

B. COLLECTION OF INFORMATION EMPLOYING STATISTICAL METHODS

B1. Respondent Universe and Sampling Methods

The target population for the survey is couples participating in the MFS-IP program. The couples are comprised of incarcerated men and their partners (who are not incarcerated). Questionnaires will be administered to the men and to their partners in separate interviews. All couples who enroll in couple-based services funded by the MFS-IP initiative at five selected program sites will be recruited to participate in the study. A cohort of comparison or control couples who are not participating in MFS-IP services will also be recruited at each of the five sites. Recruitment and baseline interview will take place during the period of incarceration. During the follow-up period some men may still be in prison, while others may have been released from incarceration. The service settings and target populations vary among the grantees, and the sites vary widely in terms of services delivered and service delivery approach (*Exhibit 3*). The broad set of program components planned among the set of grantees includes marriage education, marriage/family counseling, parenting education, enhanced visitation options, case management, education and employment services, support groups and cultural activities, financial literacy services, mentoring and coaching services, and domestic violence services. Brief program descriptions are included in **Appendix K**.

B.1.1 Site Selection

Data will be collected from program participants and control/comparison group members at a subset of five grantee sites: Shelby County Division of Correction (Memphis, TN), Minnesota Council on Crime and Justice (Minneapolis, MN), New Jersey Department of Corrections (Trenton, NJ), Osborne Association (Brooklyn, NY), and Indiana Department of Correction (Indianapolis, IN) (*Exhibit 3*). These five sites were selected based on six criteria.

- Each program had to include a couple-based relationship focus because the impact evaluation must achieve its goal of determining whether couple-based family strengthening programming has a positive impact on relationship quality, child well-being, and other outcomes.
- Program intensity was looked at because in order for a program to achieve any desirable outcomes, a reasonable level of program exposure/contact is necessary.
- Program enrollment was a key factor, as it is in any evaluability assessment. Sufficient numbers of couples receiving the treatment (as well as a sufficient number of “untreated” couples to serve as the comparison/control group) are an important prerequisite for acceptable statistical power for detecting actual treatment effects.
- Stage of implementation was a consideration because modifications to program design or delivery once the evaluation is underway are extremely undesirable. Therefore, it was necessary that programs have finalized plans for program delivery (and be ready to begin implementing their programs or have already begun implementation) in order to be selected for evaluation.
- Study design considerations were key. Specifically, we assessed whether each program was willing to randomly assign eligible couples to receive MFS-IP programming or “treatment as usual,” and if not, whether other possibilities for the identification of a comparison group existed at the site.

- Finally, the site's willingness to participate and staff capacity for participation were essential considerations because the impact evaluation cannot be conducted successfully without the cooperation of the participating sites.

Exhibit 3. Site Selection Criteria

Impact Study Sites	Target Population	Program Components	Rationale
NJ DOC	Targets men incarcerated at three state facilities who are either in a committed relationship with children or married, have six to nine months left to serve, have been identified as having an addiction problem, and are max-out offenders. Men may only participate if their partners agree to enroll.	Participants will receive case management, including visitation coordination, discharge planning, and substance abuse counseling; a 12-week marriage education and parenting curriculum (Married and Loving It); a post-release substance abuse treatment program; and referrals to support groups and marriage counseling.	<ul style="list-style-type: none"> ▪ Couple-based relationship services ▪ High intensity programming ▪ Acceptable enrollment ▪ Standardized programming ▪ <i>Other advantages:</i> intensive post-release case management
MN CCJ	Targets fathers admitted to St. Cloud state prison who are in a committed relationship, are from and returning to the Twin Cities area, and have a sentence of six months to three years. Men may only participate if their partners also enroll.	Program components, all of which are provided to both members of the couple, include the following: case management, parenting classes, relationship classes, financial literacy training, and employment referrals, training, and placement.	<ul style="list-style-type: none"> ▪ Couple-based relationship services ▪ High intensity programming ▪ Acceptable enrollment ▪ Couple-based relationship services ▪ <i>Other advantages:</i> program up and running (and recruiting very successfully), program begins at intake, intensive post-release case management, partner receives all program components
Shelby County DOC	Targets men incarcerated in the Adult Offender Center of Shelby County prison who have six to 12 months left to serve and are in a committed relationship, and their partners.	All program participants receive parenting education, employment skills training, moral reconnection therapy, and case management. Most participants receive a marriage strengthening course (PAIRS), money management education, family group conferencing, GED/higher education classes. The PAIRS component is optional for those men whose partners cannot attend; in the first cohort, about half of the men were participating in PAIRS with their partners. Child-friendly visitation is also available to interested participants who are fathers.	<ul style="list-style-type: none"> ▪ Couple-based relationship services ▪ High intensity programming ▪ Moderately high enrollment ▪ <i>Other advantages:</i> pre- and post-release components, marriage education delivered to couple jointly
Osborne Association	Targets incarcerated fathers at three New York DOCS facilities (Sing Sing, Fishkill, and Greenhaven) and their partners.	Those who enroll in the parenting class receive 16 weeks of classroom-based fatherhood training. Healthy Relationships class participants receive five or six weekly sessions. Those who take part in the couples PREP program receive an eight-hour class delivered during a single weekend.	<ul style="list-style-type: none"> ▪ Couple-based relationship services ▪ Moderate intensity programming ▪ Moderately high enrollment ▪ <i>Other advantages:</i> marriage education delivered to couple jointly

Impact Study Sites	Target Population	Program Components	Rationale
IN DOC	Targets men incarcerated at 13 state prisons who are participants in character/faith-based living units (PLUS) or general inmate population graduates of a parenting class, and their partners.	Couples retreat participants will receive eight hours of marriage education (PREP) delivered jointly to both members of the couple over a single weekend.	<ul style="list-style-type: none"> ▪ Couple-based relationship services ▪ Moderately high enrollment ▪ Standardized programming ▪ <i>Other advantages:</i> marriage education delivered to couple jointly, program up and running

Each of the selected sites has a strong couple-based relationship program, acceptable enrollment numbers, and strong possibilities for counterfactual strategies. Individually, they fulfill all key selection criteria and collectively they offer a diverse set of programs—one for which key questions can be answered regarding the effectiveness of couple-based relationship-strengthening services in general and specific program components that appear to be associated with greater effectiveness.

B.1.2 Site-Specific Study Designs

Within the five impact sites, negotiations are currently underway to finalize the most robust design for each site, particularly in terms of the counterfactual strategy. Several strategies have been discussed internally and externally, with external study design experts involved in the discussions. Proposed approaches for each site are detailed in ***Exhibit 4***. It will be essential to understand and document the counterfactual in each site, since it is unlikely that comparison and control group members will avoid services altogether. Service receipt by control and comparison group members will be documented as part of the implementation evaluation (see **B2**, below), and through the inclusion of a battery of items regarding service receipt in the survey instrument (see **Appendices A–D**). The specific strategies will be different in each site but the general approach is to apply the basic selection criteria used to identify program participants to individuals who are not eligible for the program for reasons such as 1) being incarcerated in a facility in which the program is not offered, 2) being from a community other than the community targeted by the program, or 3) having a release date outside of the parameters established by the program.

B.1.3 Selection of Respondents

Selection of subjects for the longitudinal interview component of the evaluation will be conducted with the assistance of each site participating in the impact evaluation. For “treatment” couples (i.e., those who have been officially enrolled in the MFS-IP programs), program staff will provide contact information to the study for the men and their partners. For “comparison” couples, program or agency research staff will assist in the identification of eligible subjects. If a surplus of eligible study members is identified for either the treatment or comparison group, we will either truncate the baseline enrollment period in the site or randomly sample from the eligible respondents.

Exhibit 4. Proposed Site-Specific Study Approaches and Projected Sample Sizes

Grantee	Proposed Approach	Projected Sample Size
NJ DOC	The program currently enrolls two cohorts per year, limited to inmates with projected release dates of six to nine months (at the time of recruitment) and who are eligible based on several additional characteristics. Random assignment is one possibility in this site, but such an approach would reduce the number of treatment couples from 270 over the three years to 135 each for the treatment and control group. Therefore, an alternative strategy being considered are inmates who meet all program eligibility criteria but who fall within the two annual recruitment cycles because their release dates are more than six to nine months at the first recruitment cycle and less than six months at the second recruitment cycle. This group is otherwise identical to the comparison group and the slight differences in overall duration of incarceration are not a large source of bias.	200 treatment and 200 comparison couples over the three year enrollment period
MN CCJ	The program currently enrolls inmates from the seven county Twin Cities area into their program (with several other eligibility criterion applied). After eligible men are screened, partners are recruited and then the couple is enrolled. Random assignment at the individual level (i.e., prior to the recruitment of partners) is one possibility at this site (random assignment after the recruitment of partners is undesirable from the site’s perspective due to the ethical issues involved in building partner’s trust only to be not selected for participation), but such an approach would not yield equivalent treatment and comparison groups because some “treatment” group members would end up not actually enrolled in the program (because the partner may end up not consenting to enroll). Therefore, an alternative strategy being considered is having the program refine its geographical eligibility criteria such that inmates from three counties are eligible for the program and inmates from the remaining four counties are eligible for the evaluation study only. Recruitment procedures (and all other eligibility criteria) would be identical for the two groups.	180 treatment and 180 comparison couples over the three year enrollment period
IN DOC	The program serves residents of a specialized housing unit and general population inmates who have completed a parenting class, across 13 state prisons. In order to be eligible for the PREP couples retreat (the component being evaluated), men must be in a committed relationship (with a partner who is willing to attend the retreat). Because the retreat is offered at each facility only twice a year, a possible comparison group strategy is men who want to participate in the retreat but who are released before the retreat can be offered. A large pool of inmates (across the 13 facilities) makes DOC data matching a possibility to identify inmates most comparable to the treatment group. Random assignment is unlikely due to the very small number of men at each facility who are currently enrolling in the retreats and the lack of enrollment caps on the retreats.	220 treatment and 220 comparison couples over the three year enrollment period
Osborne	The PREP couples retreat (the component being evaluated) is available to men in a committed relationship (with a partner who is willing to attend the retreat) who have completed prerequisite parenting and healthy relationship classes. Because there is not a surplus of couples eligible for the retreat (and the enrollment targets for this site are low), random assignment is unlikely. Alternative design approaches include screening graduates of parenting classes at comparable NY DOC facilities not served by the grant and identifying men in committed relationships who would be interested in attending a couples retreat.	200 treatment and 200 comparison couples over the three year enrollment period

Grantee	Proposed Approach	Projected Sample Size
SC DOC	The program serves inmates at the Adult Offender Center in a county prison. Because there is not a surplus of men eligible for the program (and the enrollment targets for this site are low), random assignment is unlikely. An alternative comparison group is inmates in the main facility of the prison who are participating in a fatherhood program offered only to inmates in that building. The program, funded under OFA's Priority Area 2 initiative, includes child-friendly visitation, parenting education and some of the ancillary services that are offered to participants in Project REACH, which is funded under OFA's Priority Area 5. Research question would focus on the added impact of a marriage intervention for couples.	200 treatment and 200 comparison couples over the three year enrollment period

B.1.4 Power Analysis

Estimates of power can be difficult with many of the multilevel analyses proposed; in several instances, closed power formulae for the designs proposed have not been developed. However, power can always be estimated using Monte Carlo simulations, where multiple data sets, using bootstrap resampling, are generated using estimates of parameters provided by the user to inform the simulation. In turn, the multiple data sets are analyzed to determine power (Thomas & Krebs, 1997). To remain consistent, this “brute force” approach, which is described fully in Peladeau and Lacouture (1993), was used to estimate power for all of the proposed analyses. Because we have conducted related studies using some of the measures we plan to use here, effect size estimates are generally available. The power analyses presented here use pooled estimates of parameters and standard errors from the following studies (a) Behavioral Couples Therapy for Drug Abuse, (b) Abbreviated Couples Therapy for Drug Abuse, and (c) Group-Based BCT for Drug Abuse which are all NIDA-funded longitudinal trials. The following assumptions are made in this analysis:

- There is a 30% missing rate at any given time point, with missing data addressed using Full Information Maximum Likelihood methods (and adjusting, of course, for increased standard error due to missing data)
- The analysis uses a longitudinal design comparing couples therapy to an equally intensive Treatment-As-Usual (TAU)
- The models assume Couple Dyadic Adjustment Scale (DAS) scores nested within time; the models are very similar if only husband or wife scores are used, so there is no gain with other methods
- The model assumes normal data (DAS tends to look somewhat normal)
- These multilevel models will be estimated in a covariance structure framework (versus traditional multilevel framework, as implemented in statistical programming packages such as HLM or MLwiN)
- The analysis uses a .05 alpha.

Power estimates in **Exhibit 5** are from Monte Carlo simulations using procedures described in Sartorra and Saris (1985) relating to the DAS and the Conflict Tactics Scale (CTS II). For the DAS and CTS (as well as most primary outcome measures on children’s adjustment, parenting behavior, substance use, employment, legal entanglements, and so forth), the effect sizes

observed in Dr. Fals-Stewart’s trials are medium-sized using Cohen’s (1988) conventions. In these contexts, a medium effect size for the DAS translates roughly to an increase in scores of about 15 to 20 points more in the treatment group than the control group. In categorical terms, this means a move from very distressed to distressed, distressed to mildly distressed, mildly distressed to normal, normal to happy, or happy to very happy. For the CTS, it means about 20% more couples in the treatment group will not commit any acts of partner violence in the post-intervention period compared with controls. In both cases, this is benchmarked against an active TAU and these effect sizes are viewed as clinically meaningful. This effect size would be detected with 80% power in samples of 200 or more couples in treatment and control conditions.

Exhibit 5. Power Estimates from Monte Carlo Simulations

Sample Size (in terms of couples per condition; assumes 2 conditions):	DAS	CTS II¹
50	0.62	0.4
100	0.76	0.56
150	0.86	0.69
200	0.94	0.8
250	0.98	0.87
300	0.99	0.92

B2. Procedures for the Collection of Information

The study is designed to assess the short- and long-term effects of participation in MFS-IP programming on key outcomes, including relationship quality and stability, intimate partner violence, parenting behaviors, child well-being, family income, and recidivism. Several relationship quality constructs will be measured, including relationship satisfaction (using the DAS-8), fidelity, marriage plans, positive couple interactions, supportiveness, shared decision making, and conflict. The relationship stability domain will include measures of marital history, marital status, presence of a romantic relationship, cohabitation, and commitment. Several variables posited as mediators between MFS-IP programming and the outcomes above will also be measured, including substance abuse, employment, parenting skills and attitudes toward marriage.

Interview data will be obtained from each member of participating couples. Baseline interview data will be collected before initiation of any MFS-IP services. Follow-up data will be collected at nine and 18 months post-baseline. The timing of the follow-up data collections from the point of enrollment is appropriate because the interventions vary widely in terms of their length, dose, and timing relative to the term of incarceration. The follow-up surveys (**Appendices C-D**) are structured so that all respondents receive some common sections, while some sections are administered only to respondents who are still incarcerated or only to respondents who have been released. Detail on the timing of each interview wave relative to program delivery and study participant incarceration term is provided in *Exhibit 6*.

¹ The power estimates for the CTS II (a measure of partner violence) require the additional assumption of a negative binomial model for the very highly skewed characteristics that usually mark violence data.

Exhibit 6. Site-Specific Implications of the Timing of Program Delivery and Participants’ Incarceration Terms on the Interview Schedule

Program	Baseline Interview	9 Month Interview	18 Month Interview
<i>The Osborne Association</i>			
Timing re: program delivery	Immediately after enrollment	Program likely completed for all	At least 12 months after program completion
Timing re: incarceration term	Variable (enrollment not based on sentence characteristics and can take place any time during incarceration)	Variable	Variable
<i>IN DOC</i>			
Timing re: program delivery	Immediately after enrollment	Program likely completed for all	At least 12 months after program completion
Timing re: incarceration term	Variable (“enrollment” into couples retreat takes place for most at the end of the 4 th quarter [12 months] of PLUS participation)	Variable	Variable (but most likely coming up on release, given average sentence lengths and the fact that they have likely already served a year at program entry)
<i>SC DOC</i>			
Timing re: program delivery	Immediately after enrollment	Program still ongoing for all	Program ends for all by this time period
Timing re: incarceration term	Six-12 months from release	Within three months prior to three months after release	Six-12 months after release
<i>NJ DOC</i>			
Timing re: program delivery	Immediately after enrollment	Program still ongoing for all	Program ends for all at this time period
Timing re: incarceration term	Seven-ten months from release	Within one month prior to release to two months after release	Eight -11 months after release
<i>MN CCJ</i>			
Timing re: program delivery	Immediately after enrollment	Program still ongoing for all	Program still ongoing for all
Timing re: incarceration term	Immediately after intake; one-three years from release	All are still incarcerated	From up to 18 months before release to six months after release

The recruitment of study participants will occur on a rolling basis, and enrolling 200 treatment and 200 control couples in each of the five sites is expected to take three years (the end point of which coincides with the end of the sites’ grants). Therefore, several waves of data collection will overlap, and the overall data collection will take 54 months.

The study will utilize field supervisors and field interviewers from RTI’s National Interviewer File, and be supplemented with new hires as necessary to afford full coverage of a site. Field supervisors will attend a one-day in-person training session focused on project management

responsibilities. Then all field supervisors and field interviewers will attend a five-day, in-person training session, covering procedures for contacting respondents, gaining cooperation, avoiding and converting refusals, administering the interview, and reporting. Training will involve a combination of lecture, demonstration, and hands-on skills practice. All field supervisors and field interviewers will be required to pass a certification exam upon the completion of training.

Interviewers (and their associated field supervisors) will receive an electronic contact summary report for all of their assigned baseline cases. Specific recruitment procedures differ for incarcerated and non-incarcerated respondents. For eligible respondents who are incarcerated, they will be brought individually to a private room in the correctional facility where the study and all respondent rights will be explained. It will have been relayed ahead of time to the facility contact (in the facility access negotiations carried out by site liaisons) that facility staff should not convey any details about our study to the potential participants who have been identified. When approaching potential candidates about meeting with the interviewer, facility staff will tell the inmates that a researcher would like to meet with them and talk about the possibility of doing an interview for a research study and that the researcher will tell them more about the study in the interview room. The field interviewer will go over the consent form and answer any questions that the respondent has about the study. Prison officials will be told that the length of the interview varies greatly (from ten minutes to two hours).

Eligible respondents who are not incarcerated (which includes most female respondents at baseline and follow-up, as well as some male respondents at follow-up) will be mailed a letter introducing or reminding them about the study. The letter will contain a toll-free number that the respondent may call to schedule the interview and increase his or her payment by \$5. If the respondent does not call the interviewer within a week of the letter being sent, the interviewer will attempt to contact the respondent (via telephone or a home visit) to schedule the interview. Copies of the lead letters (a baseline version, which will only be used for partners, and a follow-up version, which will be used for all non-incarcerated respondents) and the Q&A brochures (which will not accompany the partner lead letters but rather be used by the field interviewer when meeting with the partner in person to discuss whether she is interested in participating in the study) are attached, in addition to the “Sorry I Missed You” cards and appointment cards that will be used in the field for setting up interviews.

Interviews will be conducted in a private setting. For incarcerated respondents, (all male participants at baseline), the interview will be conducted in a private room at the correctional facility, and only the respondent and the interviewer will be present. For partners, the interview will be conducted at the respondent’s home or other private location. Men who are eligible for study participation will be escorted individually to a private room in the correctional facility where they reside. The interviewer will then describe the study to potential participants.

For all interviews, after confirming that the correct respondent is present, participants will be handed the consent form and will read along as the interviewer reads the consent form text directly from the laptop. The consent form will display the interview consent text and a signature line for participants to indicate their consent to be interviewed (and for the interviewer to sign). The consent form also has additional text and a separate signature line for the respondents to indicate their consent for having random segments of the interview audio recorded for quality

control purposes (i.e., Computer-Assisted Recorded Interview [CARI]). Participants will sign one copy of the consent form for the project files and retain an unsigned copy for themselves.

The interviewer will administer the interview in a prescribed and uniform manner. The interviews will be conducted using Computer-Assisted Personal Interviewing (CAPI). For particularly sensitive sections of community-based interviews (which will include most respondents to the baseline and follow-up female surveys, and some respondents to the follow-up male surveys), ACASI will be used. Specifically, the ACASI section includes the questions on criminal history/behavior, criminal/drug involvement of the people with whom the respondent resides, substance use, relationship fidelity, and intimate partner violence. For the non-ACASI sections, the field interviewer (FI) will read the questions from the screen and enter the respondent's answers into the laptop. If at any time, the privacy of the interview setting is compromised, the interviewer will pause the interview until privacy can be reestablished, rescheduling as necessary. Each interview will last approximately one-and-a-half hours and will cover the following topics: basic demographic information, attitudes, programs and services, family structure, relationship quality, parenting, physical and mental health, substance use, criminal behavior, employment and income, expectations for release, and future contact information. The content of all instruments (male and female, baseline and follow-up) is similar. However, the time periods about which the questions are asked differ, and there are separate skip and fill patterns depending on whether the respondent is incarcerated.

At the conclusion of community-based interviews, respondents will be given their payment and asked to initial a receipt. At the conclusion of facility-based interviews, FIs will follow the compensation procedures allowed by the facility (e.g., no compensation, a money order deposited in the inmate's account, or payment to a community designee).

During the course of the study, FIs may observe respondent distress or child abuse or neglect. Critical incident protocols have been developed for the study (with separate versions for facility- and community-based interviews). These protocols specify steps the interviewer and other project staff charged with decision-making should follow. The interviewers will be trained extensively on these protocols.

Some portions of each interview will be recorded by the laptop using CARI technology. The purpose of CARI is to detect interviewer falsification. CARI files for 5% of all interviewers' cases will be reviewed by the project quality control manager. The respondent's permission to use CARI will be requested during the informed consent process. The respondent may still participate in the interview even if he or she declines CARI. If the respondent agrees to CARI, as a necessary condition of detecting interviewer falsification, neither the respondent nor the interviewer will be aware of when the computer is making the recordings. At least three 30-second portions of the interview will be recorded as well as several responses to "other specify" questions. CARI will not be used during the portion of the interview that asks the respondent for future contact information. CARI will also not be used during any portion of the interview that asks particularly sensitive questions.

The field supervisors for 5% of community-based interviews will also conduct standard telephone verification these interviews.

B3. Methods to Maximize Response Rates and Deal with Nonresponse

In any longitudinal evaluation, much depends on retaining the comparability of the treatment and control groups over time as well as on understanding the sample composition over time. There are two reasons why the treatment groups may have higher response rates than the control groups:

- The issues of marriage, family, and child well-being may be more salient in the treatment group since some of the interventions may enhance this awareness.
 - Individuals who have benefited from the interventions who might otherwise have refused to respond to the survey may decide to participate to reciprocate for perceived benefits; likewise those who have not benefited as much from the interventions will tend not to cooperate with the survey request.

There maybe other reasons why the response rates may differ, but these two reasons represent the heart of the issue. Both reasons have the potential for biasing the estimates of the treatment effects. Increased salience might lead certain groups that typically have low response rates (such as low education and some minority groups) to respond at a higher rate if they are in the treatment group relative to the control group. Additionally a better response rate from those who benefited more from the interventions could overstated the benefits and the estimated treatment effect may be biased upward.

The impact study will implement two strategies to address these concerns. First, a data collection methodology designed to minimize the non-response bias will be used, particularly as it affects the comparison of estimates between control/comparison and treatment groups. Here it is important to note that having the same response rates in both treatment and control/comparison groups does not guarantee that bias in the treatment effect is minimized. This is because non-response bias is the product of two components: the non-response rate and the difference in the characteristics of respondents and non-respondents. Thus, the differential non-response bias in treatment versus control comparisons may still not be zero if the compositions of the non-responding populations are different with respect to the characteristics of interest. To guard against this eventuality, a commitment will be made to successfully contacting and screening sample members and achieving the highest possible response rates. Such methods include:

- In-Person Interviewing. It is expected that in general the prospective participants will be in a lower socioeconomic class than average, and that some of them may not have regular telephone access. When surveying a hard-to-reach population of this kind, experience has shown that an interviewer-administered mode yields higher response rates than self-administered modes. In-person interviews using CAPI will be the most efficient means of efficiently gathering interview data.
- Respondent Convenience and Multiple Attempts. Interviews will be scheduling interviews at the respondent's convenience. There will be multiple attempts to reach nonrespondents, including leaving "Sorry I Missed You" cards at empty households at the time of a scheduled visit. As the study progresses and the best times to reach respondents and make contact attempts is ascertained, the interviewer scheduling will be adjusted accordingly.
- Customized Lead Letters. Customized lead letters will be sent in advance of fielding to promote respondent cooperation. The lead letter will explain the study objectives, explain that the survey is voluntary, and assure confidentiality. Moreover, the letter will provide several

means for respondents to contact interviewers, including a toll-free telephone number and email address.

- Financial Incentive. A cash incentive will be offered to each respondent who makes a good faith effort to complete the survey. It is anticipated that this will increase perceived benefit so that respondents will make time for the interview. The incentive payment will also help emphasize the importance of participating in the study.
- Comprehensive Interviewer Training. Interviewing staff will participate in a multi-day, comprehensive training. Interviewers will be trained on the study purpose and procedures, interview administration, and the protection of human subjects. Past literature has shown that interviewer effect can be a source for potential survey bias. Therefore, a thorough understanding of the study and the instrument, and upholding standard protocols and ethical commitment will reduce bias and in turn help interviewers gain respondent trust.
- Refusal Aversion and Conversion. Part of the interviewer training will address in detail specific techniques to avert and convert a refusal from a respondent. Respondents who initially refuse to participate will be assigned to interviewers who have a proven record of turning refusals into completed interviews. Reasons for refusals and barriers to participation will be continually evaluated in light of the experience gained in the data collection process.
- Regular Debriefings with Data Collection Staff. The project management staff will regularly meet with data collection staff to discuss issues related to data collection operations. Methods to enhance response rates will be a standard agenda item at these meetings.

To the extent possible, response rates by pre-identified demographic and other variables during data collection will be continually monitored to ensure that both groups at a site have not only the same response rates but also the same patterns of nonresponse across the demographic groups.

If a differential rate or bias is apparent in the estimates, statistical adjustments will be used to further minimize the risks of bias. Response propensity models will allow the weighting of the data so that the demographic composition of both groups in a site are statistically equivalent on those variables that are most highly correlated with and most critical to analysis. Another approach involves sample selection models, both static and dynamic. Here, the goal is to take account of the factors influencing selection into the sample in a first stage equation and then include a selection variable in an outcome equation.

Difference between groups on observable variables will be adjusted statistically using propensity matching and other methods. Unobservable differences between treatment and control groups are more concerning, but our screening collection will offer a partial solution in that it captures equal motivation to receive services between participants. Other impact estimation methods will be used that attempt to address selection bias by using alternative comparisons and counterfactuals. Possible approaches will include comparing the outcomes of the entire eligible population in the presence of the program with the outcomes of that population without the program, comparing different cohorts of participants because participation can be lagged by as much as three years, and matching treatment and comparison groups on observable characteristics.

B4. Tests of Procedures or Methods to be Undertaken

In developing the instruments for this study, a pilot test of the male and female baseline survey instruments was conducted. The purpose of the pilot was to test the overall flow and length of the

survey, to elicit information on how respondents are forming and reporting responses, and to identify any questions that may be difficult for respondents to answer. The male instrument was piloted with a convenience sample of nine incarcerated men who were married or in a committed relationship and had minor children. The female instrument was piloted with the committed partners of those men who agreed to do the interview and agreed to provide their partners' contact information. Male interviews were conducted in the correctional facility where respondents were incarcerated, and female interviews were conducted in respondents' homes. Both versions were piloted using a paper-and-pencil version of the instrument. Respondents were paid \$25 each for the interviews. The instruments were revised based on information obtained through this process, with the goal of improving the quality of data collected and minimizing burden on respondents. Reference periods were simplified for some constructs to provide greater ease of recall for those items that pilot study participants found difficult to answer. The incentive process was also tested, and payment for female respondents was increased to \$40.

Most items included in the data collection instrument have been used successfully in previous studies with similar populations, including the Multi-Site Evaluation of the Serious and Violent Offender Reentry Initiative (SVORI), the Evaluation of the Community Healthy Marriage Initiative (CHMI), and the longitudinal Returning Home study of reentering prisoners. Investigators who were involved in each of these projects have provided guidance and feedback on the survey instrument, and their experiences collecting survey data involving similar populations and outcomes of interest has been very helpful.

B5. Individuals Consulted on Statistical Aspects and Individuals Collecting and/or Analyzing Data

a) Individuals who have participated in designing the data collection:

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