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Some Methodological Considerations in Theory-Based Health Behavior Research

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Abstract

As this special issue shows, much research in social and personality psychology is directly relevant to health psychology. In this brief commentary, we discuss three topics in research methodology that may be of interest to investigators involved in health-related psychological research. The first topic is statistical analysis of mediated and moderated effects. The second is measurement of latent constructs. The third is the Multiphase Optimization Strategy, a framework for translation of innovations from social and personality psychology into behavioral interventions.

Keywords

mediation analysis; psychological measurement; Multiphase Optimization Strategy

The articles in this special issue contain fascinating insights into the complex processes of health behavior, and how theory in social and personality psychology can be used to establish theoretical models of health behaviors. We hope that these articles inspire readers to conduct research that tests hypotheses suggested by these theoretical models, and to use these theoretical models as the basis for behavioral interventions to prevent and treat disease, to promote health, and to improve quality of life. In this brief commentary, we discuss three methodological considerations that may be of interest to investigators involved in such research: mediation analysis, measurement of constructs in social and personality psychology and health behavior, and translation of theoretical models into behavioral interventions.

Mediation and Moderation of Effects

As the articles in this special issue clearly illustrate, mediators and moderators are central to theories of health behavior change. *Mediators* are variables that measure the processes described in a theoretical model. *Moderators* are variables that identify subgroups in which

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relations among variables differ. Together, these two types of variables provide a framework for investigating theories of health behavior (MacKinnon, 2008).

Each article in the special issue describes mediating variables. For example, Sheeran, Gollwitzer, and Bargh (in press) describe how intervention changes in attentional bias may reduce relapse among drug users. Major, Dovidio, and Berry-Mendes (in press) mention how intergroup relations of group categorization, hierarchy, and competition affect how members feel about groups and how these processes are related to perceptions and behaviors in a multiple mediator and outcome model. Pietromonaco, Uchino, and Dunkel Schetter (in press) describe how type of attachment predicts emotion regulation, which, in turn, affects health.

The importance of moderators in health is also illustrated in the articles in this special issue. Pietromonaco et al. (in press) describe how the impact of conflict on marital satisfaction differs depending on type of attachment. DeSteno, Kubzansky, and Gross (in press) describe how age moderates the effects of emotion on health. Mann, de Ridder, and Fujita (in press) mention how an individual's capacity for effortful inhibition may explain how stress is related to health. Moderation is particularly important in the area of adaptive behavioral interventions (Collins, Murphy, & Bierman, 2004). In adaptive interventions, the content and/or intensity of the intervention are varied in a principled manner in response to the characteristics and needs of individual participants. For example, a drug abuse treatment protocol might be different depending on whether an individual presents with depression. The purpose of varying the treatment is to ensure that it is equally effective for depressed and nondepressed participants. In other words, if the treatment were fixed (i.e., the same for all participants), depression status would moderate the treatment effect. Thus the theory underlying most adaptive behavioral interventions specifies how moderation would occur in a fixed intervention, and the adaptive intervention is designed specifically to eliminate this moderation and produce uniform treatment effects.

Methods for Investigating Mediation and Moderation

One approach to investigating mediation and moderation is to use mediating and moderating variables in statistical analysis after an effect has been observed to investigate how an effect occurred and for whom. An alternative, more powerful, approach is to investigate mediating variables using an experimental design in which a manipulation is directly targeted at a mediating variable hypothesized to be causally related to the outcome variable. The logic is that if the mediating variable is in fact causally related to the dependent variable, then changing the mediator will change the outcome.

Many behavioral health interventions are based on this type of mediation by design, whereby theory and prior experimental research is used to suggest mediating variables to be changed with an intervention. An important challenge of this approach is the specification of two types of theory: action theory for the actions that will change the mediator and conceptual theory for how mediators are related to outcomes (Chen, 1990). The a priori specification of how the intervention will work followed by the testing of this specification with data provides a powerful method to investigate processes of health behavior. Of course, there are complications to this simplified approach, including the existence of additional, possibly unmeasured, confounding variables, timing of intervention effects, and the extent to which processes differ across persons.

One crucial aspect of any mediation model is the establishment of temporal precedence for how an intervention changes a mediator, which in turn changes an outcome. An ideal health behavior theory makes clear predictions about how variables change and the timing of cause and effect; for example, the lag between the time that the cause operates and the time at

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which change in the mediator occurs (Collins, 2006; MacKinnon, 2008). The lack of clear specification of temporal relations, both in theories of cause and effect and in timing of measurement occasions in theory-testing research, is a substantial limitation of current health behavior research.

Another important aspect of mediation analysis is the fundamental question of the causal ordering among variables. Much stronger inference about this is possible when an experiment has been conducted and therefore the treatment X represents random assignment to experimental conditions. In a randomized experiment, it is clear that X comes first causally, and other putative explanations of the observed relations between X and the mediator M and between X and the outcome Y become much less plausible. However, even with randomization of participants to levels of X, the relation of M and Y is nonexperimental and therefore more complicated. In most cases, it is impossible to determine with certainty whether M causes Y, Y causes M, or there is some third variable that explains the observed M-to-Y relation (MacKinnon, 2008). Newly developed methods for causal inference have much to offer here (e.g., Coffman, 2011; Ten Have et al., 2007).

It is possible to investigate models that incorporate both mediation and moderation. Such models may more closely reflect actual health behavior and thus may be more realistic in many cases. However, they are more complex, and the associated statistical analyses are correspondingly complex. Moderation of a mediated effect occurs when the mediated effect differs across levels of a moderator. Mediation of a moderated effect occurs when an interaction involving two or more variables predicts a mediating variable, which in turn predicts an outcome. Methods for investigating moderation of a mediated effect and mediation of a moderated effect can be found in Fairchild and MacKinnon (2009), MacKinnon (2008), and MacKinnon and Luecken (2008). The translation of the theory outlined in the articles in this special issue to predictions regarding mediating and moderating processes is a critical next step. However, the state of knowledge in these areas may not be sufficient to clearly specify these relations; a series of exploratory studies may be required to develop clear tests of intervention processes.

Measurement of Psychological and Behavioral Constructs

Attributes of a High-Quality Behavioral Measure

All of the theoretical models discussed in this special issue involve latent (unobservable) constructs that cannot be directly measured from expert observation or laboratory tests, but rather depend on capturing information directly from individuals through questionnaires (surveys). These latent constructs, for example, perceived stress, self-efficacy, positive emotion, health knowledge, and social role function, must be measured using multiple questions that form a scale.

The selection of the appropriate scale is a critically important aspect of health behavior research. There are many issues to consider when selecting a self-report questionnaire/scale for measuring a social or behavioral construct. Most importantly, *reliability* and *validity* must be established. Ideally, the scale will have been validated in the study target population or a population similar to the target population (Nunnally & Bernstein, 1994; Scientific Advisory Committee of the Medical Outcomes Trust, 2002; Frost et al., 2007). In addition, scores on a health measure should be *interpretable to decision-makers*, which could include researchers, participants, clinicians, and policymakers, and provided in a metric that is easy to understand. In particular, it should be clear how much change over time in a score represents a meaningful change in the construct as opposed to random error (noise). Recent efforts in health outcomes research have focused on establishing minimally important differences (MIDs). An MID is the minimum change in the measure that is discernable to the study participants. The MID is different from a statistically significant difference, which is affected by sample size.

It is always a good idea to *minimize respondent burden* by keeping the difficulty and length of the questionnaire, as well as frequency of assessments in a longitudinal study, to the minimum that is necessary to accomplish the research goals. Further, item clarity and comprehensibility are critical to make the questionnaire appropriate for nonnative English speakers and respondents with low literacy levels. Planned missing data designs can be helpful in reducing the number of questions an individual respondent is asked to answer while maintaining the investigator's ability to address key research questions with the resulting data (Graham, Taylor, Olchowski, & Cumsille, 2006).

The United States is an ethnically and racially diverse population, and there are an increasing number of multinational research studies. Therefore, it is essential for questionnaires to be *available in multiple languages*. It is not important to multiethnic or national studies that the words are the same in all languages per se; rather, it is critical that the construct being measured is equivalent across languages and cultures. A more detailed discussion of this issue is provided in the next section.

With advances in technology, there has been a shift from paper-and-pencil questionnaires to questionnaires administered via telephone (e.g., interactive voice response), computers connected to the Internet, and, increasingly, various handheld devices. Tailoring the method of administration to individual preferences, limitations, or lifestyles improves a questionnaire's ability to assess people who are hard to reach or who have a disability. However, there are issues here similar to those that arise when a questionnaire is translated into several languages. Developers must test for conceptual equivalence when moving from the paper format to other administration modes, especially when more than one mode is to be used in the same study.

Reducing Measurement Bias Due to Subpopulation Differences in Questionnaire Responses

Research conducted within the United States or multinationally is faced with the challenge of measuring social and behavioral constructs in a multicultural, multiethnic, and/or multiracial context. To aggregate the data across groups or to compare beliefs or perceptions among the groups, it is necessary to assume that the questionnaire items are interpreted the same way by all groups, and that the construct measured by the questionnaire is equivalent across the groups. This can be a challenge. Whether a particular question or item is presented in a common language or translated to ensure linguistic equivalence, the words may have different meanings in different cultural, ethnic, or racial groups. Thus, differences between groups in responding to the question(s) may not reflect true underlying differences on the proposed measured construct, but may be confounded by cultural perceptions yielding biased results and/or qualitative differences in the construct itself. For example, Azocar, Arean, Miranda, and Munoz (2001) found that Latino respondents endorsed the item "I feel like crying" for a depression questionnaire more often than non-Hispanic American respondents, not because they were more depressed, but because in Latino cultures crying is a more socially acceptable behavior. Without controlling for these cultural differences, the Latinos would, on average, have a higher depression score than they would without this biased question.

When one group consistently responds differently to an item than another group after controlling for group mean differences on the underlying measured construct, this is known as differential item functioning (DIF; Holland & Wainer, 1993; Teresi, 2001). There are a

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number of statistical tools for identifying DIF in questionnaires. These include item response theory (IRT), contingency tables, structural equation modeling, and regression approaches (Teresi, 2006). DIF testing may be important not only for dealing with cultural or ethnic differences, but also when studies include different subpopulations, such as different age or disease populations. DIF can also occur when different assessment modes are used (e.g., computer-administered questionnaires and phone-based interviews) to collect respondent data. Once DIF has been detected, it can be extremely helpful to use qualitative methods, such as focus groups, to help uncover reasons for the observed differences. Recognizing and controlling for DIF in research studies will enhance the validity of the results.

A Shift Toward Data Harmonization in Health-Behavior Research

Although the use of psychometrically sound and clinically relevant measures will enable researchers to test social, behavioral, and personality theories in health settings, the health behavior research field will benefit to the extent that there is an agreed on single measure (questionnaire) for each domain (e.g., perceived stress, self-efficacy, depression, positive emotion) used in the field. This would enable researchers to synthesize findings across multiple studies in the field to strengthen the evidence base for generalizing theories across multiple populations and settings.

This push for harmonizing data is the impetus for the creation of the National Cancer Institute's Grid-Enabled Measures (GEM) Database. GEM provides researchers access to a database that can be used to review on a domain-by-domain basis which measures are appropriate based on their psychometric properties. It also provides an environment for researchers to share their experiences with the measures and findings from their research. The goals for the GEM project are: (a) to promote the use of standardized measures that are tied to theoretically based constructs; and (b) to facilitate the ability to share harmonized data resulting from the use of standardized measures (http://cancercontrol.cancer.gov/brp/gem.html).

However, it is likely unrealistic for the research community to agree on one measure for each domain, because there may be several good quality measures that currently exist (e.g., for depression) that are equally valid and reliable. An attractive solution to this challenge of multiple measures for the same domain is to use IRT modeling. With the appropriate dataset (ideally, having a representative sample answer questions on each measure), IRT models can be used to simultaneously link two or more questionnaires on the same metric. This would allow the creation of crosswalk tables to link scores from one existing measure to another. An excellent example of this concept is the National Institutes of Health's Patient-Reported Outcomes Measurement Information System (PROMIS), whose goal is to develop standardized measures of health-related quality of life domains (Cella et al., 2010). A key feature of PROMIS is its item banks, which contain a large number of items and questions from existing measures and new items all found to be psychometrically valid and responsive. Thus, scores on PROMIS can be transformed to equivalent scores on other measures calibrated in the item banks. The expansion of PROMIS to capture more social, behavioral, and personality concepts will greatly advance the field toward the goal of data harmonization.

Translating Innovations in Social and Personality Psychology Into Behavioral Interventions

The Multiphase Optimization Strategy

One value of social and personality theory and research is that it provides insights into human behavior that can form the basis for behavioral interventions. In this section, we

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discuss the Multiphase Optimization Strategy (MOST; Collins, Murphy, Nair, & Strecher, 2005; Collins, Murphy, & Strecher, 2007; Collins et al., 2011), a comprehensive framework for translating theory and research drawn from social and personality psychology and other fields into behavioral interventions.

As an example, suppose an investigator has been inspired by the compelling argument for the importance of emotion regulation in health behavior presented by DeSteno et al. (in press), and wants to translate these ideas into a behavioral intervention for weight loss. The investigator has developed a theoretical model specifying that weight loss can be achieved by (a) using cognitive reappraisal when faced with temptations to overeat or to skip exercise; (b) taking pride in each week's weight loss and the cumulative accomplishment of weight loss; and (c) using gratitude to develop a social support network among family and friends. (Note that this is a mediation model; it specifies that the intervention's effect on weight loss is mediated by cognitive reappraisal ability, ability to feel pride, and ability to use gratitude to develop a support network.) This suggests that the intervention should have three components, each aimed at teaching and encouraging practice of one of these three emotion-regulation skills.

An investigator using the approach to intervention development and evaluation that is most often used today would assemble the three components into a package and evaluate the package in a randomized controlled trial (RCT). The purpose of the RCT would be to estimate the size of the intervention's effect when compared to a suitable control or comparison group. This approach is generally the most appropriate one for addressing the important question of whether a multicomponent behavioral intervention has a statistically and clinically significant effect *as a package*. However, it is not a good approach for gathering information to build a highly effective behavioral intervention, because the RCT does not provide information about which of the *individual components* making up the intervention package are having the desired effect. Without this information, it is difficult to eliminate inactive intervention components, or to weigh the contribution of an intervention component against its implementation cost when deciding whether to include it. Obtaining information about the effects of individual components opens up some intriguing possibilities for intervention science. For example, this information makes it possible to select a set of intervention components with the objective of meeting a specific criterion, such as the largest effect that can be obtained without exceeding a predetermined implementation cost.

MOST was inspired by ideas and methods that are widely used in engineering. Evaluating a treatment package via the RCT is an essential part of MOST. However, equally essential in MOST is conducting systematic and highly efficient randomized experimentation to determine what should go into the treatment package. Thus if our hypothetical investigator were using MOST, he or she would not immediately form a treatment package and mount an RCT. Instead, the investigator would conduct a component selection experiment to obtain estimates of the individual effects of each of the three intervention components, then use this information as the basis for selecting effective components and discarding any ineffective ones. The investigator would then review other considerations, such as the cost in terms of money, time, and participant burden, and then decide which of the effective components would be included in the intervention package. Only then would an RCT be conducted to evaluate the newly constructed intervention package.

A Resource Management Perspective on Choosing an Experimental Design

In many cases, it is feasible to conduct a component-selection experiment using the same level of resources as would be required for an RCT. Empirical examples of component-selection experiments can be found in Strecher et al. (2008) and Collins et al. (2011).

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Experimental design selection in MOST is guided by the resource management principle (Collins, Dziak, & Li, 2009). According to this principle, the selection of an experimental design should be based on (a) what resources are available to conduct the experiment and (b) the key scientific questions to be addressed by the experiment. Then a design can be selected that makes the most efficient use of available resources to address the key scientific questions. There are many experimental design options, each of which may be highly efficient or highly inefficient, depending on the research questions at hand and the level and type of resources available.

Suppose in the hypothetical example the investigator decides that in order to be considered for inclusion in the intervention a component must demonstrate an effect size of at least $d = .20$, which is in the "small" range according to Cohen (1988). Further suppose that the experimentation will set each component to only two levels: on (presented to the subject) or off (not presented to the subject). There are two primary domains of resource demands in experimentation, namely, the cost of implementing experimental conditions and the cost of obtaining and retaining subjects. The investigator considers several approaches to estimating individual component effects, each of which makes different resource demands. (Formulas for comparing the resource demands of experimental designs can be found in Collins et al., 2009.) One approach is to conduct three separate experiments, one for each component. Another approach is to conduct a comparative experiment (Behar & Borkovec, 2003). A third approach is a 2 × 2 × 2 factorial experiment. It should be noted that because these designs estimate different effects, they are not interchangeable from a scientific perspective. A detailed explanation can be found in Collins et al. (2009).

The comparative experiment requires the smallest number of conditions. It involves four conditions: a control group in which all three components are off, plus three conditions in which one component is on and the other two are off (e.g., pride training on, the other two components off). (The additive and dismantling designs are closely related statistically and require the same number of conditions in most cases.) Next is the three separate experiments approach. Each experiment would have two conditions: one in which the component is on, and a control condition in which the component is off, for a total of six experimental conditions across the three experiments. The factorial experiment, which requires eight conditions representing all combinations of levels of the three components, is the most resource intensive in this domain.

Now let us examine the number of subjects that would be required in each design to achieve statistical power ≥ 0.8, for detecting main effects in the factorial experiment and treatment and control differences (which are not technically main effects; see Collins et al., 2009) in the other two approaches. It may surprise some readers that with a requirement of $N = 512$, the factorial experiment is the least resource intensive of the three alternatives in this domain, by far. The comparative experiment is next with a requirement of $N = 1{,}024$, or twice as many as the factorial experiment. (In general, additive and dismantling designs have identical sample size requirements to the comparative design.) Conducting three separate experiments would require a much larger sample size of $N = 1{,}536$, or three times what the factorial experiment would require. (Formulas to obtain these relative sample sizes appear in Collins et al., 2009.)

Which experimental design would be the most efficient for this study depends on the relative cost of the overhead for implementing experimental conditions compared to the costs associated with subjects. In this example, separate experiments would be eliminated from consideration because this approach is more resource intensive than the others in both the experimental conditions and the subject domains. A macro for comparing the resource

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requirements of different experimental designs can be found online (http://methodology.psu.edu/downloads).

Conclusion

This commentary has reviewed only three of the many methodological considerations that may arise when ideas like the ones described in the articles in this special issue are investigated. The consideration of mediating and moderating variables, both in research design and statistical analysis, provides a general approach to investigating how interventions work and for which groups of persons. Measurement is a central part of research in health psychology because of the variety of psychological and behavioral constructs routinely used in research studies. Finally, one approach to translating theory from social and personality psychology into an effective behavioral intervention is MOST, which involves evaluating individual program components using an efficient experimental design suited for this purpose. Like the theories outlined in the articles, each of these three methodological considerations is best evaluated within a program of research.

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