Dismantling Studies of Psychotherapy

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There are multiple ways to assess the effects of specific components of a treatment protocol on outcome, including examining mediating or moderating effects of different components on specific outcomes. Another approach is the use of component designs. Component designs experimentally manipulate the elements of a treatment protocol, allowing stronger inferences about the effects of specific treatment elements. Component designs include either adding or subtracting specific elements of a treatment protocol to assess the relative effects of the component in the context of the other elements in the design. The latter type of study is called a dismantling or component control study. A dismantling study design typically entails comparing a full treatment protocol to a "dismantled" comparison in which one or more aspects of the treatment protocol thought to be an active change agent(s) are removed. All other aspects of the study design and implementation are held constant across conditions, and attempts are made to ensure equivalence across nonspecific aspects of care delivery that might influence outcomes. If the omitted portion(s) of the treatment protocol are an important change agent in the overall treatment package, subsequent group comparisons would demonstrate reductions in clinically significant change in the dismantled group as compared to the group that received the complete package at post-treatment and follow-up. If not, then the inference is that the omitted elements of the protocol can be removed from the overall protocol allowing for a more focused, parsimonious approach to treatment.

An example of this design is a dismantling study of cognitive therapy (CT; Beck, Rush, Shaw, & Emery, 1979) completed by Jacobson et al. (1996). In this study, 152 participants were randomized to one of three treatment conditions to control for levels of psychopathology and key demographic variables. The reference condition consisted of all of the elements found in CT. This included (a) cognitive restructuring, comprising identifying automatic thoughts, correcting dysfunctional interpretations, and modifying depressogenic schemas, and (b) behavior activation, which in this study consisted of daily activities monitoring, and scheduling of activities that promote pleasure or mastery. The behavior activation component also included a priori cognitive rehearsal of scheduled activities, skills training, and use of problem-solving techniques to facilitate completion of scheduled activities. The two comparison treatment conditions were truncated or dismantled versions of CT, the first consisting of just the cognitive restructuring component of CT, and the other consisting of just the behavioral activation component. Recruitment and assessment procedures were identical in order to control for possible potentiation effects due to assessment or assignment to treatment. In order to control for treatment nonspecifics, four experienced therapists provided care equally across all three conditions.

To compare the relative effects of each of the treatment conditions, multivariate analysis of covariance with pretreatment scores as covariates was used to compare treatment conditions at posttreatment and at a 6-month follow-up. In addition, contingency tables were used to compare those categorized as unimproved, recovered, or improved by treatment group as classified using the Jacobson and Traux (1991) method of measuring clinically significant change. Results indicated that there were no differential therapist or treatment effects

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on scores from the Beck Depression Scale or Hamilton Rating Scale for Depression at either posttreatment or at 6-month follow-up. Comparisons of clinical significant change at posttreatment also indicated there were no differential treatment effects across groups. The authors concluded that there were no significant differences in efficacy between CT and stand-alone versions of its two components. They also concluded that the hypothesized mechanism of change in CT, that change in depressogenic patterns of thinking are essential for recovery for Major Depressive Disorder, was not supported by this study. Given that both cognitive restructuring and behavioral activation alone appeared equally efficacious and more parsimonious, and that change in depressogenic thinking did not appear necessary for change, Jacobson and his colleagues pursued development of a stand-alone version of behavioral activation due to its relative ease of dissemination and implementation (see Dimidjian et al., 2006; Martell, Addis, & Jacobson, 2001).

Problems with Dismantling Designs

While promoted as a strong framework to make inferences about the relative efficacy of treatment components, dismantling designs do have limitations. In the Jacobson and colleagues study, both the dismantled behavioral activation and cognitive restructuring conditions received up to 20 sessions of solely behavior activation (BA) or cognitive restructuring (CR) in order to match the time for the combined standard CT condition. This is because most researchers think it is crucial to match client-therapist contact time in all conditions of dismantling studies. This means that, in the standard CT condition, the intensity of the dose of each component (BA and CR) was necessarily half given the equivalent time frame for treatment delivery. Thus, while pragmatically one might select the easiest component to train and disseminate, the conclusion that all treatments are equal does not speak to why the standard CT package was equivalent to the components given lesser does of both active interventions—a critical determination in modifying existing treatments to increase efficacy or developing new treatments. In this case, it is unclear if there was a synergy between CR and BA that allowed for equivalent outcomes despite lesser doses of each active element found in the component groups or that one or the other components was the active ingredient but there were diminishing returns after the initial sessions.

Overall, dismantling designs do not assess possible potentiation of treatments due to interactive or order effects. For example, CT in this study was delivered in delineated order in which behavior change via BA techniques was the first target of intervention followed by examination of automatic interpretations of these and similar events (Jacobson et al., 1996, p. 295). If one interpreted the results of Jacobson and colleagues study as evidence that BA techniques in CT potentiate the effectiveness of CR techniques, then efforts to identify the most parsimonious, cost-effective way to deliver CT might focus on identifying the minimum dose of BA necessary to potentiate restructuring rather than the more comprehensive BA procedures delineated in current CT protocols. Alternatively, if one hypothesizes that the equivalency of the full CT protocol was due to BA assignments providing content for CR, then one might test the multiplicative effect of these components by assessing if certain aspects of between-session BA assignments delivered concurrently with CR might interact to magnify the effects of both over time. This highlights an important issue when deciding to use a dismantling study design. Within a multicomponent therapy protocol with three or more active ingredients, a reduction in efficacy in a reduced protocol might not necessarily be a result of differential effectiveness of specific components but may reflect how those components perform given changed order and interactive effects. Thus, in designing a dismantling study of multicomponent therapies, in order to draw clear conclusions about the efficacy of any particular component, the design would need dismantled groups that would account for order effects.

Adding to the issue of dose, designing equivalent dismantled groups in terms of treatment nonspecifics or common factors can also be difficult. For example, in the dismantling study of cognitive processing therapy for posttraumatic stress disorder by Resick et al. (2008), the researchers found it necessary to deliver the full CPT package and the stand-alone cognitive intervention component in 1-hour biweekly sessions. The stand-alone component consisting of writing and reading accounts of individuals' worst traumatic events did not fit this format and required 2-hour weekly sessions. This decision to structure the treatments in this way makes it unclear to what to attribute the finding of relative equivalency across treatment groups found in this study. Is it the case that massed practice enhanced the effect of the writing group and if the cognitive-alone group and the CPT group were to be delivered in 2-hour blocks, would they be twice as effective, or is it that the writing group might have been effective if it could have been broken into bi-weekly sessions and had a higher frequency of practice? Given the lack of reported process measures and lack of an attention control group it is unclear if the equivalent results were a result of treatment nonspecifics equivalent across groups or equal efficacy of treatments across conditions.

In dismantling studies, the mechanisms of change can often be difficult to tease apart if the component interventions each have separate or interactive effects on the proposed mechanism of change. In the Jacobson et al. study, was behavior activation effective because individuals acted and interacted with their environment in nondepressed ways, or did acting differently challenge depressogenic beliefs about individuals self, world, and future? Similarly, is the mechanism of change in cognitive restructuring acting or thinking differently in response to environmental stimuli? To address these questions, Jacobson and colleagues included process measures. They found that changes in depressogenic thinking was associated with outcome in the BA condition and that increases in pleasant events were associated with adjustment in the CR condition—the opposite of what one might expect given the targets for these interventions. However, without the inclusion of these process measures, it would have been difficult to assert that the tested therapeutic techniques were differentially effective when compared to general treatment nonspecifics.

Indeed, many dismantling studies often find that all therapy groups perform equally. This is often interpreted as evidence in favor of using the more parsimonious therapies included in these studies. However, there may be alternative explanations. A meta-analysis by Ahn and Wampold (2001) of component studies looking at 20 dismantling and additive studies completed between 1990–99 found that the differences between active therapies were minimal and statistically nonsignificant. These results lead the authors to conclude that the effects of psychotherapy are not related to the specific effects of any techniques but instead were related to common factors across psychotherapy techniques—a finding consistent with the general findings that all active therapies are typically found to be equivalent across multiple mental health problems in well-controlled research (Baardseth et al., 2013; Speilmans, Pasek, & McFall, 2007; Wampold, Minami, Baskin, & Tierney, 2002). Common factors include therapist allegiance to treatment, general therapist attributes (such as ability), client attributes (motivation, expectancies for outcome, etc.), client belief that the therapist understands the problem, study design features (e.g., that facilitate Hawthorn effect), and social and organizational factors (clinical atmosphere, community stigma; see Staines, 2008, for full review). Much of the effect of treatment may be attributed to the effect of these factors on patient understanding of problems, belief in the credibility of the treatment to fix their problem, installation of hope, increased self-efficacy, and so forth. A more comprehensive study recently found modest additive effects for treatment components (Bell, Marcus, & Goodlad, 2013), but the complexities of doing dismantling studies remain much more difficult than one might initially expect.

In addition to common factors, other influences might explain the frequent finding of equivalency or near equivalency. Most published dismantling studies are typically underpowered to detect small differences in effect sizes between two active treatments—this is over and above the shared variance in both treatments due to common factors. Sample-size limitations are common in clinical research given the rarity of the clinical phenomenon studied and the difficulty in recruiting participants. However, this can lead to inclusion of less severe cases in treatment groups as a way to boost sample sizes. The result of this decision is the potential introduction of measurement floor effects in a part of the sample, reducing overall variability that can be analyzed, and violating homoscedacity assumptions inherent in many types of parametric analyses (e.g., Crits-Christoph, 1997).

Potential Solutions to Problems with Dismantling Designs

One solution is to design dismantling studies with adequate statistical power to identify small effects. The standard dismantling design of comparing two or more potentially active interventions limits the differential effect size to be detected as does designing for equivalence across common treatment factors such as therapist allegiance given the large effect these factors have on outcomes as outlined in the literature. Solutions to other problems include accounting for equivalency of dose in the study planning stages. Planning for follow-up studies to examine order effects and synergies between components would also improve dismantling studies when supported by the results.

The issue of the overlapping effect of treatment components on outcomes is not easily solved given that components were likely included in the treatment package because of their effects on those same outcomes. Instead, a well-designed dismantling study might look at effects of components on a range of related outcomes through their effects on a number of different hypothesized process variables. Presumably components are included in the larger treatment package for the potential additive effect of addressing more than one process variable. The relative effect of components versus whole treatments would allow more focused treatment decision making and hypothetically allow for better matching between presenting problems and treatment planning. Moreover, new advances in mediation modeling can allow researchers to examine multiple mediator models using nonparametric bootstrapping methodologies, which offer advantages over older regression-based techniques. The advantages these methods offer in terms of the increased power that bootstrapping resampling allows in estimating the effects of each indirect path, especially in relatively small samples, can help mitigate the reality of limited sample sizes in clinical studies (see Hayes, 2009; Preacher & Hayes, 2008 for further details).

Another important design consideration is to ensure adequate measurement of common, nonspecific treatment factors associated with outcome, or to plan the study with these in mind. Therapist allegiance and attributes, client attributes, study design features, and social and organizational factors can be measured and included as a covariate in analyses or addressed during the research design phase through randomization of both clients and therapists. Organizational and social factors can be controlled through planning multisite studies. However, treating these factors as covariates, rather than controlling for them, requires careful recruitment of both participants and therapists such that both groups have sufficient variation in the targeted common factors to analyze the effects of these factors. An advantage to this approach is that it allows exploration of the potential moderating effects of these factors on component efficacy or on the effect of components on process variables, which in turn can inform treatment development over time.

In many cases, these solutions to the problems in dismantling studies may not be feasible. In these cases, there are other novel approaches to building efficacious treatment protocols in which both components and dose of components are optimized for the target population.

The Multiphase Optimization Strategy (MOST; see Collins, Murphy, Nair, & Strecher, 2005 for details) for treatment development involves three phases to identify active components in a proposed protocol and the dose of each component most associated with optimal outcomes. In phase one, treatment components and components related to delivery of the treatment are assessed using either a fully crossed or fractional factorial design in which participants are randomly assigned to all potential combinations of components in a fully crossed design, or in the case of fractional designs, a subset of combinations. The goal is to identify the most efficacious combination of components. A similar design is used to refine the protocol in phase 2, where a factorial approach is used to assess the optimal dose of each component of the most efficacious combination identified in phase 1. In the last phase, the finalized treatment protocol is assessed using standard randomized clinical trial methodologies. A related approach called the Sequential Multiple Assignment Randomized Trial (SMART; see Murphy, 2005, for details) can be used either separately or within the MOST framework to design protocols that also account for when a component would be best applied and if the dose might vary based on the stage of therapy in order to optimize outcomes and resources.

SEE ALSO: Benchmarking Studies of Psychotherapy; Common (Nonspecific) Factors in Psychotherapy; Empirically Supported Treatments (ESTs) and Empirically Supported Principles of Change (ESPs); Mediation Analysis; Psychotherapy Process and Outcome Research; Randomized Clinical Trial

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Further Reading

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