SOCIAL INNOVATION FUND

EVALUATION PLAN GUIDANCE

A Step-by-Step Guide to Designing a Rigorous Evaluation

Corporation for
NATIONAL & COMMUNITY SERVICE

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A federal agency, CNCS engages more than five million Americans in service through AmeriCorps, Senior Corps, Social Innovation Fund, Volunteer Generation Fund, and other programs, and leads the President’s national call to service initiative, United We Serve.

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Guidance Overview

The purpose of the Social Innovation Fund (SIF) is to grow the impact of innovative community-based solutions that have compelling evidence of improving the lives of people in low-income communities throughout the United States. As one of the federal government's "tiered-evidence initiatives," the SIF embodies a commitment to use rigorous evidence both to select recipients of federal funding and to validate the impact of their program models. The SIF specifically directs resources toward increasing the evidence base, capacity, and scale of the organizations that it funds in order to prove and improve the lives of people served by those organizations.

What do we mean by increasing the evidence base? Like other "tiered-evidence" initiatives, the SIF sees levels of evidence as a continuum with preliminary evidence on one end and strong evidence on the other. Increasingly rigorous evaluation designs and repeated program assessment in different situations help build an evidence base and move a program from one end of the continuum to another. The SIF expects programs to use our funds to move towards the strong end of the continuum and have thus set an ambitious requirement that each funded program model must achieve moderate or strong evidence of impact (as defined here) by the end of its three to five year grant period. To achieve this, grantees and subgrantees must commit significant time and resources to conduct formal evaluations of each program model that receives SIF funding.

About this Guidance

This guidance is intended to be used by SIF grantees, subgrantees, and evaluators in preparing a SIF Evaluation Plan (SEP). Because each of these audiences may have a different level of evaluation knowledge, non-technical terms are used where available, and technical and SIF-specific terms are defined throughout.

This document has four sections:

1. This Introduction
2. The Suggested Evaluation Plan Outline
3. The Detailed Evaluation Plan Guidance
4. Appendices which include:
   - References;
   - Resources;
   - Examples and Templates; and
   - A Glossary of Terms.

Information on additional available resources pertaining to certain guidance sections is also included throughout the document.

Through these evaluations, programs will not only assess their effectiveness, but will also build the knowledge base for other initiatives addressing similar community issues across the nation. A key step in conducting a rigorous SIF evaluation is the development of a SIF Evaluation Plan (SEP) that details the program model being evaluated and describes and justifies the evaluation approach selected. This document is a version that has been adapted for public use, based on the one provided to SIF grantees, subgrantees, and evaluators containing guidance for developing their evaluation plan.

Developing an evaluation plan helps programs and evaluators think through the issues involved in carrying out their evaluations. For the SIF, this plan also helps ensure a shared understanding between CNCS, funded programs, and the evaluator about the evaluation to be conducted and the level of evidence it proposes to provide.

CNCS sees the process of ongoing evaluation and knowledge building as a key aspect of the SIF that can improve grantee and subgrantee programs, and also benefit other organizations throughout the nonprofit and public sectors. SIF works closely with grantees to meet this goal by providing them with technical assistance on the design, implementation, and monitoring of their SEPs, collecting best practices to share with the broader social sector, and disseminating the evidence of effectiveness for each program model within the SIF.

As a part of our commitment to sharing lessons learned through the implementation of our programs, the SIF is pleased to share this guidance document with the field. It can serve as a detailed, step by step planning guide for organizations that wish to implement rigorous evaluations that strive to be in alignment with standards set by the SIF and other federal agencies that emphasize evidence and impact of program effectiveness. Many SIF-funded programs have found that completing this kind of plan to the level of detail specified is an intensive undertaking. All SIF programs have found that the plan development is an iterative process requiring several drafts and careful communication between evaluator and programs. However, SIF grantees report that this investment of time and effort has proved greatly helpful in ensuring that plans meet the desired level of rigor, anticipating potential challenges, shoring up sufficient resources, and establishing a firm set of timeframes and deliverables. The end result is a stronger and robust plan that both program staff and contracted evaluators can execute.

Changes to the Guidance

A limited original version of the Guidance was released to grantees in January 2010 by the Corporation for National and Community Service (CNCS). Based on suggestions from the 2010 grantees and SIF stakeholders, CNCS released an updated and expanded version of the Guidance in January of 2012. The current edition of the Guidance (2013) encompasses the same elements as the Guidance provided in 2012, with updates to improve clarity and ensure that SEPs meet both stakeholder and SIF grantee information needs. The current version of the guidance was created by JBS International, a training and technical assistance provider to CNCS.

Levels of Evidence

A key goal of the SIF is for grantees and subgrantees to conduct evaluations that expand the level of evidence available about their program interventions. It is required that SIF-funded interventions enter the SIF with at least some preliminary evidence of program effectiveness.

As our past Notices of Available (NOFA) states, it is expected that SIF programs will engage in evaluations that provide either moderate or strong levels of evidence by the end of their subgrant period (see below for definitions). A submitted SIF evaluation plan will clearly identify the level of evidence it is targeting.

Levels of Evaluation Evidence

As outlined in the SIF NOFA, the tiers of evidence are defined as follows:

Preliminary evidence means the model has evidence based on a reasonable hypothesis and supported by credible research findings. Examples of research that meet the standards include: 1) outcome studies that track participants through a program and measure participants' responses at the end of the program; and 2) third-party pre- and post-test research that determines whether participants have improved on an intended outcome.

Moderate evidence means evidence from previous studies on the program, the designs of which can support causal conclusions (i.e., studies with high internal validity) but have limited generalizability (i.e., moderate external validity) or vice versa - studies that only support moderate causal conclusions but have broad general applicability. Examples of studies that would constitute moderate evidence include: (1) at least one well-designed and well-implemented experimental or quasi-experimental study supporting the effectiveness of the practice strategy, or program, with small sample sizes or other conditions of implementation or analysis that limit generalizability; or (2) correlational research with strong statistical controls for selection bias and for discerning the influence of internal factors. Moderate evidence requires third-party or external and impartial evaluators.

Strong evidence means evidence from previous studies on the program, the designs of which can support causal conclusions (i.e., studies with high internal validity), and that, in total, include enough of the range of participants and settings to support scaling up to the state, regional, or national level (i.e., studies with high external validity). The following are examples of strong evidence: (1) more than one well-designed and well-implemented experimental study or well-designed and well-implemented quasi-experimental study that supports the effectiveness of the practice, strategy, or program; or (2) one large, well-designed and well-implemented randomized controlled, multisite trial that supports the effectiveness of the practice, strategy, or program. Strong evidence requires third-party or external and impartial evaluators.

Suggested Evaluation Plan Outline

Each SIF evaluation plan will contain the following sections (suggested lengths for each section are provided in parentheses below to provide a sense of relative emphasis):

Executive Summary (estimated 1-2 pages)

I. Introduction (estimated 1-2 pages)
  A. Program Background and Problem Definition
  B. Overview of Prior Research
  C. Overview of Study
  D. Connection of this Study to Future Research

II. Program Theory, Logic Model, and Outcomes of Interest (estimated 2-4 pages)

III. Research Questions and Contribution of the Study (estimated 2-4 pages)
  A. Research Questions
    1. Impact
      a. Confirmatory
      b. Exploratory
    2. Implementation
  B. Contribution of the Study

IV. Study Components (estimated 10-15 pages)
  A. Impact Evaluation Design
  B. Implementation Evaluation Design
  C. Sampling, Measures, and Data Collection
    1. Sampling
      a. Sampling Plan and Power Calculation
      b. Recruitment, Retention, and Informed Consent
    2. Measures
    3. Data Collection Activities
  D. Statistical Analysis of Impacts
  E. Multiple Outcome Measures

V. Protection of Human Subjects Protocol (estimated 0.5-1 page)

VI. Reporting Results, Timeline, and Budget (estimated 2-4 pages)

VII. Evaluator Qualifications and Independence (estimated 2-4 pages)

VIII. Grantee/Subgrantee Role and Involvement (estimated 0.5-1 page)

Detailed Guidance

Executive Summary

Include an overview of the program, the context of the evaluation and the evaluation approach you intend to use. As with any Executive Summary, the purpose is to present the reader with the highlights of the full document, either to brief very busy readers or to serve as an overview to better prepare readers for the full document.

Specific Guidance: Executive Summary

The Executive Summary should contain the following:

- Name of organization;
- A one-paragraph synopsis of the program and what it intends to change/impact;
- A brief synopsis (one to two sentences) of prior research done on the program;
- A brief description of the measures/instruments to be used, and the types of data to be collected from them;
- A brief description of the proposed analysis approach(es);
- A summary of the key timeline elements/dates (e.g., dates that participant recruitment and data collection need to start/end, and when analysis and reporting will take place);
- The estimated budget; and
- A brief description of the evaluation team and members' experience related to the effort.

I. Introduction

The introduction to your evaluation plan establishes the context of the evaluation that you are proposing. It should further describe the program background and problem definition, and give an overview of prior research on the program. A program description and overview of prior research are crucial to setting up the overall evaluation design. They provide a starting point for assessing outcome measurement in the current program. At the same time, they also provide an understanding of how the program has worked in the past and what evidence of effectiveness may be expected, as well as information on potential control or comparison groups.

Program Background and Problem Definition

A strong description of the program and its origins contextualizes the evaluation. The relationship of the program to the problem it is designed to address is also important tounderstand the overall evaluation design.

Specific Guidance: Program Background and Problem Definition

The Program Background and Problem Definition section describes the problem or issue the program addresses. As part of this description, the evaluation plan should briefly discuss the program theory and logic model. This section should also include information concerning program components, intervention level, beneficiaries, and key outcomes.

Overview of Prior Research

SIF-funded programs are expected to meet an evidence threshold to be funded and must build upon this evidence as a requirement of the grant. In general, it is important to identify what research has been done and how the planned evaluation will contribute or build upon this existing research base. In this section, document the previous research that has been completed and contextualize the evaluation plan.

Specific Guidance: Overview of Prior Research

Describe any evaluations or other research on the program, and include the following for each:

- When the study was done;
- Who conducted the study;
- The program population (number and brief description) involved in the studies;
- Any comparison or control group involved (number and brief description);
- The evaluation approach or methods used (e.g., randomized controlled trial, quasi-experimental design, case studies, implementation study);
- A brief description of the findings; and
- The level of evidence attained (e.g., moderate, strong).

Include the same information listed above for any relevant research or evaluation findings on similar programs (e.g., programs using the same intervention in other locations or similar interventions with either the same or different populations).

Note: If the current program has a large pre-existing evidence base or if there is extensive research on similar programs, it may be more practical to include information from key evaluation studies or any available metaanalyses, summarizing the bulleted information above. For programs that have extensive research, you may want to put this information in tabular form (see Appendix C for a sample table). Please also include references to the source of the prior research or evaluation.

II. Program Theory and Logic Model

The evaluation plan should include an overview of your program's theory and a logic model that guides the evaluation and provides the reader with a better understanding of how your program is expected to achieve its targeted outcomes and impacts.

A description of program theory, coupled with an informative logic model, frames the evaluation design. Understanding the theory and assumptions behind how a program is designed is an important precursor upon which all subsequent evaluation activities fundamentally rest (Rossi, Lipsey, & Freeman, 2004). A program logic model usually includes both a graphic display and a narrative description of the resources/inputs, the program activities that constitute the intervention, and desired participant outcomes/results.

Additional Resources

The Kellogg Foundation's 2004 Logic Model Development Guide (*http://www.wkkf.org/knowledge-center/resources/2006/02/wk-kellogg-foundation-logic-model-development-guide.aspx*) provides advice and examples for constructing a logic model.

Logic models, which are grounded in a theory of change, use words and graphics to describe the sequence of activities thought to bring about change and how these activities are linked to the results the program is expected to achieve. This process includes sequentially thinking through and aligning the following areas:

- *Resources/Inputs:* Include resources that are available and directly used for the program activities, including human, financial, organizational, and community resources;
- *Intervention:* Include program activities (i.e., what constitutes the program intervention) with the resources you listed previously that lead to the intended program results/outcomes; and
- *Outcomes/Results:* Include changes that occur because of the program intervention previously described, using the resources previously described. These can be any of the following:
  a) Short-term outcomes (outputs) may include the amount of intervention (i.e., the quantity and type(s) of program activities a participant actually takes part in) an individual receives or specific changes in knowledge or skills;
  b) Intermediate outcomes may include changes in individuals' behaviors or attitudes; and
  c) Long-term outcomes (impacts) may include the changes occurring in communities or systems as a result of program interventions.

An example of a logic model can be found in Appendix C (Examples and Templates) section of this guide.

Specific Guidance: Program Theory and Logic Model

The program theory and logic model accomplish the following:

1. Briefly describe the basis for the logic model (theory or prior research, or both) along with aspects of the model, if any, which have been confirmed (or refuted) by previous research;
2. Ensure alignment among the logic model elements. Focus on key factors in the cause-and-effect relationship. Think about and describe how the resources/inputs link to the intervention and then link to the desired outcomes/results;
3. Detail the elements of the resources/inputs and articulate the paths through which the services provided affect individual outcomes (e.g., employment, health, education). Make sure that the activities directly lead to the explanation of short-term outcomes/outputs (e.g., increased student attendance)

and intermediate-term outcomes (e.g., improved reading scores) that would be necessary to achieve the program's long-term outcomes/impacts (e.g., high school completion, college readiness);

4. Emphasize the outcomes that the proposed evaluation will measure; and
5. Include only information that is directly related to the theory of change. Keep the logic model simple to minimize attention to things that cannot be controlled.

The logic model should ultimately be used to guide the collection of data to inform an assessment of program effectiveness. The logic model should emphasize details of the program that directly relate to the core aspects of the evaluation.

III. Research Questions and Contribution of the Study

Research Questions

The evaluation plan should include both the questions that the evaluation hopes to answer and a description of how answering these questions will contribute to a greater understanding of programs, outcomes, and/or policies. Research questions are inquiries that are measurable, and thus empirically answerable. SIF evaluation designs frequently include both impact and implementation components.

Impact evaluations pose questions about the *outcome* of the program for beneficiaries/participants and on the *impact* of program services or participation, more generally, in relation to the comparison group, control group, or pre-participation baseline of the participants themselves.

Implementation evaluations pose questions related to the *process* of developing, running, or expanding a program, and potentially also about participants' experiences of program participation. Questions should focus on the process by which the program operates, rather than the outcomes among beneficiaries or impacts on them in relation to non-participants.

For example, for a program that delivers job training services, an implementation question might be:

Do participants receive all components of the job training program upon completion of the program?

In contrast, a comparable impact question might be:

Do participants have more job-related skills upon completion of the program components compared to the control group?

Most SIF-funded evaluations contain both types of research questions.

Specific Guidance: Confirmatory and Exploratory Impact Questions

The confirmatory and exploratory impact questions should describe the impact evaluation questions, and note whether they are confirmatory or exploratory in nature. Exploratory questions include those that are posed during the design phase, and implied by, or stated in the logic model, but cannot be answered with adequate statistical power. If you have multiple confirmatory questions related to the same outcome, consider prioritizing them to avoid difficulties with multiple statistical comparisons during the analysis phase.

Additional Resources

For more information on addressing questions about multiple comparisons, see Peter Schohet's (2008a) "Technical Methods Report: Guidelines for Multiple Testing in Impact Evaluations" (NCEE 2008-4018).

For example, a confirmatory question might examine changes in knowledge levels among program participants at the beginning of program participation compared to the end of the program. An additional exploratory question about the evidence of effectiveness of program exposure to all components of a program may be posed if it is unclear whether all participants follow the same path through a program.

Questions should be based on outcomes specified in the logic model. Developing the logic model and the evaluation questions in concert can help ensure that the impact evaluation measures the right outcomes and accurately monitors program implementation. If the evaluation design includes a control or comparison group, questions should refer to each group as appropriate. For example, "Do program participants show greater positive changes relative to comparison group members?" Not all impact evaluation questions will apply to both groups, but using the same questions across groups allows for assessing the net effects of the program.

Specific Guidance: Implementation Evaluation Research Questions

Include implementation questions that focus on how and how well the program is put into operation. These questions should be separate from the impact questions, aligned with the program logic model, and measureable.

Implementation evaluation measures may include, for example, determining the extent to which a service is provided to beneficiaries, the number/type of components in an intervention, or who is (and is not) offered or receives services. In addition, implementation evaluation may address participation, quality of services, costs, satisfaction with the services received or other aspects of the program, including inputs and outputs specified in the logic model.

Contribution of the Study

Evaluation studies should contribute to the broader understanding of programs and policies. In developing these studies, evaluators should consider:

- How will this study build on the research that already exists about the program, intervention, or the problems addressed?
- How will this study serve to confirm or refute existing theories of change surrounding the problem?
- How will this study expand the overall evidence available about the program or problem?
- What future evaluation efforts will this study position you to take on in the future?

Specific Guidance: Contribution of the Study

Using the overview of prior research as context, clearly state what contribution the proposed study will make to the overall knowledge about the program's interventions and outcomes. The evaluation plan should clearly state what level of evidence, according to the SIF definition the proposed evaluation plans to attain (e.g., moderate, strong), and why this level is appropriate for the program. For example, "Prior studies have reached moderate evidence and demonstrated effectiveness with strong internal validity through well implemented quasi-experimental designs (QEDs). However these studies had limited, non-generalizable, samples. The current study targets strong evidence through the use of a multi-site randomized controlled trial (RCT) with a diverse sample."

IV. Study Components

Overview

The Study Components section provides guidance on the design elements of impact and implementation evaluations, and issues that need to be addressed regarding study measures, sampling, and data collection. It also details the criteria for descriptions of statistical analyses of data and approaches to missing data. In this section of the guidelines, information, specific guidance, and criteria for different design types are presented separately. Since your evaluation plan may address one or more types of impact/outcome evaluation designs as well as implementation evaluation or feasibility studies, please review the guidance sections for the designs and other study components shown in the table below.

Study Component	Follow the guidance for this section:
Impact Evaluation-Design Types, including:	Yes (An evaluation plan will typically choose one or more types of Impact/Outcome Evaluation designs)
- Randomized Between Group Design	If applicable
- Groups Formed by Matching	If applicable
- Groups Formed by Cutoff Score	If applicable
- Single Group Design	If applicable
- Interrupted Time Series Design	If applicable
- Pre-experimental Design (i.e., no comparison group/counterfactual)	If applicable
- Impact Feasibility Study	If applicable
Implementation Evaluation	If applicable
Sampling, Measures, and Data Collection	Yes (All evaluation plans should include)
Statistical Analysis of Impacts	Yes (All evaluation plans should include)

Program evaluations may have multiple components, including implementation analysis, outcome analysis, impact analysis, and cost-benefit, cost-effectiveness or other cost inclusive analyses.

Impact Evaluation

Impact evaluation designs address the issue of how an intervention is related to changes in its participants or beneficiaries, and ideally provide evidence about whether it causes the observed changes. The strength of evidence of an impact evaluation is determined by the extent to which it maximizes internal and external validity.

Internal validity refers to the ability of the evaluation findings to accurately reflect the impact of the intervention on participants or beneficiaries. Research designs are thought to have good internal validity if they incorporate design features that limit the extent to which the changes seen in the study could be caused by things other than the intervention being measured. These alternative causes could include:

- Who gets into the program (or control or comparison group) and who stays in each group during the study;
- Things that happen during the time window of program participation that are not part of the program components (e.g., children get older, the economy rebounds);
- The evaluation process (e.g., completing measurement instruments, being observed); and
- Decisions made by evaluators and the impact of their actions (e.g., unreliable instruments, instruments that are invalid for the intended purpose).

Strong external validity occurs when a study’s findings can be generalized to a diverse target population. Even if the population in question is a targeted one, external validity pertains to diversity in time (i.e., the program is effective across several years), and location. Designs that have strong external validity adequately address concerns about the following:

- How well the findings apply to other settings, sites, or institutions; and
- How well the findings apply to other groups of people, who may or may not be like those in the current study.

Evaluation designs that minimize threats to internal validity and maximize external validity are most likely to yield strong evidence. Designs that address key threats to internal validity, although they may not have strong external validity, can yield moderate evidence. Designs that do not sufficiently address threats to internal validity will yield preliminary evidence, as will designs that do not incorporate a counterfactual scenario (i.e., a comparison with what would happen in the absence of the program). For more details, see the information on “Internal and External Validity and Strength of Evidence” below.

Internal and External Validity and Strength of Evidence

Strong Evidence: Different types of evaluation designs are likely to yield different levels of evidence of program impact. These designs are likely to maximize both internal and external validity if they use reliable measures, take into account pre-existing participant characteristics, have program participants (or groups or sites) receive identical services, and include a large and diverse sample drawn from the target population in both the group that participates as well as the group that does not.

Additional Resources

For more information on design types and threats to validity see Table 2 in Appendix C (Examples and Templates).

Moderate Evidence: Evaluation designs that have strong internal validity, but weaker external validity, are anticipated to produce moderate levels of evidence. Moderate evidence comes from studies that are able to show that a program produces changes among participants (or groups or sites), but cannot demonstrate how well the program would work among other groups besides those included in the study, or which may have a very limited number of threats to internal validity unaddressed.

Different types of evaluation designs may produce moderate evidence, such as the following:

- Randomized control group designs that include small numbers of respondents or draw participants from a group that is not representative of the target population as a whole;
- Cut-off score matched group designs;
- Interrupted time series designs drawn from representative samples of the target population; or
- Single case study designs that involve frequent data collection across time.

Preliminary Evidence: Evaluation designs that address either no, or only a few, threats to internal validity produce preliminary evidence. Preliminary evidence comes from studies that cannot demonstrate a causal relationship between program participation and measured changes in outcomes, although in some cases they may be able to show a strong association (statistically) between program participation and measured changes. Different types of evaluation designs may produce preliminary evidence, such as:

- Any study (even an otherwise well designed randomized controlled trial [RCT] or quasi-experimental design [QED]) without sufficient sample size/statistical power;
- Any study (even a RCT or QED) that fails to address threats to validity due to instrumentation or experimenter effects;
- Interrupted time series designs with insufficient pre- and post-measurements;
- Non-randomized two-group post-test or pre-and post-test comparison without adequate matching or statistical controls; or
- Pre- and post-test or post-test only design with a single group.

These designs are unable to provide strong or moderate evidence because they cannot sufficiently reduce other possible explanations for measured changes.

Design Types

This section outlines the two major categories of impact evaluation: (1) experimental or quasi-experimental evaluation designs, and (2) pre-experimental designs. Experimental or quasi-experimental evaluations can be either between-group impact studies or single-group impact studies. Between-group designs compare at least two groups of individuals who differ in terms of their level of program participation on one or more outcomes. The people (or groups of people) who receive services are referred to as the treatment group, the group of program participants, or the group that receives the intervention, while the group that does not participate is referred to as the control group (when people are randomly assigned to that group) or the comparison group (when people are assigned to that group through non-random matching). Single subject/group designs collect data on only one group (or respondent) for multiple time points pre- and post-intervention, or at different points during the program intervention. All of these designs are explained in greater detail below.

Specific Guidance: Impact Evaluation Design Selection

Describe the characteristics of the impact evaluation proposed. Impact evaluations provide statistical evidence of how well an intervention works and what effect it has on participants or beneficiaries. The type of information that should be provided in the impact evaluation section will differ according to the type of design proposed (e.g., randomized control group, matched comparison group, single case with repeated measurements, a pre-experimental evaluation using pre- and post- testing). However, all proposals should include a clear description of the design, including its strengths and limitations. Where possible, the proposed design should draw upon previous examples of the design type from the literature and explain why the proposed evaluation design was selected over alternative designs.

The subsections below describe the different types of research designs most commonly used in impact evaluations. The evaluation plan should describe in detail the selected research design, noting the specific requirements from the checklist for the particular design type. This list is comprehensive, but not exhaustive, and other research designs may be appropriate depending upon program specifics.

Randomized Between-Groups Design

The strongest evaluation design available for establishing causality is random assignment of program participants (or groups of participants, program sites, etc.) to either a program participation group or a control group that is not exposed to the program (often referred to as the treatment or intervention). If individuals are randomly assigned to the program and control groups, the groups are statistically equivalent on measured and unmeasured characteristics—including unmeasured characteristics that evaluators may not have considered when designing the evaluation (Boruch, 1997). Random assignment allows evaluators to infer that changes in the participants are due to the intervention, regardless of the characteristics of any of the individuals that are easily recorded (such as race or gender) or less easily recorded (such as motivation or beliefs).

This statistical equivalence comes from the treatment and control groups being formed in an unbiased way. If the evaluation were, theoretically, replicated a large number of times, the groups would be perfectly balanced in terms of individual characteristics. However, in any one sample, the groups may not be perfectly balanced on all characteristics. Even so, when groups are formed before individuals start the program, they are assumed to be statistically equivalent on measured and unmeasured characteristics prior to program participation. If the groups remain the same throughout the evaluation (i.e., there is minimal attrition or those who drop out of both groups are similar in terms of key characteristics), then the difference in the average outcome between the intervention and control groups can be attributed to the program without reservations. However, issues (e.g., the number of randomized units being too small, participants dropping out differentially from the intervention and control groups, unequal participation rates in the treatment and control groups), may create systematic differences in the groups during the study.

Additional Resources

See Boruch (1997) for information on conducting experimental design evaluations.

See Song and Herman (2010) for guidance on methods of random assignment for experimental designs.

See McMillan (2007) for a discussion of threats to internal validity in RCT designs, (available here: http://pareonline.net/pdf/v12n15.pdf)

Specific Guidance: Randomized Between-Groups Design

Discuss steps that will be taken to ensure that the groups remain equivalent, or steps to impose statistical corrections for non-equivalence.

For a randomized design, provide comprehensive and clear information as to what constitutes taking part in the program (i.e., what constitutes treatment or exposure, does some program participation count), and what the control conditions will be (i.e., no knowledge of the program, not participating in some parts of a program). Fully describe the randomization process, including how individuals will be assigned to program participation, how the random numbers will be generated, who will conduct the random assignment, and any matching that might be used prior to randomization. The unit of randomization should be the same as the unit at which the outcome is measured; this means that if individuals are the unit of analysis, they should be randomly assigned to each group, but if program sites are the unit, the sites should be randomly assigned to each group.

Non-Randomized Group Designs – Groups Formed by Matching

Sometimes it is not feasible to randomly assign potential program participants (or groups or sites) to treatment and control groups. In these situations, a comparison group can be formed by matching study participants, or clusters of study participants, on a set of pre-intervention measures of the program outcome (e.g., pre-test scores for an academic program, pre-participation employment status for a job related program) and/or pre-intervention measures that are likely correlated with the program outcome, and/or other characteristics. The main goal is to have two groups of individuals (or sites) that are as similar as possible on as many characteristics as possible.

These types of designs are called quasi-experimental because they limit the evaluator’s ability to make causal claims about a program’s impact, compared to experimental designs with random assignment. This is because the groups formed by non-random methods will be, at best, equated on measured characteristics only, whereas random assignment ideally randomly distributes unmeasured characteristics, such as motivation, between the treatment and control groups. Working with a quasi-experimental design, the evaluator can attribute the observed effect on the outcome to the program, but with reservations, because unmeasured characteristics may unknowingly be responsible for the outcomes observed after program participation (Shadish, Cook, & Campbell, 2002; Rossi, Lipsey, & Freeman, 2004). However, matching *in advance of the treatment* with variables that are either pre-intervention measures of an outcome or are highly correlated with the outcome will minimize the chance of differences between the treatment and comparison group. Doing so lessens the possibility that observed differences are in fact due to extraneous differences between the treatment and comparison groups, rather than due to program participation.

Additional Resources

See Rossi, Lipsey, and Freeman (2004) for a general overview of research design in evaluation.

See Shadish, Cook, and Campbell (2002) for details on experimental research design.

For more information on quasi-experimental evaluation designs see: http://ssmon.chb.kth.se/safebk/Chp_4.pdf

Methods for matching people or sites to groups differ in their effectiveness. Propensity scoring methods, or statistical assessments of similarity among participants, are preferred when groups are matched with multiple pre-intervention measures. However, the type of matching algorithm used to implement the propensity scoring should be carefully selected based on simulation studies, previous research that demonstrates the validity of the algorithms, and the goals of the evaluation. See Song and Herman (2010) for information and guidance on methods of matching for quasi-experimental designs.

Specific Guidance: Non-Randomized Group Designs – Groups Formed by Matching

For a proposed quasi-experimental design where the comparison group is formed by matching, clearly describe the proposed comparison group and each step in the matching procedure. List all measures to be used in the matching, and provide details for these in the measures section, below. Provide information (e.g. from existing literature) that justifies the inclusion of all measures in the matching procedure.

If possible, describe any matching characteristics used that were drawn from previous evaluations. Further, this evaluation should include all variables that are typically included in matching procedures in similar evaluations. To anticipate potential threats to internal validity (the certainty that the evaluation results accurately account for program impact on documented outcomes), be sure to discuss reasons why the comparison group might differ from the treatment group and the ways in which the proposed methods adjust for those differences.

Between-Groups Designs - Groups Formed by a Cutoff Score

Another way of assigning individuals into groups is by examining a quantifiable indicator related to the key outcome collected prior to study participation, such as reading ability measured by standardized test scores (for a tutoring program) or income per household member (for a program addressing economic opportunity). If the indicator is reliably and validly measured, a well-defined cutoff score can be used to form the intervention and comparison groups, with those in more need (as measured on the indicator) assigned to the program group and those with less need assigned to the comparison group. The difference (or discontinuity) in the average outcome between those in the intervention group just below the cutoff score (e.g., those with lower test scores) and those in the comparison group just above the cutoff score (e.g., those with higher test scores) can be attributed to the program, albeit with reservations. This design is known more formally as a regression discontinuity design (RDD).

Additional Resources

For more information on RDD, see the Resources for Further Reading list in Appendix B: Resources.

A description of discontinuity designs is given by Imbens and Lemieux (2008), available here: http://faculty.smu.edu/millimet/classes/eco7377/papers/imbens%20lemieux.pdf)

A clear outline of the method is also given here: http://www.socialresearchmethods.net/kb/quasird.php

Specific Guidance: Between-Groups Designs - Groups Formed by a Cutoff Score

If this type of design is proposed, clearly delineate and justify the cutoff point (thereby describing the program group versus the comparison group), including whether an estimated or exact cutoff score is to be used. The unit of measurement to be used for the cutoff score (e.g., an individual student's score) should correspond to the outcome measure, and to the unit of assignment (e.g., an individual student was measured and students were the unit of assignment). The cutoff score should have sufficient range to constitute meaningful differences between the two groups to ensure internal validity. The cutoff score should have a relatively linear relation to post-test measures, because non-linear relations can erroneously be interpreted as a regression discontinuity. It is important not to mistake the continuation of a non-linear trend in data for a break in a test score.

Single-Group Designs - Single Subject (or Case Study Designs)

The single case design is recognized in the literature and by the federal Department of Education's *What Works Clearinghouse* as enabling evaluators to attribute changes in the outcome based on a single case. Single case designs can do a good job of evaluating how a program affects participants (internal validity) approaching that of experimental designs, but use of a single case limits the generalizability (external validity) of the results to other groups. One form of the single case design—repeated measures taken prior to treatment delivery and repeated measures after treatment delivery—is a form of the interrupted time series, but the time series is typically measured on one individual.

Specific Guidance: Single-Group Designs - Single Subject (or Case)

If a single group design is proposed, each intervention stage of the design should be detailed, including the baseline stage. The number of measurement points at each phase should be adequate such that trends can be established and threats to internal validity can be minimized.

Interrupted Time Series Design

Occasionally, an evaluation examines a single group of individuals before and after participation in an intervention, with no attempt at controlling who is part of the group. This form of evaluation, referred to as an interrupted time series design with a single group (such as a school or classroom), attempts to capture any change that occurred to the individuals after program participation by examining the general trend found in multiple measures of an outcome over time.

Data collected across time are referred to as longitudinal data (in comparison to cross-sectional data, which are collected at one point in time). Longitudinal data are defined as repeated measurements on individuals (sites, groups, etc.) over time. Many researchers use the specific term "time series" to refer only to data collected over time from different sets of individuals (as compared to "longitudinal data," which refers to data collected from the same individuals over time). However, the term "interrupted time series" is used commonly in evaluation research to refer to a specific form of longitudinal data analysis, and so is used here to refer to all types of data used in this type of evaluation. The frequency of these measurements and units, and the length of time, depends on the program or policy being assessed. Simply put, repeated measurements are made on units prior to implementation (the comparison group) and after implementation (the treatment group). The interruption in the time series (or patterns before and after program implementation) can be estimated graphically or statistically (Imbens & Lemieux, 2008).

The interrupted time series design has been most frequently and effectively used when the intervention is a program or policy of significant scope, such as community interventions to improve child-rearing practices or mandatory seat belt laws in a state. The social evaluation research literature includes many descriptions and examples of the development and implementation of this design and the analysis of data generated by it (Bickman & Rog, 2009; Khandker, Koolwal, & Samad, 2010; Rossi, Lipsey, & Freeman, 2004; Shadish, Cook, & Campbell, 2002). Additionally, although this design is listed as a single-group design, a comparison group is usually needed to address the possibility that extraneous events linked to how data were collected or outcomes were developed

Additional Resources

The Department of Education's *What Works Clearinghouse* (at http://ies.ed.gov/ncee/wwc/) provides many resources related to evaluation design.

A description of interrupted time series designs is given by Imbens and Lemieux (2008), (available here: http://faculty.smu.edu/millimet/classes/eco7377/papers/imbens%20lemieux.pdf)

See Bickman and Rog (2009) for information on many different evaluation designs.

See Khandker, Koolwal, and Samad (2010) for information on propensity score matching and regression discontinuity design.

See Shadish, Cook, and Campbell (2002) for citations of studies that have used the interrupted time series design.

could lead to an observed intervention effect. Similar to a single case design, interrupted time series designs are likely to have limited generalizability.

Specific Guidance: Interrupted Time Series Design

If an interrupted time series design is proposed, demonstrate that the number of time points to be measured prior to and after the intervention is sufficient to establish a trend and rule out rival explanations. Describe the timing of measures and their appropriateness to the intervention. Plans to include comparison cases should also be provided, if at all possible. If a comparison case is included, it should be clearly described.

Pre-Experimental Designs

If it is not feasible or appropriate to conduct an experimental or quasi-experimental evaluation of a program, an evaluation design targeting preliminary evidence may be most appropriate. In the vast majority of these cases, an initial evaluation of a program's effectiveness can serve as a precursor to a more rigorous evaluation.

Additional Resources

For an in-depth description of pre-experimental models see http://www.socialresearchmethods.net/kb/destypes.php.

Pre-experimental design evaluations are characterized by lack of a control or statistically matched comparison group, and often incorporate more limited data collection. These include studies with a single group that uses a post-test, or pre- and post-test and studies with two groups that are not randomly assigned, statistically matched, or controlled that use post-test or pre- and post-test comparisons. They also include approaches that address causality through alternative methods, such as intensive case studies, general elimination methods, and reflexive control designs (e.g., single pre-test with multiple post-tests).

Specific Guidance: Pre-Experimental Designs

If an evaluation using a pre-experimental design and targeting preliminary evidence is proposed, the evaluation plan should:

- Provide justification for its use and detail reasons why an impact evaluation with fewer threats to internal and external validity cannot be conducted;
- Describe the full study design in detail along with any assignment to groups, and outline the potential of non-equivalence of groups;
- Explain the treatment and any counterfactual conditions, as needed;
- Provide details of any additional threats to internal and external validity of the design; and
- Indicate plans, if any, to conduct an impact evaluation meeting criteria presented above, with attention paid to the ways in which the proposed research study will inform and facilitate this future research.

Feasibility Studies

In some evaluation plans, in conjunction with an implementation evaluation and/or with an outcome or impact evaluation, a program may conduct a feasibility study in preparation for conducting a more rigorous evaluation. Such a study might be appropriate if a program is developing new delivery mechanisms, creating additional content, or expanding services to a new organization or institution. A feasibility study is a preliminary study designed to test measures, instruments, data collection techniques, or other aspects of an outcome or impact evaluation.

Specific Guidance: Feasibility Studies

Describe the barriers to implementing a full-scale study yielding moderate or strong evidence and provide a complete description of the feasibility study to be conducted, including, as appropriate, study elements related to a description of the program, engagement with stakeholders, or assessments of data availability and quality. The evaluation plan should address the plans to test the practicality of the use of a control or comparison group or other counterfactual. Describe how the feasibility study prepares the program and the evaluators for planning, implementing, and completing a more rigorous evaluation.

Combination of Designs and/or Analyses

In some situations, using a combination of several study designs may be useful to capture the evidence of effectiveness of the program. For example, a small scale RCT may be used alongside a larger matched comparison study with groups formed by propensity score matching.

Specific Guidance: Combination of Designs and/or Analyses

If multiple study designs or analyses are combined, the proposal should provide clear details of all components, in accordance with the specifications provided above. Further, the proposal should provide a rationale for using a combination approach, with a focus on how using multiple analyses offsets threats to internal and external validity.

Note that the design types discussed above are not intended to be a full list, and other design types and combinations of design types appropriate to your program and evaluation goals are welcome.

Implementation Evaluation

Implementation evaluation is an assessment of how well a program does what it sets out to do. Rather than focusing on the *outcomes*, however, implementation evaluations focus on the *process* by which a program provides services or otherwise accomplishes its mission. Implementation studies center on discerning how closely the actual running of the program matches the theory that generated both the program in general, as well as the particular components that participants experience.

Additional Resources

See Weiner (2004) for information on implementation evaluation research design.

Specific Guidance: Implementation Evaluation

Implementation studies are strongly encouraged as part of any evaluation plan. The program theory and logic model should guide the implementation evaluation. An implementation evaluation examines both how well the program matches the theory behind its creation, and what the program actually does on the ground. In addition, an implementation evaluation strives to uncover the exact processes that lead to outcomes. The plan should measure variation in implementation within the program as well as across program sites, as warranted.

The plan should assess both the quantity and quality of services delivered to the target population. Additionally, the plan should examine if the program is indeed serving the people for whom it was intended, and if appropriate, if there is a control or comparison group who may or may not be receiving similar services through another program or programs.

If the evaluation plan includes a comparison or control group, the plan should include a way of assessing whether or not the comparison or control group was in any way exposed to the program components. Ideally, the plan should include a way of determining the amount the program may have diffused in a given area to establish how much non-participants may have been exposed to aspects of the program. Include in the plan ways of estimating differences between program participants and control or comparison group members in terms of access to program components and delivery of services.

The implementation evaluation plan should include specific measures that will be used to assess how the program was implemented. The plan should address how data will be collected regarding service provision, program participants, and the control or comparison group, if applicable. Include specifics about the types of data to be collected, how they will be collected (such as data collection instruments), and how they will be analyzed.

Sampling, Measures, and Data Collection

Sampling

The sampling plan will provide a complete description of who will participate in the study and how they will be selected. A well thought out sampling plan can help prevent problems with internal and external study validity.

A power analysis is a calculation that estimates, given a specific sample size and analysis design, how likely it is that a program effect will be significant. There are different techniques available for calculating a power analysis, and exactly which power analysis formula to be used will depend on the details of the study, including the amount and types of information collected (the independent variables), and the desired level of explanation the study hopes to provide (the size of the expected R-squared). Power analysis calculators for different techniques, such as ANOVA and regression analyses, are available online.

Additional Resources

More details, and instructions for calculating statistical power, can be found on the UCLA statistical computing software website: http://www.ats.ucla.edu/stat/dae/

See Bloom (2005) for more information on sample size and MDES in evaluation research.

A common way of demonstrating statistical power is to provide the calculation of the Minimum Detectable Effect Size (MDES) that has an 80 percent chance (the conventional level used in evaluation research) of being statistically significant at a specific level of significance (alpha level). The MDES is a calculation designed to detect the smallest effect statistically notable in the study. MDES calculations help researchers determine if the findings from a study of a particular sample size are to be believed, statistically speaking. In practice, MDES helps determine what effect the size of the sample – the number of participants (or groups or sites) – will have on statistical calculations. It also helps researchers examine if the effects seen in the study group are significant given the sample size (Bloom, 2005).

Specific Guidance: Sampling Plan and Power Analysis

Sampling Plan

First, describe the population to which the study results will be generalized - that is, the group of individuals to whom the study results would apply. This might include details such as the social, economic, or demographic characteristics (race, gender, etc.), at-risk status, or geographic location of the population. Importantly, the evaluation plan should demonstrate that the sample that will be drawn will be representative of that population.

To do this, describe exactly how the participants who will be in the sample will be selected from the entire possible population of participants. That is, participants might be selected through one of several techniques: simple random sampling, stratified random sampling, convenience sampling, or some other sampling procedure. The sampling plan, which describes the technique and includes the size and composition of the sample, should reflect the budget and timeline of the project.

Power Analysis and Minimum Detectable Effect Size

The evaluation plan should include a power analysis and MDES calculations for the proposed research design and estimated sample sizes. These calculations should capture whether the sample used in the evaluation will be large enough for the impact analysis to provide meaningful results. A power analysis and MDES calculation should be presented for each analysis in the evaluation plan, including all sub-groupanalyses. Power analyses can be conducted using the free programs R and G*Power, as well as SPSS, SAS and STATA. For designs that have multiple levels of analysis or assign treatment and control at the group, rather than individual level, the free Optimal Design Software (http://sitemaker.umich.edu/group-based/optimal_design_software) can be used to estimate power and MDES.

Sample Retention

To ensure that the evaluation is as strong as possible, it is important to try to maximize the number of participants (or groups or sites) who take part in the study. This means not only recruiting participants, but also making sure that as many people as possible are part of data collection efforts during the study, as well as in any follow-up period. This is true for program participants, as well as for any control or comparison group members.

Specific Guidance: Sample Retention

The evaluation plan should describe how the study will keep track of participants and their data (ideally, when generating the sample specifications and data collection strategy). This plan should include mechanisms for monitoring, tracking, and troubleshooting issues related to keeping participants in the study.

These practices might include systematic checks of data collection practices to ensure that data are being collected on a regular basis from all participants, thus helping identify any participants who have not been contacted in a given period of time. This could also include having strategies in place for following up with participants who are not responding or participating in either program or data collection activities. It is important to remember that individuals who leave a program prior to the completion of all components still need to be tracked as part of the data collection efforts. All participants, and control or comparison group members, are ideally tracked and provide data during the entire study period, regardless of program participation, location, or ease of access.

Measures

Ensuring that the measures selected are reliable, valid, and appropriate to use for the study is a key way to reduce threats to internal validity caused by the study itself. Selection of poorly validated or unreliable measures can lead, for example, to an effective program showing no results.

Reliability refers to the generalizability of a particular measure. That is, if a measure is used over and over again on the same group of people, it should yield the same results. Validity refers to the extent to which a measure adequately reflects the concept under consideration; if a measure matches the concept in question, most respondents will interpret it in similar ways. Appropriateness refers to matching measures to the population or scale of a study. To illustrate, measures that require literacy should not be used with young children or under-educated populations. Multiple ways exist to test validity and reliability of measures; see May et al. (2009) or the University of Leeds' (2011) guide to designing questionnaires for more information.

Specific Guidance: Measures

The evaluation plan should provide a clear indication of how each measure aligns with the outcomes in the logic model. As such, each outcome should be clearly defined as a confirmatory measure, or an exploratory measure (see above for definitions of 'exploratory' and 'confirmatory' research questions). In-depth detail regarding each variable to be measured should be provided. If the variable is to be measured by a survey, test, interview, or structured observation, describe, to the extent feasible, the following:

- The intended respondents;
- The proposed administration method;
- The number of questions included;
- The anticipated administration time;
- How the questions are organized and worded;
- The response categories used (if appropriate);
- Potential score/response ranges; and
- Typical measure distributions in a general sample (if available – such information allows evaluators to detect any potential mismatch between analysis technique and the proposed measures).

Additional Resources

More details on generating reliable, valid, and appropriate measures, see May et al. (2009) "Using state tests in education experiments: A discussion of the issues" at http://ies.ed.gov/ncee/pdf/20090013.pdf.

For information specifically on questionnaire design, see the University of Leeds "Guide to the design of questionnaires" at http://iss.leeds.ac.uk/downloads/file/125/guide_to_the_design_of_questionnaires

If the measure has already been developed and tested, then its origin should be described, including relevant citations. Of particular importance is whether the measure was developed by the researchers themselves, or if it is a commercially available measure.

If the measure is part of an existing data set (e.g., program records of service provided, medical test results, financial data), describe the following:

- The proposed data source (e.g., program records, public data sets, patient records);
- When the data were collected and by whom;
- Who funded the original data collection; and,
- The type of data provided by the data source (e.g., Are financial/medical data provided for each participant or are they aggregated? Are data on program services provided available as counts, percentages, for individual services or for groups of individuals?).

If different measures of the same constructs are to be used for different portions of the sample, this should be clearly indicated and the implications this has for the analysis plan should be described.

If data are to be provided by a third party, such as a school, it is highly recommended that a letter of agreement from that third party be included in the evaluation plan, if available.

Specific Guidance: Measure Validity, Reliability, and History of Use

Describe validity, reliability, and history of use for all measures used in data collection activities. At a minimum, each survey, test or interview measure used should have evidence of the following:

- Reliability (e.g., test-retest reliability for measures with individual items, Cronbach's alpha for tests/scaled surveys);
- Face validity (e.g., pilot testing with appropriate populations, review by service providers); and,
- Content validity (e.g., review by content experts, systematic alignment with key areas to be measured).

Additional Resources

See Pett, Lackey and Sullivan (2003) for a discussion of exploratory factor analysis. See Brown (2006) for more information on confirmatory factor analysis.

In addition, surveys or tests that use scales or generate scores should provide details of exploratory factor analysis (Pett, Lackey, & Sullivan, 2003), confirmatory factor analysis (Brown, 2006), and validation against outcomes, as well as history of use, if available.

If reliability and validity of measures have yet to be determined, an analysis plan for doing so should be presented. If non-validated, self-developed measures of key outcomes are to be used, provide a justification for the use of this method over any pre-existing and pre-validated measures.

Data Collection Activities

A systematic plan for collecting data for the evaluation must be in place to ensure strong results. The measures indicated in the evaluation plan dictate the data collected. Data for participants and for control or comparison group members may come from a variety of sources, not all of which may be part of the program.

Data collection ideally starts prior to program participation to best capture participants' baseline status. In the same vein, identical data from both participants and control or comparison groups should be collected whenever possible. Data collection often continues even after participants are no longer part of the program. It is important to map out the timing of data collection to ensure the best possible evaluation.

Specific Guidance: Data Collection Activities

Identify data sources for all measures, as well as the type of data to be collected along with how they will be collected. Descriptions should cover data for program participants as well as control or comparison group members.

Establish a baseline status for program participants and control or comparison group members. Baseline data are important for assessing change in participants over time. Describe when the collection of baseline data will occur. Also, discuss whether the baseline measures being used are comparable for both program participants and control or comparison group members.

Data may be collected by the evaluation team, by project staff, or by some other party. Indicate who will collect all data used in the evaluation. Note the role of project staff members in relation to the data collection process. If administrative data will be used, specify the source(s) and availability of the data, and the evaluation team's experience working with that type of information.

Describe the way in which data are to be collected. Specify if data will be collected through administrative records, program systems, or through instruments specifically created for the purpose. For example, will participants complete surveys, will project staff maintain records that will be used, or will administrative data such as academic transcripts be used? It is likely that some combination of efforts will be required to capture all the data the measures warrant.

The way in which data are collected may not be identical for both program participants and control or comparison group members, but ideally the same data will be collected across both groups. Indicate any differences in data collection between the two groups, including sources, means of data collection, and who will collect the data.

Finally, include a timeline outlining the data to be collected at various points (for example, what data are collected prior to program participation, during, and after). Indicate expected sample sizes at various points in time in the data collection process.

Statistical Analysis of Impacts

To ensure the strongest possible evidence results from the evaluation, the correct statistical analysis techniques must be employed. The statistical technique chosen will depend on the types of research questions and outcomes or impacts specified in the research design; they will also depend on the type(s) and quantity of data collected. For example:

- Studies that collect data across time will need to use statistical models that are appropriate for such data (e.g., fixed effects models);
- Research designs with comparison groups may need to statistically control for attributes of group members (e.g., regression models); and
- Designs that involve nested data, such as students within classrooms will need models that handle that type of interaction (e.g., hierarchical linear models).

Different types of data will also necessitate different statistical models. For example, determining if someone uses more or less service after program participation implies that the outcome measure varies along a continuous spectrum (i.e., number of visits or service utilizations). This type of question would require the use of a model that fits this type of data and that can also adequately take into account other factors besides program participation (ideally) to ascertain the impact of program participation on the number of visits (e.g., a linear regression or related model). Conversely, if the outcome of interest is whether or not a student enrolled in college, the outcome has only two categories (enrolled or did not enroll), and the statistical model must be appropriate for that type of categorical data (e.g., a logistic regression or related model).

Additional Resources

See Schochet and Chiang (2009) for information on causal inference and instrumental variables framework in TOT randomized clinical trials.

More details on specific, commonly used statistical models are detailed in Table 1 in Appendix C (Examples and Templates) of this document.

Intent to Treat (ITT) and Treatment on Treated (TOT) Analysis Frameworks

Two main perspectives exist for guiding analysis in evaluations. If the proposed study has a randomized between-groups design, an intent-to-treat (ITT) framework is suggested (though not required). This framework starts with the premise that analytically, evaluators are assessing outcomes based on program components rather than participant experience. This framework is useful because it (1) requires the evaluator to compare the outcomes of participants (or groups of participants) between those who were assigned to receive program services and those who were not assigned to the program, and (2) generates an unbiased estimate of the average program effect for participants offered the program. The ITT framework is often used because the impact estimate is the one that is often of most interest to policymakers (Bloom, 2005).

However, in practice, interventions are more likely to be implemented with varying degrees of fidelity to the intended implementation plan. Similarly, levels of participation can range from participants not taking part at all to those completing all aspects of a program. The unbiased estimate from an ITT analysis framework will reflect this range. For this reason, the impact estimate from the ITT is often considered the most policy relevant because it is based on the experimental structure of the data (i.e., results depend upon either participating, at any level, or not at all).

Additional Resources

More information on and examples of statistical analysis can be found on the UCLA statistical computing website: http://www.ats.ucla.edu/stat/examples/default.htm.

The treatment-on-treated (TOT) analysis, which is based on what participants actually experience, must typically be obtained from an extension of the non-experimental structure of the data (Bloom, 2005). Moreover, the unbiased estimate of a program's effect on an outcome can be compromised by non-random missing data on the outcome. This possibility, and how it will be handled if it occurs, should be addressed in the evaluation plan under missing data.

Conversely, TOT analysis typically requires the evaluator to analyze data collected on individuals based on their level of program participation. Because program participants self-select their level of program participation, analyses that estimate program impacts according to these levels are by nature quasi-experimental.

Specific Guidance: Data Analysis

Open the statistical analysis section of the evaluation plan by describing the ITT analysis framework and then describing if this framework will be supplemented with another framework, such as TOT. For designs that form groups through other approaches, such as matching, the principle of beginning with an ITT framework still applies and should be reflected in the evaluation plan. For example, when matching is used to form groups prior to program delivery, individuals (or groups of individuals) should be analyzed in the groups they were in at the time of matching.

Provide clear details of the data analysis used to determine program effects. Describe the types of statistical analysis to be undertaken, specifying descriptive analysis and/or inferential analysis as appropriate. Include descriptions of statistical models. Note the covariates – the characteristics or variables that will be used in the model – in the discussion, and clearly delineate the sample (full or partial; including anticipated size) to be used for each analysis.

List and describe the statistical procedures to be used in analysis (for example, describe the OLS regression, logistic regression, ANOVA, or whatever model fits the analysis plan best). Table 1 (in the Examples and Templates section [Appendix C] at the end of this document) lists common statistical models used in evaluation design along with examples of their application. The reasons why procedures were selected, including the study design and outcome(s) considered, should also be described. Importantly, the statistical procedures proposed must align with the research questions, and correspond to the power analysis used to determine the minimum sample size.

The evaluation plan should be clear on the level of statistical analysis (e.g., individual or site) and this analysis should be at the same level as the random assignment or matching (e.g., individual or site). The analysis needs to match the level of assignment in order to ensure that evaluators do not make an erroneous inference about the statistical significance of the program effect (i.e., ascribing a treatment impact on individuals while analyzing program sites in aggregate). Further, the evaluation plan should include the specification of the statistical model used to estimate the program effect, including the addition of any covariates, weights, or other adjustments. Provide the assumptions made in the model and indicate whether any of these assumptions are likely to be violated. For quasi-experimental and pre-experimental studies, the evaluation plan should also include a detailed discussion of the statistical methods used to control for selection bias on observed characteristics correlated with the outcomes of interest.

Missing Data

It is to be expected that some participants (or control or comparison group members) may drop out of the study, the program, or otherwise become impossible to contact. This creates "missing data," or holes in the collected data, that need to be dealt with in order for a statistically sound analysis to take place. When researchers and evaluators refer to missing data, they generally are referring to information that was not collected, but could have been.

Missing data can be a problem when trying to understand the effects of a program. For example, if only half of all participants complete an entire program, but all of them show positive change, it remains unclear if the impact is due to the program or if it is due to the characteristics of the people who completed the program. If nothing is known about the people who did not complete the program, it would be difficult to say with certainty that any change found among participants was due to program participation.

Specific Guidance: Missing Data

The evaluation plan should describe how attrition rates will be calculated, both for the study as a whole (in general, taking into account the number of participants who were continuously part of the study versus the number of participants who were part of the study only during a particular, truncated period of time). Similar to the overall attrition rate, differential attrition rates, or the rates at which particular subgroups (e.g., men versus women) continue to take part in the study or not, should also be addressed in the plan. The plan should describe how rules will be constructed for deciding how long participants have been in the program in order for them to count as having completed the program or not, or, if appropriate, what constitutes the various amounts of program exposure or completion.

To illustrate, it may prove useful to compare participants who completed an entire program with participants who completed part of the program, as well as the control or comparison group to better understand how different amounts of exposure or participation affect outcomes. Keeping track of both absolute and differential attrition can also be helpful for implementation evaluation, too. That is, knowing when participants leave a program may help identify any strengths or weaknesses of the program.

Sometimes, but not in every case, adjustments are made to data to adjust for biases related to missing data. Describe any procedures planned for adjusting data to assess and/or deal with these biases in the data. In particular, any use of multiple imputation, the replacement of missing data with substituted values, should be explained fully and clearly. Data on outcomes of interest should never be imputed, however.

Specific Guidance: Multiple Outcome Measures

Describe the ways in which the evaluation design will take into account the use of, and potential problems related to the use of, multiple outcome measures. When multiple outcome measures are specified, evaluators need to be careful of errors that may develop in comparing them. If there are multiple related confirmatory questions, or a single confirmatory question evaluated using multiple outcomes, detail the adjustments to be made to analyses to reduce the likelihood of a Type-I error (incorrectly rejecting a hypothesis that is in fact confirmed) occurring. Techniques that can be used to adjust for such a possibility are the ordering of multiple outcomes (primary, secondary, etc.) and accordingly adjusting the p-values for each outcome, the use of a statistical procedure such as a Bonferroni adjustment, or using MANOVA if there are multiple outcomes.

Additional Resources

See Blakesly, et al. (2009) on multiple outcome measures in evaluation at http://www.ncbi.nlm.nih.gov/pmc/articles/PMC3045855/.

V. Human Subjects Protection

In general, SIF-funded evaluations (and other federally-funded evaluations) will require either Institutional Review Board (IRB) approval, or an explanation of why IRB approval is not needed/appropriate for the proposed study. Evaluation plans generally need to be designed to meet standards for human subjects protection if they use individual level data collected from people (whether participants, controls or comparison groups) that could be personally identifiable. If the evaluation collects new data from human subjects, it is likely that the evaluation plan will need to include how the program or evaluator will obtain approval for the data collection process from an IRB. If the data has already been collected, for example, from a previously existing survey, IRB approval may not be needed. However, it is always a good idea to obtain a waiver from an IRB stating that human subjects' protection was not needed. This is particularly useful if, in the future, the results will be submitted to certain peer-reviewed journals. IRB approval is not needed for data that is not collected at the level of the individual person. For example, financial data, housing completions, or jobs created at an organization would not need IRB approval to be collected or analyzed.

The specific information required by an IRB may vary, however. Generally, the application will include a description of the study, sampling methods, data collection methods and instruments, and types of analysis to be conducted. If needed, it may include the justification for collecting data from vulnerable populations. If informed consent is required, the IRB application will likely ask for the type of consent, as well as procedures and documents to be used in obtaining informed consent. Finally, the IRB will be interested in how the personally identifiable data will be handled and securely stored, as well as when and how the data will be destroyed.

Specific Guidance: Human Subjects Protection

State how Human Subjects Protection will be ensured and whether or not IRB approval will be necessary. If IRB approval will not be sought, explain why.

If the evaluation includes personally identifiable data, then the evaluation plan should include the following:

- The name of the IRB that will be used;
- The type of approval sought;
- The process used by the named IRB for securing the approval;
- What data will be subject to informed consent; and
- An overview of how informed consent will be obtained.

In addition, IRB approval generally lasts for a specific period of time, and that time should be described and conform to the evaluation timeline submitted.

The IRB approval process does take time and there may be associated costs as well. Check with the IRB you plan to use during your evaluation plan development to find out its timeline and whether there are costs. Include timing and cost information in the evaluation plan timeline and budget.

VI. Reporting Strategy, Timeline, and Budget

Overview

The reporting strategy, timeline, and budget are critical components of the evaluation plan. The process of aligning these components with the technical aspects of your evaluation design will help you determine whether your evaluation is actually feasible. In addition, they provide the reader with key information regarding how the plan will actually be implemented and monitored.

Reporting Strategy

The focus of an evaluation reporting strategy is to ensure that timely information is available for decision making at key points in the program's progress. These key points will likely align with the program logic model. While the sections above have outlined how the program will obtain and analyze information critical for assessing the implementation and outcomes or impacts of the program, the reporting strategy will explain how the information will be disseminated.

In developing the reporting strategy, focus on what key information is needed by decision makers including organization staff, as well as funders, board members and others invested in the outcome of the program. Then, develop a series of reports that meet these information needs and are aligned with the evaluation timeline (i.e., what data is available at those points). If the logic model permits, it is often useful to align the evaluation timeframe with the timing of information needs. That way, the evaluation can be the most useful. It is likely that at least a baseline and final report will be needed. Intermediate reports are valuable if there are significant monitoring or data collection points along the way. If the evaluation is multi-year, then an annual report might include the significant milestones for that year and could count as the intermediate report.

Since programs often are required to report to funders and boards on multiple evaluation activities, it is good practice for evaluators to complete reports of each component of the study as they occur (for both implementation and impact evaluation). This should be part of the evaluator's scope of work and will allow you to have the most recent information available for decision making, to monitor the evaluation, and to be prepared for reporting to grantors. Evaluation activity reports could include the following:

- Report summarizing the design;
- Summary of instrument development;
- Detailed description of how sampling was done, including numbers of controls and participants and any issue(s) encountered;
- Report on each data collection, including each instrument used, how many were collected, from whom (sites, controls, participants, number completed), and when; and
- Detailed report of data analysis conducted, including any issues that arose or deviations from the original analysis plan and summaries of results to date.

Specific Guidance: Reporting

List all reports that will be produced along with dates. Specify how each report aligns with the evaluation plan (e.g., baseline, intermediate, final) and what significant activities will be included (e.g., follow up data collection). List the major sections of the report and what they will cover. Be sure to report on implementation as well as impact evaluations.

Timeline

The study timeline, in coordination with the budget, serves as a crucial checkpoint for the practicality of the design. A detailed timeline can address how key study elements fit together. Key elements to consider incorporating are:

- Study planning;
- IRB;
- Sampling;
- Data collection instrument creation;
- Data collection;
- Analysis;
- Interviews with key staff;
- Report writing;
- Report deadlines; and
- Meetings with staff.

Additional Resources

A sample timeline is included in Appendix C (Examples and Templates) at the end of this document.

Specific Guidance: Timeline

Include a detailed and feasible timeline that lists all the major events in the evaluation and their start and end dates. At a minimum, list the major components used for budgeting. The timeline should, at minimum, include sections for design (including IRB clearance if needed), instrument development, sampling and assignment of participants and controls, data collection, data analysis, and report writing and dissemination.

Within these major components, subcategories for key tasks are useful for tracking and monitoring. For example, under data collection, you might list each data collection point such as baseline, intermediate, follow-up, and/or final data collection. For a multi-year evaluation, the timeline should show each year and the activities within that year.

The reporting schedule will need to conform to the reporting approach outlined above, with each report listed under the timeline section and ideally, sub-tasks for each report (outline, draft, review schedule, finalizing and dissemination).

Budget

Developing the evaluation budget is an important step in determining the feasibility of the plan. Identifying costs for each of the steps in an evaluation, if done thoroughly, can clarify not only what components are included in the evaluation and who is doing them, but also whether those steps are practical given the time and resources available to the project. The budget should reflect not only tasks, but responsible entities/individuals and the estimated time that tasks will take. During the budgeting process, it may be necessary to revisit the evaluation plan and revise or provide further details on how evaluators and program staff will carry out each part of the evaluation.

Additional Resources

Please see the Sample Budget and LOE in Appendix C (Examples and Templates) at the end of this document.

Specific Guidance: Budget

Time Frame

The evaluation budget should reflect both the time and costs that it will take to achieve the evidence goals outlined. If current guaranteed funding is only for part of that time, identify which portions of the budget are funded and which are projected.

Budget Components

The evaluation budget should contain hours and costs by person or position for each major step in the evaluation, and for any other services the evaluator is providing, such as technical assistance for project staff. Your budget should also identify any other direct costs associated with the evaluation including travel, printing, materials and supplies, and communications. Indirect and overhead costs should be included, either by providing separate lines for them, or by loading them onto direct costs. Your budget should note which of these approaches you will take.

It may be useful, particularly if your evaluation plan has many components, to develop (and include as an appendix) a Level of Effort (LOE) chart that identifies hours by task and sub-task, by person. Developing a detailed LOE chart can take time, but it provides a valuable tool to check that your plan is feasible. For example, an LOE chart can tell you whether your projected number of data collectors will have enough hours available to collect pre-implementation data before your program starts, or if you will need to have more data collectors working at the same time.

A note about budgeting for evaluations:

The amount of an evaluation budget will vary depending on the specific design selected, the number of sites and participants, the measures proposed, the timeframe for measurement, the type of analysis proposed, the number and type of reports, and other factors. Budgets from a group of 70 SIF evaluations received average 19% of the program budget and reflect in many cases very tight resource allocation. In general, evaluation budgets should be:

- Commensurate with stakeholder expectations and involvement;
- Appropriate for the research design used and key questions to be answered;
- Adequate for ensuring quality and rigor, and;
- In line with the level of program and organizational resources available.

Further, a review of evaluation and program budgets for 2010 and 2011 SIF intermediaries and information from their experiences and reflections indicate that:

- The rule of thumb ratios in use to date (i.e., between 5% and 10% of the total program budget allocated for evaluation) result in serious under-budgeting of evaluations seeking to address both impact and implementation. Available data indicate that between 15% and 20% is more realistic for single site quasi-experimental designs (QEDs) and randomized controlled trials (RCTs), with some designs (e.g., multisite RCTs, designs with intensive implementation studies) requiring 25% or more.

- In general, using a percentage of program budget is not an ideal method for allocating evaluation funds. Evaluation and program costs (costs associated with program staff's support of evaluation activities) should be considered in absolute dollar amounts as well as in relative terms. For example, you likely cannot conduct an evaluation that targets a moderate level of evidence as defined by the SIF for less than $75,000 per year.

- Evaluation costs and evaluation-to-program budget ratios vary based on the study design chosen and increase with designs that seek to establish causal impact.
- The price of evaluation goes up as the level of evidence desired goes up. Strong evidence is disproportionately more expensive. One driving factor is whether or not the study is conducted across multiple sites.
- All design types have the potential to be expensive.

VII. Evaluator Qualifications and Independence

Overview

Evaluator qualifications are critical to a successful evaluation that will strengthen the level of evidence. A single evaluator or a team of evaluators is fine, so long as all the necessary skills are covered. When selecting an evaluator, it helps if the evaluator has worked with similar programs and has demonstrated experience in conducting the specific type of evaluation described in the evaluation plan.

Specific Guidance: Evaluator Qualifications

This section should focus on the program evaluator and the evaluator's background and qualifications. Explain why the evaluator was selected, including the extent of the evaluator's experience with both the content area and the type of evaluation. This will likely include listing the evaluator's experience with similar interventions and with the type of RCT or QED that the evaluation is using (e.g., an RCT in which schools, rather than students, are randomly assigned to treatment or control). List the key people designing and overseeing the evaluation and ensuring its quality along with their education/training and type and years of experience.

Verify that the evaluator can handle the scale and size of the proposed evaluation. Provide at least one example of an evaluation that is similar in size, complexity, and number of sites. Discuss the experience the evaluator has in managing similar evaluation protocols (e.g., this type of sampling, data collection, analysis). If relevant, does the evaluator have the capacity to conduct an evaluation with multiple sites across a broad geographic area?

Specific Guidance: Evaluator Independence

Include information that describes the independence of the evaluator from the program. To achieve a high quality evaluation, it is important that the evaluator have enough independence to render an honest and unbiased opinion of the program's outcomes and impacts. However, it is also important that the project staff be able to provide the oversight needed to ensure that the evaluation meets expected standards. Finally, it is important that the evaluator not have conflicts of interest regarding the evaluation.

To address these issues, provide a description of the relationship between the program and the evaluator, and what steps are being taken to ensure independence and alleviate any apparent or real conflicts of interest. In doing this, it is important to explain the structure of the relationship between the intervention and the evaluation, including both the role of the program staff with respect to the evaluator. This includes determining whether the evaluator is hired by the program organization, and if that evaluator is an employee, consultant, or outside firm. In addition, what are the reporting and financial arrangements between the program and the evaluator? Who is responsible for giving direction and providing oversight?

Address whether or not there are conflicts of interest related to the evaluation. Conflicts of interest could be related to a part of the program, the evaluator, or the relationship between the two. For example, has the evaluator played a role in designing the program, or is the person supervising the evaluator also responsible for program implementation and success? If there are conflicts of interest, they should be disclosed and measures taken to mitigate them discussed.

In terms of oversight, include a description of who, at the program level, is reviewing evaluation plans provided by the evaluator(s) and their qualifications to do so. In addition, the description of oversight should discuss how the program will make sure that the evaluator stays on schedule, produces the required evaluation products (e.g., design, instruments, data, analysis, reports), and how the individual(s) providing oversight can determine the quality of the products and whether they meet standards of scientific evidence and conform to rigorous requirements such as those expected by the SIF.

Finally, the section on evaluator independence is the place to explain how the findings will be released. What are the roles of the program in releasing the evaluation findings? Does the evaluator have the ability to release findings independent of the program? How will all parties ensure that findings are released in a timely manner?

Appendix A: References

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Appendix B: Resources

Resources for Further Reading

Sampling

Holmes, W. & Olsen, L. (2010). *Using propensity scores with small samples*. Presented at American Evaluation Association Conference. http://www.faculty.umb.edu/william_holmes/usingpropensityscoreswithsmallsamples.pdf

Kiernan, N. E. (2009). *Sampling a diverse audience: Tipsheet #58*. University Park, PA: Penn State Cooperative Extension. http://extension.psu.edu/evaluation/pdf/TS58.pdf

Pierce, S. (2010). *Fundamentals of power analysis and sample size determination*. Presented at American Evaluation Association Conference. http://comm.eval.org/EVAL/Resources/ViewDocument/?DocumentKey=e0e70bbc-0e05-49fd-9074-3f1d69fe33d5

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University of Reading Statistical Services Centre. (2000). *Some basic ideas of sampling*. Reading, United Kingdom. http://www.reading.ac.uk/ssc/n/resources/Docs/Some_Basic_Ideas_Of_Sampling.pdf

Evaluation Approach and Data Collection

Greenseid, L. (2011). *Conducting high quality surveys*. Presented at American Evaluation Association Conference. http://comm.eval.org/EVAL/Resources/ViewDocument/?DocumentKey=b4ea6ba2-ffaf-492c-a484-99d82f7135c5

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Measure Creation, Validity, and Reliability

Brock, D. J. (2011). *A process for determining the acceptability of measurement tools: How to decide?* http://comm.eval.org/EVAL/Resources/ViewDocument/?DocumentKey=6bccff9f-d3a2-4e23-a148-aaa3f081c6c3

Cui, W.W. (2003). Reducing error in mail surveys. *Practical Assessment, Research & Evaluation, 8* (18). http://pareonline.net/getvn.asp?v=8&n=18

Kiernan, N. E. (2001). *Designing a survey to increase response and reliability: Tipsheet #53*. University Park, PA: Penn State Cooperative Extension. http://www.extension.psu.edu/evaluation/pdf/TS53.pdf

University of Leeds. (2011). *Guide to the design of questionnaires.* http://iss.leeds.ac.uk/info/312/surveys/217/guide_to_the_design_of_questionnaires

Human Subjects Protection/IRB

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Maintaining Sample Datasets and Handling Missing Data

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University of Reading Statistical Services Centre. (1998). *Data management guidelines.* Reading, United Kingdom. http://www.reading.ac.uk/ssc/n/resources/Docs/Data_Management_Guidelines.pdf

Multilevel and Hierarchical Linear Modeling

Kalaian, H.A. & Raudenbush, S.W. (1996). A multivariate mixed linear model for meta-analysis. *Psychological Methods, 1*(3), 227-235. http://www.ssicentral.com/hlm/techdocs/KalaianandRaudenbush96.pdf

Raudenbush, S.W., & Sampson, R. (1999). Assessing direct and indirect effects in multilevel designs with latent variables. *Sociological Methods & Research, 28*(2), 123-153. http://www.ssicentral.com/hlm/techdocs/smr99.pdf

Full Review Checklist

Executive Summary

The following items are briefly described:

□ The program and intended outcomes/impacts;
□ The prior research;
□ The targeted level of evidence;
□ The evaluation design including comparison/control group approach;
□ The measures/instruments;
□ The proposed analysis approaches;
□ The evaluation timing/timeline and budget; and,
□ The evaluation team.

Program Background and Problem Definition

□ The policy issue or problem the program is designed to address and why it should be addressed is succinctly described.
□ The issue or problem is framed based on a review of research.
□ The program model is described briefly in the introduction, and includes key information such as who participants will be, the intervention level, and key outcomes.

Overview of Prior Research

□ Prior research conducted on this program is succinctly described.
□ Sufficient information is provided to identify the prior level of evidence attained by the program.
□ Prior research conducted on similar programs is succinctly described.
□ Sufficient information is provided to identify the prior level of evidence attained for similar programs.

Logic Model

□ Both a narrative and a graphical display that follows the chain of reasoning are included.
□ The logic model concepts, including all outcomes to be measured, are clearly defined.
□ How the resources and activities lead to the outcomes is described.
□ Only aspects directly related to the theory of change are included.

Impact Questions

□ Program impact questions that the study will address are clearly stated.
□ The questions are consistent with the logic model's stated outcomes.
□ If there are multiple questions related to the same outcome, confirmatory and exploratory questions are clearly defined.
□ The confirmatory research question(s) represent(s) the main impact question(s) for the primary outcome that the study can address with a known level of statistical precision (based on statistical power).
□ Questions address programmatic activity in the program participant group and, when feasible, the comparison or control group.
□ Questions are phrased such that groups can be compared to understand net effect of intervention and differences.
□ Impact questions do not include implementation questions.

Implementation Questions

☐ Program implementation questions are clearly stated.
☐ When feasible, the implementation questions should address the level of program-like services received by the comparison group.
☐ Implementation questions do not include impact questions.

Contribution of the Study

☐ The contribution to understanding that the evaluation will make is clearly stated.
☐ The level of evidence the program is targeting is described.
☐ How the proposed evaluation meets the criteria for this level of evidence is included.

Impact Evaluation Design Selection

☐ The SEP clearly identifies the study design selected.
☐ The description of the design draws upon previous research or literature, where available.
☐ The SEP presents a rationale for the design selected.
☐ The SEP justifies the target level of evidence based on a discussion of internal and external study validity.

Randomized Between-Groups Design (if applicable)

☐ Unit of random assignment is clearly identified (and aligned with the unit of analysis).
☐ Procedures to conduct the random assignment, including who implemented the random assignments, how the procedures were implemented, and procedures used to verify that probability of assignment groups, are described and generated by random numbers.
☐ Blocking, stratification, or matching procedures used—to improve precision in the estimate of the program effect or to balance groups on measured characteristic(s)—are described.
☐ The program group and to the extent possible, the control group conditions are described.
☐ Any concerns that proposed strategies or approaches will lead to nonequivalent groups are discussed.

Between-Groups Design- Formed by Matching (if applicable)

☐ Unit of matching is clearly identified (and aligned with the unit of analysis).
☐ Procedures to carry out the matching to form a comparison group are described.
☐ A precedent in the literature for including the variables used in the matching is included.
☐ Methods used to form the proposed comparison group are described such that the validity of the matching is explained.
☐ Reasons why the comparison group might differ from the treatment group and threaten internal validity, and the ways in which the proposed methods adjust for those differences, are discussed.

Between-Groups Design- Formed by Cut-off Score (RDD) (if applicable)

☐ Measure and cutoff score are clearly identified (and aligned with the unit of analysis).
☐ Cutoff score is clearly delineated and justified.
☐ Methods used to apply the cutoff score are described in detail.

Single Group Design (if applicable)

- □ Each intervention phase of the design, including the baseline condition, is clearly described.
- □ Number of measures during each measurement phase is detailed.
- □ Number of measures during each measurement phase is sufficient to establish trend and rule out rival explanations.
- □ Timing of measures pre/post interruption is appropriate to the intervention.

Interrupted Time Series Design (if applicable)

- □ Number of measurement points before and after the intervention is described.
- □ The number of measures during each measurement phase is shown to be sufficient to establish a trend and rule out rival explanations.
- □ The timing of measures pre/post interruption is shown to be appropriate to the intervention.
- □ Comparison cases are clearly described.

Pre-Experimental Design (if applicable)

- □ Full study design is clearly and comprehensively explained.
- □ Description of the treatment and counterfactual groups are included.
- □ Where appropriate, assignment of study participants to groups is described.
- □ Additional threats to the internal validity of the design are discussed.
- □ Ways future evaluations can be designed to rule out these threats are described.

Feasibility Study (if applicable)

- □ Full study design is clearly and comprehensively explained.
- □ Description of the treatment and counterfactual groups are included.
- □ Where appropriate, assignment of study participants to groups is described.
- □ The instruments or processes to be tested in the feasibility study are described.

Combination Design (if applicable)

- □ Clear details of all design components are provided.
- □ A rational for using a combined approach that identifies how that approach addresses threats to internal and external validity is discussed.

Implementation Evaluation

- □ Specific plans for measuring fidelity of program implementation (i.e., how well the program was actually implemented) in the program group are presented.
- □ Plans for measuring the level of program services the program group actually received, including the criteria for assessing whether an adequate amount and quality of the program was delivered to participants, are described.
- □ Plans for assessing whether the control or comparison group received program services, including the criteria for assess the extent to which there was diffusion of the program to the control or comparison group are provided.

Sampling Plan and Power Analysis Evaluation

☐ The size and composition of the sample is described and is consistent with the "Budget" section.

☐ Sampling plan is designed to select participants that are representative of the population from which they were selected.

☐ The target population from which the sample was selected and the sample selection procedures, including any geographic regions targeted are described.

☐ Statistical power is estimated and consistent with the study design.

☐ The statistical power analysis used to arrive at the sample size is described, and includes the minimum detectable effect size (MDES) that has an 80 percent chance of being statistically significant at a specific alpha level.

☐ Outcome(s) and assumptions used in the statistical power calculations are described.

☐ When there are plans to conduct analyses of subgroups, additional statistical power analyses are presented to estimate those MDES.

Sample Retention

☐ Strategies to recruit study participants are described.

☐ Alternative strategies that can be implemented if the initial strategy is unsuccessful are outlined.

☐ Plan describes strategies and incentives to recruit and retain study participants (e.g. remuneration for all participants). Justification is given for amount of remuneration.

☐ A management plan to monitor, track, and troubleshoot issues that arise in retaining the study sample during program implementation is described.

Measures

☐ How each outcome is aligned with the logic model.

☐ If there are multiple outcomes, as indicated by the logic model and the research questions, the plan differentiates between confirmatory and exploratory outcomes consistent with the logic model and research questions.

☐ How each variable from the logic model will be measured is detailed.

☐ For each measure, whether the measure will be developed or has already been developed (i.e., a commercially available, off-the-shelf measure) is explained.

☐ If the outcome measure differs across sites or groups/sub groups of sample members, how the different measures will be combined is described.

Measure Validity, Reliability and History of Use

☐ Information regarding each measure's reliability and validity (e.g., Cronbach's alpha), and validation against outcomes as well as historical use, if available, is provided.

☐ If reliability and validity of measures have yet to be determined, an analysis plan for doing so is presented.

Data Collection Activities (for each data source the SEP includes)

☐ A description of baseline measures is provided.

☐ The timing of baseline collection relative to start of the intervention is described.

☐ Whether baseline measures are adequate for assessing baseline equivalence is explained.

☐ A description of who will collect the data is included.

☐ A description of the role of staff members delivering the intervention with regard to data collection is described.

☐ The timing of the data collection, relative to delivery of the program is explained.

☐ Discussion of how the data will be collected is included.

□ Discussion of whether the mode of data collection is the same for the intervention and control groups is included.

□ If administrative records (e.g. school academic/truancy records; unemployment insurance data) will be used, the source and availability of these data as well as the evaluator's experience using these data sources are described.

□ Expected sample sizes at each data collection point are included.

Statistical Analysis of Impacts

□ If a between-groups design is planned, an Intent-to-Treat (ITT) analysis is described, which compares outcomes between those assigned program services and those not assigned.

□ A clear description of the steps of the analysis is provided.

□ How the statistical analysis of the data is aligned with the research questions is explained.

□ How the statistical analysis is aligned such that the unit of analysis corresponds to the unit of assignment is described.

□ The statistical model used to estimate the program effect is fully specified and all variables in the model (and their coefficients) are defined.

□ Assumptions of the model are listed.

□ The program effect model is shown to be consistent with the statistical model used for the statistical power analysis during the design stage.

□ Model estimation procedures are included.

□ If applicable, covariate adjustments to estimated program effects, adjustments for missing data, estimation of standard errors, and corrections for multiple comparisons are described.

□ If using a non-randomized between-groups design, statistical methods that adequately control for selection bias on observed characteristics are described.

Sample Retention and Missing Data

□ How overall and differential attrition will be calculated and assessed is detailed.

□ An outline of specific procedures planned for *assessing* and *adjusting* for potential biases, due to non-consent and data non-response, is included.

□ Any intentions to use multiple imputation for missing data are discussed. This imputation should match the analysis to be conducted (the imputation model should use multi-level procedures if the analysis is multi-level; if the statistical analysis uses maximum likelihood or Bayes to incorporate missing data patterns, then this should also be noted).

□ A brief description of how the plan is designed to minimize missing data with particular focus on minimizing differential attrition is provided.

Multiple Outcome Measures

□ If the proposal has multiple related confirmatory research questions, or a single confirmatory question evaluated using multiple outcomes, adjustments made to reduce the chance of a Type-I error are described.

Human Subjects Protection

□ If the researchers will work with personally identifiable data, procedures to secure IRB approval are discussed. Plans requiring IRB approval should include:

□ IRB(s) that will review the submitted application;

□ Type of approval sought and process for securing this approval;

□ Duration that the approval will be in effect, and expected approval date; and,

□ Timeline for when and how this approval will be secured.

□ Plans that do not require IRB approval explain why they do not think approval is necessary.

Reporting

- □ The reporting strategy lists the reports that will be submitted, their timing and content.
- □ Reports include at least a baseline and final report.
- □ For multi-year evaluations, an annual report is included.
- □ The content of the reports covers all the major evaluation events/activities, their results/findings, and recommendations.

Timeline

- □ The timeline notes the period for design (including IRB clearance if needed), baseline data collection, pretesting, random assignment, intervention implementation, post-testing, and each follow-up period, if applicable.
- □ A timeline for data collection, follow-up, analysis, and reporting is included.
- □ Schedule for collecting and reporting on intermediate outcomes that are aligned with logic model and evaluation design is included.
- □ IRB approval is included in the timeline.

Budget

- □ A list of key evaluation and program staff/consultants, including roles and total number of hours, is provided.
- □ Approximate costs by major components of the study (e.g., planning and project administration, instrument development, IRB approval, sampling, data collection, data analysis, report writing, presentations), by year, are included. Please refer to the "Examples of Budget" in Appendix C.
- □ Unfunded, but anticipated, evaluation years are included as estimates.
- □ IRB costs are included in the budget.
- □ The budget is aligned with the research design and target level of evidence.

Evaluator Qualifications

- □ The extent to which the evaluator/team has sufficient capacity to conduct the proposed evaluation, in terms of technical experience and the size/scale of the evaluation, is described.
- □ The evaluator/team's prior experience with similar evaluations is described.

Evaluator Independence

- □ Steps that will be taken to ensure the independence of the evaluation are explained.
- □ The role that the program will play in overseeing the evaluation is described.
- □ The process to ensure that the evaluator's findings are released in a timely fashion, and, when the evaluation is completed, the extent to which the evaluator will be able to release the findings without prior approval by the program/organization is described.
- □ How the initial release of the results will be handled by the evaluator and what the program/organization role will be in disseminating the findings is explained.
- □ Discussion of the review process that will be used to ensure that the evaluation meets standards of scientific evidence is included.
- □ Any potential evaluator or program conflicts of interest are explained.

Appendix C: Examples and Templates

This section contains examples of evaluation plan components referenced in the Detailed Guidance section. These examples are not mandated, but are provided for your reference. Please adapt, as appropriate, to your evaluation plan.

Prior Evidence of Program Effectiveness- Sample Table

Title, Author, Publication Year and Source of Study	Peer Review (Y/N)	Date of Data Collection	Study Type (RCT, QED, Outcomes only)	Sample Size	Sample Description	Outcome Measures	Key Study Results	Effect Sizes	Similarity to Proposed Program
Effects of After School Character Education on Reading Scores, Smith, R.L. and Doe, J.M. 2002 Private evaluation available at www.eval.org	N	1999-2000	RCT	400 T 300 C	Elementary School students grades 2-5, in a district with 65% Hispanic population.	CTBS, Participant and Parent Surveys	Parents of participants were more likely than non-participants to report that their children had improved reading skills. No significant difference was found on test scores.	.2 for parent group differences N/A for children	This study focused on the same intervention at a different site, with a different population.
"Our Annex" 2009 Program Evaluation Report, 2009	N	2010	Outcomes	80	Elementary School students in a district with 35% African American and 35% Hispanic population	TOWRE Participant Surveys	Tested children gained an average of 2 grade levels on the TOWRE	Not calculated	Study of the same intervention conducted at the same program site

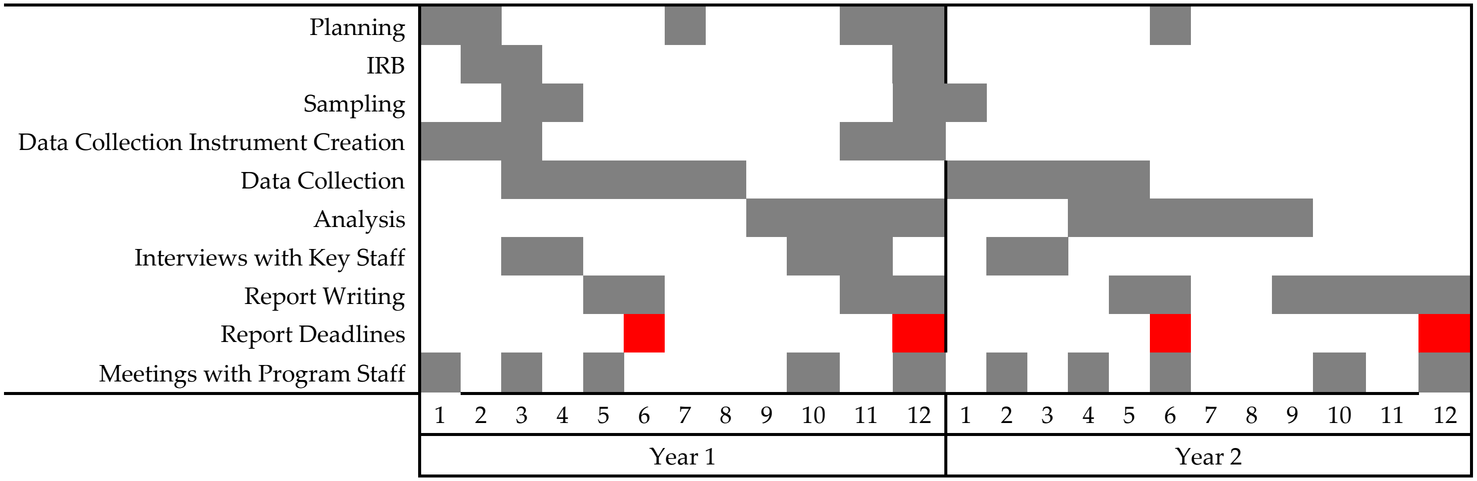
Logic Model Example

Sample Logic Model: Sample Program, College Preparation "Go College!"

Inputs	Activities	Outputs	Outcomes	Impacts
Collaborating schools and staff Go College! board Go College! leaders and national staff Financial support for Go College! from foundations, private individuals, businesses, and government Data system to track students' progress and provide ongoing feedback	Quarterly leadership workshops for peer leaders Tailored professional development for teachers and counselors Year-long class for seniors on college/ postsecondary planning Go College! program for ninth through eleventh-graders	30 Peer leaders trained 30 Teachers and counselors receive 40 hours of professional development 200 seniors attend the year-long class 600 ninth through eleventh graders participate in the Go College! programming	Staff and students increase expectations for students' high school academic achievement Staff and students increase expectations for postsecondary education Seniors gain knowledge about the college application and enrollment process	School-wide college-going culture that looks beyond high school graduation Increase in completed college applications and in other milestones in the college application process Increase in high school attendance, persistence, and graduation Increase in college enrollment and success

Examples of Timelines

Sample Evaluation Timeline in Graphic Form



Sample Evaluation Timeline in Table Form

Task	Year 1	Year 2
Planning	Months 1-2	
IRB	Months 2-3	Month 1
Sampling	Months 3, 4, 12	Month 1
Data Collection Instrument Creation	Months 1-3, 11-12	
Data Collection	Months 2-8	Months 1-6
Analysis	Months 9-12	Months 3-9
Interviews with Key Staff	Months 3-4, 10-11	Months 2-3
Report Writing	Months 5-6, 11-12	Months 5-6, 9-12
Report Deadlines	Months 6, 12 (last day of month)	Months 6, 12 (last day of month)
Meetings with Program Staff	Months 1, 3, 5, 10, 12	Months 2, 4, 6, 10, 12

Examples of Budget

Example 1: Evaluation Budget

The template below is one approach to providing a detailed Evaluation Budget.

PART 1: Detailed costs

1. Planning and Project Administration	XX hrs
2. Instrument Development:	XX hrs
3. IRB Approval:	N/A
4. Sampling (including creation of control or comparison groups):	XX hrs
5. Data Collection:	XX hrs
6. Data Analysis:	XX hrs
7. Report Writing:	XX hrs
8. Presentations:	XX hrs

Costs by Evaluation Component: Evaluator, Project Staff, and Others

Year 1	Personnel (staff and contractual)	Other Direct Costs
Major Evaluation Component	Evaluator: TBN Evaluation Coordinator: TBN Research Manager: TBN	Travel, printing, materials, supplies, communications, participant honoraria
Planning and Project Administration		
Instrument Development		
IRB Approval		
Sampling		
Data Collection		
Data Analysis		
Report Writing		
Presentations		
Total Costs		

Year 2	Personnel (staff and contractual)	Other Direct Costs
Major Evaluation Component	Evaluator: TBN Evaluation Coordinator: TBN Research Manager: TBN	Travel, printing, materials, supplies, communications, participant honoraria
Planning and Project Administration		
Instrument Development		
IRB Approval		
Sampling		
Data Collection		
Data Analysis		
Report Writing		
Presentations		
Total Costs		

Year 3 **(Provisional based on Grant Renewal)**	Personnel (staff and contractual)	Other Direct Costs
Major Evaluation Component	Evaluator: TBN Evaluation Coordinator: TBN Research Manager: TBN	Travel, printing, materials, supplies, communications, participant honoraria
Planning and Project Administration		
Instrument Development		
IRB approval		
Sampling		
Data Collection		
Data Analysis		
Report Writing		
Presentations		
Total Costs		

PART 2: Budget justification/narrative

Narrative providing the explanations needed to understand the budget. The budget narrative should begin by indicating what percentage of the total SIF sub grant (including matching funds) is being dedicated to evaluation, as opposed to other activities.

Example 2: Evaluation Budget

The template below is another approach to providing a detailed Evaluation Budget.

Cost by Evaluation Component: Evaluator, Project Staff, and Others
The template below is for Year 1 but the final evaluation budget should cover the full length of the evaluation (e.g. Year 2, Year 3, etc.).

Year 1 **Evaluation Components**	**Project Director/PI**	**(Staff or Consultant Name/ Position)**	**(Staff or Consultant Name/ Position)**	**(Staff or Consultant Name/ Position)**	**Total Hours**
Planning and Project Administration					
Evaluation Plan Design	(#Hours)				
(Other subcomponents, if applicable)					
Instrument Development					
(Subcomponents, if applicable)					
IRB Approval					
Sampling					
Data Collection					
Data Analysis					
Report Writing					
Presentations					
(Other Components, if applicable)					
Total Staff Hours					
Hourly Rate	**$**	**$**	**$**	**$**	**$**
Fringe (if applicable and not included in hourly rate)	**$**	**$**	**$**	**$**	**$**
SUBTOTAL	**$**	**$**	**$**	**$**	**$**
OTHER DIRECT COSTS	*Include details and purpose, by evaluation component, for each Direct Cost in the budget narrative.*				
Travel (transportation, lodging, incidentals)					$
Printing					$
Communications					$
Supplies					$
IRB Approval Costs					$
Honoraria (if applicable)					$
Subtotal Other Direct Costs					**$**
Indirect Costs (if applicable and not included in other rates, please specify subcategories, if needed)					**$**
TOTAL BUDGET					**$**

Table 1: Statistical Techniques, Descriptions, and Applications

Statistical Technique	Description	Data Requirements	Sample Application
Descriptive Statistics (distribution, mean, median, mode)	These statistics describe variation and the central tendencies of variables. The distribution is the number and percent of respondents in any given category of a variable. The mean is the arithmetic average. The median is the value that is the middle (50th) percentile of the distribution. The mode is the most common category of the distribution. Use descriptive statistics in exploratory research, to evaluate the feasibility of testing hypotheses, and to check assumptions of inferential statistics.	Any continuous (numeric) or categorical (e.g., gender, race, or yes/no) variables. Not all statistics are calculable for all types of variables.	What percent of program participants (distribution) learned about the program from a friend versus a family member? What was the average (mean) number of visits by a social worker to a family? What was the most common way (modal category) that tenants learned about a program?
Correlation	An examination of the strength and direction of a relationship between two continuous variables. This statistic shows whether or not the two are statistically related, but does not present any causal link.	Two continuous variables, such as age, income, years of education, or number of days in program.	Is greater program participation as measured by number of modules completed associated with years of education?
Chi-Square and Fisher's Exact Test (and related tests of association within cross tabulations)	An examination of two categorical variables to assess if the combinations of characteristics is due to chance.	Two categorical variables with two (or more) possible categories, such as gender, program participation (yes/no), or race/ethnicity.	Is there a difference between men and women in seeking mental health services (yes/no) in the program area?
T-Test (two group comparison)	A comparison of the means of two groups based on a single variable (that has only two values, such as yes/no) to see if observed differences in groups are larger than would be expected due to chance.	Variables that are continuous, such as test scores, indices from mental health instruments, income levels, or that can be divided by a single categorical variable with only two possible categories, such as gender or program participation.	Is program participation (yes/no) related to average score on an intake instrument?
ANOVA (and related techniques)	A comparison of means of an outcome variable is expected to vary based on one other variable (with 3 or	A categorical variable with at least three categories, such as types of program	Does participation in one of three different exercise programs affect young

Statistical Technique	Description	Data Requirements	Sample Application
	more categories) or based on multiple other variables. It is used to determine if the means the various groups of the target variable (based on a second variable) differ statistically. Related models such as MANOVA can use multiple dependent variables, while ANCOVA (and MANCOVA) use multiple independent covariates.	participated in, and a continuous variable, such as test scores, BMI, or income in dollars. Other model forms may include more variables.	people's BMI after completion of the programs?
Linear and Hierarchical Regression (and related regression techniques)	A model used to examine the relationship between one or more predictive (independent) variables and a continuous outcome (dependent) variable. This model allows for statistically accounting for the effects of multiple factors on a single outcome.	A continuous variable that is an outcome of interest, such as test scores, number of days in a program, or number of visits to a health care provider, and at least one continuous variable (and the model may include more continuous or categorical variables as well).	What factors, such as age, gender, or education level, predict test scores on a knowledge exam of health behaviors?
Logistic Regression (and related techniques)	A model used to examine the relationship between independent variables and a dichotomous categorical dependent or dependent variable with limited categories. This model allows for statistically accounting for the effects of multiple factors on a single outcome.	A categorical variable that is an outcome of interest, such as completed program or not, and at least one continuous variable and/or categorical variable(s)).	Assess what factors, such as age, gender, or education level, are related to attending an afterschool program versus not attending.
Fixed Effects Models	A form of regression analysis that holds constant factors which do not change over time, allowing for analysis of factors that do change over time. This model allows for statistically accounting for the effects of multiple factors on a single outcome.	Data collected from the same subjects at least twice (and preferably multiple times). Examples of outcomes (dependent variables) include test scores from students taken in three different years, blood cell counts taken multiple times across several months, or employment status documented every month for one year.	What factors were associated with a net increase in income levels over time for a group of unstably employed individuals?
Hierarchical Linear Models (HLM)	A form of regression that takes into account the unique contribution of variables within and across hierarchical levels to predict a single outcome. This model allows for statistically accounting for the effects of multiple factors on a single outcome.	Independent variables within a level that are nested within one another, such as students within classrooms within schools or neighborhoods. The dependent variable can be continuous or categorical.	How well did students do on a test within an entire school district, taking into account program participation, different classroom experiences, and different school level programs?
Path Analysis	A series of regression models formulated into a path diagram that is used to describe both the size and directionality of the relationship between predictor variables and an outcome variable. It is used to test	Continuous variables that are outcomes of interest, such as test scores, number of days in a program, or number of visits to a health care provider, and at least one	Given that family stability, education level, and previous level of service utilization all contribute to predicting a score on a psychometric instrument,

Statistical Technique	Description	Data Requirements	Sample Application
	theories of causal relationships between variables. This model allows for statistically accounting for the effects of multiple factors on a single outcome. A model that is used to describe both the size and directionality of the relationship between predictor variables and an outcome variable. This model allows for statistically accounting for the effects of multiple factors on a single outcome.	continuous variable (and the models may include more continuous or categorical variables as well).	which relationships are causally most important?
Structural Equation Modeling	A modeling technique that takes into account observed (or measured) variables and latent (or unmeasured) variables by specifically modeling measurement error. It is used to examine causal relationships between variables taking into account the influence of unmeasured variables.	Continuous variables that are outcomes of interest, such as test scores, number of days in a program, or number of visits to a health care provider, and at least one continuous variable (and the model may include more continuous or categorical variables as well).	Can the outcome of increased college matriculation be assessed taking into account both known characteristics, such as test scores and parental education, as well as unobserved measures, such as motivation and ambition?

Table 2: Types of Matching for Control and Comparison Group Formation

General Design and Description	Matching Type	Description	Relation to Validity
Single Group In general, single group designs do not involve a comparison group. One way of approximating a comparison group is to use an interrupted time series design.	No Comparison Group	No comparison group is specified.	Single group designs that do not have many data points across time do not have strong internal validity. Only by drawing a large, statistically random sample from the target population can a study with a single group and no comparison group attain either internal or external validity.
	Interrupted Time Series	In an interrupted time series design, program participants serve as their own comparison group. Relatively large amounts of data are collected on participants (or groups or sites) prior to program participation and then again after program participation. This volume of data allows for the pre-treatment data, or data collected prior to program participation, to serve as a control for the post-treatment data, the data collected after program participation.	In general, single group designs do not deal with threats to validity very well. Time series designs do deal with some of them; effects related to being in the program and the passage of time are controlled for in this design. Biases related to how people are selected to the program are not well controlled for, and generalizability (external validity) is weak. Only by drawing a large, statistically random sample from the target population can a single group without comparison study attain either external validity.
Comparison Group Comparison groups are composed of individuals (or groups or sites) that are selected through a non-random means. Different ways of selecting the comparison group result in different levels of statistical assurance concerning the representativeness of the comparison group (which does not receive treatment or participate in the program) in relation to the program participant group. Comparison group members (or groups or sites) may be identified through various means.	Matched based on Convenience	Comparison group chosen without consideration to characteristics of that group in relation to the characteristics of the program participant group.	Unmatched comparison groups do not control for selection bias or other issues related to the formation of treatment or control groups.
	Matched based on Characteristics	Matching based on a limited number of observed characteristics of program participants and non-program participants, such as age, gender, race, years of education, etc. In certain situations, a comparison group initially formed based on convenience may, after post hoc statistical analysis, be considered a comparison group matched on characteristics if the matching is shown to be strong.	Matching based on characteristics somewhat controls for selection bias and other issues related to formation of treatment and comparison groups. However, because of the limited number of known characteristics upon which the match is based, the comparability between the two groups is less than in other matching methods.
	Regression Discontinuity	Regression discontinuity implies assigning individuals into groups by examining a quantifiable indicator related to the key outcome. If the indicator is reliably and validly measured, a well-defined cutoff score can be used to form the program participation (treatment) and comparison groups, with those in more need (as measured on the indicator) assigned to the program group and those with	Regression discontinuity matching controls for selection bias and other biases related to group assignment (although not perfectly). Provided both groups are drawn from the target population and are generally representative of it, this form of matching may lead to generalizable results.

General Design and Description	Matching Type	Description	Relation to Validity
		less need assigned to the comparison group. The difference (or discontinuity) in the average outcome between those in the intervention (program participant) group just below the cutoff score (e.g. those with lower test scores) and those in the comparison group just above the cutoff score (those with higher test scores) can be attributed to the program, albeit with reservations.	
	Propensity Score	Propensity scores are based on multiple characteristics of many individuals, and allow for a more accurate matching between program participants and control group members. The scores represent how likely any individual is to be in the treated (program participant group) based on known characteristics of individuals in each group. Scores are calculated for the program participation group and for the comparison group, and the composition of the comparison group is generated to best resemble the program participation group.	Propensity score matching controls for selection bias and other biases related to group assignment (although not perfectly). Provided both groups are drawn from the target population and are generally representative of it, this form of matching may lead to generalizable results.
Control Group The non-program participation group is selected randomly along with the program participant group. Several techniques for randomly assigning people (or groups or sites) exist.	Random Assignment	Random assignment specifies that control groups are created when individuals, groups, or sites are randomly assigned to either the treatment (program participation) group or to a control group (that does not receive treatment). Random assignment can take several forms, ranging from simple random assignment in which each person (or group or site) has an equal probability of being selected for program participation to more complicated schemes involving stratification in which characteristics of the population are taken into account.	Provided study participants are drawn from the target population and are generally representative of it, this form of matching leads to generalizable results. However, program participation must not distort the situation under examination greatly. Additionally, external validity is boosted when multiple sites are used in the study.

Appendix D: Glossary

Alpha Level (α): The criterion that allows researchers to use the p-value (see below) to determine whether an estimated program impact is statistically significant. The p-value is the probability that an estimated impact that large, or larger, in magnitude could have occurred by chance, even if the program had no actual impact. The alpha level should be specified by the researcher before outcome data collection begins. Many researchers set alpha equal to 0.05, by convention, but under certain circumstances a larger value (such as an alpha level of 0.10) or a smaller value (or an alpha level of 0.01) may be preferable.

Baseline Differences: For between-group designs, the difference between the average characteristic of one group versus the average characteristic of another, prior to program (or intervention) delivery. A statistical hypothesis test is typically applied to evaluate whether this difference is due to chance.

Between-Group Designs: Designs that compare the average outcome from each of at least two groups.

Bias: In the context of program evaluation, this refers to the extent to which the program impact estimated using the study sample approximates the true impact in the population, across many replications. When an estimate is biased, it will be higher or lower than the true impact.

Blocking: This approach is used during the assignment phase of a study to improve precision of the estimated program impact, to balance the groups on certain characteristics, or both. This is accomplished by determining a characteristic (such as locale), then ordering study units by levels of that characteristic (e.g., urban, suburban, and rural). Within each level, study units are assigned to groups (using random assignment or matching).

Comparison Group: A group of study participants who do not receive program services, usually formed through methods other than random assignment. This group serves as the counterfactual relative to the program (or intervention) group. Because this group is formed by methods other than random assignment, it is considered a "weaker" comparative group than a control group formed by random assignment.

Confirmatory Research Question: The primary research question that the study is statistically powered to address and the answer to which can be used to inform policy.

Control Group: A group of study participants, formed by random assignment, who do not receive program services, and is assessed in comparison to the group receiving services (or the intervention). A randomly assigned control group of participants, statistically, should be similar in both known and unknown ways to the group of participants receiving services. It is considered the strongest possible group to compare to the intervention group.

Correlation: A statistical relationship between two characteristics that vary in a continuous way, such as a relationship between number of hours receiving training and wages after completing a program. This term is used to describe how one characteristic of study participants or group of study participants varies with another characteristic of the same study participants (or group of participants). This covariation is typically measured by a correlation coefficient that ranges from -1 to +1. However, the observation that two characteristics are correlated does not imply that one caused the other (correlation does not equal causation).

Counterfactual: A term used in evaluation to denote a hypothetical condition representing what would have happened to the intervention group if it had not received the intervention. The counterfactual cannot be directly observed, so it is usually approximated by observing some group that is "nearly identical," but did not receive the intervention. In random assignment studies, the "control group" formed by random assignment that is equivalent to the intervention group in every way, on average, except for receiving the intervention serves as the counterfactual.

Covariates: A statistical term that describes the relationship between characteristics of study participants that are, typically, correlated with the outcome. These characteristics could explain the differences seen between program participants and the control or comparison group. As such, these variables are often used as statistical controls in models used to estimate the impact of the intervention on study participants' outcomes.

Effect Size: A way of statistically describing how much a program affects outcomes of interest. Effect size is the difference between the average outcomes of the intervention and control group expressed in standard deviations. This expression is derived by dividing the difference by a standardized unit.

Evidence Base: The body of research and evaluation studies that support a program or components of a program's intervention.

Experimental Design: A research design in which the effects of a program, intervention, or treatment are examined by comparing individuals who receive it with a comparable group who do not. In this type of research, individuals are randomly assigned to the two groups to try to ensure that, prior to taking part in the program, each group is statistically similar in both observable (i.e., race, gender, or years of education) and unobservable ways (i.e., levels of motivation, belief systems, or disposition towards program participation). Experimental designs differ from quasi-experimental designs in how individuals are assigned to program participation or not; in quasi-experimental design, non-random assignment is used, which prevents evaluators from feeling confident that both observable and unobservable characteristics are similar in each group since group assignment is based on observable characteristics usually.

Exploratory Research Question: In contrast to a confirmatory research question, an exploratory research question is posed and then addressed to inform future research rather than to inform policy. This question type includes questions that examine, for example, which specific subgroups respond best to an intervention; questions such as that are less likely to be answered with strong statistical certainty, but may be helpful for program implementation and future evaluation. If a question arises as a result of analyzing the data and was not originally posed as a fundamental impact of the program before data is collected, it is categorized as exploratory.

External Validity: The extent to which evaluation results, statistically, are applicable to groups other than those in the research. More technically, it refers to how well the results obtained from analyzing a sample of study participants from a population can be generalized to that population. The strongest basis for applying results obtained from a sample to a population is when the sample is randomly selected from that population. Otherwise, this generalization must be made on extra-statistical ground – that is, on a non- statistical basis.

Idiographic: Research that focuses on the outcomes for individuals rather than for groups. This is in contrast to research that is nomothetic, which is research that focuses outcomes at the group level. For between-group designs, the strength of the causal attribution depends on how the control or comparison group was formed (random assignment, matching, non-random assignment).

Impact Evaluation: An evaluation designed to determine if the outcomes observed among program participants are due to having received program services or the intervention.

Implementation Fidelity (Fidelity of Intervention Implementation): The extent to which the program or intervention was implemented as intended. The intention usually is expressed prior to intervention delivery and, when available, in intervention developer documents such as the program theory and logic model.

Informed Consent: A dimension of Human Subjects Protection that requires researchers to make sure that potential study participants (both program participants and control or comparison group members) are fully informed of the potential risks or benefits, if any, and conditions of study participation.

Intent-to-Treat (ITT): An approach for analyzing data from between-group designs in which study participants are analyzed, (1) in the group they were assigned to at the start of the study, regardless of the group they end up in at the end of the study, and (2) for individuals in the intervention group, whether they participate in the intervention or not. In intent-to-treat analysis, the aim is to estimate the impact of the "offer" of the intervention regardless of whether it is received, as opposed to focusing on how participants' experience of program participation affects an outcome.

Internal Validity: For a given design, the extent to which the observed difference in the average group outcomes (usually program participants versus control or comparison group members) can be causally attributed to the intervention or program. Randomized controlled trials allow for high causal attribution because of their ability to rule out alternative explanations (usually unobserved characteristics) other than the intervention as the reason for the observed affect.

Intervention: A term used to describe the services or activities a program does to achieve its stated outcomes, goals, or desired results.

Intervention Level: The level (e.g., at the individual, group, community, or structural level of society) at which a specific program offers treatment or services to address a particular problem or issue.

Level of Evidence: The quality of findings, based on empirical data, from an evaluation or research study. Although there is no consensus within the evaluation field concerning what constitutes a particular level of evidence, the SIF program divides evidence into three categories: preliminary, moderate, and strong. These divisions are based on how well a particular evaluation is able to address concerns about internal and external validity, with evaluations that do a better job generating strong or moderate levels and those that are less able to do so generating preliminary levels of evidence.

Matching: A technique used to pair participants in an evaluation based on observed characteristics that are correlated with the outcome of interest. The pairing is then used to create intervention and control groups that are similar based on these characteristics.

Minimum Detectable Effect Size (MDES): The smallest effect size (usually, the comparative difference measured in an outcome between program participants and control or comparison group members) that can be detected for a given design and under certain assumptions with a specified probability (typically .80). Typically, increasing the sample size leads to a smaller MDES (that is, enables the study to detect a smaller impact).

Multiple Comparisons: When between-group designs are used, there are opportunities to compare multiple groups on the same outcome or two groups (program participants versus control or comparison group members) on multiple outcomes. This comparison can artificially inflate the alpha level and require the researcher to adjust it downward. That is, if many outcomes are addressed in a study, it is possible that some will be erroneously viewed as statistically significant even though they are in reality due to chance.

Nomothetic: In contrast to idiographic, nomothetic focuses on group outcomes typically based on the average.

Post Hoc: Means “after the fact.” In the context of evaluation, the term refers to analysis of data that was not specified prior to analyzing the data.

Propensity Score: A score calculated using logistic regression techniques based on known characteristics of an individual or group, which predicts probability of group membership (e.g., intervention or program participation group versus comparison group).

Propensity Score Matching: The use of propensity scores to identify participants for inclusion in the comparison group. Propensity score matching can decrease pre-treatment differences in the treatment and comparison group, thereby reducing selection bias, which constitutes a key threat to internal study validity.

P-values: In the context of an impact evaluation, a statistical term used to describe the probability that the impact observed in the sample could have come from a population in which there is no impact.

Quasi-Experimental Design: A design that includes a comparison group formed using a method other than random assignment, or a design that controls for threats to validity using other counterfactual situations, such as groups which serve as their own control group based on trends created by multiple pre/post measures. Quasi-experimental design, therefore, controls for fewer threats to validity than an experimental design.

Random Assignment: A process that uses randomly generated numbers (or other method of randomizing study participants) to assign study units (people, program sites, etc.) to either the program participant or control group. The use, or lack of use, of this process differentiates experimental designs from non-experimental designs.

Regression: A statistical model used to examine the influence of one or more factors or characteristics on another factor or characteristic (referred to as variables). This model specifies the impact of a one unit change in the independent variable or variables (sometimes referred to as the predictor variable or variables) on the dependent variable (sometimes referred to as the outcome variable). Regression models can take a variety of forms (ordinary least squares, weighted least squares, logistic, etc.) and require that the data meet certain requirements (or be adjusted, post hoc, to meet these requirements). Because regression models can include several predictor variables, they allow researchers to examine the impact of one variable on an outcome while taking into account other variables' influence.

Regression Discontinuity Design: A form of research design used in program evaluation to create a stronger comparison group (i.e. reduce threats to internal validity) in a quasi-experimental design evaluation study. The intervention and control group are formed using a well-defined cutoff score. The group below the cutoff score receives the intervention and the group above does not, or vice versa. For example, if students are selected for a program based on test scores, those students just above the score and those students just below the score are expected to be very similar except for participation in the program, and can be compared with each other to determine the program's impact.

Selection Bias: When study participants are assigned to groups such that the groups differ in either (or both) observed or unobserved characteristics, resulting in group differences prior to delivery of the intervention. If not adjusted for during analysis, these differences can bias the estimate of program impacts on the outcome.

Standard Error: In the context of an impact evaluation, this is the standard deviation of the sampling distribution of the estimate of the program impact. This estimate is divided by the standard error to obtain the test statistic and associated p-value to determine whether the impact is real or due to chance (i.e., sampling error).

Statistical Equivalence: In research, this term refers to situations in which two groups appear to differ, but in truth are not statistically different from one another based on statistical levels of confidence. In a sample, two groups may have what appears to be an average difference on a baseline characteristic. However, when this difference is assessed relative to the population from which this group was drawn (using a statistical hypothesis test), the conclusion is that this difference is "what would be expected", due to sampling from the population, and there is really no difference, statistically, between the groups.

Statistical Power: A gauge of the sensitivity of a statistical test. That is, it describes the ability of a statistical test to detect effects of a specific size, given the particular variances and sample sizes in a study.

Theory of Change: The underlying principles that generate a logic model, a theory of change clearly expresses the relationship between the population/context the program targets, the strategies used, and the outcomes (and/or impact) sought.

Treatment-on-Treated (TOT): In contrast to Intent-to-Treat (ITT), Treatment on Treated (TOT) is a type of analysis that measures the program impact going beyond just the “offer” of the program to consider the level of program uptake. In contrast with ITT, TOT is typically thought of as a measure of the impact on those who actually got the treatment, rather than those who were offered it.

Unit of Analysis: Study participants that comprise the sample that will be used to produce study results; this may or may not be individuals, as sometimes studies compare program sites, groups of participants and non-participants at the aggregate level, or states, for example.

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