Study Retention as Bias Reduction in a Hard-to-Reach Population

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Abstract

Collecting data from hard-to-reach populations is a key challenge for research on poverty and other forms of extreme disadvantage. Even among the disadvantaged, men and women recently released from prison experience relatively severe hardship that is closely associated with diminished study participation. With data from the Boston Reentry Study (BRS), we document the extreme marginality of released prisoners and the challenges for study retention and analysis. The BRS design and data yield three findings. First, released prisoners show high levels of "contact insecurity," correlated with social insecurity, in which residential addresses and contact information change frequently. Second, strategies for data collection are available to sustain very high rates of study participation. Third, survey nonresponse in highly marginal populations is strongly nonignorable, closely related to social and economic vulnerability. The BRS response rate of 94 percent over a one-year follow-up period allows analysis of hypothetically high nonresponse rates comparable to those in earlier studies. High rates of nonresponse attenuate regression estimates in analyses of housing insecurity, drug use, and re-incarceration. The analysis underlines the utility of small specialized data collections for highly disadvantaged populations.

Extreme social and economic disadvantage poses a fundamental methodological challenge for sociological research. Deeply disadvantaged populations are under-enumerated and at high risk of survey nonresponse (Hunter, de la Puente, and Salo 2003; de la Puente 2004; Martinez-Ebers 1997). Thus studies of homelessness, severe poverty, and drug addiction, for example, have developed special methods for data collection to include those living outside of conventional households and to maximize study retention (Desmond et al. 1995, Cottler et al. 1996, Faugier and Sargeant 1997).

Under current conditions of historically high incarceration rates, people moving through America's prisons and jails have become an important population for research and policy (Travis et al. 2014). The formerly-incarcerated often present a range of co-occurring disadvantages and vulnerabilities that are of key sociological interest, but that also make them difficult to contact or unwilling to participate in research studies. As a result, follow-up studies of the formerly-incarcerated have suffered from high rates of attrition that are likely correlated with outcomes of key interest. Becky Pettit (2012) called the incarcerated population "invisible men" because of their under-enumeration in social survey estimates of socioeconomic well-being. Because of their deep marginality in social and economic life, this invisibility in social science data collection also extends to those who have moved from incarceration to communities.

Recent longitudinal studies of the formerly-incarcerated suffered from 30 to 60 percent attrition over a one to two year follow-up period (e.g., Nelson et al. 1999, Visher et al. 2004). In contrast, the Boston Reentry Study (BRS) followed Massachusetts state prisoners through the first year of prison release and achieved a retention rate of 91 percent. This paper uses data from the BRS to explore three main challenges for longitudinal data collection among the formerly-incarcerated.

First, we document extreme social insecurity in the BRS sample that is closely associated with a high level of contact insecurity—the frequent changing of residential addresses and phone numbers that makes nonresponse more likely. Second, we describe a variety of strategies for data collection that yield a high rate of study participation. Third, we show that the risk of survey nonresponse is associated with deep social disadvantage including histories of addiction and mental illness. In this setting, nonresponse contributes to a loss of statistical precision in data analysis, and nonresponse is nonignorable, biasing estimates of risk and vulnerability in the population. Exploiting the high rate of study participation in the BRS, we explore the sensitivity of sample quantities to survey nonresponse providing evidence of selection bias in analyses of housing insecurity, drug use, and re-incarceration.

Our analysis focuses on the formerly incarcerated but the methods and findings reported here are generally relevant to the study of other hard-to-reach populations that are unstably housed, difficult to contact, or otherwise weakly attached to conventional households. Although large household surveys have been central to the study of poverty and related topics, our results suggest the utility of specialized, and often small-scale, data collections and methods for highly marginal populations that fall outside of the scope of conventional survey methods. We discuss the implications of these designs for research on extreme poverty and other severe disadvantage below.

1 Released Prisoners as a Hard-to-Reach Population

Hard-to-reach populations present a variety of challenges for data collection. Marginalized groups, such as the homeless, drug users, or sex workers, are significantly under-represented in conventional household surveys. Researchers have developed special methods for recruitment and analysis involving, for example, snowball and respondent-driven sampling (Faugier and Sargeant 1997; Salganick and Heckathorn 2004). Even where vulnerable populations can be sampled, attrition and other survey nonresponse pose serious threats in follow-up studies (Farrington et al. 1990; Hough et al. 1996). Researchers report low rates of follow-up among drug users (Desmond et al. 1995; Cottler et al. 1996; Messiah et al. 2003), low-income minority parents (Martinez-Ebers 1997; Teitler et al. 2003), women at risk of HIV infection (Brown-Peterside et al. 2001), the homeless mentally ill (Hough et al. 1996), and youth engaged in crime and delinquency (Farrington et al. 1990; Cotter et al. 2002). In research on the homeless mentally ill, for instance, study recruitment rates are often quite high—from 80 to 90 percent of targeted individuals—but retention rates can be significantly lower, from 30 to 80 percent over six months to two years of follow up (Hough et al. 1996).

Among vulnerable populations, prison releasees are unusually disadvantaged often contending with multiple risks and adversities. Besides their involvement in serious crime, people who serve time in prison suffer from high rates of mental illness and drug addiction. After prison release, the formerly incarcerated are often homeless or insecurely housed (Metraux, Roman, and Cho 2007; Travis 2005) and they are more likely to reside in group quarters—in shelters or other transitional housing—rather than in conventional households (Sirois 2015). Those with outstanding warrants may be on the run, evading both researchers and police. Employment is also unstable and frequently undocumented, and social programs are under-used. The formerly-incarcerated are thus weakly connected to mainstream social institutions and as a result they are often inaccessible to standard data collections using surveys or administrative records (Kornfeld and Bloom 1999; Harding et al. 2014).

High rates of nonresponse reduce the precision of estimates and may be a source of bias. With the small sample sizes typical of the specialized data collections for hard-to-reach populations, even low rates of nonresponse yield a large increase in sampling variance. Where survey nonresponse is related to the values of variables of key interest, nonresponse is called non-ignorable and can be a source of selection bias (Little and Rubin 1987, 15). Say drug users are difficult to contact for interviews and substantive interest focuses on drug use after incarceration for respondents with a history of addiction. More formally, for respondent i at wave t, drug use measured by y_{it} can be written as a function of x_i, a dummy variable indicating drug addiction that is measured at baseline and is completely observed. Addiction may also be a risk factor for nonresponse.

The regression of interest is,

$$y_{it} = \beta_0 + \beta_1 x_i + e_{it}. \tag{1}$$

where e_{it} is a random error. To see the effects of nonresponse we write a response indicator, R_{it}, that equals 1 if a survey is completed and 0 in the case of nonresponse. A sample selection model writes

$$R_{it} = \begin{cases} 1 & \text{if } \eta_{it} \geq 0 \\ 0 & \text{if } \eta_{it} < 0 \end{cases}$$

where η_{it} describes the respondent's propensity to be observed,

$$\eta_{it} = \delta_0 + \delta_1 x_i + \delta_2 z_i + u_{it}, \tag{2}$$

z_i is another completely-observed covariate predicting nonresponse, and u_{it} is a random error. The dependence of nonresponse on the level of drug use y_{it} is usually modeled by allowing the errors e_{it} and u_{it} to be correlated. Note that equation (1) cannot be fit directly because y_{it} is unobserved when $R_{it} = 0$. Analyzing just the observed data, the conditional expectation of the dependent variable depends on covariates and an adjustment factor reflecting the predicted probability of being observed,

$$E(y_{it}|x_i, z_i, R_{it} = 1) = \beta_0 + \beta_1 x_i + h(\delta_0 + \delta_1 x_i + \delta_2 z_i), \tag{3}$$

where h is a function whose form depends on assumptions about the bivariate distribution of e_{it} and u_{it}. The adjustment factor, h, in equation (3) is common to models for sample selection bias (e.g., Fitzgerald et al. 1998; Heckman 1979).

In the naive regression of the observed values of drug use on addiction—a regression of y_{it} on x_i—bias results from the correlation between the covariate, x_i, and the residual which includes the omitted term, h. Below, we assess the magnitude of selection bias in a (near) completely observed data set reporting estimates that exclude observations that are at high risk of nonresponse. The key intuition is that if drug users with a history of addiction are difficult to contact for interviews, the true level of drug use among drug addicts will be understated by the observed data and the regression coefficients will be under-estimated. The attenuation of regression relationships is a common product of nonrandom sample selection on dependent variables, though different patterns of nonresponse might also cause regression coefficients to be over-estimated.

In the study of marginal populations, risks and adversities such as drug use, criminal involvement or housing insecurity may all contribute to attrition from panel surveys and are also outcomes of key interest. In these cases, nonignorable nonresponse biases sample statistics but can be difficult to diagnose in the absence of additional information such as a refreshment sample or population statistics for comparison (Hirano et al. 2001). In sum, besides the widely acknowledged problem of undercount in highly disadvantaged populations, risk of nonresponse is likely confounded with quantities of key interest and is a source of bias in data analysis.

2 Attrition and Retention in Studies of Released Prisoners

Table 1 lists nine studies since 1999 that have collected data from samples of newly-released prison and jail inmates. Most of these so-called reentry studies have sampled prison releasees and follow-up periods have generally extended from a month to two years. Most studies sampled from cities, although the large-scale Serious and Violent Offender Reentry Initiative conducted interviews in 14 states. Sample sizes in reentry studies are typically relatively small, in the hundreds. The challenges of study participation are reflected in the retention rates at exit interviews. In the earliest study, the Vera Institute's First Month Out, only around half of a sample of prison and jail inmates released in New York City were re-interviewed after 30 days. Most reentry studies record retention rates between 50 and 70 percent. The Michigan Reentry Study provides an important exception, retaining 86 percent of a small sample of parolees over a two-year follow-up period.

The final study in Table 1, the Boston Reentry Study, forms the focus of this paper. Following a sample of Massachusetts state prisoners over a

Table 1. Retention rates of longitudinal studies of released prisoners.

	Year(s)	Site	Sample Size	Follow-Up (months)	Retained at Exit (%)
1. First Month Out	1999	New York	88	1	56.0
2. RH Maryland	2001–03	Baltimore	324	6	32.1
3. RH Illinois	2002–03	Chicago	400	16	49.5
4. RH Ohio	2002–03	Cleveland	424	12	69.0
5. RH Texas	2004–05	Houston	676	8–10	55.9
6. SVORI	2004–07	14 states	2391	15	68.5
7. Michigan Reentry	2007–09	SE Michigan	22	24	86.4
8. Smartphone Project	2012–13	Newark	152	3	70.0
9. Boston Reentry Study	2012–14	Boston	122	12	91.0

Note: RH=Returning Home; SVORI=Serious and Violent Offender Reentry Initiative. References are as follows: (1) Nelson et al. (1999), (2) Visher et al. (2004), (3) Kachnowski (2005), (4) Visher and Courtney (2007), (5) LaVigne et al. (2009), (6) Lattimore and Steffey (2009), (7) Harding et al. (2014), (8) Sugie (2014). First Month Out interviewed prison and jail releasees. Returning Home and the Michigan Reentry Study interviewed former prisoners. SVORI interviewed former adult prisoners and juveniles. The Newark Smartphone Project interviewed parolees.

Table 2. Study participation across five waves of the Boston Reentry Study.

	Number of Interviews	Response Rate (%)	Respondents Attritted (N)	Median Days from Release to Interview	IQR of Days to Interview
Baseline	122	100.0	–	−8	11
1 week	117	95.9	0	7	3
2 month	113	92.6	2	64	8
6 month	113	92.6	4	186	17
12 month	111	91.0	5	373	29
Total (N)	576	94.4	11	–	–

Note: Survey nonresponse includes all those who could not be contacted or scheduled for a follow-up interview plus those unreachable through incarceration or hospitalization as a percentage of those eligible to be interviewed. Attrition in a given wave is defined as missing the current interview and all subsequent interviews. The two-month interview count includes one respondent who was administered a re-incarceration interview in prison. The six-month interview count includes six respondents who were given re-incarceration interviews in prison.

year after prison release, the BRS achieved a retention rate of 91 percent. The study consisted of a baseline interview one week before prison release, a follow-up interview one week after release, then further interviews at two months, six months, and twelve months after release. With a baseline sample size of $N = 122$, and 5 scheduled interviews for each respondent, the initial design included 610 interviews. A total of 576 interviews were completed yielding an overall response rate of 94.4 percent over a year of follow-up.

Table 2 provides more detail about the pattern of nonresponse in the BRS. Nonresponse increased over time as 5 out of 122 respondents were missed at the first follow-up survey and 11 respondents were missed at the 12-month exit interview. Only a minority of nonrespondents were dropouts in the sense of being permanently lost to follow up. Although the study sustained a high response rate, data on the days to interview suggest the increasing difficulty of completing interviews. The median days to interview shows that half the sample were interviewed on the scheduled follow-up day or earlier. Latecomers, at the 75th percentile of the days-to-interview distribution, got progressively later even though nonresponse did not markedly increase after the two month follow-up. The full distribution of the days to interview for each survey wave is shown in Figure 1. By the 12-month interview, a quarter of the sample were interviewed at least a month later, and 10 percent of the sample were interviewed at least ten weeks late. Groves and Couper (1998) distinguish between nonrespondents and "reluctant respondents" who are either difficult to contact or hesitant to consent to an interview. We can think of the latecomers, in the final quartile of the time-to-interview distribution, as reluctant respondents who are at risk of nonresponse without additional efforts at study retention. Latecomers and nonrespondents consist mostly of those who were who were just intermittently missing or late for 1 or 2 waves (85 percent) and a smaller number who were persistently late or missing in at least 3 of the 4 waves (15 percent).

3 Study Retention in a Cohort of Prison Releasees

The BRS was designed to collect data intensively in the early stages of prison release. Respondents were recruited to the study with an information sheet distributed by prison staff to prisoners who were approaching release and planned to live in Boston. Respondents who were interested to participate were scheduled for an interview about a week before release. There appears to be little nonrandom selection on observable characteris-

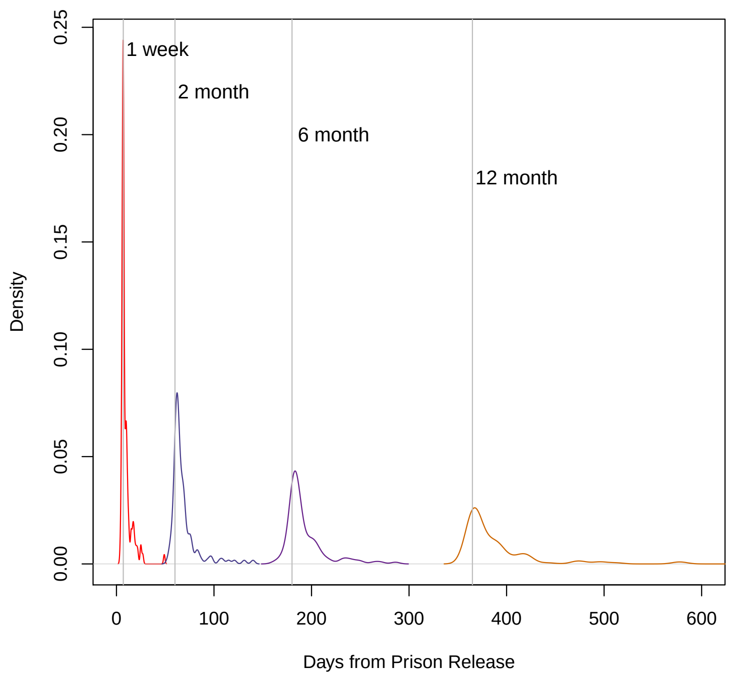

Figure 1. Density plot showing time to interview from prison release. Interviews were scheduled for one week, two months, 6 months and 12 months after prison release, Boston Reentry Study ($N = 122$).

tics into the study sample. The offense profile, criminal history, level of risk and demographic characteristics of the BRS sample are similar to those for the Boston-area reentry population as a whole. After release, rates of rearrest and reincarceration in the BRS sample are also similar to those for the whole Boston reentry population (Western et al. 2015). Study recruitment varies less across respondent characteristics than across correctional facilities where implementation of the research protocol varied significantly. In some facilities, prison staff showed a keen interest in the research and were active in their efforts to distribute information about the study. The BRS respondents represent a one-quarter sample returning to the Boston area from Massachusetts state prisons.

Following recruitment, the research design consisted of five face-to-face interviews over a one-year follow up period. The baseline interview was conducted in prison by a university researcher with a staff member from the research unit at the Department of Correction (DOC). At the baseline interview, contact information was obtained for the first follow-up interview two weeks later in the community. All follow-up interviews were conducted face-to-face by university research staff who worked in pairs. If a respondent was re-incarcerated, follow-up face-to-face interviews were conducted inside correctional facilities.

3.1 Contact Insecurity and Social Insecurity

How did the BRS sustain such a high response rate? Characteristics of the BRS sample, as in other surveys of released prisoners, place them at high risk of interview nonresponse (Table 3). The sample is entirely urban, living in and around Boston, mostly male, African American and Hispanic with very low levels of schooling. In addition, a large proportion of the

Table 3. Means and percentage distribution for selected characteristics of Boston Reentry Study respondents ($N = 122$).

Demographics	
Average age (years)	36.5
Female	12.3
Black	50.1
Hispanic	18.9
HS dropout	59.8
Baseline characteristics	
Substance abuse history	54.1
Mental illness history	44.3
Average time in prison (months)	32.4
Unemployed before arrest	41.0
Unstable housing before arrest	19.7
Post-release characteristics	
No probation/parole after release	38.5
Incarcerated after release	22.1
Unstable housing after release	62.0

respondents have histories of mental illness or heavy drug use, and had previously lived in unstable housing, either dividing time between multiple residences, living on the streets, or in shelters or other group quarters. In contrast to most prison reentry studies, BRS respondents were not recruited from probation or parole agencies and over a third of the sample was released to the street with a completed sentence and no post-release supervision. After release, nearly a quarter of the sample returned to incarceration in the one-year follow-up period and nearly two-thirds lived in some kind of unstable housing. In short, like most newly-released prisoners, the BRS respondents were socially unstable and often difficult to contact for follow-up interviews.

A direct sense of the difficulties of follow up is given in Table 4. As we detail below, respondents' cell phones were the main means of contact for check-ins and scheduling interviews. At each wave of the survey, 5 to 7 per-

Table 4. Change in phone contact, residence, or criminal justice status at 1 week, 2 months, 6 months, and 12 months after prison release, Boston Reentry Study (N=122).

	Release to 1 week	1 week to 2 months	2 months 6 months	6 months to 12 months
No phone (excl. incarcerated)	5.0	6.7	4.5	17.3
Changed phone	56.8	44.8	42.4	52.3
Unstable/unknown residence	39.3	38.5	44.3	53.3
Changed residence	40.2	34.7	49.1	57.9
New charge/arraignment	0.0	5.7	9.8	27.9
Entered prison or jail	0.8	0.8	7.4	13.9

Note: Unstable residence includes staying in multiple residences, treatment programs, transitional housing, shelters, correctional facilities, and homeless or on the street. Data on new charges and prison or jail stays is drawn from administrative records and thus is complete for the whole sample.

cent of respondents did not have working phones and 40 to 50 percent of respondents changed phones between survey waves. In addition to being hard to reach by phone, 40 to 60 percent of respondents frequently changed residence and around half were living in unknown or unstable residences. Criminal justice involvement may also contribute to nonresponse for respondents with outstanding warrants or who are detained while awaiting trial. The level of criminal justice involvement increased over time. In the final six months of the follow-up period, over a quarter of the BRS sample were arrested on new charges and 14 percent were re-incarcerated. When contact information—chiefly phone numbers and residential addresses—is intermittently unavailable or changes frequently, respondents exhibit a high level of *contact insecurity* that makes survey nonresponse more likely.

3.2 Strategies for Study Retention

In this challenging setting for study retention, a variety of strategies were adopted to reduce nonresponse. In a context of extreme contact insecurity, we collected an extensive array of supplementary contact information, maintained a high level of contact between interviews, and tried to increase the willingness of respondents to participate. Four specific features of the BRS research design were designed to maintain study retention:

1. Interview incentives. BRS respondents were paid $50 for each interview. Incentives were previously found to increase participation among low-income respondents and parolees (Martin et al. 2001; Harding et al. 2014). Following Harding and his colleagues (2014), BRS respondents were paid in cash at the conclusion of an interview (Harding et al.). In the case of re-incarceration, research staff deposited $50 into the respondent's prison commissary account. Besides the cash incentives, interviewers were trained to present a strongly nonjudgmental posture in interactions with respondents. Respondents reported that this approach contrasted with official interactions with criminal justice authorities, and that the attitude of interviewers provided an additional incentive for participation.

2. Phone check-ins and letters. Research staff also conducted regular phone check-ins with study respondents. Between the baseline, 1-week, 2-month, and 6-month interviews, interviewers phoned respondents every few weeks. Between the 6-month and the 12 month waves, interviewers checked in by phone each month. Phone check-ins were used to update the respondents' residential information and to maintain contact and rapport. Phone contact involved both conversations and texting. Where phone contact was lost, we emailed or wrote letters to the respondents, often using address information previously provided by family members and friends.

A strongly nonjudgmental approach in interviews combined with frequent phone check-ins were intended to build rapport and trust between interviewers and respondents.

3. Proxy interviews and secondary contacts. Proxy interviews were conducted with close family and friends who could also help us locate hard-to-find respondents. In the Fragile Families Study of Child Well-Being, mother-father couples were interviewed and proxy responses by mothers compensated for a high rate of nonresponse among formerly-incarcerated fathers (Lopoo and Western 2005). We elaborated this approach by obtaining at the baseline interview a list of secondary contacts to be used to help locate respondents after prison release. The contact list was updated at each subsequent interview. Because we expected proxy respondents to provide useful information about the respondent's family contacts and well-being, we also aimed to conduct at least one substantive interview with a family member or close friend for each focal respondent. We completed 81 proxy interviews.

4. Justice agencies and community contacts. When conventional retention strategies were exhausted, a variety of justice agencies and community partners helped re-establish and maintain contact with the study subjects. The DOC provided weekly updates on the criminal justice status of all respondents and monitored whether respondents returned to state custody. For subjects under criminal justice supervision in the community, the Massachusetts probation and parole agencies—and in a few cases, the Boston Police Department—helped locate subjects for interviews. Parolees have been the focus of earlier reentry studies and below we study whether parole/probation status is associated with study retention. For those who were not on probation or parole supervision, we tried to reestablish contact through a variety of street and community workers operating in inner-city neighborhoods where study respondents resided.

In addition to the four main strategies for study retention, survey records were also linked to administrative records on arrests, charges, sentencing, and incarceration. Although not a strategy for study retention, record linkage yields a completely-observed record of criminal justice involvement for all respondents for the entire follow-up period. Besides providing a complete criminal justice history for the sample, administrative records help us to explore nonrandom survey nonresponse. Differences in arrest or incarceration rates between respondents and nonrespondents may suggest nonrandom differences for survey variables for, say, self-reported crime, drug use, or illegal income.

3.3 Day-to-Day Operations

In the day-to-day operation of the reentry study, the retention strategies were applied continuously and concurrently, so much of the work of the data collection revolved around maximizing study participation. Interview incentives were particularly important for study participation immediately after prison release. At the first meeting in the community, respondents received a total of $100, $50 for the baseline interview in prison and $50 for the one-week interview. One hundred dollars was a significant payment for people with little income, particularly for one fifth of the sample who were released from prison with less than $100. The last question for each post-release survey asked respondents why they were participating in the study. A quarter of the sample cited the interview payment as a strong incentive at the one-week interview. The interview incentive appeared to be less crucial to study retention over time. By the 12-month interview, about 10 percent of respondents indicated they were participating for the money.

Interviews were conducted by a pair of researchers and one member of the pair was charged with maintaining consistent contact with the respondent for the duration of the study. Consistent contact—reinforced by a non-judgmental approach to interviews and regular phone check-ins—fostered trust and rapport between researchers and study participants. Reflecting the quality of the interviewer-respondent relationship, around one-third of the sample reported they were participating in the research because they were committed to completing the study or because they liked the interviewer. An additional 20 percent said it was helpful for them to be able to share their experiences after incarceration. At each post-release interview, nearly half of the sample reported that they hoped their participation would ultimately help others or have an impact on criminal justice policy.

For most respondents, phone calls were our primary mode of contact. While the majority of respondents provided the number of a family home or program at the baseline interview, half of the sample obtained a cell phone within seven days of release. Respondents often informed researchers of their new number when we called to schedule the one-week interview. After the first week out, 23 out of 122 respondents maintained the same phone number throughout the year out of prison. These 23 respondents with a high level of contact security completed all post-release interviews and were less likely to be scheduled late than others in the sample.

More commonly, phone status was unstable. Respondents changed their phone number eight times on average over the course of the study. In some cases, people's primary phones would not work if phone bills went unpaid or if residential status changed, such as when they left a program or returned to prison or jail. Others obtained new phones when they started working or changed their phone numbers every few months. Some reported that regularly changing phones was a habit picked up when they were involved in illegal activity and feared their phones were being tracked. In addition, a number of phones went out of use when they were lost or stolen, particularly in the chaotic environments of homeless shelters and transitional housing. When respondents were unreachable by phone, proxy contacts were helpful for maintaining contact. Several mothers with stable home or cell phone numbers would provide us with updated contact information for their sons and daughters every few months when their phones went out of service.

In a small number of cases, phone check-ins and secondary contacts were insufficient to maintain study participation, and criminal justice agencies and community contacts also helped to follow-up with study respondents. Criminal justice queries often took the form of a request to probation, parole, or the police for a phone number or address for hardest-to-reach respondents. Respondents had earlier consented to such contact and the queries were designed to be unobtrusive both for the agencies and the respondents.

Community contacts were also invaluable for reaching the most elusive respondents. In particular, streetworkers (or gang outreach workers) were especially helpful for reestablishing contact with younger respondents who were associated with gangs or other street groups. Because gangs are a source of connection to social networks and neighborhoods (Papachristos et al. 2013), and because Boston's streetworkers are assigned to neighborhoods and gangs, streetworkers were well positioned to assist in connecting with individuals with gang and neighborhood ties. In some cases, streetworkers were able to provide a direct link—through a working phone number or current address—for a study respondent that had gone missing. More often, however, streetworkers "knew someone who knew someone," and connected the research team to a third party (say an ex-girlfriend, neighborhood acquaintance, or a former youth worker) that could arrange contact with a missing respondent. When subjects dropped out, community contacts also offered explanations for nonresponse. For example, we learned that one subject had developed extreme paranoia about being "set up" following a shooting in his area in between survey waves and was reluctant to meet or talk with all but a small group of trusted friends. Mistrustful and occupied with the challenges of survival, participation in the study was not something he could arrange.

Consistent with the nonignorable risk of nonresponse, contact insecurity was often linked to insecurity in daily life. In these cases, different strategies for study retention were useful at different points throughout follow-up. Omar, a Puerto Rican man in his mid-forties, changed his phone number 17 times over the course of the study. Omar had been a regular heroin user for much of his life. At 16 years old, he suffered brain damage after a car accident and reported it had been difficult to function without drugs since that time. His physical and mental health problems compromised his ability to maintain a steady phone number and to remember dates of medical appointments and study interviews. Still, Omar was eager to participate and on days when he was clear-headed and had a working cell phone, he would sometimes call to check in.

For the first three months after incarceration, Omar resided at several addresses and was sometimes homeless on the streets. He soon rekindled a relationship with a former girlfriend, Jessie, who was just released from prison herself. Jessie had been a heavy drug user but was in better health and assisted Omar with attending appointments, including interviews. She stayed with him on the streets and moved with Omar into a friend's apartment four months after his release. When Omar's cell phone was out of service, we would often call Jessie, and she would hand her phone to him so that we could check in.

Omar was sentenced to probation upon release but never reported to his probation officer, he told us, due to his drug use. Just before his six-month interview, he received a warrant for not reporting. Though Omar mentioned his trouble with probation during interviews and phone calls, we received further information from our contacts at the Department of Correction. Omar's warrant was dropped on the condition that he complete a residential drug treatment program. However, when we called the program where he was assigned, a staff member reported he was no longer there. We had trouble reaching Omar to schedule the 12-month interview, perhaps because his legal status was compromised and he was no longer living with Jessie.

When Omar and Jessie were both out of contact over the course of the year, we would call Omar's sister Lena, an older woman in her late fifties. Lena was 16 years old when the family moved from Puerto Rico to Boston and was more comfortable communicating with Spanish-speaking members of the research staff. For nearly 15 years, she had maintained a public housing unit in Boston's inner city. Lena was the most stable person in Omar's life, and he used her residence as his mailing address. Lena would get updated contact information from him every so often when he stopped by her place for meals. After we learned that Omar had left his treatment program, we contacted Lena who gave us his new cell phone number. Two days later, we conducted Omar's 12-month interview. Though the interview took place two and a half months late, it provided valuable information about Omar's housing insecurity, drug use, and probation late in the year after incarceration.

Between the 6- and 12-month interviews, we completed five phone check-ins with Omar, failed to reach him on 18 call attempts, spoke to his sister or girlfriend five times, and received information on his legal and residential status from local criminal justice agencies. His case points to the necessity of a range of redundant strategies to retain vulnerable study participants and the high risk of nonresponse for people who are struggling with housing insecurity, drug relapse, mental illness, and criminal involvement.

Efforts at sample retention are summarized in Table 5. At each wave of the survey, the distribution of retention effort is given for subjects who are interviewed on time, interviewed late (at the 75th percentile of the days-to-interview distribution or later), and who missed an interview. Successful contacts include the average number of successfully completed phone calls, text, or email exchanges. Respondents who completed late or who missed interviews received a much higher number of unsuccessful contact efforts. By the final six months of the survey, late respondents and non-respondents were receiving two to three times more unsuccessful contact efforts than successful. The proxies of these hard-to-observe respondents also received many more calls than the proxies of respondents who were interviewed on time. Finally, justice system queries, although relatively few in number, were almost entirely focused on the late and missing respondents. In sum, the late and missing respondents received disproportionate retention effort. Late respondents received twice as many phone call con-

Table 5. Average level of retention effort between survey waves for on-time interviews, late interviews, and missed interviews.

Interview Status	Successful Contacts	Failed Contacts	Calls to Proxies	Letters Sent	Justice System Query	*N*
Baseline to One Week						
On time completion	1.48	.63	.68	.03	.00	90
Late completion	2.00	1.30	1.11	.19	.04	27
Missed	1.00	1.40	.60	.40	.60	5
One Week to Two Months						
On time completion	3.31	.47	.15	.00	.00	85
Late completion	2.65	1.38	.27	.15	.04	28
Missed	2.50	7.50	1.83	.83	.33	9
Two Months to Six Months						
On time completion	3.55	1.28	.17	.00	.04	83
Late completion	2.80	1.63	.27	.10	.27	30
Missed	.75	6.25	1.75	1.50	1.00	9
Six Months to Twelve Month						
On time completion	3.77	3.10	.45	.05	.15	86
Late completion	2.92	7.24	2.04	.76	.80	25
Missed	1.80	8.60	1.20	.40	.60	11
Total (*N*)	1,331	890	250	53	58	122

tacts than those who were interviewed on time, and the most intensive retention efforts—through secondary contacts, letters, and justice system queries—were focused on the nonrespondents.

3.4 A Model of Reluctance and Nonresponse

We can link respondent characteristics to nonresponse across the whole sample with a multinomial logit regression that takes late and missing interviews as response categories. Completely observed covariate characteristics can be taken from the baseline interview and from linked administrative data on arrests and incarceration. Such a model allows us to test two hypotheses. First, including lagged dummy variables for late and missed interviews indicate respondents with an enduring propensity to nonresponse. Lagged measures also provide estimates of the relationship between lateness and nonresponse indicating whether late respondents are at high risk of nonresponse. Second, we can also assess the association of lateness and nonresponse to measures of extreme disadvantage that are closely related to contact insecurity. These markers of social insecurity include histories of mental illness, addiction, and marginal housing, all measured at baseline. Contact insecurity is also closely related to time-varying measures of new charges and re-incarceration. In addition to these measures, the analysis also controls for age, sex, and race and ethnicity. Finally, to study the risk of nonresponse for those who have no correctional supervision after prison release, a dummy variable is included for those on probation or parole during follow-up.

More formally, for respondent i in wave t, we can write the probability of being late or missing, p_j ($j = L, M$) compared to being interviewed on

Table 6. Multinomial logit regression results in an analysis of late and missed interviews, compared to on-time interviews, 3 panels of the Boston Reentry Study.

	Model 1		Model 2	
	Late	Missing	Late	Missing
Intercept	−.356	−2.179**	−.675	−2.667**
	(.66)	(2.80)	(1.17)	(3.21)
Lag late interview	.474	1.213*	.495	1.270*
	(1.35)	(2.52)	(1.37)	(2.49)
Lag missed interview	1.880**	3.470**	1.858**	3.647**
	(3.30)	(5.95)	(3.30)	(6.40)
Age	−.048**	−.031	−.047**	−.025
	(2.94)	(1.50)	(2.76)	(1.12)
Female	−.056	−1.642	−.122	−1.673*
	(.16)	(1.78)	(.32)	(2.05)
Addiction	.420	1.067*	.415	.919*
	(1.29)	(2.53)	(1.20)	(1.96)
Mental illness	.773*	.0554	.850*	.0172
	(2.51)	(.13)	(2.54)	(.04)
Prior unstable housing	−.863*	−.818	−.823*	−.926
	(2.38)	(1.59)	(2.09)	(1.60)
No supervision	.253	.326	.239	.529
	(.85)	(.72)	(.75)	(1.14)
New charge			.971*	−.144
			(2.50)	(.17)
New Incarceration			1.122	2.581*
			(1.91)	(2.38)
Log likelihood	-250.246		-237.502	
Respondent-waves (N)	366		366	

$^{*}p < .05$ $^{**}p < .01$

Note: Numbers in parentheses are z statistics. Late respondents are those in the top quartile of the days-to-interview distribution. Standard errors have been adjusted for clustering. One survey wave is omitted to allow lagged measures of interview status.

time, p_O,

$$\log\left(\frac{p_{jit}}{p_{Oit}}\right) = \alpha_{1j} l_{it-1} + \alpha_{2j} m_{it-1} + \boldsymbol{x}_i' \boldsymbol{\beta}_{1j} + \boldsymbol{z}_{it}' \boldsymbol{\beta}_{2j},$$

where dummy variables indicating lagged interview status are included for respondents who were late ($l_{it-1} = 1$) or missing ($m_{it-1} = 1$) in the prior wave, $\boldsymbol{x}_i$ is a vector of time-invariant covariates, and $\boldsymbol{z}_{it}$ is a vector of time-varying covariates. (With lagged predictors, analysis is confined to the last three waves of the survey at $t = 2$ months, 6 months, and 12 months.) Note that the multinomial logit model yields two sets of coefficients, $j = L, M$, one showing the odds of late respondents relative to those who were on time, and another showing the odds of missing respondents relative to those who were on time.

Multinomial logit results are reported in Table 6. We fit one model that includes lagged interview status and baseline characteristics, and another model that adds the effects of new charges and incarceration (Table 6). Across both models, respondents who were late or missing in a given wave were highly likely to be missing at the following wave. This suggests that the propensity to nonresponse is in part an enduring trait that is observable from wave to wave. Difficulty in scheduling an interview, resulting in lateness, is also associated with the risk of nonresponse. The underlying risk of nonresponse is related to the baseline traits of a history of drug addiction, mental illness, and a history of unstable housing. Unexpectedly, living in unstable housing prior to incarceration is asociated with a relatively low risk of nonresponse, when other covariates are controlled. Unusually, the BRS sample also includes a relatively large number of people who have completed sentences and are not supervised by probation or parole. Coefficients for no supervision are relatively small and insignificant in both models indicating that respondents who have “maxed out” (completed their maximum sentence) face no higher risk of nonresponse than the rest of the sample. This result underlines the utility of the sample retention strategies that do not directly involve assistance from community corrections agencies. Estimates from the final model show that the risk of nonresponse—either being late for an interview or missing—are also associated with new charges and reincarceration. The odds of being late for an interview are more than doubled by a new arrest. Respondents who return to prison or jail are also at very high risk of a noninterview. Although the relative risk of a noninterview is very high, overall nonresponse rates in the BRS were very low. Still, the positive association of new charges and incarceration with the risk of nonresponse suggests that, in a sample with less retention effort and a lower response rate, survey nonresponse may be strongly nonignorable.

4 Consequences of Reluctance and Nonresponse

To study the effect of survey nonresponse on sample statistics, we can construct a measure of the risk of nonresponse and examine the sensitivity of sample quantities to different hypothetical levels of missing data.

An index for the risk of nonresponse can be constructed from data on late interviews and actual missed interviews. Late interviews are completed with additional retention effort, so treating late interview subjects as potential nonrespondents yields a sample that would have been observed with less retention effort. We construct a summary index of respondent's risk of nonresponse using information on the days late for a scheduled interview and nonresponse. Because the days late varies across waves, we transform the measures to percentiles, P_{it} within each wave and assign nonrespondents to the highest percentile. A simple average across the four waves of the survey yields a measure of the risk of nonresponse, $\bar{P}_i$, for each respondent. High values of $\bar{P}_i$ indicate respondents who were consistently very late or who missed follow-up interviews. Low scores on $\bar{P}_i$ indicate respondents who completed all interviews on time and required relatively little retention effort. Following the multinomial logit results on lagged effects, the risk of nonresponse, $\bar{P}_i$, is treated as a stable trait. Still, nonresponse risk also has a time-varying component. For example, respondents who relapse to addiction at some point in the follow-up period may be more likely to miss an interview compared to respondents with histories of addiction who do not resume drug use. Sensitivity of sample statistics to the risk of nonresponse is thus likely to be under-estimated by $\bar{P}_i$.

Figure 2 reports the means of key variables for respondents grouped in the lower, middle and upper terciles of the risk of nonresponse, $\bar{P}_i$. Respondents who are at low risk of nonresponse are in the bottom tercile of the distribution of $\bar{P}_i$. Medium risk respondents are in the middle tercile and high-risk respondents are in the top tercile. All variables except post-incarceration employment are completely observed, either recorded at the baseline interview or from criminal justice administrative records. Respondents at high or medium risk of nonresponse reported high levels of drug addiction, high school dropout, arrest, and incarceration compared to those at low risk of nonresponse.

Mean differences in key variables across levels of the risk of nonresponse may be associated with nonignorable nonresponse that drives sample selection bias in regression. Often in longitudinal studies with hard-to-reach populations, regression analysis focuses on the effects of baseline characteristics on outcomes at follow-up. We pool together the four follow-

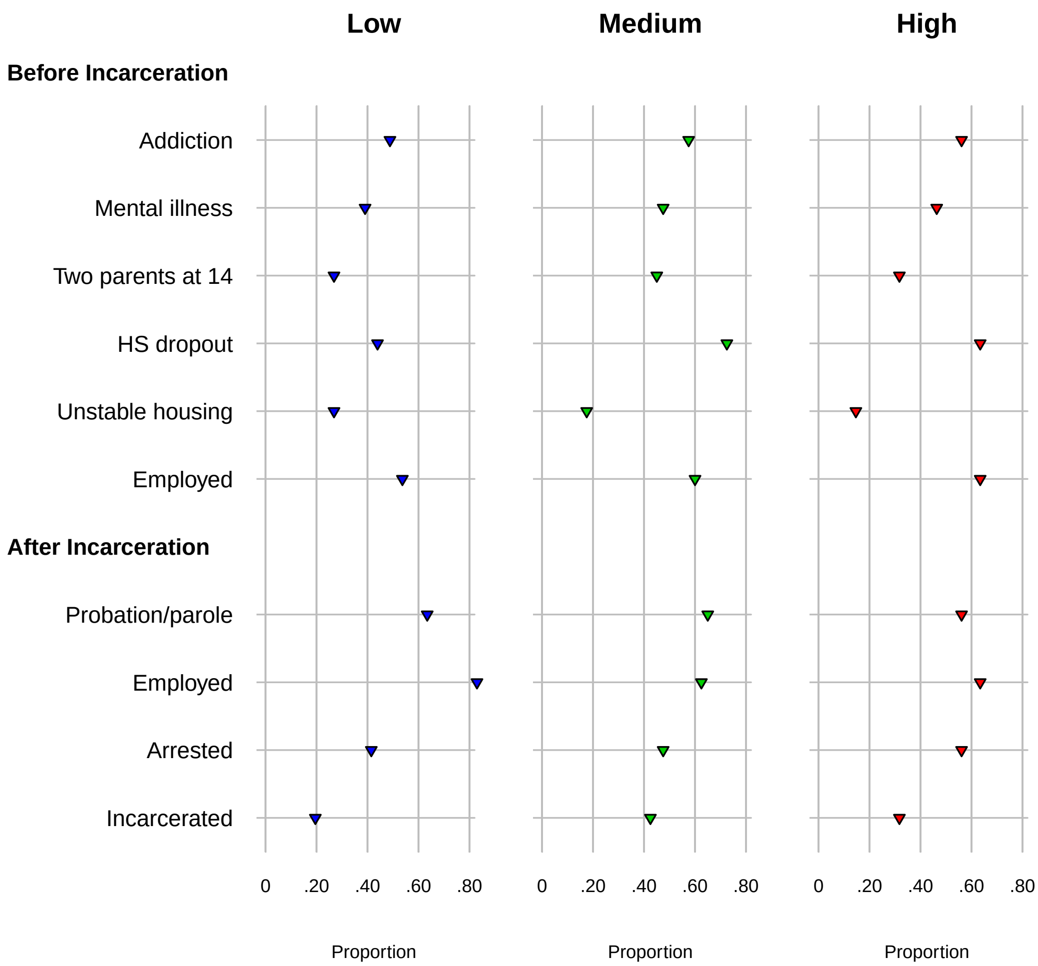

Figure 2. Means of key pre-incarceration and post-incarceration variables by low, medium, and high risk of nonresponse, Boston Reentry Study.

up waves from the BRS and fit linear probability models to the pooled data using as dependent variables measures of post-incarceration unstable housing, drug use, and re-incarceration. Regression analysis focuses on the coefficients for a history of drug addiction, unstable housing prior to arrest, and probation and parole supervision after release. In addition, regressions control for the wave of the survey, age, sex, and race and ethnicity.

To study the sensitivity of regression coefficients to different levels of nonresponse, we conduct a simulation experiment analyzing subsets of the data where observations are included in the analysis conditional on the nonresponse risk, $\bar{P}_i$. As the average level of percentiles from four follow-up surveys, the nonresponse risk, $\bar{P}_i$, varies from 10 to 97 with a mean of 50. We then define a nonresponse threshold, T, that is used to drop all observations from respondent i for $\bar{P}_i > T$. With the reduced data set, we estimate new regression coefficients, β_T. These coefficients can be interpreted as the estimates that would have been calculated with a response rate at a level of retention effort at threshold T. Allowing the retention effort to vary from $T = 97, 96, \ldots, 50$ we can examine how coefficients change as nonresponse increases. With T varying over this range, the simulated nonresponse rate varies from 0 to 45 percent, approximately the range of nonresponse reported in recent longitudinal prisoner reentry studies. With nonignorable nonresponse and the corresponding sample selection bias, we expect coefficients to get larger in absolute value as nonresponse declines. Statistical precision will also increase with larger sample sizes, so confidence intervals will also shrink as nonresponse declines.

Figure 3 reports results from the simulation experiment. Results for each dependent variable—housing instability, drug use, and re-incarceration—are shown down the columns. The coefficients—for pre-arrest addiction, pre-arrest unstable housing, and probation or parole status—are shown across the rows. Hypothetical nonresponse rates are reported on the horizontal axis and coefficients and confidence intervals are shown on the vertical axis. In the model of housing instability, the effect of pre-arrest unstable housing and probation/parole behave consistently with classical sample selection bias in which effects are significantly attenuated with high rates of nonresponse. Neither effect would be detectable with a 55 percent response rate, but both effects become large and significant as the response rate approaches 100 percent. For post-incarceration drug use, the coefficient for pre-arrest addiction also becomes larger with the rising response rate. The probation/parole coefficient gets slightly smaller, but the confidence interval shrinks to statistical significance as sample size gets larger. In the models for reincarceration, the coefficients for addiction, unstable housing and probation/parole also get larger as sample selectivity declines and sample size increases. These results indicate how high rates of nonresponse in hard to reach populations contribute to sample selection bias, driving null effects in the analysis of social disadvantage and vulnerability.

5 Discussion

This paper explores study retention and the consequences of survey nonresponse in a highly marginal population. Surveying a sample of released state prisoners returning to neighborhoods in Boston, we describe a one-year follow-up study involving a group of men and women with histories of drug addiction, mental illness, and housing insecurity. The Boston Reentry Study maintained a high response rate, over 90 percent, that provides an opportunity to explore strategies for study retention in hard-to-reach pop-

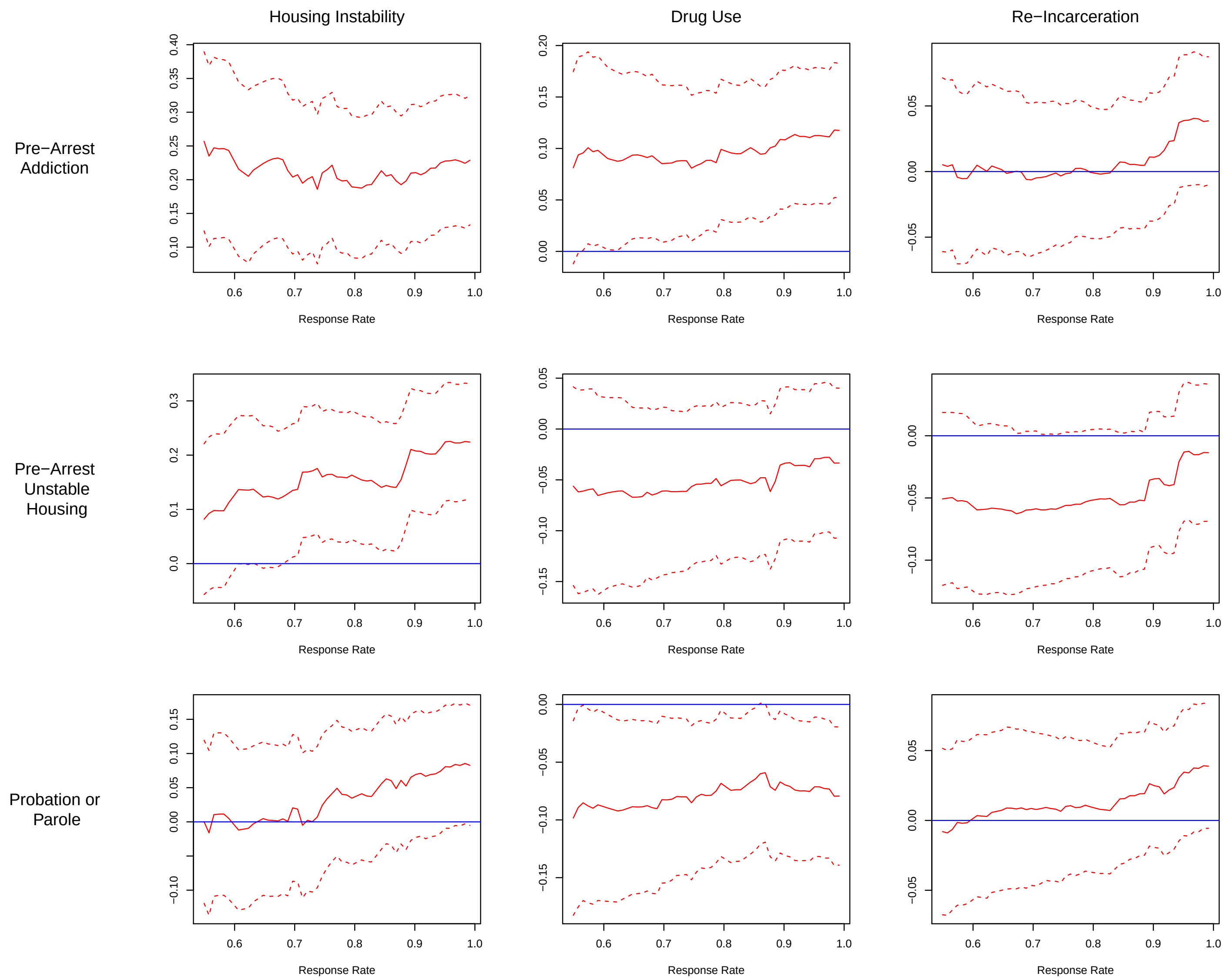

Figure 3. Coefficients for pre-arrest addiction, pre-arrest unstable housing, and probation or parole status in panel regressions on post-incarceration housing instability, drug use, and re-incarceration. Coefficients vary with the response rate sorting sample respondents from low to high risk of nonresponse. (Dashed lines indicate 95 percent confidence intervals. A zero coefficient is marked by the solid horizontal line.)

ulations as well as examine the effects of survey nonresponse that has been common in prior research.

Three main conclusions can be drawn from the analysis of study retention in the BRS. First, formerly-incarcerated men and women show a very high level of contact insecurity. Most of the BRS respondents experienced significant residential instability, were weakly attached to traditional households, changed phones frequently, were periodically out of phone contact, and in some cases returned to incarceration. These respondents would be overlooked in conventional household surveys and we would expect very high rates of attrition without special measures for study retention. High rates of contact insecurity help explain the high rates of nonresponse reported in prior studies of prisoner reentry.

Second, the challenges to study retention in hard-to-reach populations can be successfully addressed through research design. Successful retention depends on a variety of redundant strategies for minimizing attrition in a highly unstable sample facing extreme contact insecurity. Key strategies in the Boston Reentry Study included significant interview incentives, a nonjudgmental approach to interviews along with frequent phone check-ins that helped build rapport, lists of proxy contacts, and in the last resort, justice system and community streetworker queries. These measures provided strong incentives for participation and helped quickly find those whose contact information was lost.

Third, contact insecurity and the risk of survey nonresponse were closely linked to the social risks and vulnerabilities of sociological interest. Thus respondents at high risk of nonresponse had histories of drug addiction and mental illness, and were also more likely to be arrested and incarcerated after prison release. Because the risk of a noninterview is closely linked to the extreme social disadvantages of substantive interest, nonresponse in such a socially marginal population is strongly nonignorable. In the Boston Reentry Study we found that regression estimates were highly sensitive to survey nonresponse in analyses of housing insecurity, drug use, and re-incarceration. Regression estimates were greatly attenuated at hypothetically high rates of nonresponse, around 35 to 45 percent.

Earlier studies of nonresponse in hard-to-reach populations report that significant resources are required to collect data from the most elusive respondents (Farrington et al. 1990; Teitler et al. 2003). Researchers have suggested that the benefits in data quality that result from a higher retention rate may not be worth the cost of tracking down those who are most difficult to contact (Teitler et al. 2003).

Our findings point to three additional considerations when evaluating the benefits of high study retention. First, while retaining the most elusive respondents does require significant effort, developing a culture within the research project that promotes rapport and connectedness with study subjects can improve retention at little extra financial cost. Second, the effort to sustain 100 percent retention likely improves the quality of interviews that are completed. Where interviews are conducted in a climate of trust with an interviewer that a respondent has come to know through numerous interactions, survey responses are likely to be more forthcoming and complete, particularly in sensitive areas. Third, as our field period progressed, 100 percent study retention became as much a humanistic as a methodological objective. A single missing observation out of nearly 600 interviews would barely increase bias or standard errors, but an opportunity would be missed to register the voice and experience of a respondent who has rarely been heard in science or policy. In very poor or socially marginal research sites, building trust and connection with respondents yields the benefits of accurate measurement and a high response rate, but also gives voice to those who are largely invisible.

More generally, attrition in panel studies and other forms of nonresponse represent a fundamental methodological challenge to the study of extreme poverty and other hardship. Research in this area often features large-scale data collections with household surveys or administrative data bases (e.g., see the reviews of Brooks-Gunn and Duncan 1997 and Haveman et al. 2015). With these data, the problems of under-enumeration and survey nonresponse are well-documented (Groves and Couper 1998). Specialized data collections focused on those who are missed by a household sampling frame or at risk of attrition in a panel study can usefully supplement the large-scale data collection. Counting those who are the most difficult to count holds the promise of observing the deepest disadvantage and the full extent of its social contours. In these very marginal social spaces—where nonresponse is strongly nonignorable—strategies for study retention are critical for reducing bias and accurately observing extreme material hardship and social insecurity.

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