass it on to others (C6 in Figure 3). For the random selection assumption (AS3 in Figure 3) to be true, all six conditions listed in Figure 3 must be true. These include: Recruiters do not recruit outside of their network (C1 in Figure 3); Recruiters always select, approach, and reach randomly from their network for recruitment purposes (recruiter behavior, C2 in Figure 3); Eligible potential recruits will uniformly accept the invitation and report to the study site (recruit behavior, C3 in Figure 3); Unless one is already in the study, the alter who accepted a coupon will not fail to report to the study site, or will fail to report randomly (recruit behavior and interaction with study site, C4 in Figure 3); and the alter who accepted a coupon will not pass it on to others (recruit and third party interaction). Among these, conditions C2 to C6 must also be met for assumption AS4 to be true.

To date, very few empirical studies (39, 72, 73) have attempted to assess the random selection assumption without a conceptual distinction between recruiter behaviors and recruit behaviors or have even considered recruit behaviors. Some researchers have assessed the possibility of condition C1 in Figure 3, which is B0 in Figure 1. These researchers (4) suggest asking recruiters at the time they collect successful referral incentives about their relationship type with the person who passed them the coupon. Others suggest asking recruiters about recruits' coupon acceptance and participation in the study (29). However, none of the existing RDS estimators or RDS researchers have, to date, directly assessed or considered the possibility of indirect recruitment through secondary passage of coupons. To address this gap, our second set of analytical questions is: To what degree does indirect recruitment occur (a subset of those recruited through steps 5–8 in Figure 1)? How do recruits who are directly recruited (without coupon redistribution) and those indirectly recruited (after coupon redistribution) compare? What proportion of indirect recruits were also in the initial coupon holder's network? To answer these questions, we compared three sources of data: recruiter-reported coupon recipient's names, recruit-reported name of the person who gave him/her the coupon, and coupon ID-suggested name of the initial coupon holder. If all these matched, a direct recruiter-recruit relationship was asserted. Otherwise,

the discrepancy suggests coupon redistribution on the street, so we assumed an indirect recruiter-recruit relationship.

The third analytical question is, what factors are associated with each step of the peer recruitment process, indicated by recruitment intention ties, recruitment attempt ties, coupon passing ties, and enrollment ties (including both direct and indirect enrollment)? To address this question, we ran multiple level logistic regression with random effect at the dyadic relationship level. This analysis included 3392 ego and PWID alter ties that were formed by 2633 non-duplicated nominees of the 528 sampled individuals. Independent variables included tie strength between ego-alter and ego perceived alter HIV risk variables. Ego and alter degree and “alter already got coupon” were included as co-variates in the model. The answer to the third question provides data to assess RDS’s random peer selection assumption as well as the proportion to degree assumption.

RESULTS

Descriptive statistics of various degree measures and their bi-variate correlations are presented in Tables 2 and 3. The typical RDS degree measure has the largest variation and a significant number of outliers. To minimize the influence of the outliers, we choose to use univariate Ordinary Least Square (OLS) regression and Poisson regression with robust standard error (SE) to assess the correlations among the degree measures. All previous RDS statistical models have assumed that recruitment occurs proportional to a recruit’s true degree. Therefore, enrollment degree should be correlated with both total degree and the typical RDS degree, if both of these measures are reliable. However, Table 3 shows that typical RDS degree is only associated with recruitment intent degree ($\beta=2.18$, $p<0.05$) among all degree measures. This means that the typical survey questions for degree estimations used by most RDS studies are not a reliable measure of the actual social network degree.

In addition to enrolled matched degree, total degree is associated with all other degree measures (Table 3), including, in order of strongest to weakest association, peer-reported degree ($\beta=1.07$, $p<0.000$), self-reported degree ($\beta=1.03$, $p<0.001$), coupon enrollment degree ($\beta=.39$, $p<0.001$), coupon passing degree ($\beta=.39$, $p<0.10$), actual enrollment degree ($\beta=.29$, $p<0.001$), recruitment intent degree ($\beta=.28$, $p<0.000$), and recruitment encounter degree ($\beta=.25$, $p<0.10$). The association between total degree and self-reported degree suggests that participants can fairly accurately report their degree when using probing questions to assist them to focus on specific individuals under different categories. This result is encouraging because full sociometric network data from a hidden population is difficult and expensive to obtain. This result suggests that its collection may not be necessary, as long as network elicitation questions similar to ours are used to assist participants’ recall in specific areas.

Regarding the second set of analytical questions about indirect recruitment, we first analyzed the individual level data. Among 408 non-seed participants who returned for the 2-month follow-up, only 49.6% of the reported recruiter name matched with the original coupon holder, 41.8% were non-matched, and 8.5% were unclear. Unexpectedly, nearly half

of the coupons were redistributed on the street before arriving at the final recruit enrolled in the study. When recruits received more than one coupon, they were more likely to enter the study and pass the redundant coupons to others, resulting in a significant chance of indirect recruitment. These results failed the most important RDS estimator assumption on which all RDS models are based, that is, recruitment occurs over the recruiter's social network ties (10–13, 74). It also challenges the foundation of H1997, H2002, and SH 2004 estimators, which heavily rely on accuracy of recruiter-recruit relationship based only on a clear chain of coupon custody.

Some researchers (4) have been concerned about the possibility of recruiting outside the recruiter's network. They asked recruits about their relationship with the person who gave them the coupon. If not a stranger, a recruiter-recruit relationship was assumed between the initial coupon holder (suggested by the coupon code) and the final recruit returned the coupon. Similarly, we also asked participants at the follow-up survey about their relationship with the person who passed them the coupon. Among those non-matched ties in the dyadic level database, relationship types were 3.5% spouse or sex partners, 3.9% family members, 58.7% friends, 32.8% acquaintances, and 1.0% other non-family members or drug associates. In other words, most recruits reported receiving coupons from someone within their own network. However, the coupon givers were not the initial recruiters suggested by the coupon ID; thus, responses do not provide answers to the question researchers presume to be asking. There is no way to know through their responses whether recruits were in the network of the original coupon holder. It furthermore suggests that this kind of survey question cannot be used to assess whether recruitment occurred outside of the initial recruiters' network. We then selected the 223 participants recruited indirectly through a third person, and found 33.2% were named in the initial coupon holders' ego network. This information can be used in the future to further assess how indirect recruitment affects existing RDS estimators.

Results to answer the third analytical question are presented in Table 4. Dependent variables are the various recruitment behaviors at different stages of the recruitment process. Among them, both recruitment intention and recruitment effort are recruiters' behavior, coupon passing involves recruiter and recruit interaction, and enrollment only involves recruits' action and interaction with the study site. Independent variables are classified into three blocks: ego-alter relationship factors, ego perceived alter risk behaviors, and co-variables.

The results show that each type of recruitment behavior was affected by these factors differently. "Number of days ego-alter contacted" appears to have had trivial influence on any recruitment behavior. "Ego-alter lived together" reduced recruitment intention (OR= 0.61, CI=0.41, 0.89, $p<0.01$) but increased the chance of recruitment effort (OR= 1.33, CI= 1.02, 1.75, $p<0.001$), coupon passing (OR=1.84, CI=1.35, 2.48, $p<0.001$), and actual enrollment (OR=2.15, CI=1.66, 2.78, $p<0.001$). "Ego can go to alter for a place to stay" only increased the chance of recruitment intention (OR=1.55, CI=1.07, 2.26, $p<0.001$) but not other behaviors. "Ego can go to alter for money" reduced the chance of recruitment intention (OR=0.66, CI=0.46, 0.95, $p<0.001$) but was not associated with other recruitment behaviors. High level of trust increased the chance of actual enrollment (OR=1.40, CI= 1.03, 1.89, $p<0.001$). Surprisingly, ego-alter hang out together did not affect recruitment intention and

reduced the chance of recruitment effort (OR=0.37, CI=0.29, 0.49, $p<0.001$), coupon passing (OR=0.31, CI= 0.23, 0.41, $p<0.001$) and actual enrollment (OR=0.63, CI=0.48,0.82, $p<0.001$). These findings do not provide a clear assessment of whether recruiters select from their network randomly. It seems they use a variety of criteria to decide whom to recruit. Our in-depth interview data did find that perceived alter reliability in returning to the study site was often the primary reason when they choose candidates to pass coupons (75). However, perceived alter reliability was not included in the survey questionnaire. It is possible that egos consider most of the people they hang out with as not reliable.

Among ego-perceived alter risk variables, “ego uses drugs with alter” increased only the chance of recruitment intention (OR=1.95, 95% CI: 1.44, 2.64, $p<0.001$) but was not associated with other behaviors. “Alter injects with others” increased the chance of all recruitment behaviors, with odds ratio values ranging from 1.41 (Actual recruitment) to 1.81 (Recruitment effort). “Alter is homeless” was associated with neither recruitment intention nor actual enrollment, but increased the chance of recruitment effort (OR=1.37, CI: 1.13, 1.67, $p<0.01$) and coupon passing (OR=1.36, CI: 1.09, 1.71, $p<0.001$). “Alter injects in shooting gallery” and “alter is HIV positive” (ego perception) were not associated with any recruitment behaviors. After controlling for “alter already got coupon,” surprisingly, ego out-degree decreased the chance of all recruitment behaviors and alter total degree was not associated with any recruitment behavior. Consequently, the assumption “recruitment occurs in proportion to (alter’s) degree,” central to all RDS estimators, was not supported by these data. Besides the fixed effect model for coupon passing, all other models have a significant random effect, which suggests behavior patterns within ego networks were similar. All model constants were significant, suggesting that other factors were also associated with various recruitment behaviors. Further research will be needed to identify these factors and quantify their influence.

Table 5 reports comparison between enrolled and non-enrolled alters on relationship characteristics. Enrolled alters were more likely to have had more days in contact with ego (Mean=19.2 vs. 15.3, $p<0.001$), support ties (34.0% vs. 30.8%, $p<0.05$), lived with ego in the last 6 months (21.7% vs. 10.3%, $p<0.001$), and scored high in trust (Mean=2.98 vs. 2.78, $p<0.001$). In terms of risk factors, enrolled alters were more likely to be perceived by ego as having injected with others (69.3% vs. 64.7%, $p<0.05$) or injected in shooting galleries (69.3% vs. 64.7%, $p<0.01$), to be homeless (40.7% vs. 36.4%, $p<0.05$), or to be HIV positive (9.6% vs. 6.5%, $p<0.01$) and less likely to be a crack user (39.7% vs. 45.2%, $p<0.01$).

To further assess the impact of relationship factors, risk factors, and degree on the recruitment process and sample composition, we used the “sum” function to aggregate these dyadic level measures by alter’s unique ID to obtain an individual level dataset. For example, participant 1007 named 12 PWIDs as his network members but was only mentioned by 4 PWIDs as their network member. We sum up all 4 egos’ reported relationship measures and ego-alter shared risk measures, into one score per item as 1007’s aggregated relationship score and risk score. This way, the dyadic relationship database (N=3392) was collapsed into an individual level dataset containing data on aggregated relationship and ego-perceived risk measures for 2636 unique alters. Note again that the number of unique alters is always smaller than the sum of alter entries in the ego network

survey, because one alter can be named by multiple egos. Among the 2636 unique PWID alters, 2167 of them have follow-up survey measures, including total number of coupons received from different recruiters. The mean of relationship measures and ego perceived alter risk measures were compared among PWID who received multiple coupons and those who received zero or one coupon (Table 6). The mean of almost all relationship and risk variables for multiple coupon receivers were several times higher than those who received one or no coupons ($p < 0.001$ for all), except for ego perceived alter HIV status. Alters with multiple coupons were more likely to be those with larger degrees and more likely to be enrolled in the study. We also ran multiple regression analysis with the number of coupons as a continuous dependent variable, and the same list of aggregated relationship, risk, and degree variables as independent variables. The results (not included in any tables) show that effects of ego-alter contact days, support, ego being able to go to alter for a place to stay, and alter injecting in a shooting gallery diminished. Factors positively associated with number of coupons received include alter injects with others ($\beta = 0.160$, $P < 0.001$), ego-alter lived together in the last 6 months ($\beta = 0.112$, $P = 0.001$), alter was homeless ($\beta = 0.097$, $P < 0.001$), alter was HIV positive ($\beta = 0.073$, $P < 0.025$), alter total degree ($\beta = 0.051$, $P < 0.001$), and trust ($\beta = 0.034$, $P < 0.001$). Note that alter total degree is not associated with any recruitment behavior in Table 4 but shows a positive association in Table 6 and multiple regression analysis here. It is worth pointing out that un-sampled alters' degree may be underestimated, and therefore results suggesting an association with enrollment and other recruitment behavior may be inflated. With this in mind, along with other non-uniform selection patterns, our results do not support the proportion to degree assumption.

From the aggregated dataset, we then selected 405 sampled participants who were mentioned at least once by other participants as their network members. Table 7 indicates the comparison of relationship factors, risk factors, and degree between directly recruited participants and indirectly recruited participants, which shows no significant difference on any of the variables. In other words, direct and indirect recruits were not very different in aggregated relationship, risk and degree factors. Recall that Table 6 results suggest that coupons were more likely to end up in the hands of strong ties, those at high risk of HIV infection, and those with large degree. These findings together suggest that indirectly recruited participants were also at high risk of HIV and more likely to maintain close ties with other PWIDs.

DISCUSSION

Intensive social network data collection and construction of a sociometric network enabled us to identify otherwise unobserved network nodes from a smaller number of sampled hard to reach urban PWIDs. This sociometric network dataset, along with comprehensive survey measures, captured complex and nuanced peer recruitment processes and dynamics at the individual, dyadic relationship, and whole network level. These data revealed several significant yet overlooked threats to RDS estimators. Several most common assumptions about peer recruitment behavior and processes that are essential for the validity of estimators were not supported by these data. These include that peer recruitment: (1) occurs over the recruiters' direct network ties, (2) is a random selection of direct network members, and that (3) recruits' probability of being in the sample is proportional to their degree.

The problems are rooted in an overly simplistic conceptualization of the peer recruitment process. One contribution of our study is conceptualization of network multiplexity associated with recruitment processes that involve multiple stages as illustrated in Figure 1. The process includes the initial recruiter, their initial recruit candidates, alternative candidates, initial coupon receivers and their network members, final coupon receivers, people in between, and staff. Greater attention to this multiplexity and further examination of the various subsectors of the recruitment networks could contribute to both analytical and practical answers to the problems of assumption violations in future research. Previous RDS development work (1, 2, 9–13) and estimator validity assessments (18, 28, 29) failed to recognize the multiplexity network structure of RDS and the complexity of the peer recruitment process. Recruiter's decisions, behaviors, and interactions with others occur along a time horizon that starts with the recruiter planning, trying to reach recruit candidates, interacting with the candidates, passing the coupon to them, or seeking other recruit candidates to pass on the coupon or let it expire. After a coupon is successfully passed, what happens is beyond the initial recruiter's control. The initial coupon receiver could either overcome various barriers and enter the study, let the coupon expire, or pass it on to others. Once the latter happens, the final successful recruit, an indirect recruit, may not be in the initial recruiter's network.

Our data show that nearly half of the sample was indirectly recruited. Among those indirectly recruited participants, only 33.19% were identified in the initial recruiters' direct network. Given careful adherence in this study to standard NHBS implementation procedures, there is little reason to conclude that this high rate of indirect recruitment was substantially different from what may have occurred in many other studies that used RDS. These results are consistent with other RDS implementations [76]. Thus, lack of support for the assumption that "peer recruitment occurs over the recruiter's network ties" is perhaps the greatest threat to validity of all the RDS estimators.

Findings from a nested sample of 60 in-depth interview participants conducted as part of our study suggest reasons for the high rate of indirect recruitment. These interviews, reported elsewhere (77), indicated that recruiters purposefully attempted to select the "right people" (i.e., both eligible and reliable), and that they impressed on the coupon recipient the importance of following through and taking the survey. Recruiters use techniques such as asking recipients to give their word or return the study coupon if they did not use it, offering recruits use of their cell phones to make appointments, and even accompanying them to the study site. Despite these clear recruiter efforts to follow protocol, coupon recipients who could not do so or chose not to go to the study site still felt compelled to be sure the coupon was redeemed by passing it to another eligible person. One stated reason participants gave in qualitative interviews for coupon redistribution was to ensure the original recruiter was able to recover his/her expected recruitment incentive and thereby maintain the good graces within that relationship. (77) Though monetary incentives for both survey completion and successful coupon passing recruitment may in part have motivated coupon redistribution processes (77–79), such dual incentive structure is standard practice in RDS studies and is considered necessary to ensure sample recruitment success.

In our study, monetary incentives were well within the local standards for research compensation with low-income drug-using populations, and also comparable with our practice in a previous NHBS cycle. We also conducted a supplemental research ethics study to assess potential coercion related to RDS peer recruitment. Previously published findings suggest that even though persistent recruitment strategies could add social pressure and relationship complications, most recruits consider these within acceptable “norms” of their relationships, and did not pose a threat to voluntary participation (77).

Related to the failure of the assumption about recruitment occurring over the recruiter’s direct network ties are problems with the random selection assumption and proportion to degree assumption. These two assumptions are highly related, and this relationship needs to be unpacked and examined in a nuanced way. If the random selection assumption is true, recruiter behavior (choosing, reaching, and passing a coupon to recruit candidates) and recruits’ behavior (accepting the coupon, entering the study, or denying/rejecting recruitment) must all occur at random. Only if the recruits behave uniformly can the proportion to degree assumption be true. However, our data demonstrate that none of these behaviors occurred uniformly at random.

Recruiters tended to consider alters who used drugs with them and to whom they can go to when they need a place to stay, and were less likely to consider those who hang out with them, lived with them in the past 6 months, or those they can go to when they need money. During implementation of peer recruitment, ego network analysis demonstrated that recruiters were more likely to interact with alters they feel close to or received support from, those who lived with them in the past 6 months, and those who also inject drugs with others or were homeless. These factors affected coupon passing behavior and actual enrollment (recruit behavior) in similar ways, with stronger effects of ego-alter having lived together and weaker effects from support ties for each behavior.

Multiple coupon receivers were several times more likely to be those with high scores on tie strength measures, especially with aggregated trust and ego-alter having lived together in the last 6 months. Multiple coupon receivers were also several times more likely to be those with higher perceived risks, such as injecting with others, being homeless, and being HIV positive. Since the parameters associated with random effects (Table 4) were all significant, there may be other omitted factors associated with non-uniform selection patterns and unobserved heterogeneity. Our results not only reject the random selection assumption, but also show that the non-uniform pattern is associated with the outcome variable of interest and results in a sample at higher risk than the whole network. With rejection of the random selection assumption, the proportion to degree assumption is also rejected as previously explained.

A possible limitation to our study and our conclusions about violation of assumptions in RDS sampling could be related to poor field implementation of RDS recruitment. As indicated above, we believe this is unlikely to be significant. Our research team has nearly 30 years of history studying drug users in the region and has gained extensive experience in RDS field implementation through several NIH funded studies and a CDC funded NHBS IDU cycle (21). We followed the NHBS recruitment protocol in this study so that our data

would be comparable to those of many other researchers who use RDS. With only six seeds, our RDS procedure resulted in four very long recruitment chains ranging from 11 to 29 waves. This is a good indicator that the assumption violation findings are less likely due to poor field implementation of RDS and more likely related to intrinsic recruitment dynamics that occur through real world recruiter and recruit practices.

As with all RDS studies, this one is also limited by the need to rely primarily on self-reported behavior. This limitation is somewhat reduced through our ability to complement self-reported with alter-reported behavior through matching ties in the sociometric network, though these comparisons often relied on different measures. Another limitation of this study is that available node/individual level and network tie data about un-sampled individuals, those not reached by RDS, are limited to peer report, compared with both self-report and peer report from sampled individuals. Thus, network degree of un-sampled individuals could be underestimated. Network composition and structure of the un-sampled cannot be fully observed. However, the data we collected through the first empirical sociometric social network study associated with RDS peer recruitment among PWID are very valuable for uncovering significant information that was previously believed inaccessible.

RECOMMENDATIONS AND IMPLICATIONS FOR FUTURE STUDIES

Understanding the explicit assumptions that underlie statistical analysis of RDS data is important for HIV and other researchers who use RDS for two reasons. The first is as a caution in RDS field implementation to try to meet the assumptions whenever possible. However, due to practical challenges in working with hard to reach populations, strategies for improvements to field operations can be limited. Second, better understanding of recruitment processes is needed to improve the development and performance of estimators. Understanding the causes and patterns of assumption violations, real world recruitment behavior, and empirical network properties can provide statisticians valuable information and insight so that they can address these issues.

This latter direction is more meaningful, because vast resources have already been invested and copious data have been collected and are being analyzed resulting from RDS sampling methods, including many years of CDC funded NHBS surveys and more than 448 NIH funded studies with RDS as a topic or method (80). RDS data can be more useful when better estimators less sensitive to the usual assumptions are available. To improve RDS estimators, statisticians should test model sensitivity to identified assumptions. Heckathorn highlighted in a forthcoming paper the discrepancy between the performance of variance estimators on simulated and real word networks, and attributed that variance to differences in network structure and the distribution of the attribute of interest (22). To this end, HIV researchers' future contributions to identifying assumption violations and their patterning are important, so that the sensitivity test can be done not only analytically, via simulation, but also using empirical data.

The best way to move this development work forward is for statisticians, modelers, and HIV researchers who are familiar with study populations, their empirical networks, and field

implementation issues to work together. One possible benefit of such multi-disciplinary efforts could be to reduce some of the confusion within the RDS literature in the use of terms that are similar but conceptually different. For example, concepts like homophily versus differential recruitment and differential recruitment bias versus differential recruitment effectiveness or pattern are sometimes not clearly defined, have been used interchangeably, or are named differently in different papers, even from the same group of scholars (19, 22, 25, 81).

Further definition and clarification of related concepts requires better conceptualization and understanding of nuanced recruitment process dynamics and related multiple sources of bias. Building consensus around language and specification of key concepts could benefit efforts to understand the limitations to RDS methodology and build ways to address or minimize those limitations. Further collaborative research could also generate better tests of model sensitivity and structure through mixed methods research. When estimator development and analytical and simulation assessment studies are published by statistical and mathematical focused journals, the underlying assumptions about recruitment process, patterning, and network structure are sometimes not explicit or in language that is understandable for HIV researchers (12, 13, 18), or sometimes even for other groups of statisticians (25). Interdisciplinary RDS research could reduce some of this confusion and lack of clarity.

During field operation, it is important but very difficult to assess the degree of indirect recruitment, especially the proportion outside of the initial recruiter's direct network. Building trust and rapport between researchers and the study population could assist with uncovering coupon redistribution practices. Collecting identifying information of recruiters and the ultimate coupon givers, as well as perceived network linkage between them could improve the ability to measure and reduce the effects of coupon redistribution. Deeper understanding of the population being sampled through direct relationships between researchers and the study sample also opens the door to examination of qualitative processes that shape RDS implementation and the resulting potential threats to statistical assumptions in estimators, providing valuable information needed to address these problems.

Our results also demonstrated that typical RDS survey degree estimation questions (e.g., "How many people do you know who also inject drugs?", "How many of them are Black, Latino, etc.") are far from reliable. But collecting comprehensive social network data and construction of sociometric network data to improve degree measures are often not practical. Nevertheless, the self-reported degree measure can be improved if the survey includes questions similar to our name-generation questions. Examples include, "Think about all the people who have used drugs with you in the past 6 months...", "... who you feel close to or received support from", or "... who lived with you in the last 6 months", "Write down their name/initials/nickname on a piece of paper here...." Questions like these can help participants have a more reliable estimate of network size by recalling network members with specific characteristics. Besides the benefit of a more reliable degree estimate, collection of ego network data could have the potential to be used in new RDS estimators that incorporate these data, such as Lu's estimator (82) or other network sampling model estimates (83).

Researchers have few options for sampling hard to reach populations. Despite its limitations, RDS has provided one of the most reliable methods for doing so, although it remains unclear how to use the resulting data to make inferential statements about the larger population from which they were drawn. Nevertheless, both implementation issues and inherent RDS methodological issues appear to compromise RDS population estimates. As a relatively new sampling method, RDS still needs further assessment of performance, sensitivity testing of assumption violations, and statistical development that is less dependent on counterfactual assumptions and that builds on more realistic assumptions. It is our hope that findings presented here could inspire statisticians to find ways to address these problems in future RDS estimator development.

Acknowledgments

We are extremely grateful to our study participants who shared extensive network and other sensitive information with us. We appreciated Forrest Crawford and two anonymous reviewers' helpful insights. We also thank many student interns who assisted in various aspects of data collection and processing. They are: Mark Romano, Jason Weiss, Shajeda Chowdhury, Irene Shaver, Mengjia Li, Nina Soar, Barbara Byrne, David Andrew, Loren Sanchez-Radda, Hayley Berg, and Raymond Li. The study is supported by award R01DA031594 from the National Institute of Drug Abuse. It is affiliated with the Center for Interdisciplinary Research on AIDS (P30 MH62294). AZ would like to acknowledge the NIDA career development award K01 DA37826. The content is solely the responsibility of the authors and does not necessarily represent the official views of the National Institutes of Health, the National Institute on Drug Abuse or the National Institute on Mental Health.

Funding: This study was funded by National Institute of Drug Abuse, grant number R01DA031594

References

1. Heckathorn DD. Respondent-driven sampling: A new approach to the study of hidden populations. *Social Problems*. 1997 May; 44(2):174–99.
2. Heckathorn DD. Respondent-Driven Sampling II: Deriving Valid Population Estimates from Chain-Referral Samples of Hidden Populations. *Social Problems*. 2002 Feb; 49(1):11–34. 2002.
3. Abdul-Quader AS, Heckathorn DD, McKnight C, Bramson H, Nemeth C, Sabin K, et al. Effectiveness of respondent-driven sampling for recruiting drug users in New York City: findings from a pilot study. *J Urban Health*. 2006 May; 83(3):459–76. Epub 2006/06/02. eng. [PubMed: 16739048]
4. Johnston LG, Sabin K, Mai TH, Pham TH. Assessment of respondent driven sampling for recruiting female sex workers in two Vietnamese cities: reaching the unseen sex worker. *J Urban Health*. 2006 Nov; 83(6 Suppl):i16–28. Epub 2006/10/13. eng. [PubMed: 17031567]
5. Kral AH, Malekinejad M, Vaudrey J, Martinez AN, Lorvick J, McFarland W, et al. Comparing Respondent-Driven Sampling and Targeted Sampling Methods of Recruiting Injection Drug Users in San Francisco. *J Urban Health*. 2010 Jun 26. Epub 2010/06/29. Eng.
6. Iguchi M, Ober A, Berry S, Fain T, Heckathorn D, Gorbach P, et al. Simultaneous Recruitment of Drug Users and Men Who Have Sex with Men in the United States and Russia Using Respondent-Driven Sampling: Sampling Methods and Implications. *Journal of Urban Health*. 2009; 86(0):5–31.
7. Yeka W, Maibani-Michie G, Prybylski D, Colby D. Application of respondent driven sampling to collect baseline data on FSWs and MSM for HIV risk reduction interventions in two urban centres in Papua New Guinea. *J Urban Health*. 2006 Nov; 83(6 Suppl):i60–72. Epub 2006/10/27. eng. [PubMed: 17066328]
8. Malekinejad M, Johnston L, Kendall C, Kerr L, Rifkin M, Rutherford G. Using Respondent-Driven Sampling Methodology for HIV Biological and Behavioral Surveillance in International Settings: A Systematic Review. *AIDS and Behavior*. 2008; 12(0):105–30.
9. Heckathorn DD. Extensions of Respondent-Driven Sampling: Analyzing Continuous Variables and Controlling for Differential Recruitment. *Sociological Methodology*. 2007; 37(1):151–207.

10. Salganik MJ, Heckathorn DD. Sampling and Estimation in Hidden Populations Using Respondent-Driven Sampling. *Sociological Methodology*. 2004; 34(1):193–240.
11. Volz E, Heckathorn DD. Probability based estimation theory for respondent driven sampling. *Journal of Official Statistics*. 2008; 24(1):19.
12. Gile KJ. Improved inference for respondent-driven sampling data with application to HIV prevalence estimation. *Journal of the American Statistical Association*. 2011; 106(493)
13. Gile KJ, Handcock MS. Network model-assisted inference from respondent-driven sampling data. *Journal of the Royal Statistical Society: Series A (Statistics in Society)*. 2015; 178(3):619–39.
14. Heimer R. Critical Issues and Further Questions about Respondent-Driven Sampling: Comment on Ramirez-Valles, et al. (2005). *AIDS and Behavior*. 2005; 8(4):403–8.
15. Frost SDW, Brouwer K, Firestone Cruz M, Ramos R, Ramos ME, Lozada R, et al. Respondent-Driven Sampling of Injection Drug Users in Two U.S.–Mexico Border Cities: Recruitment Dynamics and Impact on Estimates of HIV and Syphilis Prevalence. *Journal of Urban Health*. 2006; 83(0):83–97.
16. Johnston LG, Malekinejad M, Kendall C, Iuppa IM, Rutherford GW. Implementation challenges to using respondent-driven sampling methodology for HIV biological and behavioral surveillance: field experiences in international settings. *AIDS & Behavior*. 2008; 12(4 Suppl)
17. Li, J., Moorthi, G., Mosher, H., Obidoa, C., Weeks, M., Valente, T. Skepticism about RDS Inference Validity: Preliminary Empirical Findings from a Recruitment Dynamic and Network Study; American Public Health Association Annual Conference; November 2–6; Boston, MA. 2013.
18. Gile KJ, Handcock MS. RESPONDENT-DRIVEN SAMPLING: AN ASSESSMENT OF CURRENT METHODOLOGY. *Sociological methodology*. 2010; 40(1):285–327. [PubMed: 22969167]
19. Heckathorn DD. Comment: snowball versus respondent-driven sampling. *Sociological methodology*. 2011; 41(1):355–66. [PubMed: 22228916]
20. Volz, E., Wejner, C., Degani, I., Heckathorn, DD. Respondent-Driven Sampling Analysis Tool (RDSAT) Version 5.6. Ithaca, NY: Cornell University; 2007.
21. Lansky A, Abdul-Quader AS, Cribbin M, Hall T, Finlayson TJ, Garfein RS, et al. Developing an HIV behavioral surveillance system for injecting drug users: the National HIV Behavioral Surveillance System. *Public Health Rep*. 2007; 122(Suppl 1):48–55. Epub 2007/03/16. eng.
22. Heckathorn DD, Cameron CJ. Network Sampling: From Snowball and Multiplicity to Respondent-Driven Sampling. *Annual Review of Sociology*. Aug.2017 43(1)doi: 10.1146/annurev-soc-060116-053556
23. Aronow PM, Crawford FW. Nonparametric identification for respondent-driven sampling. *Statistics & Probability Letters*. 2015 Nov.106:100–2. [PubMed: 26327739]
24. Handcock MS, Fellows IE, Gile KJ. RDS Analyst: Software for the Analysis of Respondent-Driven Sampling Data. *UCLA Statistics* (0.42). 2014
25. Shi Y, Cameron CJ, Heckathorn DD. Model-Based and Design-Based Inference Reducing Bias Due to Differential Recruitment in Respondent-Driven Sampling. *Sociological Methods & Research*. 2016 0049124116672682.
26. Gregoire TG. Design-based and model-based inference in survey sampling: appreciating the difference. *Canadian Journal of Forest Research*. 1998; 28(10):1429–47.
27. SARNDAL C-E. Design-based and Model-based Inference in Survey Sampling1. *Scand J Statist*. 1978; 5:27–52.
28. Goel S, Salganik MJ. Assessing respondent-driven sampling. *Proceedings of the National Academy of Sciences*. 2010 Apr 13; 107(15):6743–7. 2010.
29. Gile KJ, Johnston LG, Salganik MJ. Diagnostics for respondent-driven sampling. *Journal of the Royal Statistical Society: Series A (Statistics in Society)*. 2015; 178(1):241–69.
30. Skvoretz J, Agneessens F. Reciprocity, Multiplexity, and Exchange: Measures. *Qual Quant*. 2007 Jun 01; 41(3):341–57. 2007. English.
31. Krohn MD. The web of conformity: A network approach to the explanation of delinquent behavior. *Social Problems*. 1986; 33(6):s81–s93.

32. Krohn MD, Massey JL, Zielinski M. Role overlap, network multiplexity, and adolescent deviant behavior. *Social Psychology Quarterly*. 1988;346–56.
33. Fujimoto K, Valente TW. Multiplex congruity: Friendship networks and perceived popularity as correlates of adolescent alcohol use. *Social Science & Medicine*. 2015; 125:173–81. [PubMed: 24913275]
34. Ramirez-Valles J, Douglas DH, Vazquez R, Diaz RM, Campbell RT. The Fit Between Theory and Data in REspondent-Driven Sampling: REsponse to Heimer. *AIDS and Behavior*. 2005 Dec; 9(4): 409–14.
35. McCreesh N, Frost S, Seeley J, Katongole J, Tarsh MN, Ndunguse R, et al. Evaluation of respondent-driven sampling. *Epidemiology (Cambridge, Mass)*. 2012; 23(1):138.
36. Abdul-Quader AS, Heckathorn DD, Sabin K, Saidel T. Implementation and analysis of respondent driven sampling: lessons learned from the field. *J Urban Health*. 2006 Nov; 83(6 Suppl):i1–5. Epub 2006/10/24. eng. [PubMed: 17058119]
37. Uuskula A, Johnston LG, Raag M, Trummal A, Talu A, Des Jarlais DC. Evaluating recruitment among female sex workers and injecting drug users at risk for HIV using respondent-driven sampling in Estonia. *J Urban Health*. 2010 Mar; 87(2):304–17. Epub 2010/02/05. eng. [PubMed: 20131018]
38. Gwadz MV, Leonard NR, Cleland CM, Riedel M, Banfield A, Mildvan D. The effect of peer-driven intervention on rates of screening for AIDS clinical trials among African Americans and Hispanics. *American journal of public health*. 2011; 101(6):1096–102. [PubMed: 21330587]
39. Liu H, Li J, Ha T, Li J. Assessment of random recruitment assumption in respondent-driven sampling in egocentric network data. *Social networking*. 2012; 1(2):13. [PubMed: 23641317]
40. Friedman SR, Kottiri BJ, Neaigus A, Curtis R, Vermund SH, Des Jarlais DC. Network-related mechanisms may help explain long-term HIV-1 seroprevalence levels that remain high but do not approach population-group saturation. *American Journal of Epidemiology*. 2000 Nov 15; 152(10): 913–22. [PubMed: 11092433]
41. Neaigus A, Friedman SR, Curtis R, Jarlais DCD, Furst RT, Jose B, et al. The relevance of drug injectors' social and risk networks for understanding and preventing HIV infection. *Social Science & Medicine*. 1994; 38(1):67–78. [PubMed: 8146717]
42. Latkin CA. A personal network approach to AIDS prevention: an experimental peer group intervention for street-injecting drug users: the SAFE study. *NIDA Research Monograph*. 1995; 151:181–95. [PubMed: 8742766]
43. el-Bassel N, Cooper DK, Chen DR, Schilling RF. Personal social networks and HIV status among women on methadone. *AIDS Care*. 1998; 10(6):735–49. [PubMed: 9924528]
44. Lakon CM, Ennett ST, Norton EC. Mechanisms through which drug, sex partner, and friendship network characteristics relate to risky needle use among high risk youth and young adults. *Social Science & Medicine*. 2006; 63:2489–499. [PubMed: 16875770]
45. Latkin CA, Mandell W, Oziemkowska M, Vlahov D, Celentano D. The relationships between sexual behavior, alcohol use, and personal network characteristics among injecting drug users in Baltimore, Maryland. *Sexually Transmitted Diseases*. 1994; 21(3):161–7. [PubMed: 8073344]
46. Shaw SY, Shah L, Jolly AM, Wylie JL. Determinants of injection drug user (IDU) syringe sharing: the relationship between availability of syringes and risk network member characteristics in Winnipeg, Canada. *Addiction*. 2007 Oct.102(10):1626–35. [PubMed: 17854339]
47. Sherman SG, Latkin CA, Gielen AC. Social factors related to syringe sharing among injecting partners: a focus on gender. *Substance Use & Misuse*. 2001; 36(14):2113–36. [PubMed: 11794586]
48. Li, J, Weeks, MR., Convey, M., editors. *The Social Relationship Context and Cultural Perspective of Injection Risk, a Comparison between US and Chinese Injection Drug Users*; Society for Prevention Research 16th Annual Meeting: Context and Prevention Science; May 27 – 30 2008; San Francisco, CA. 2008.
49. Valente TW, Vlahov D. Selective risk taking among needle exchange participants: implications for supplemental interventions. *American Journal of Public Health*. 2001; 91(3):406–11. [PubMed: 11236405]

50. Lansky A, Drake A, Pham H. HIV-associated behaviors among injecting-drug users-23 cities, United States, May 2005–February 2006. *Morbidity and Mortality Weekly Report*. 2009; 58(13): 329–32. [PubMed: 19357632]
51. Johnston LG, Whitehead S, Simic-Lawson M, Kendall C. Formative research to optimize respondent-driven sampling surveys among hard-to-reach populations in HIV behavioral and biological surveillance: lessons learned from four case studies. *AIDS Care*. 2010 Jun; 22(6):784–92. Epub 2010/05/15. eng. [PubMed: 20467937]
52. Magnani R, Sabin K, Saidel T, Douglas DH. Review of sampling hard-to-reach and hidden populations for HIV surveillance. *AIDS*. 2005; 19(suppl 2):S67–S72.
53. Trotter RT II, Bowen AM, Potter JM Jr. Network models for HIV outreach and prevention programs for drug users. *NIDA Research Monograph Series: Social Networks, Drug Abuse and HIV Transmission*. 1995; 151:144–80.
54. Mitchell, JC. The concept and use of social networks. In: Mitchell, JC., editor. *Social Networks in Urban Situations: Analysis of Personal Relationships in Central African Towns*. Manchester: Manchester University Press; 1969. p. 1-50.
55. Granovetter MS. The strength of weak ties. *American Journal of Sociology*. 1973; 78(6):1360–80.
56. Maguire, L. *Understanding social networks*. Beverly Hills, Calif.: Sage Publications; 1983. p. 119
57. Marsden PV, Campbell CE. Measuring Tie Strength. *Social Forces*. 1984; 63(2):482–501.
58. Weeks MR, Li J, Dickson-Gomez J, Convey M, Martinez M, Radda K, et al. Outcomes of a Peer HIV Prevention Program with Injection Drug and Crack Users: The Risk Avoidance Partnership. *Substance Use & Misuse*. 2009 Jan; 44(2):253–81. [PubMed: 19142824]
59. Weeks MR, Clair S, Borgatti SP, Radda K, Schensul JJ. Social networks of drug users in high-risk sites: Finding the connections. *AIDS & Behavior*. 2002 Jun; 6(2):193–206.
60. Li J, Weeks MR, Borgatti SP, Clair S, Dickson-Gomez J. A Social Network Approach to Demonstrate the Diffusion and Change Process of Intervention from Peer Health Advocates to the Drug Using Community. *Substance Use & Misuse*. 2012 Apr; 47(5):474–90. Epub March 18. [PubMed: 22428816]
61. Duke, M., Li, J., Singer, M. Drug use, Syringe Sharing and HIV Risk in the People's Republic of China. In: Pope, C.White, R., Malow, R., editors. *HIV/AIDS: Global Frontiers in Prevention/ Intervention*. New York, London: Routledge; 2008. p. 40-9.
62. Bell DC, Belli-McQueen B, Haider A. Partner naming and forgetting: Recall of network members. *Social Networks*. 2007; 29(2):279–99. [PubMed: 17940583]
63. Brewer DD, Garrett SB, Kulasingam S. Forgetting as a Cause of Incomplete Reporting of Sexual and Drug Injection Partners. *Sexually Transmitted Diseases*. 1999; 26(3):166–76. [PubMed: 10100775]
64. Brewer DD, Webster CM. Forgetting of friends and its effects on measuring friendship networks. *Social Networks*. 2000; 21(4):361–73.
65. Young AM, Abby ER, Su AE, Lee King, Jent S, Havens J. Accuracy of name and age data provided about network members in a social network study of people who use drugs: Implications for constructing sociometric networks. *Annals of Epidemiology*. 2016 Nov 30.26(11):8029.
66. Bartunov, S.Korshunov, A.Park, S-T.Ryu, W., Lee, H., editors. Joint link-attribute user identity resolution in online social networks. *Proceedings of the 6th International Conference on Knowledge Discovery and Data Mining; Workshop on Social Network Mining and Analysis* ACM; 2012.
67. Vosecky, J.Hong, D., Shen, VY., editors. User identification across multiple social networks. *Networked Digital Technologies, 2009 NDT'09 First International Conference on; IEEE; 2009*.
68. Bilgic, M.Licamele, L.Getoor, L., Shneiderman, B., editors. D-dupe: An interactive tool for entity resolution in social networks. *Visual Analytics Science And Technology, 2006 IEEE Symposium On; IEEE; 2006*.
69. Borgatti, SP., Everett, MG., Freeman, LC. *Ucinet for Windows: Software for Social Network Analysis*. Harvard, MA: Analytic Technologies; 2002.
70. Borgatti, SP. *NetDraw: Graph Visualization Software*. Harvard, MA: Analytic Technologies; 2002.
71. Center for Disease Control and Prevention. *National Health Behavior Surveillance Injection Drug User Cycle Survey Instrument*. 2005

72. Rudolph AE, Young AM, Lewis CF. Assessing the geographic coverage and spatial clustering of illicit drug users recruited through respondent-driven sampling in New York City. *Journal of Urban Health*. 2015; 92(2):352–78. [PubMed: 25694223]
73. Young AM, Rudolph AE, Quillen D, Havens JR. Spatial, temporal and relational patterns in respondent-driven sampling: evidence from a social network study of rural drug users. *Journal of epidemiology and community health*. 2014; 68(8):792–8. [PubMed: 24692631]
74. Salganik M. Variance Estimation, Design Effects, and Sample Size Calculations for Respondent-Driven Sampling. *Journal of Urban Health*. 2006; 83(0):98–112.
75. Moorthi, G., Li, J., Mosher, H., Weeks, M. An ethnographic exploration of RDS coupon distribution dynamics: motivation constraints and contextual factors; International Network for Social Network Analysis Annual Meeting; February 18–23; St. Pete Beach, Florida. 2014.
76. Valente TW, Zogg JB, Christensen S, Richardson J, Kovacs A, Operskalski E. Using social networks to recruit an HIV vaccine preparedness cohort. *Journal of acquired immune deficiency syndromes (1999)*. 2009 Dec 1; 52(4):514. [PubMed: 19584741]
77. Mosher HI, Moorthi G, Li J, Weeks MR. A qualitative analysis of peer recruitment pressures in respondent driven sampling: Are risks above the ethical limit? *International Journal of Drug Policy*. 2015; 26(9):832–42. [PubMed: 26141164]
78. Semaan S, Santibanez S, Garfein RS, Heckathorn DD, Des Jarlais DC. Ethical and regulatory considerations in HIV prevention studies employing respondent-driven sampling. *Int J Drug Policy*. 2009 Jan; 20(1):14–27. Epub 2008/02/05. eng. [PubMed: 18243679]
79. DeJong J, Mahfoud Z, Khoury D, Barbir F, Afifi RA. Ethical considerations in HIV/AIDS biobehavioral surveys that use respondent-driven sampling: illustrations from Lebanon. *Am J Public Health*. 2009 Sep; 99(9):1562–7. Epub 2009/07/18. eng. [PubMed: 19608961]
80. Verdery AM, Mouw T, Bauldry S, Mucha PJ. Network structure and biased variance estimation in respondent driven sampling. *PloS one*. 2015; 10(12):e0145296. [PubMed: 26679927]
81. Crawford FW, Aronow PM, Zeng L, Li J. Identification of homophily and preferential recruitment in respondent-driven sampling. *Am J Epidemiol*. 2017; :kwx208.doi: 10.1093/aje/kwx208
82. Lu X. Linked Ego Networks: Improving estimate reliability and validity with respondent-driven sampling. *Social Networks*. 2013; 35(4):669–85.
83. Mouw T, Verdery AM. Network Sampling with Memory A Proposal for More Efficient Sampling from Social Networks. *Sociological methodology*. 2012; 42(1):206–56. [PubMed: 24159246]

* PWID = people who inject drugs

Figure 1.
IDU Networks in RDS Recruitment Process

Figure 2.
Sample Recruitment graph
(Solid black nodes: seeds; other nodes: non-seed participants; shape: chain affiliation)

Figure 3.
Conditions underlying RDS Assumptions, and Analytical Questions

Table 1**RDS Sample[§] Characteristics (N=528)**

	N	(%)
Gender- Male	402	76.14
Ethnic group:		
Black/African American	96	18.18
Puerto Rican/other Latino	304	57.58
White/other groups	93	17.61
Marital status:		
Single	317	60.04
Married/Living together	95	17.99
Divorced/Separated	89	16.86
Homeless	236	44.7
Monthly income <\$500	248	46.97
Work Situation		
Unemployed or disabled	453	85.79
Work part time or full time	53	10.04
Substance injected ^a		
Inject heroin by itself	496	94.3
Inject cocaine by itself	293	55.7
Inject speedball	293	55.7
HIV Positive	70	13.26
Shared drugs or injection equipment ^a	174	33.08
Recruited others	325	61.55
	Mean (Range)	SD
Age	45.5 (18.8–74.7)	9.4
Times injected drugs ^a	76.3 (1–470.0)	77.3
Years injected drugs	21.25 (0.2–60.8)	12.3
Number of injector ties	5.32 (0.0–15.0)	3.07

[§]. Include all baseline participants regardless of being a seed or not or invited as recruiter or not.

^a. Behavior occur in the last 30 days before baseline interview

Table 2
Descriptive Statistics of Multiple Degree Measures among Sampled Individuals

Recruiter degree measures	Descriptive Statistics			
	Valid N	Mean	Mode	SD Range
Typical RDS degree	522	19.49	10	43.42 0~500
Total degree	522	8.65	9	4.05 1~29
Self reported degree	522	6.42	5	3.18 0~19
Peer reported degree	522	2.23	1	2.29 0~18
Recruitment intent degree	522	4.5	5	2.59 0~14
Recruitment encounter degree §	410	2.88	3	1.60 0~11
Coupon pass degree §	411	2.32	3	0.97 0~5

§ Excludes participants who did not complete follow-up survey.

Table 3

Association among Multiple Degree Measures among Sampled Individuals

Recruiter degree measures	Univariate OLS and Poisson Regression with Robust SE (β)					
	Valid N	Typical RDS degree ^d	Total degree ^a	Coupon Enrollment degree ^a	Actual enrollment degree ^b	Coupon enrollment matched degree ^a
Typical RDS degree	522	-	.00	.00	.00	.001
Total degree	522	.61	-	.02 [*]	0.05 ^{***}	0.0
Self reported degree	522	1.32	1.03 ^{***}	.02	0.03	-.02
Peer reported degree	522	-.68	1.07 ^{***}	.03 ⁺	0.10	0.03
Recruitment intent degree	522	2.18 [*]	.28 ^{***}	-.002	.05 ⁺	-.01
Recruitment encounter degree [§]	410	.05	.25 ⁺	0.11 ^{***}	.08	-.02
Coupon pass degree [§]	411	1.11	.39 ⁺	.29	.24	0.10 ⁺

⁺ p<0.10;
^{*} p < .05;
^{**} p < .01;
^{***} p < .001

[§] . Excludes participants who did not complete follow-up survey.

^a . OLS regression with robust standard error (SE) was performed.

^b . Poisson regression with robust SE was performed.

Association between Peer Recruitment Process Measures, Ego-Alter Relationship and Alter HIV Risk Factors (Multilevel Logistic Regression at Dyadic level, N = 3392)

Table 4

	Recruitment Intent OR (95% CI)	Recruitment effort OR (95% CI)	Coupon Passing ^c OR (95% CI)	Actual Enrollment OR (95% CI)
Ego-Alter relationship				
Ego-alter hang out ^a	0.79 (0.51, 1.23)	0.37 (0.29, 0.49) ***	0.31 (0.23, 0.41) ***	0.63 (0.48, 0.82) **
Days ego-alter contacted ^b	1.01 (1.00, 1.03) ***	0.99 (0.98, 1.00) *	0.98 (0.97, 0.99) ***	1.02 (1.01, 1.03) ***
Ego close to or received support from alter ^a	0.99 (0.69, 1.42)	1.47 (1.17, 1.84) ***	1.29 (1.00, 1.66) **	1.13 (0.90, 1.43)
Ego-alter lived together ^a	0.61 (0.41, 0.89) ***	1.33 (1.02, 1.75) *	1.84 (1.35, 2.49) ***	2.15 (1.66, 2.78) ***
Ego can go to alter for a place to stay	1.55 (1.07, 2.26) **	1.18 (0.92, 1.51)	1.21 (0.92, 1.60)	0.87 (0.68, 1.10)
Ego can go to alter for money	0.66 (0.46, 0.95) **	1.09 (0.85, 1.39)	1.26 (0.96, 1.65) +	0.90 (0.71, 1.15)
How much ego trusts alter		(moderate level of trust as reference group)		
Low trust	0.79 (0.54, 1.17)	0.89 (0.70, 1.15)	0.80 (0.55, 1.14)	0.99 (0.77, 1.27)
High trust	0.66 (0.42, 1.05) +	0.85 (0.62, 1.16)	0.97 (0.73, 1.30)	1.40 (1.03, 1.89) **
Ego perceived alter risks				
Ego uses drugs with alter ^a	1.95 (1.44, 2.64) ***	0.89 (0.73, 1.09)	0.84 (0.67, 1.05)	0.97 (0.79, 1.19)
Alter injects with others ^a	1.59 (1.01, 2.46) +	1.81 (1.37, 2.39) ***	1.64 (1.20, 2.25) ***	1.41 (1.04, 1.91) **
Alter injects in shooting gallery ^a	1.10 (0.69, 1.84)	0.85 (0.66, 1.09)	0.86 (0.66, 1.12)	1.28 (0.96, 1.72) +
Alter is homeless ^a	1.16 (0.80, 1.68)	1.37 (1.13, 1.67) **	1.36 (1.09, 1.71) ***	1.15 (0.94, 1.41)
Alter is HIV positive	0.99 (0.61, 1.64)	0.97 (0.70, 1.37)	1.08 (0.73, 1.60)	1.22 (0.87, 1.71)
Co-variables				
Alter total degree	1.01 (0.95, 1.07)	0.97 (0.92, 1.02)	1.03 (0.97, 1.09)	1.00 (0.96, 1.04)
Ego out-degree	0.85 (0.80, 0.91) ***	0.88 (0.84, .92) ***	0.82 (0.79, 0.86) ***	0.89 (0.86, 0.93) ***
Alter already got coupon	0.01 (0.00, 0.02) ***	0.28 (0.18, 0.44) ***	0.19 (0.10, 0.36) ***	6.97 (4.70, 10.3) ***
Constant	21.3 (10.4, 43.4) ***	3.94 (2.51, 6.20) ***	2.21 (0.54, 3.25) ***	0.59 (0.37, 0.92) ***
Random effect SD (95% CI)	1.18 (0.99, 1.42) ***	0.54 (0.42, 0.70) ***	-	0.74 (0.61, 0.89) ***

Author Manuscript

Author Manuscript

Author Manuscript

Author Manuscript

$p < .001$

 $p < .01$;
**
 $p < .05$;
*
 $p < .10$;
+
a. Behavior occurred in the last 6 months before survey.
b. Behavior occurred in the last 30 days before survey.
c. Fixed effect logistic regression model is used because: a) the random effect coefficient was close to 0; and b) the model with random effect does not converge with alter and ego degree as co-variables.

Table 5

Comparison of Ego-Alter Relationship Factors and Ego-Alter Risk Factors between Enrolled and Not-Enrolled Ties at the Dyadic Level (N=3392)

	Enrolled Ties	Not-Enrolled Ties
Ego-Alter Relationship		
Ego hangs out with alter ^a	947 (80.80%)	1828 (82.34%)
Days ego-alter contacted ^b (Mean)	19.19 (11.75)	15.25 (12.22) ***
Ego feels close to or received support from alter ^a	399 (34.04%)	684 (30.81%) *
Ego lived with alter ^a	254 (21.67%)	228 (10.27%) ***
Ego can go to alter for a place to stay	322 (28.33%)	565 (25.45%) ⁺
Ego can go to alter for money	435 (37.12%)	756 (34.05%) ⁺
How much ego trusts alter (Mean)	2.98 (1.37)	2.78 (1.29) ***
Ego-Alter Risk		
Ego injects drugs with alter ^a	651 (55.55%)	1200 (54.05%)
Alter injects with others ^a	682 (69.31%)	1147 (64.69%) *
Alter injects in shooting gallery ^a	423 (42.99%)	670 (37.79%) **
Alter is homeless	400 (40.65%)	646 (36.44%) *
Alter smokes crack ^a	465 (39.68%)	1003 (45.18%) **
Alter is HIV positive	112 (9.56%)	145 (6.53%) **

⁺ p<0.10,

* p .05,

** p .01,

*** p .001

^a . During the last 6 months

^b . During the last 30 days

Table 6

Comparison of Aggregated Ego-Alter Relationship Factors and Ego-Alter Risk Factors between Those who Received a Different Number of Coupons (N=2167 unique alters with complete follow-up data)

Sum of dyadic measures by alter unique ID	Number of Coupons Received		
	No CP (N=1343)	1 CP (N=754)	2 or more CPs (N=81)
Ego-Alter Relationship	Mean (SD)		
Ego hangs out with alter ^a	.92 (.49)	1.09 (1.07)	4.04(3.05) ***
Days ego-alter contacted ^b	17.25(14.47)	24.22(26.82)	101.59(72.08) ***
Ego feels close to or received support from alter ^a	.34(.49)	.48(.68)	1.44(1.38) ***
Ego lived with alter ^c	.11(.32)	.23(.48)	1.15(1.11) ***
Ego can go to alter for a place to stay	.28(.48)	.39(.63)	1.35(1.36) ***
Ego can go to alter for money	.38(.52)	.53(.71)	1.75(1.44) ***
How much ego trusts alter	3.04(1.77)	4.12(3.70)	15.09(9.95) ***
Ego-Alter Risk	Mean (SD)		
Ego injects drugs with alter ^a	.59(.55)	.73(.92)	2.62(2.04) ***
Alter injects with others ^a	.77(.49)	1.02(.77)	3.30(2.37) ***
Alter injects in shooting gallery ^a	.50(.53)	.60(.72)	2.25(2.22) ***
Alter is homeless	.39(.51)	.54(.69)	2.05(1.96) ***
Alter is HIV positive	.79(.29)	.11(.37)	.52(1.17) ***
Actual Enrollment	.12(.53)	.77(1.73)	5.20(3.33) ***
Alter total-degree	1.48(1.99)	3.21(3.65)	10.70(4.59) ***

p .001

^a. During the last 6 months

^b. During the last 30 days

Table 7

Comparison of Aggregated Ego-Alter Relationship Factors and Ego-Alter Risk Factors between Sampled participants who were Directly Recruited and Indirectly Recruited^s

Sum of dyadic measures by alter unique ID	Dyadic Measures Aggregated at Individual Level ^c Mean (SD)	
	Direct Recruit N=204	Indirect Recruit N=201
Ego-Alter Relationship		
Ego hangs out with alter ^a	2.14(1.89)	2.15(1.91)
Days ego-alter contacted ^b	53.06(48.13)	48.89(45.43)
Ego feels close to or received support from alter ^a	.91(.91)	.95(.97)
Ego lived with alter ^a	.60(.77)	.59(.78)
Ego can go to alter for a place to stay	.81(.91)	.72(1.08)
Ego can go to alter for money	1.03(1.06)	.95(1.00)
How much ego trusts alter	8.16(6.50)	7.61(7.06)
Ego-Alter Risk		
Ego injects drugs with alter ^a	1.45(1.36)	1.39(1.45)
Alter injects with others ^a	1.69(1.53)	1.73(1.66)
Alter injects in shooting gallery ^a	1.15(1.44)	1.08(1.42)
Alter is homeless	.95(1.28)	1.00(1.23)
Alter is HIV positive	.23(.69)	.25(.70)
Actual Enrollment	2.69(2.11)	2.56(2.34)
Alter total-degree	7.94(3.80)	8.58(3.72)

^s. Exclude participants who were not mention in any other network.

^a. During the last 6 months

^b. During the last 30 days

^c. Only includes sampled participants who were also mentioned as alters of someone.