

SCALE-UP METHODS AS APPLIED TO ESTIMATES OF HEROIN USE

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The feasibility of using the network scale-up method to estimate heroin use is described. A random sample was asked "How many people do you personally know" who use heroin, and how many in other subpopulations – robbery, assault, burglary, auto-theft victims, binge drinkers, and marijuana users – whose size is more accurately known. A model estimated the overall number of persons each respondent knew and the size of each subpopulation. Estimates of the subpopulation are compared with known subpopulation sizes to assess the plausibility of the model. Data came from the 1999 survey evaluating the "Fighting Back" substance prevention program. Fourteen sites with clear political boundaries were used (n=5892). Heroin use varied from city to city. Rates estimated for heroin use correlated .832 with the level of respondents' sense of "crime in their neighborhood." The average ratio between the known populations and the estimates is .943. Members of each subpopulation, especially drug users, tended to know more people within their own subpopulation.

INTRODUCTION

We report here on the feasibility of applying network scale-up methods to estimate the prevalence of heroin use in 14 communities. The scale-up method has

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been widely applied with promising results (Shelley, Bernard, Killworth, Johnsen, & McCarty, 1995; Shelley, Killworth, Bernard, Johnsen, McCarty, & Rice 2003). The premise of scale-up methods, to be expanded on below, is that if we knew the size of a respondent's social circle for a random population sample, and if we knew the number of persons in their circle who used heroin, then, by using an appropriate model, we could estimate the number of heroin users in a population. Prior work shows that, for optimum results, 20 to 30 subpopulations of known size are needed to back estimate the size of each respondent's social circle (Bernard, Johnsen, Killworth, & Robinson, 1989; Bernard, Johnsen, Killworth, & Robinson, 1991; Killworth, Johnsen, McCarty, Shelley, & Bernard, 1998). In the experiment reported here, only six such populations are used. The experiment was successful in producing heroin rates that order the 14 communities in a reasonable manner, and the size of the estimates seem in line with those produced by other methods. There are limitations, however. The small number of known-size subpopulations produces large standard deviations of a key parameter – the estimate of the size of a person's circle – that is relevant to making these estimates. In addition, the ability of the model to accurately produce back estimates of the six known rates once the maximum likelihood model is calculated is variable. Some of the rates are close to the known rates, while others are not. There are systematic distortions, some of whose causes we will explore.

Heroin wreaks medical, social, political, and economic havoc far out of proportion to the number of its users. Assessment of the prevalence and social distribution of heroin and other infrequently used but dangerous drugs is critical to the development of sound preventive and treatment strategies. Nonetheless, it is notoriously difficult to assess heroin use rates in the general population. Authors of a recent study of "hardcore drug users" in the United States observed, "People who are heavily involved in drug use often refuse to admit that they engage in such activity, and they are characteristically difficult to locate for an interview. These limits – on veracity and access, respectively – tend to produce negative bias in any estimates of size that are made with survey data" (Simeone, Rhodes, Hunt, & Truitt, 1997, p. 4). The National Household Survey of Drug Abuse (NHSDA), which utilizes a large sample and is the only source for estimating the prevalence of the use of illegal drugs in the entire U.S. noninstitutionalized population age 12 and older, reports that "Estimates of heroin use from the NHSDA are considered very conservative due to the probable undercoverage of the population of heroin users" (Office of Applied Studies, 2000a, p. 75). The most recent NHSDA estimate (2003) is that 3.7 million Americans 12 and over have used heroin during their lifetime and that 314,000 used heroin in the last year (Substance Abuse and Mental Health Services Administration, 2004, p. 36).

Nonetheless, the incidence and prevalence of heroin use in the United States appear to vary considerably from city to city depending on the nature of the local distribution system, the type of heroin available, and the local drug-using culture. Notwithstanding the critical importance of national policies with respect to the prevention of heroin use and distribution, effective policies must also be adapted to local needs and conditions. Consequently, the Community Epidemiology Work Group (CEWG) meets twice a year to assess trends in drug abuse. In an effort to achieve triangulation, the CEWG uses “data from a variety of health and other drug abuse indicator sources” (CEWG, 1999). Included are the treatment episodes data set (TEDS) developed by the Substance Abuse and Mental Health Services Administration (SAMSHA); the Drug Abuse Warning Network (DAWN) data developed by SAMSHA from drug-related incidents reported by emergency departments and coroners’ reports; the Arrestee Drug Abuse Monitoring (ADAM) program funded by the National Institute of Justice (NIJ); data from the Drug Enforcement Administration (DEA), such as information on drug seizures; the Uniform Crime Reports (UCR) of the FBI; and information from Regional Poison Control Centers. Qualitative information from ethnographies and other community-based sources is also used. Local epidemiological surveys are generally not available. All of these materials are invoked in an attempt to uncover trends and changes from year to year. In contrast, the NHSDA’s national estimate of heroin incidence is said to be too low to establish reliable year-to-year trends (Office of Applied Studies, 2000b).

Simeone and colleagues (1997, p. iv) made the most sophisticated attempt to estimate heroin use at the local level: “Data [were] collected on arrestees at Cook County Jail, on people admitted to publicly- and privately-funded drug treatment programs in Cook County, and on people staying overnight in residential homeless shelters in Cook County [Illinois].” Random hair tests were used to estimate the proportion of drug users in these institutions. The number of times self-admitted drug users said they had contact with these institutions over a given period of time was used to estimate the number of “drug events” for each institution. And a mathematical model was used to multiply the number of expected events by the number of expected users at these institutions. In 1995, the estimate for Cook County showed 63,000 heroin users based on the arrest data, 60,000 based on the treatment data, but only 4,300 based on the shelter data (Simeone et al., 1997). The researchers concluded that the shelter data yielded too low an estimate. Our projections of these figures to the Cook County population aged 21 to 65 in 1965 yield 2.1% heroin users based on the arrest data and 2.0% for the treatment database. Nevertheless, the study takes into account only persons who came to the attention of authorities. Based on analyses of our data and the NHSDA, we find that one half to three quarters of

heroin users (depending on the severity of use) have been in treatment in the last year. The number of Cook County heroin users may, therefore, be between 75,000 and 120,000, or between 2% and 4% of the population aged 21 to 65. Clearly, more refined estimates of the number of heroin users are needed. Further triangulation is desirable, and, even if network scale-up methods are far from perfect, they may provide insight into estimates of heroin use.

METHOD

THE NETWORK SCALE-UP MODEL

The network scale-up method has been developed since 1986 to estimate hard-to-count populations such as those who are HIV positive, women who have been raped, and the homeless (Bernard et al., 1991; Killworth, Johnson et al., 1998; Killworth, McCarty, Barnard, Shelley, & Johnsen, 1998; McCarty, Killworth, Bernard, Johnsen, & Shelley, 2001). This method involves asking a statistically representative set of respondents how many people they know in a variety of subpopulations (in the papers cited, these subpopulations include people named Michael, people who are diabetic, people who have a twin, etc.). “Know” is defined for respondents as their having had contact within the last two years. These subpopulations fall into two categories. The majority are subpopulations whose size, relative to the larger population in which they are embedded, is known. The examples above fall into this category. The second category is subpopulations (in this, case heroin users) whose sizes are to be determined.

The method assumes that, other things being equal, the probability that any member of the respondent’s network belongs to a particular subpopulation is the fraction of the larger population constituted by that subpopulation. In other words, if $1/100^{\text{th}}$ of the population of the U.S. have some characteristic, then on average, *ceteris paribus*, $1/100^{\text{th}}$ of any person’s network will possess that characteristic as well.

Various forces work against this assumption. First, there is random influence; one cannot assume that the simple proportionality applies to any individual. To deal with the issue of randomness, the network scale-up method assumes that a given respondent has a total network size of some value c , which is unknown. For any subpopulation that occupies a fractional size p of the entire population, the probability that any member of the network is in the subpopulation is assumed to be p . Thus, the number of people reported to be known in the subpopulation (m) has a binomial distribution with probability p and mean cp . The respondent is asked about many subpopulations and from the responses given, a maximum-likelihood estimate of the respondent’s c (i.e., one which best fits the pattern of responses elicited) is computed. Providing that “many” is equal to or greater than approximately 20, the

estimates of purely statistical error (i.e., those not involved with the assumptions of the methodology) become small. In our test of the method for estimating the number of heroin users, we were able to use only six subpopulations. We will discuss the potential consequences of this on our estimates later.

Once the network size, c , for each respondent has been estimated (a distribution of great theoretical interest), a similar maximum-likelihood estimate is computed for the size, m , of the unknown subpopulations. The maximum-likelihood estimate is constructed from the pattern of responses across all respondents.

The papers cited above report tests of the method to estimate the size of each of the known subpopulations as well as the size of populations whose size is unknown. The results have been generally accurate, although there remains a tendency for the size of large subpopulations to be underestimated and for that of small subpopulations to be overestimated (e.g., Figure 2 of Killworth, McCarty et al., 1998).

Another complicating factor is that because there is stigma involved or because it is not common knowledge, the respondent may be unaware that someone in his or her network is a member of a subpopulation. This transmission effect can be mitigated to a large extent by the selection of obvious or easy-to-recognize subpopulations: news about a death in an auto accident almost certainly travels more reliably and more widely to members of the victim's network than does the fact that someone has a twin sibling. In what is termed a "barrier effect," the respondent may not know many (or any) members of the subpopulation because people in that subpopulation are unlikely to be members of the respondent's personal network. People in Oklahoma, for example, are far more likely to know an American Indian than are people in New Jersey. If heroin users are a closed community, then their isolation may represent a barrier. In principle, the scale-up method allows for the testing of both barrier and transmission effects (though this has not been previously systematically incorporated into the method). Indeed, as we show below, if respondents are also members of a subpopulation, they are likely to know more persons in that subpopulation. This is especially true for respondents who are substance abusers.

Finally, respondents may simply be unable to estimate accurately. To a great extent, the natural averaging that occurs with representative samples of respondents neutralizes random effects. This has been tested (Killworth, McCarty et al., 1998). However, systematic errors are different. McCarty et al. (2001) discuss the question of respondents' tendency to round off their estimates (typically to multiples of 5) and show that this has a weak effect only.

In sum, the main problems for the present study in applying the network scale-up method to estimates of heroin use are the relatively small number of subpopulations available, which produces larger variances than would be desirable, and transmission

and barrier effects, whose magnitude will be estimated and whose implications both for the method and for understanding substance abuse will be discussed.

METHOD

THE FIGHTING BACK SURVEY

The data reported here were obtained from the second wave of a telephone survey conducted in the spring of 1997 as part of the evaluation of a nationwide, community-based drug and alcohol prevention program, Fighting Back, supported by the Robert Wood Johnson Foundation. This 23-minute telephone survey collected data on 17,900 individuals aged 16 to 44 in 12 communities that participated in the Fighting Back initiative and in 29 control-site communities. The survey was designed to assess the relationships among physical and social environmental variables, the existence of a community-based prevention program, and self-reports of the use of various substances. Comparison sites were from the same states as the Fighting Back sites to maximize the comparability of data. In most of the communities, the survey focused on the inner city, matching the areas targeted by the Fighting Back program, which often were not whole cities, but rather specific regions or neighborhoods (Beveridge, Kadushin, Saxe, Rindskopf, & Livert, 2000). For reasons that will become apparent, the test of the scale-up method is restricted to those 14 sites in which the survey sample closely matches official political boundaries. They include nine cities from the Northeast, four from the South, and one from California. The populations of these medium-sized cities and the sample size for each city (smaller samples are from the control sites) are shown in Table 1. For present purposes, the experimental/control design of the study is ignored.

THE SIX BENCHMARKS IN THE FIGHTING BACK STUDY

Given that the entire survey was limited to 23 minutes, every question had to be related to the purpose of the evaluation, which was to assess substance use and related harms. Since crime may be associated with drug and alcohol use and crime reduction was one of the goals of the intervention, crime (rather than, say, the number of persons with a given name whom the respondent knew) was used as one of the predictors or benchmarks for estimating heroin use. In theory, crime rates for each city are available from the FBI Uniform Crime Reports (UCR), although, as will be seen, this supposition may be questionable. To test the utility of the UCR, a small-scale crime-victimization survey was included in the study.

Respondents were asked to state the number of persons known to them in the city being surveyed who:

“... in the last 12 months had their apartment, home, or garage broken into?”

TABLE 1
POPULATION OF AREAS SAMPLED, 1997

COMMUNITY: State, FB/Not FB, Name	Population 16-44	Sample Size
AR FB Little Rock	78623	1023
AR NB Fort Smith	31478	292
CA FB Vallejo	53522	1029
CT FB New Haven	65015	1008
CT NB Bridgeport	62947	199
CT NB Hartford	65821	197
CT NB Waterbury	46682	210
MA FB Worcester	78028	607
MA NB Fall River	37805	201
MA NB Lowell	53180	210
MA NB Springfield	69317	203
NC NB Greensboro	96120	201
NC NB Raleigh	136007	201
NJ NB Camden	37815	311
Total sample		5892

“... in the last 12 months were beaten up, attacked, or hit?”

“... in the last 12 months had something taken from them or were robbed by force, such as by a stickup, mugging, or threat?”

“... in the last 12 months had any cars, vans, trucks, motorcycles, or other motor vehicles stolen or used without permission?”

“Knowing” was explained as follows: “By knowing someone we mean you know them and they know you by sight or by name, that they are living, that you could contact them by phone, mail, or through someone else if you had to, and that you have had some form of contact with them in the past two years.”

In addition, just before each of these questions (except for vehicle theft), respondents were asked whether such a crime had been committed against them in the last 12 months. The purpose of these questions was to obtain an estimate of the crime rate in each locality independent of that provided by the UCR. The wording was taken from the Crime Victimization Survey so as to ensure maximum overlap with the crime definitions used in the UCR.¹ For example, one question asked, “During the last 12 months, did anyone break into, or somehow illegally get into, your apartment or home, garage, or another building on your property?” The question of whether one’s own vehicle had been stolen was omitted to save time

(in retrospect, this omission may have been an error). Murder, although generally viewed as one of the UCR's more accurate rates, was not used as a subgroup – first, because we obviously could not ask respondents whether they themselves had been murdered and second, because the 1993 survey had shown that nearly everyone living in inner cities claimed to know someone who had been murdered in the past year, despite the injunction that the person be “known personally.” On the other hand, because deaths annually from traffic accidents were too few in these non-automobile-using inner cities to be statistically reliable, vehicular deaths also were not useful as benchmarks.

To the crime-related items used as predictors we added two related to substance use:

“How many people do you know personally, age 16 to 44, living in [SITE] who, in the last 30 days, had five or more drinks on at least one occasion?”

“How many people do you know personally, between the ages 16 and 44 living in [SITE], who use marijuana or hashish?”

These questions were asked in the context of questions that ascertained the respondent's own bingeing behavior (five or more drinks on one occasion) and marijuana use. Finally, respondents were asked, “How many people do you know personally, between ages 16 and 44 living in [SITE], who use heroin?” The prior question asked, “In the last 12 months have you used . . . heroin (HAIR-oh-in)?” Given the history of responses to questions about heroin use in sample surveys, the answers to this question are suspect. In the analyses that follow, personal heroin use is regarded as unknown, to be estimated by the model.

Asking “who do you know” questions requires some reference points in time and space. In our study, the time reference was “in the last 12 months.” In 17 of the 31 Fighting Back locales, respondents were asked about people whom they knew in their neighborhood (like “East Side” in San Antonio) who had been attacked, been in a car wreck, etc. UCR statistics, however, are reported for whole cities, not for neighborhoods. We restrict our analysis, then, to the 14 out of 31 locations in the Fighting Back study in which respondents were asked the “who do you know” questions about the entire city.

WHY NOT USE UNIFORM CRIME REPORTS STATISTICS AS BENCHMARKS?

The scale-up method usually uses publicly available, official rates – like the number of women who have given birth in the last year in a particular place – since they are usually the most reliable. In the present case they are not.

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Two versions of crime rates as benchmarks are used in this test. To what extent do crimes reported by the survey respondents in the 14 selected sites in 1997 match the crime rates in the 1997 UCR data set? There may not be a close match between official statistics and reports garnered in a survey because people do not necessarily report crimes to the police. Of those in our sample who reported having been burglarized, 29% said that they did not report this to the police; 45% of those who said they had been attacked did not report it; and 40% of those who said they had been robbed did not report it. Overall, survey respondents (not the entire population at risk, only the segment between the ages of 16 and 44) reported, on average, rates about three times higher of robbery and burglary than do the UCR data. As shown in Table 2, however, the ratio varies by city. Respondents reported a slightly lower number of assaults than UCR data (a ratio of .88 of the UCR assault number). It is not clear why this is so, since, as noted, 45% who said they had been attacked did not report this to the police. There may be considerable differences between respondents' understanding of the definition of assault and the formal definitions used by the police and the FBI. As expected, there is not a very good overall match between the UCR data and the survey data. Note that the ratio between victimization rates in the survey and in the UCR varies considerably from city to city. Since the survey questions are a constant across cities, a reasonable hypothesis is that police procedures in reporting crimes vary from city to city.

TABLE 2
RATIO OF SURVEY RESPONSES TO UCR DATA

Site	Ratio Survey to UCR Robbery	Ratio Survey to UCR Assault	Ratio Survey to UCR Burglary
Little Rock	2.48	0.51	2.57
Fort Smith	9.58	1.18	5.76
Vallejo	3.89	1.29	4.21
New Haven	2.83	0.68	2.77
Bridgeport	3.00	0.80	2.42
Hartford	2.48	0.57	3.91
Waterbury	5.44	1.17	1.97
Worcester	5.29	1.20	3.63
Fall River	10.11	0.60	2.27
Lowell	10.62	3.53	4.99
Springfield	1.93	0.41	2.77
Greensboro	1.35	2.25	2.07
Raleigh	5.55	0.58	3.02
Camden	2.38	0.77	1.92
Average	3.32	0.88	2.85

DOES THE METHOD PRODUCE REASONABLE RATES OF HEROIN USE?

Table 3 shows the resulting estimates for heroin use in the 14 cities in our study. The estimates range from a low of .006 for Greensboro and .007 for Raleigh, both in North Carolina, to .05 for Camden, New Jersey, and .055 for Bridgeport, Connecticut. One impression is that the rank ordering of the figures roughly corresponds to how “tough” the city is. But why rely on the impressions of social scientists? At the beginning of the interview, before any questions on their personal exposure to crime or their personal use of substances or their knowledge of people who use substances, respondents were asked, “How serious a problem is crime in your neighborhood (very serious, somewhat serious, not at all serious)?” Table 3 also shows the proportion of person who picked the “very serious” alternative. The Spearman rank order is .83, significant, even with 14 cases, at the .01 level.

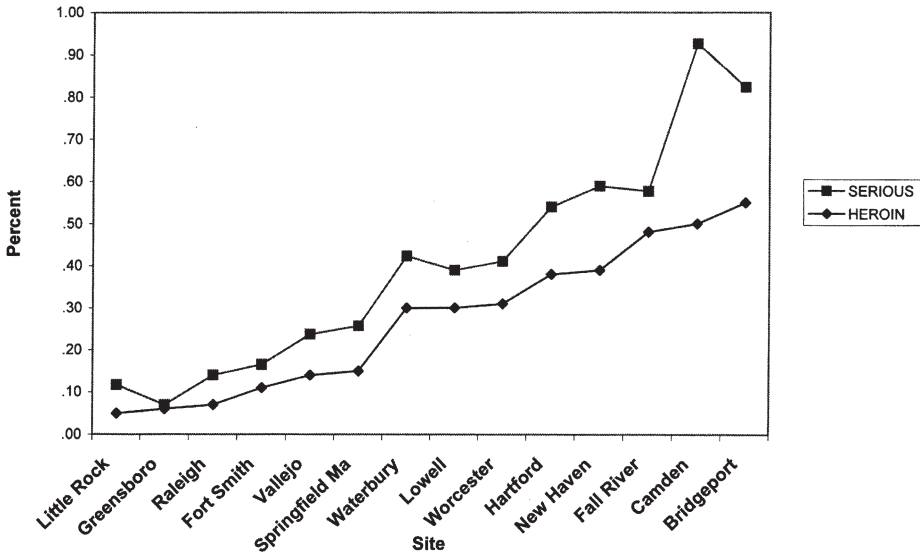
How reasonable is the size of the figures? This is, of course, hard to tell if we believe that there are no adequate heroin use rates available. But recall that the most sophisticated estimate for a city, Chicago, produced by Simeone and colleagues (1997), was reworked by us, taking account of the likelihood that heroin users came to the attention of authorities, to be between .02 and .05. This more or less

TABLE 3
ESTIMATED HEROIN RATE AND PERCEPTION THAT “CRIME IS SERIOUS IN MY NEIGHBORHOOD”

Site	Estimated Heroin Rate	“Crime, Serious”
Little Rock	.005	.07
Fort Smith	.011	.01
Vallejo	.014	.07
New Haven	.039	.06
Bridgeport	.055	.10
Hartford	.038	.11
Waterbury	.030	.12
Worcester	.031	.09
Fall River	.048	.10
Lowell	.030	.16
Springfield MA	.015	.20
Greensboro	.006	.10
Raleigh	.007	.43
Camden	.050	.27
Spearman Rho		.832**

**Significant .01, two-tailed test.

FIGURE 1
ESTIMATED HEROIN RATE (x10) AND NEIGHBORHOOD CRIME SERIOUSNESS



encompasses the range reported for the “tougher” cities in the Northeast in our study.

HOW WELL DOES THE MODEL FIT?

BACK ESTIMATES

There is no objective way to determine whether the estimated rates, using the scale-up method, for unknown-size subpopulations is correct. The procedure used is a maximum-likelihood iterative calculation that, by definition, produces the best fit to the data input. But are these the right data to use in the first place? There are three reasonable ways to decide whether the model fit is reasonable. First, if the model predicts accurately populations whose size we already know (like the number of people who were robbed), then this lends credence to the model’s prediction of a population whose size we do not know (like the number of people who use heroin). These are called backward predictions. In our case, the benchmark data (the populations of putatively known size) include respondents who were robbed, assaulted, or burglarized (which we try to estimate using our mini-victimization survey) and who used marijuana and binge drank during the 12 months prior to the survey (which we try to estimate using our epidemiological survey). Remember that one of the real data points was not obtained from our survey: rates of stolen vehicles were those obtained from the UCR data. A rough and ready way of making

these comparisons is to look at the ratio between the backward predicted rates and the real rates for each community as shown in Table 4 below.

TABLE 4
MEAN RATIOS – COMPUTED AS ACTUAL

Site	Robbery	Assault	Burglary	Vehicle Theft	Binge	Marijuana	Mean
Fort Smith	0.606	0.505	0.229	0.192	1.105	2.705	0.890
Vallejo	0.696	0.661	0.324	0.176	1.036	2.156	0.841
New Haven	0.610	0.803	0.219	0.235	1.102	2.666	0.939
Bridgeport	0.717	2.357	0.163	0.106	1.156	2.974	1.245
Hartford	0.838	0.877	0.153	0.157	0.835	2.547	0.901
Waterbury	0.715	1.247	0.244	0.175	1.010	1.976	0.895
Worcester	0.929	0.867	0.350	0.220	0.968	2.115	0.908
Fall River	0.380	1.389	0.305	0.129	0.955	2.336	0.916
Lowell	0.722	1.266	0.358	0.260	0.781	2.481	0.978
Springfield Ma	0.614	0.622	0.294	0.188	0.883	3.072	0.946
Greensboro	0.827	0.502	0.328	0.118	0.956	2.417	0.858
Raleigh	0.806	0.483	0.357	0.180	0.840	3.003	0.945
Camden	0.919	1.077	0.259	0.209	1.124	2.845	1.072
Mean	0.717	0.965	0.281	0.186	0.981	2.526	0.943
SD	0.145	0.498	0.070	0.049	0.117	0.367	0.105

The first thing to notice is that while the average ratio for the entire set of predictions looks fairly close at .943, there is considerable variation according to what is being compared, as well as some variation by community. We do well with robbery and assault among the crimes, but there is considerable variation by community in the match between our estimates and the victimization survey for assault. It is not clear what the source is of variation by community in this case. Not surprisingly, the one statistic that we did not measure, vehicle theft (taken from the UCR) is the most poorly matched to the back estimate. On the other hand, so is burglary, which we measured ourselves. It may be that robbery and assault, which require physical action against a victim, are both more easily remembered and more easily reported than burglary, which is entering and taking something without permission, a category that makes sense to law enforcement but perhaps less so to the average citizen. As for the epidemiological measures, everybody seems to understand bingeing, or at least it seems to be accurately reported and accurately observed in others.

Marijuana data are interesting. Respondents report knowing many people who use it, but fewer report using it themselves. As social scientists, we are inclined

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to believe the reports given about others' use rather than self-reports of use for an illegal substance. There is also fairly low variation in this ratio by community. There seem to be many reasons why these data might be biased both in attempts to collect so-called "real data" as well as in the ability of respondents to know about others.

SIZE OF THE CIRCLE: THE NUMBER OF PERSONS THE RESPONDENT KNOWS

Part of the processes of the scale-up method is to estimate the number of persons an average respondent knows, at least for the purposes at hand. This is a critical estimate because, in theory, it drives all the others. The size of this circle, as estimated in the present data set, is much below that derived from other, more nationally representative data sets. The value of c , shown in Table 5, is 55 (SD 80) when calculated from our survey data. Note that the standard deviation in all sites includes 0. These estimates are smaller than the mean of 105 (SD 89) given in Killworth, Johnson et al., (1998) for seven subpopulations used to estimate seroprevalence, or

TABLE 5
SIZE OF 'c'

Site	N	Mean	Standard Deviation
Little Rock	893	53.68	80.07
Fort Smith	269	64.19	90.13
Vallejo	817	47.75	76.20
New Haven	865	63.07	100.40
Bridgeport	171	57.82	83.90
Hartford	169	45.72	60.35
Waterbury	176	50.79	81.72
Worcester	522	48.40	76.39
Fall River	169	48.74	70.90
Lowell	167	45.73	69.14
Springfield Ma	175	50.59	68.83
Greensboro	186	65.77	86.43
Raleigh	158	51.46	80.15
Camden	261	70.62	94.41
Mean		54.59	79.93

the 1700 or so for the population of the United States reported in that paper. Using a variety of different methods, Bernard and colleagues² as well as others (Freeman & Thompson, 1989) have found an average c value in a highly stable range of 250 to 290. On the other hand, the stigma of a particular behavior to be estimated may reduce both c and the general size of people's networks. Networks of people who are living with HIV or AIDS are a third the size of networks in the general population (Johnsen, Bernard, Killworth, Shelley, & McCarty, 1994). The size of c – the circle known to respondents – clearly varies considerably depending on the subject studied and the benchmarks used for the estimates. The size of c in the present case may be small because all six benchmarks of the known variables as well as the unknown one to be estimated all suffer from both barrier and transmission effects.

BARRIER AND TRANSMISSION EFFECTS: "IT TAKES ONE TO KNOW ONE"

A serious impediment to all network scale-up methods remains to be discussed. It is assumed that the average respondent is able to know how many people in his or her circle possess a given characteristic – in this case, being a heroin user. Two kinds of distortion were noted above: transmission effects, in which the attribute is known but people are not inclined to spread this knowledge or to pay much attention to it, and barrier effects, in which distortion is caused by the invisibility of the attribute in the first place. This invisibility may be literal (for example, having a mole on one's thigh), or it may be caused by a social-structural barrier that prevents one from seeing a particular trait. Substance abuse is classically a social phenomenon: binge drinking is often done on social occasions, and illegal drugs are distributed through a social network. If one is not part of the network of abusers, then one may have very limited access to knowing abusers. The number of persons one knows, especially the number of heroin users one knows, is almost directly related to estimates of the number of heroin users in a given population (Spearman rank order .982). Further, in previous work it was shown that the estimates of c are highly related to the number of persons respondents say they know, as indeed the formulas suggest. Therefore, biases affecting the number of persons known to respondents for each of the seven subpopulations in this study may also bias respondents' estimates of the number of persons they know who use heroin.

In other studies, there appear to have been a variety of barriers as well as facilitators to knowledge of such intimate details about another person as whether they were sero-prevalent or used drugs (Shelley et al., 1995; Killworth, McCarty et al., 1998). The data from the Fighting Back study allow us to estimate these biases. The hypothesis to be tested is that if one belongs to a circle of friends who drink or who use drugs, then one is more likely to know more people who drink or who use drugs than if one is not a member of such a circle. In short, "It takes

one to know one.” Selective attention may also facilitate knowing someone in any given subpopulation if one is a member of that subpopulation. If a person has been robbed, then he or she may be more likely to notice that other people have been robbed.

The seven domains of circles in this study are investigated separately to test whether being a member of the class of persons under investigation – for example, being a marijuana user if one is being asked about marijuana – influences the size of the circle. There are other relevant factors, of course. One might expect that younger persons might be more aware of drug users and more generally sensitive to crime; on the other hand, older persons might have a larger circle of acquaintances. Women are generally less likely to be users and also less likely to be on the street to observe or know about crime. The data generally suggest that African Americans are more likely to be crime victims. Finally, crime and other normatively deviant acts are more likely to occur to persons of lower socio-economic status (SES).

Both crime and drug use are sensitive to SES in large respect because both are related to living in a disadvantaged neighborhood. We were able to geocode 94% of our respondents and then assign them to census tracts. Using the tract-level data, five neighborhood-context indicators were used to create a neighborhood disadvantage index (see Ricketts & Sawhill, 1988; Saxe et al., 2001). All U.S. census tracts (*circa* 50,000) were given percentile scores on the following frequently used measures of disadvantage: percentage of the adult population unemployed, percentage of high-school dropouts, percentage of female-headed households, percentage of persons receiving some form of public assistance, and percentage living in households below the poverty level. For each census tract, the five percentile scores were summed to form a 1 to 500 index in which higher numbers indicated greater disadvantage. The resulting score was used to rank all census tracts in the U.S. on a scale from 1 to 100.

The analysis includes a variable that represents attachment to a worship community – how often one attends religious services. Whether or not this is an accurate assessment on an individual’s part (Hadaway, Marler, & Chaves, 1998; Hadaway, Marler, & Chaves, 1993; Caplow, 1998; Presser & Stinson, 1998; Smith, 1998; Woodberry, 1998), the question does seem to reflect respondents’ sense that they are or at least ought to be affiliated with a religious community. To the extent that belonging to a religious circle widens the set of people known to a respondent, this variable should be positively related to the seven dependent variables. On the other hand, some of the variables represent unpleasant behavior that persons identified with religion might not wish to know about or be involved with; hence, they will be less likely to report such knowledge.

Each of these variables was regressed on the seven “How many people do you know...?” questions. To increase the power of the analysis, all 41 sites in the Fighting Back evaluation survey were included. The results are shown in Table 6. Each column represents the coefficients for each of the seven questions about how many people a respondent knows. The mean number of persons the sample knew who were burgled is shown, for example, in the upper left corner and is 1.583, or

TABLE 6
IT TAKES ONE TO KNOW ONE – PREDICTORS REGRESSED ON NUMBER OF PERSONS KNOWN WITH A GIVEN CHARACTERISTIC (METRIC B SHOWN)

Independent Variables	Dependent Variables						
	Burgled	Attacked	Robbed	Car Stolen	Binge	Marijuana	Heroin
N	16646	16617	16700	16734	15831	16177	16263
Mean	1.583	1.968	1.247	1.345	9.748	12.021	1.045
SD of Mean	4.769	6.607	5.442	5.171	17.755	22.491	5.539
Equation 1							
R Squared	0.005	0.014	0.011	0.009	0.015	0.033	0.012
Female	-0.403	-0.689	-0.547	-0.380	-3.400	-3.984	-0.180
Services#	0.144	ns	ns	0.141	-0.896	-2.071	ns
Disadvantage*	0.081	0.222	0.179	0.155	0.285	0.925	0.222
Constant	1.114	0.824	0.279	3.077	10.427	9.586	-0.293
Equation 2							
R Squared	0.018	0.033	0.026	0.016	0.041	0.127	0.030
Female	-0.400	-0.613	-0.492	-0.342	-2.353	-2.872	ns
Services#	0.143	ns	ns	0.140	ns	-0.778	ns
Disadvantage*	0.065	0.205	0.160	0.143	0.330	0.921	0.215
Are One	1.742	4.175	3.379	2.201 ^a	7.303	22.241	15.871
Constant	1.015	0.663	0.242	0.337	7.540	5.480	-0.325

ns= not significant

#Coded as 0 to 2; *Coded as 0 to 10

^a Robbed replaces car stolen

Full equation also includes number of residents, centered on 2; one or more telephones

The present equations are a "reduced form": Individual SES variables and race were not significant after tract disadvantage index was introduced. Age was also not significant.

roughly 1.5 persons. There are actually 14 different regressions shown in the table: the upper panel in the table shows the results of the regression equation for each of the questions when being a member of the group itself is not considered. The lower panel adds “Are One,” which means that whether or not the respondent himself or herself experienced the phenomenon in question is also taken into account. For example, in the first column, “Are One” represents respondents who themselves were burgled. There is one exception: we did not ask each respondent whether they themselves had their car stolen, so “Are One” in this case represents people who

were robbed. We originally controlled for the individual SES of the respondent as well as for age, but these were not significant variables and were dropped. Each equation includes a control for the number of household residents and the number of telephone numbers coming into the household. These were part of the sampling plan, and including them is equivalent to weighting the data by the sample design. However, these variables are not shown in the table.

Now to the top panel. Again, let us look at the first column for knowing someone who was burgled. The independent variables are female and male, with male coded as zero; the number of religious services per week that the respondent claims to have attended, coded as 0, 1 or 2 or more; and the neighborhood disadvantage score coded 0 through 10. The constant therefore expresses the number of persons known to someone who was male, did not attend religious services, and who had no neighborhood disadvantage. Under these circumstances, the number of persons known who were burgled is a bit over one (1.114). Being female reduces this number by almost half (-.403). For each increment in neighborhood disadvantage the number of persons known is increased by .081. So for persons who live in the maximum disadvantaged neighborhood, the number of persons known to males is almost 2 ($10 \times .081 + 1.114 = 1.924$). The strong effects of being female and being in a disadvantaged neighborhood are clearly shown for every question about who do you know, although the effects of neighborhood vary according to the dependent variable. For example, in the most disadvantaged neighborhoods, other variables being equal, a person was likely to know nine more persons who used marijuana than in the best neighborhoods. In regressions not shown here, ethnicity dropped out as a significant factor once disadvantage was introduced. Religion shows equivocal relations: knowledge of burglary and cars stolen is positively affected; knowledge of attacks, robberies, and heroin is not affected; and knowledge of bingeing and marijuana use is negatively affected.

With these variables taken into account, the bottom panel of Table 6 introduces a respondent's own membership in the class of people to be known – the variable of key interest to us. First, note that there are minor changes in the effects of the some of the variables introduced in the top panel. Claimed attendance at religious services has the same effect as before on burglary and vehicle theft and does not significantly affect the likelihood of one's having been attacked or robbed. However, the effect of attending religious services on knowledge of substance users is sharply reduced when it comes to being a substance user oneself.

More important, in every case, a member of the class is more likely to know others in that class, but this varies dramatically according to the target class. Those who were burgled knew about 1.7 more people who were burgled than did those who were not themselves burgled. Next in the effect of being a member of the class is car theft, for which membership in the class adds 2.2 persons to the average,

although, in this case, membership was measured by having been robbed since we did not ask about one's own vehicle loss. Robbery adds 3.379, and being attacked adds 4.175. Alcohol bingeing adds 7.3 persons, but marijuana adds 22.2 and heroin adds 15.9 to the number in those classes known to the respondent. These findings were obtained even when controlling for living in a disadvantaged neighborhood, which remains a potent variable.

As these figures show, being a member of the class of persons using drugs makes an enormous difference in knowing other persons using drugs. Binge drinking also seems to be a community in which persons know one another, although these effects are not quite as strong as for drug use. Thus, there may be two processes operating in creating barriers or facilitators to knowing others: selective perception in the case of crime (people are more likely to pay attention to instances of crime if it also happened to them) and community membership in the case of drug use and perhaps also in the case of heavy drinking. Those who are not members of this community have few connections to it and therefore are less likely to know users personally. The consequences of these distortions for the scale-up method are discussed below.

DISCUSSION

The network scale-up method is a promising addition to the methods currently available for estimating the size of hard-to-count populations such as heroin users in different communities. With just six subpopulations used as predictors, we estimated the prevalence of heroin use in 14 middle-sized U.S. cities. The estimates obtained by this method are reasonable when compared to how "tough" the cities are in terms of crime according to survey respondents and a sophisticated model of heroin use (Simeone et al., 1997).

The rates also vary appreciably from city to city, consistent with the experience of the CEWG and with those of observers in the cities studied. These results are achieved with quite small samples, in part because each respondent serves as a participant-observer of his or her own network – networks that average 55 persons.

The average overall correlation between the model back estimates of known sizes of the six subpopulations used as predictors is excellent at .943, but there is considerable variation between the cities as well as the six subpopulations. Notable is that the benchmarked populations are derived from the survey itself, rather than some "official" rates that in the case of crime can be biased by the policies of the local police. Further, the survey counts are easily and immediately available.

There are, of course, some problems: the variance of c is high (there are considerable differences in the apparent accuracy of the back estimates of different

subpopulations), and there are strong differences in the ability of respondents of differing backgrounds to see into the worlds of substance abusers. The present test used only six subpopulations to estimate the seventh. This is a major source of the large standard deviation of c – the number of persons known. The recommendation of the literature on network scale-up is that at least 20 subpopulations be used (Killworth, McCarty et al., 1998). Spending precious survey time asking how many people one knows named Matthew is unreasonable if it is also important, for example, to know attitudes toward harm and toward legalization of marijuana, at what age respondents started to use substances, and what prevention messages respondents have heard. On the other hand, the test reported here showed that the “known” subpopulations chosen can include populations such as crime victims that are inherently interesting in studies of illegal drug use. Still, given the broad requirements of an evaluation study, the number of known subpopulations that can be asked about is strictly limited.

Although the subpopulations chosen for this test averaged a very close back-estimate fit, giving confidence in the overall estimates of heroin use, there were major differences among the individual subpopulations in different cities in their closeness of their fit. This finding is typical in the network scale-up literature and argues again for including a fairly wide range of subpopulations, even though in the present test the discrepancies averaged out very well.

This study revealed strong differences among persons of different backgrounds and practices in their access to the information needed to report on the behaviors of the members of their circles. This variation suggests that there is no substitute for getting as representative a sample of respondents as possible, with a wide range of backgrounds, so that the circles of hard-to-find populations can be penetrated. If the wrong starting point is used in network studies, then one ends up with the wrong network. The Fighting Back study used a very carefully designed random sample across a set of cities and had a response rate of 76%. It is instructive, for example, that at least some heroin users must be included in the sample as achieved. If none were available, then the analysis presented here suggests that heroin use, even with network scale-up methods, would be underestimated because access to the network of heroin users would be limited. Network scale-up methods cannot correct for a poor sample. Further, the method requires that a clearly defined area be studied, one whose outlines are intuitively clear to respondents. For this reason, the technique could not be applied generally to evaluate the effect of the Fighting Back program on heroin use because many of the sites and the controls were not defined as political units.

There is a very important by-product of including questions on the number of persons a respondent knows who are abusers of various substances. As has been

shown elsewhere, these questions can be used as important indicators of the extent to which a person is involved in the substance use and abuse system (Kadushin, Reber, Saxe, & Livert, 1998). Substance use and abuse are not only individual traits but are also part of an entire social system that supports use and abuse.

Finally, further tests of the network scale-up method with respect to estimation of heroin use are needed. Perhaps we were simply fortunate in that the particular mix of subpopulations chosen worked out so that the average discrepancy in back estimates was so small. It is important to test other subpopulations as benchmarks and to increase the number from six to closer to the 20 believed to be optimal. For example, although the choice of crime victimization as a benchmark was justified by the reasoning given above, the effect of including less "loaded" items such as "How many people do you know named Matthew?" remains to be investigated. In addition, it would be helpful to find ways to decrease the large variance of c . If this variance were reduced by adding more subpopulations, it might be possible to obtain estimates for heroin use by segments of the population, such as males between 16 and 29, as well as estimates for overall use such as those obtained here.

Notwithstanding these limitations of the network scale-up method as developed and applied to date, it is our recommendation that network questions on "Who do you know who ...?" be routinely included in future epidemiological studies of hard-to-count populations. The questions are relatively easy to administer, they appear to work, and the network questions on drug use that suggest which individuals have connections to drug-using circles are useful in many policy related analyses other than estimation of rates of incidence (Kadushin et al., 1998; Saxe et al., 2001). With respect to such rates themselves, it is impossible to evaluate social policies designed to prevent heroin use without reasonably reliable estimates of current use.

NOTES

- ¹ The Crime Victimization Survey cannot be used for our purposes because it covers only national estimates. In 1998, for the first time in 20 years, the Bureau of Justice Statistics, which conducts the survey, also surveyed 12 cities including Kansas City (most of which is a Fighting Back site) and Madison (part of which is a control site). The data are not publicly available.
- ² A good nontechnical introduction to their search for "c" is Bernard & Killworth, 1997. Also see Killworth & Bernard, 1978; Bernard et al., 1989; Killworth, McCarty et al., 1998 and works cited in those sources.

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