Health psychology, broadly speaking, examines the influence of behaviors, psychological conditions, and social factors on health outcomes (Kaplan, 2009; see Chapter 1). Key to this enterprise is the development, rigorous testing, and implementation of evidence-based interventions targeting these influences, with the overarching goal of improving individual and population health. This chapter provides an overview of intervention development and empirical testing, with special emphasis paid to the randomized controlled trial (RCT). Specifically, we focus on the parallel-group, randomized efficacy and effectiveness superiority trial design, in which the active intervention is hypothesized to produce improved health outcomes relative to the comparison or “control” condition. Group- or cluster-randomized controlled trials, non-inferiority or equivalence trials, and sequential multiple assignment randomization trials are but a few of the many RCT designs not covered in this chapter but that play a critical role in advancing the field of health psychology intervention research.

A detailed description of RCT design principles could fill volumes. Readers interested in learning more about fundamentals of clinical trial design are referred to Friedman, Furberg, DeMets, Reboussin, and Granger (2015).

Key Features of Randomized Controlled Trials

RCTs have two defining features. First, they compare one or more “active” interventions to at least one comparator condition, also known as a comparison or control condition, which can be another active intervention, treatment as usual, an inert treatment, or no treatment, to name a few. Second, participants are randomly assigned to a condition. This allows the researchers to control for known and unknown confounders that could otherwise bias results. The inclusion of two or more conditions (i.e., active treatment and comparator) and random assignment to these conditions defines the RCT. Other non-experimental designs are common, such as single-arm trials that lack a comparator and quasi-experimental designs in which participants are assigned to a condition through means other than random assignment. Unfortunately, many types of bias in non-randomized intervention trials can attenuate study conclusions. A detailed discussion of these biases is beyond the scope of this chapter and readers are referred to Sterne and colleagues (2016) for a more detailed explanation of types of bias in non-randomized trials. Methodologic and statistical techniques can be used with non-randomized trial designs to reduce risk of bias, but it is rarely possible to account for all known confounders and impossible to fully account for unknown confounders. For these reasons, the RCT
remains the gold standard for testing health psychology interventions, as trial conditions are equated through randomization, thus alleviating known and unknown confounders.

The Translational Science Continuum

Fifteen years ago, the U.S. National Institutes of Health (NIH) published a “roadmap” of scientific discovery to promote the translation of basic biomedical, behavioral, and social science findings into clinical practice (Zerhouni, 2003). Originally characterized as a continuum (Westfall, Mold, & Fagnan, 2007) progressing from “bench” (basic science research) to “bedside” (human clinical research) to “practice” (dissemination and implementation research), the translational science continuum has evolved into a multidirectional, recursive “spectrum” in which each phase of research informs and is informed by the others, as shown in Figure 4.1. Consistent with principles of participant-centered care (Epstein & Street, 2011), at the center of this model resides the activated and informed participant.

![Figure 4.1 U.S. National Institutes of Health Translational Science Spectrum](Image)

Reprinted with permission from the National Center for Advancing Translational Sciences, National Institutes of Health.
Considering Internal and External Validity in Clinical Trial Design

The multi-phase progression of clinical research that establishes evidence for an intervention begins with controlled study designs, restricted participant samples, and well-trained research interventionists to maximize internal validity and establish that the active intervention, and not other factors, caused observed changes in the outcome. These trials are referred to as clinical efficacy (i.e., Phase III) trials. Contrast these with pragmatic effectiveness (i.e., Phase IV) trials in which an intervention is administered by clinical staff to participants with multiple comorbidities, as seen in actual practice, with the goal of maximizing external validity and determining if an intervention produces desired outcomes in “real world” settings. While efficacy and effectiveness trials can both vary in size and scope, effectiveness trials often enroll a larger number of participants across multiple centers and assess a primary biological or clinical endpoint that has been shown to change as a result of improved behavioral, psychological, or social factors that are the targets of the health psychology intervention.

A successful Phase III RCT would establish efficacy and support a pragmatic effectiveness trial to determine if the efficacious intervention is effective in practice. However, many efficacious health psychology interventions are never tested for effectiveness and their utility in clinical settings remains unknown. Indeed, some have called for a moratorium on efficacy RCTs of complex behavioral, psychosocial, and systems-level interventions due to their lack of impact on practice and policy, even when efficacy has been established (Kessler & Glasgow, 2011). Greene (2008) cited the 17-year length of time it takes for only 14% of original research to benefit target populations, with the vast majority of positive research findings never being applied in practice. He instead called for more rapid evidence generation, pragmatic research, and emphasis on external over internal validity. Still others have proposed “hybrid” designs that can help to speed implementation of effective interventions into clinical practice by combining traditionally distinct phases of the research continuum into a single trial (Curran, Bauer, Mittman, Pyne, & Stetler, 2012).

We present these criticisms not to disparage the efficacy trial but rather to raise awareness of salient issues in the field. At a minimum, researchers should consider the purpose of their planned clinical trial and evaluate features of their design along the internal-external validity continuum to ensure the design aligns with the study purpose. A tool recently developed to evaluate clinical trials on this continuum is the PRagmatic Explanatory Continuum Indicator Summary 2 (PRECIS-2; Loudon et al., 2015) shown in Figure 4.2. The PRECIS-2 model depicts spokes on a wheel, with each spoke representing a design consideration of a clinical trial.

Researchers rate where on the continuum of explanatory (i.e., internal validity, toward the center of the wheel) to pragmatic (i.e., external validity, toward the outer end of a spoke) each design feature of their own clinical trial falls and place a dot on each spoke accordingly. Connecting the dots on each spoke around the wheel yields a shape that visually depicts the extent to which a clinical trial is explanatory versus pragmatic. An interactive website complete with a user toolkit is available at www.precis-2.org, and descriptions of PRECIS-2 constructs are described in Loudon and colleagues (2015). Figure 4.2 depicts a PRECIS-2 wheel for an ongoing RCT, Project BRIDGE, described in greater detail at the end of this chapter.

Developing and Testing Health Psychology Interventions

Health psychology intervention research typically falls within the basic, pre-clinical, and clinical research domains. A useful heuristic for developing health psychology interventions for chronic diseases was recently proposed by the Obesity-Related Behavioral Intervention Trials (ORBIT) consortium, in conjunction with NIH representatives and other experts in health-related behavioral treatments (Czajkowski et al., 2015). Although the authors of the ORBIT model focus on individual behavior, its principles also apply to interventions that target psychological constructs and social
systems. The ORBIT model, presented in Figure 4.3, parallels the multi-phase drug development scientific process, including intervention development and dose-response testing (Phase I), feasibility and acceptability evaluations (Phase II), and eventual Phase III efficacy and Phase IV effectiveness trials.

According to the ORBIT model, the research process begins with identification of a key clinical problem that is catalyzed or perpetuated by a behavioral, psychological, and/or social factor, and that could thus be remediated with a health psychology intervention. Once a key problem has been identified, research activities follow a four-phase process.

In the Design Phase, researchers conduct systematic reviews, meta-analyses, epidemiologic research, small-sample experimental studies, and/or qualitative research to establish evidence for the pathway between a behavioral, psychological, or social risk factor and a meaningful clinical or biological outcome and identify the magnitude of the treatment effect. This preliminary work leads to candidate intervention components that can be combined, refined, and tested to maximize efficiency while still effecting clinically meaningful change. Refinement activities include testing the ordering of intervention components and modes of delivery, as well as frequency and duration of contact between participants and interventionists.

Figure 4.2 Pragmatic-Explanatory Continuum Indicator Summary 2 (PRECIS-2) Wheel of the Project BRIDGE Randomized Controlled Trial
Adapted from Loudon and colleagues (2015).
Critically important to the Design Phase is the solicitation of key stakeholder feedback during the iterative intervention development and refinement process. Stakeholders may include participants who are representative of the target population, clinicians, hospital administrators, and policy makers, among others. Data point to a lack of implementation of evidence-based interventions in clinical practice, in part due to the incompatibility of interventions with the “real world” settings in which their use was intended (Roy-Byrne et al., 2003). Engaging stakeholders from the outset can aid in the development of interventions that have been informed and vetted by end users, thus increasing the likelihood of implementation if found to be effective (Vastine, Gittelsohn, Ethelbah, Anliker, & Caballero, 2005). Completion of the Design Phase yields an intervention “package” that can be tested for feasibility and acceptability.

Preliminary Testing follows the Design Phase and comprises two research activities: single-arm proof-of-concept studies and small-scale pilot RCTs. Proof-of-concept studies employ quasi-experimental, within-subjects designs with a small sample of participants to determine if an intervention can produce clinically significant changes in the target behavioral, psychological, or social risk factor from pre- to post-intervention. A successful proof-of-concept study justifies conduct of a more rigorous pilot test, which includes a larger sample and a control group to which participants may be randomized. The inclusion of a control group allows the researchers to (a) account for effects due to both the passage of time and non-specific factors (e.g., therapeutic alliance; Martin, Garske, & Davis, 2000) and (b) ensure the control condition does not produce clinically meaningful change in the target risk factor.

Notably, the purpose of a pilot study is not to provide an estimate of the treatment effect for powering a larger clinical efficacy trial (Leon, Davis, & Kraemer, 2011). Rather, pilot studies examine feasibility and acceptability of the study protocol and provide important estimates of statistical parameters such as intraclass correlations of repeated measures, variability of continuous outcome variables, response rates for dichotomous and time-to-event outcome variables, and rates of participant attrition. These estimates can be used, along with extant literature, to calculate the sample size for the Phase III efficacy trial needed to detect a clinically meaningful effect at pre-specified levels of statistical significance and power. Successful preliminary testing justifies conduct of a Phase III Efficacy Trial and, if found to be efficacious, a Phase IV Effectiveness Trial. The remainder of this chapter focuses on design and conduct of these larger trials, with emphasis paid to the Efficacy Trial.

It should be noted that non-randomized designs contribute to the body of evidence for the efficacy and effectiveness of health psychology interventions. These include natural experiments and observational cohort studies, among others. Such studies are commonly used when manipulation of exposure to the active intervention is either not possible or unethical. Nonetheless, and despite its limitations (Bothwell, Greene, Podolsky, & Jones, 2016; McCambridge, Kypris, & Elbourne, 2014),
the RCT remains the dominant research paradigm for establishing the efficacy and effectiveness of health interventions (Burns, Rohrich, & Chung, 2011).

Planning a Randomized Controlled Trial

Many decisions go into planning a large-scale RCT, some of which are made during the Design and Preliminary Testing Phases of intervention development (Czajkowski et al., 2015), while others are made following preliminary testing but prior to RCT execution. These include identification of the target population from which the study sample will be drawn, establishing study inclusion and exclusion criteria that balance considerations of internal and external validity, selection of the primary and secondary outcome variables, operationalization of the active intervention and comparator, specifying a randomization scheme, calculating sample size, pre-specifying the data analytic plan, and establishing an independent data and safety and monitoring protocol. Notably, randomization in an RCT can occur at the individual participant, provider, clinic, or community level. In this chapter, we focus on individual-level RCTs. Readers are referred to Friedman and colleagues (2015) and Murray, Varnell, and Blitstein (2004) for additional details on the design and analysis of group-randomized controlled trials.

Identifying a Target Population

In most health psychology intervention trials, the target population will have, or be at risk for, an identifiable health condition (e.g., diabetes, cancer, cardiovascular disease, HIV, chronic pain) and a behavioral, psychological, or social risk factor that leads to the onset or exacerbation of disease symptoms. The risk factor serves as the target of the intervention. The target population thus includes all persons with an identifiable health condition and a risk factor. Recruitment of a random sample from a target population is seldom practical, and as a result, participant samples in clinical trials most often comprise convenience samples. This practice has contributed to a lack of racial, ethnic, gender, and geographic diversity in RCT participant samples and criticism that RCT findings lack generalizability (Oh et al., 2015). A well-constructed RCT will enroll a diverse sample of participants who are collectively representative of the target population.

Establishing Study Inclusion and Exclusion Criteria

In addition to being a member of the target population, an RCT will often have additional inclusion and exclusion criteria. Some agencies that fund clinical trials, such as the NIH, now require researchers to scientifically justify the planned inclusion or exclusion of women, racial and ethnic minorities, and children. The NIH further requires, when applicable, that researchers overtly address sex as a biological variable in study designs and analyses. Additional considerations include the homogeneity versus heterogeneity desired in the study sample. Greater restrictions on study inclusion can yield more consistent treatment effects, boosting internal validity, but may limit the generalizability of study findings and reduce the likelihood that an intervention will be implemented.

Selecting and Measuring Study Outcomes

Selection of study outcomes and their measurement will have been performed in earlier phases of intervention development and preliminary testing. Smaller efficacy trials will often target a behavioral, psychological, or social endpoint purported to impact a clinical or biological outcome. Larger efficacy and effectiveness trials will evaluate the clinical or biological endpoint itself as the primary
outcome. The behavioral, psychological, and social risk factors will serve as secondary outcomes or mediators of the intervention’s effect on the clinical or biological endpoint.

Objective measures of behavioral, psychological, and social outcomes are preferred to participant self-report measures. For example, in the context of HIV medication adherence, validated pill count methods (Kalichman et al., 2010), or measurement of drug concentrations in hair follicles (Olds et al., 2015) are preferred to self-reported medication adherence. If a self-report or interviewer-administered measure is the gold standard, as is the case for depression symptom severity (Nelson, Cho, Berk, Holland, & Roth, 2010) or pain intensity (Twycross, Voepel-Lewis, Vincent, Franck, & von Baeyer, 2015), validated instruments should be used. Selecting validated instruments commonly used in the field also aids meta-analysts and systematic reviewers who may aggregate results of multiple RCTs in the future, as greater homogeneity in the measurement of study outcomes reduces bias.

**Active Interventions and Comparators**

Active health psychology interventions are the product of iterative design, refinement, and preliminary testing. They target a specific behavioral, psychological, or social risk factor shown to impact a clinical or biological endpoint. A manual clearly operationalizes intervention delivery and is used by interventionists to maintain fidelity to the treatment.

An RCT also includes a comparator, also known as a comparison or control condition. Comparators in an RCT can take many forms. Based on the extent to which they are hypothesized to affect the outcome variable, comparators range from weak (i.e., unlikely to impact the outcome) to strong (i.e., likely to have a clinically significant effect on the outcome; Freedland, Mohr, Davidson, & Schwartz, 2011; Mohr et al., 2014). Weak comparators include no treatment and wait-list; in the latter, participants will receive a treatment after a pre-specified period of time in which they participate in the study as a control. Moderate comparators include attention-equivalent inert interventions (e.g., an educational intervention), components of an active treatment, and existing practice such as usual or standard care. Although researchers are able to create manuals for inert treatments and components of active treatments, usual care can be very heterogeneous and often lacks a standard administration protocol. Strong comparators include other evidence-based active treatments; this type of comparator is commonly used in comparative effectiveness trials and non-inferiority trials. In non-inferiority trials, an active intervention is hypothesized to be no-less efficacious than another evidence-based intervention.

All else being equal, weaker comparators will generally yield larger treatment effects than stronger comparators (Mohr et al., 2014). However, a weaker comparator is not always desirable, and selection of a comparator depends on the purpose of the trial (Mohr et al., 2009). For example, a trial that aims to test specific intervention components would require a comparator that includes all the other elements of the intervention minus the specific components. A trial that aims to test the delivery mechanism of an evidence-based intervention may compare telehealth to in-person delivery of the intervention. Similar to manualization of the active intervention, a comparator intervention should also include a manual and appropriate training for study interventionists to enhance fidelity of treatment delivery. Readers are referred to Mohr and colleagues (2009) and Freedland and colleagues (2011) for a more detailed discussion of comparator selection.

Unlike drug trials, health psychology intervention trials are unable to use double-blind procedures, in which both participants and researchers are blind to the condition to which participants are randomly assigned. Even blinding participants alone to the randomization scheme may be difficult or unethical. If a study outcome is assessed by a researcher (e.g., through direct observation or via interview), health psychology RCTs should blind these outcome assessors to intervention assignments to help reduce potential bias.
Specifying a Randomization Scheme

A defining feature of an RCT is the random assignment of participants to treatment or comparator conditions. Many randomization schemes exist, including complete, simple, stratified, and block randomization (Friedman et al., 2015).

In complete randomization (i.e., a coin flip), the probability of assignment to a condition is independent of who was assigned to each of the conditions previously. For example, the probability of assignment in a two-arm RCT would always remain 50%. It is thus impossible to predict assignment of the next participant to a condition, but this method can also lead to group imbalances, particularly in smaller trials. In contrast, simple randomization mimics a draw from a hat without replacement. The number of participants in a trial and the conditions to which they will be randomized are pre-specified. A slip of paper specifying a condition is put into a hat for each participant and these slips of paper are drawn randomly until the last slip of paper is drawn. In practice, this procedure is carried out with computer software. By the end of the trial, an equal number of participants will be assigned to each condition. However, there is an increased likelihood with simple randomization that researchers can predict the assignment of the last few participants in a trial. In addition, both complete and simple randomization increase the likelihood that a large number of participants in a row are assigned to the same condition. This can lead to bias in RCTs with slow rates of enrollment and when secular trends in the treatment of a disease are rapidly changing, as outcomes could be impacted by the secular trends rather than the intervention itself.

Stratified randomization divides the sample into strata, such as sex or age group, and randomizes within each stratum. This procedure ensures balance on important factors across conditions but can become unwieldy when trying to stratify across multiple variables. Block randomization performs a simple randomization procedure within successive blocks of pre-specified numbers of participants. For example, given intervention A, comparator B, and a block size of four participants, the possible orderings are AABB, ABAB, ABBA, BBAA, BABA, and BAAB. This randomization scheme equates sample sizes in each group and eliminates the possibility that a large number of participants in a row are assigned to the same condition. It is important to note that a pre-specified block size, as in the preceding example, allows researchers to know the condition to which every fourth participant will be assigned. To protect against this bias, researchers can have the study biostatistician randomly select from multiple block lengths (e.g., 2, 4, 6) and not reveal the block lengths to investigators or study staff that carry out the randomization. Readers are referred to Friedman and colleagues (2015) for a more detailed description of these and other randomization schemes.

Calculating Sample Size and Pre-Specifying the Data Analytic Plan

The planned data analyses inform how many participants will be needed for the study in order to achieve statistical power (Aberson, 2010), that is, that the study has enough participants to detect a pre-specified clinically meaningful effect if it occurs. Data obtained from preliminary studies and the extant literature are often used to provide estimates of the parameters needed to determine sample size. These include variability of continuous outcome variables, response rates for dichotomous and time-to-event outcome variables, intraclass correlations of repeated measures for longitudinal analyses, and rates of participant attrition. Pre-specified clinically significant effect sizes (from previous studies), acceptable Type I error rate, and desired statistical power also contribute to sample size calculations.

An important approach to data analysis is intent-to-treat analyses. Intent-to-treat analyses include all participants randomized to an active intervention or comparator, regardless of whether they actually participated in the intervention, and is recommended for the primary outcome analysis. The intent-to-treat approach provides a conservative estimate of the treatment effect, as participants who
were randomized to the treatment condition but receive incomplete treatment are still analyzed as if they had received the full treatment. In contrast, per protocol analyses include only those participants who received all of the allocated intervention, thus undermining the power of randomization as not all randomized participants are analyzed. This analysis is not recommended for the primary outcome, as it can systematically bias results, but can be used for exploratory analyses.

**Data and Safety Monitoring**

Most sponsors of Phase III and Phase IV clinical trials require an independent Data and Safety Monitoring Board (DSMB). The board comprises a chair and additional voting members with relevant expertise (e.g., clinician, statistician, bioethicist, clinical trialist, stakeholder). Voting members should have no actual or perceived conflict of interest with any member of the investigative team. Non-voting members may include a sponsor (i.e., funder) representative and the study’s biostatistician, who can provide interim analyses to the DSMB as requested.

The DSMB is organized by a charter that outlines its membership and function. Prior to initiating an RCT, the DSMB should approve the study’s clinical intervention protocol and operations and procedures manual. The DSMB meets periodically over the course of the trial (e.g., twice yearly) to evaluate study progress, including rate of enrollment, participant retention, and adverse event monitoring. If specified in the protocol, interim analyses may be conducted by the study biostatistician. The DSMB serves as an advisor to the study team and sponsor, makes recommendations regarding modifications to the study protocol, and votes on the continuance versus stoppage of the trial. Stoppage of a trial may be recommended if a treatment clearly shows superior effects in interim analyses. Conversely, a trial may be stopped if serious adverse events (e.g., death) threaten the safety of participants; this is more common in drug and medical device trials than in health psychology intervention trials.

**Executing a Randomized Controlled Trial**

**Recruiting, Enrolling, and Retaining Study Participants**

Researchers should aim to recruit diverse samples of participants who are representative of the target population. To maximize recruitment, various forms of recruitment media can be used, such as flyers and brochures, direct clinician referrals, and advertisements in print and digital media. Social and other online media allow for targeted outreach but are limited to users of these platforms. Barriers to study participation should be identified and plans put in place to overcome these barriers. For example, flexible scheduling of eligibility screening and other study visits for nights and weekends, provision of transportation vouchers, and offering child care could help to recruit participants who would otherwise be unable to participate in the RCT. Prior to recruiting participants, study staff should be trained in all study procedures. Over the course of the trial, recruitment, enrollment, and retention should be monitored closely, comparing planned versus actual rates.

Retention of study participants in a clinical trial is paramount. Goldberg and Kiernan (2004) described an innovative group-based enrollment orientation session used prior to enrolling participants in a weight-loss RCT. This single session provided information about the clinical trial, provided education about the process of randomization and scientific purpose of this design, and used motivational interviewing principles to help attendees identify pros and cons of trial participation and process ambivalence they may have about being in the trial. The session was led by the study’s principal investigator, who emphasized the partnership between researchers and study participants in advancing scientific knowledge. Retention in the trial was outstanding, with 96% of participants completing the 18-month follow-up assessment. Additional retention strategies also have been suggested (Goldberg & Kiernan, 2004). These include flexible scheduling of study appointments,
sending birthday cards or notes acknowledging important participant milestones, conducting participant outreach using multiple methods of contact (i.e., phone, text, e-mail), and obtaining approval to contact individuals within a participant’s social networks if the participant is lost to follow-up.

**Treatment Fidelity Monitoring**

Treatment fidelity concerns the extent to which a participant received the intended treatment. Fidelity is critically important because without it, a clinically significant treatment effect could have no effects because of an inefficacious treatment or poor fidelity of an efficacious one. Three elements comprise treatment fidelity (Lichstein, Riedel, & Grieve, 1994): participants attend intervention sessions, interventionists deliver the intervention, and, if applicable, participants engage in between-session activities that are part of the treatment protocol. Retention strategies described previously can be employed to encourage participant attendance at all intervention sessions. Interventionists should be thoroughly trained in the intervention protocol, meeting a priori standards of competence before delivering the intervention to enrolled participants. Ongoing fidelity monitoring can be performed through direct observation or coding of recorded intervention sessions. This is particularly important when the same interventionists deliver both the active intervention and comparator, as ongoing monitoring and feedback can help prevent intervention crossover and drift. To monitor participants’ engagement in between-session activities, researchers could employ diaries (e.g., pain diaries, thought logs) or other ecological momentary assessment techniques, which evaluate participants’ real-time behaviors and experiences in their natural environments (Shiffman, Stone, & Hufford, 2008).

**Clinical Trial Registration and Reporting**

The integrity of medical and behavioral sciences depends on the rigorous conduct of RCTs and accurate reporting of trial results. Steps have been taken to improve transparency in the research enterprise. Most trials in the U.S. are registered at www.clinicaltrials.gov, prior to enrolling participants in a clinical trial. Other international trial registries exist, such as the World Health Organization International Clinical Trials Registry Platform (see www.who.int/ictrp/en/). Registrants provide descriptions of the study methodology, including pre-specified primary and secondary outcome variables, and data analytic plans. This ensures that when submitted for publication, trial methodologies and data analytic plans conform to methods described a priori, ensuring researchers do not “cherry pick” results. Trial registration is a requirement for publication in most medical journals and upper-tier health psychology and behavioral medicine journals. Many funding agencies, including the NIH, require trial registration. In addition, greater scrutiny is being placed on pre-specified analyses published during initial trial registration. Recent studies have identified discrepancies between outcome variables and analyses specified when registering the trial prior to initiation and those described in publications of trial results in scientific journals once the trial has concluded, with discrepancies approaching 50% (Becker, Krumholz, Ben-Josef, & Ross, 2014; Zarin & Tse, 2013). This indicates that some researchers may be reporting results from exploratory analyses and describing them as if they were specified a priori.

The uniformity of reporting RCTs in published manuscripts has advanced since initial publication of the original Consolidated Standards of Reporting Trials (CONSORT) Statement in 1996. Revisions in 2001 and 2010 incorporated a growing body of empirical evidence that informs CONSORT. Today, most journals require inclusion of the CONSORT 25-item checklist with manuscript submissions. The checklist specifies minimum RCT reporting criteria for all sections of a manuscript, including the title, abstract, introduction, methods, results, discussion, and other information (trial registration number, protocol location, and source of funding). Key documents for the 2010 CONSORT Statement can be found at www.consort-statement.org.
An Example of an RCT That Tests the Efficacy of a Health Psychology Intervention

For illustrative purposes, we present details of an ongoing clinical trial that tests the efficacy of a telephone-administered motivational intervention to reduce condomless sex in HIV-positive older adults. Details about this RCT can be found in Table 4.1 and at www.clinicaltrials.gov, trial registration number NCT03004170.

Table 4.1 Characteristics of an ongoing RCT that tests the efficacy of a behavioral intervention to increase condom use in HIV-positive older adults

<table>
<thead>
<tr>
<th>Target Population</th>
<th>• HIV-positive adults 50 years of age and older who engage in condomless sex with partners who are HIV-negative or whose HIV status is unknown</th>
</tr>
</thead>
<tbody>
<tr>
<td>Target Behavior</td>
<td>• Condomless sex</td>
</tr>
<tr>
<td>Clinical and Biological Endpoints</td>
<td>• Rates of HIV transmission</td>
</tr>
<tr>
<td></td>
<td>• Rates of transmission of other sexually transmitted infections</td>
</tr>
<tr>
<td></td>
<td>• Participants’ HIV viral load</td>
</tr>
<tr>
<td>Inclusion Criteria</td>
<td>• HIV-positive</td>
</tr>
<tr>
<td></td>
<td>• At least 50 years old during study participation</td>
</tr>
<tr>
<td></td>
<td>• English speaking</td>
</tr>
<tr>
<td></td>
<td>• Access to a landline or cellular telephone</td>
</tr>
<tr>
<td></td>
<td>• Self-reported condomless anal or vaginal sex in the past three months with an HIV-negative partner or partner whose HIV status is not known</td>
</tr>
<tr>
<td></td>
<td>• No active suicidal ideation</td>
</tr>
<tr>
<td>Sampling Scheme</td>
<td>• National sample of persons who receive care or services from community AIDS service organizations or infectious disease clinics</td>
</tr>
<tr>
<td></td>
<td>• Oversampling of racial and ethnic minority, rural, and female HIV-positive older adults</td>
</tr>
<tr>
<td>Primary Outcome</td>
<td>• Change from baseline to 12-month follow-up in number of self-reported condomless anal and vaginal sex acts over the past three months with HIV-negative partners or partners whose HIV status is unknown, as measured by the Timeline Follow-Back interview (TLFB)</td>
</tr>
<tr>
<td>Secondary Outcomes</td>
<td>• Among participants with mild depressive symptomatology at baseline, change from baseline to 12-month follow-up in depression symptom severity, as measured by the Patient Health Questionnaire 9-Item depression measure (PHQ-9)</td>
</tr>
<tr>
<td>Assessment Timing</td>
<td>• Participants complete Internet or mailed surveys and telephone interviews at baseline, 3-, 6-, 9-, and 12-month follow-up</td>
</tr>
<tr>
<td>Blinding</td>
<td>• Interviewers collecting data on primary and secondary outcomes are blind to participant condition</td>
</tr>
<tr>
<td>Active Intervention Condition</td>
<td>• Five sessions of telephone-administered motivational interviewing plus behavioral skills training</td>
</tr>
<tr>
<td>Comparator Condition</td>
<td>• Time-matched five sessions of telephone-administered coping effectiveness training</td>
</tr>
<tr>
<td>Randomization Scheme</td>
<td>• Individual participants randomized to one of two study conditions using block randomization, with blocks of varying size</td>
</tr>
<tr>
<td></td>
<td>• The study biostatistician developed the randomization scheme and randomly selected block sizes. All other research personnel are blind to the randomization scheme</td>
</tr>
</tbody>
</table>

(Continued)
By 2020, 70% of people living with HIV/AIDS in the United States will be greater than 50 years of age (Karpiak, 2014), and many engage in sexual behaviors with HIV-negative partners that risk HIV transmission (Golub et al., 2010; Lovejoy et al., 2008). Preliminary studies established feasibility and acceptability of a telephone-administered intervention targeting HIV sexual transmission risk behavior in this aging population (Lovejoy, 2012; Lovejoy et al., 2011), providing the scientific foundation for a larger-scale efficacy RCT. Funded by the NIH National Institute on Aging, Project BRIDGE tests the efficacy of a five-session telephone-administered motivational interviewing plus behavioral skills training intervention with HIV-positive older adults to reduce condomless sex with HIV-negative sexual partners and partners whose HIV status is not known, relative to a five-session coping effectiveness training comparator condition. While this trial is ongoing and results are not yet available, Table 4.1 illustrates aspects of the study’s \textit{a priori} methodology decisions. The trial is characterized on the pragmatic-explanatory heuristic presented in PRECIS-2 (see Figure 4.2). Although Project BRIDGE is characterized as an efficacy trial, several of its characteristics align more closely with pragmatic trials. For example, few exclusion criteria were employed, practicing HIV social service providers deliver the intervention as would be the case in actual practice, no effort other than

\begin{table}[!h]
\centering
\begin{tabular}{|l|l|}
\hline
\textbf{Intervention Fidelity} & \textbullet Both the active intervention and comparator intervention are manualized  \\
 & \textbullet All intervention sessions are audio-recorded and a random 10\% are coded  \\
 & \textbullet Interventionists administer both the active and comparator interventions  \\
 & \textbullet Sessions are coded to ensure the active intervention “looks like” motivational interviewing, while the comparator intervention does not  \\
\hline
\textbf{Interventionists} & \textbullet Community HIV social service providers (social workers, addiction counselors, nurses) who provide HIV medical case management to clients in Portland, Oregon, USA  \\
\hline
\textbf{Interventionist Training} & \textbullet All interventionists completed multi-day workshops on delivery of the active intervention and comparator  \\
 & \textbullet Each interventionist completed a minimum of four mock sessions in each condition (at least eight sessions total) and were “cleared” by trainers once they met the required levels of competency for the study  \\
\hline
\textbf{Sample Size} & \textbullet N = 336 HIV-positive older adults: 168 in the active intervention condition and 168 in the comparator intervention condition  \\
\hline
\textbf{Data-Analytic Plan} & \textbullet Zero-inflated negative binomial mixed-effects regression to compare differences in the number of condomless sex acts between the two conditions (primary outcome)  \\
 & \textbullet Linear mixed-effects regression to compare depressive symptom severity between the two conditions (secondary outcome)  \\
\hline
\textbf{Data and Safety Monitoring Board} & \textbullet Composed of a chair, two additional voting members, the study’s biostatistician (non-voting), and an NIH representative (non-voting)  \\
 & \textbullet The board possesses expertise in clinical trials, HIV, motivational interventions, biostatistics, and bioethics  \\
\hline
\textbf{Clinical Trial Registration} & \textbullet The trial is registered at \url{www.clinicaltrials.gov}  \\
 & \textbullet Trial registration number: NCT03004170  \\
\hline
\end{tabular}
\caption{Table 4.1 (Continued)}
\end{table}

\footnote{The primary outcome for this RCT is the behavioral endpoint of self-reported condomless sex. If found to be efficacious, this intervention would be tested in a pragmatic effectiveness trial with biological and clinical endpoints.}
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reminder phone calls and text messages is made to ensure participants adhere to the intervention, and planned data analyses will use all available data and intent-to-treat procedures. This RCT is an example of how the efficacy-effectiveness continuum is sometimes blurred to help balance the need for both internal and external validity.

Conclusion

Clinical trial methodology has made considerable advancements since the first RCT was published in 1948 by the British Medical Research Council on the effect of streptomycin for the treatment of tuberculosis (Streptomycin in Tuberculosis Trials Committee, 1948). As noted in an editorial in Health Psychology, the field’s flagship journal, the editor in chief highlighted the importance of conducting large-scale, multicenter RCTs using multidisciplinary team science to definitively determine if modification of behavioral, psychological, and social risk factors can improve the prognosis of chronic medical conditions (Freedland, 2017). Such studies have the potential to fundamentally shift clinical practice by providing empirical support for a truly integrated biopsychosocial approach to integrated health care.

References


