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Health Behavior Change: Moving from Observation to Intervention

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Keywords

experimental medicine, interventions, trials, theory, construct, correlational data

Abstract

How can progress in research on health behavior change be accelerated? Experimental medicine (EM) offers an approach that can help investigators specify the research questions that need to be addressed and the evidence needed to test those questions. Whereas current research draws predominantly on multiple overlapping theories resting largely on correlational evidence, the EM approach emphasizes experimental tests of targets or mechanisms of change and programmatic research on which targets change health behaviors and which techniques change those targets. There is evidence that engaging particular targets promotes behavior change; however, systematic studies are needed to identify and validate targets and to discover when and how targets are best engaged. The EM approach promises progress in answering the key question that will enable the science of health behavior change to improve public health: What strategies are effective in promoting behavior change, for whom, and under what circumstances?

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INTRODUCTION: A VIEW FORWARD

Over the past three decades, there have been tremendous advances in our understanding of the basic processes that shape people's behavior. We have come to recognize that different systems can regulate behavior (e.g., reflection versus impulse; Strack & Deutsch 2004), that different classes of cognitions shape and are shaped by behavior (i.e., implicit versus explicit cognition; Gawronski & Payne 2011), and that these processes unfold within a complex social world [e.g., relationships (Smith & Christakis 2008); discrimination (Pascoe & Smart Richman 2009)]. At the same time, it has only become more apparent that rates of morbidity and mortality depend critically on people's behavior (e.g., Mokdad et al. 2004, Yoon et al. 2014). National and international mandates to improve public health rely on changes in a broad array of behaviors, from diet and physical activity to tobacco and substance use to adherence to treatment and screening guidelines. Thus, efforts to meet these mandates depend upon intervention strategies effectively and efficiently modifying people's behavior (Rothman et al. 2015). However, when faced with the challenge of pursuing these mandates, interventionists and policy makers are left with more questions than answers as they grapple with limited theoretical and empirical guidance about what intervention strategy to use to modify a specific behavior and whether the intervention strategy is more or less effective for particular types of people or under particular conditions.

Given this challenge, researchers are increasingly recognizing the need for new approaches that can forge tighter links between advances in basic behavioral sciences and innovation in the design and delivery of strategies to improve public health (Czajkowski et al. 2015, Klein et al. 2015). The development of interdisciplinary research teams provides one valuable strategy because it facilitates communication about and use of information that too often remains siloed within a given discipline or area of study (Hall et al. 2012). However, there is also a need for an approach that shapes how basic and applied behavioral scientists specify the questions that need to be addressed and how they pursue the evidence needed to test those questions. To address this need, this review adopts the approach used to discern the mechanisms underlying treatments for disease (Bernard 1957) and advocates for an experimental medicine (EM) approach to research on health

behavior change (Riddle et al. 2015). We first describe the EM approach, how it differs from traditional efficacy trials, and how it relates to other research frameworks. We devote the body of the review to evaluating the theories, constructs, and evidence used in health behavior change research from an EM perspective. Finally, we offer directions for future research that are informed by an EM analysis of the current state of the field.

Efficacy trial: an experimental test of the impact of an intervention strategy on a health behavior or outcome that may eschew issues of target engagement

Targets: constructs that serve as mechanisms of action for health behavior change

Assays: measures of targets in experimental medicine

THE EXPERIMENTAL MEDICINE APPROACH

The EM approach offers a vantage point on what programs of research need to be undertaken, why research questions need to be tackled experimentally, and how different research programs can be marshaled to forge a more cumulative science of health behavior change. EM involves four steps, beginning with the identification of factors that relate to behavior and are potentially modifiable and that thus qualify as targets for interventions to change health behaviors (i.e., **Figure 1**, Path A). The second step is the validation of those targets by developing assays (measures) of the targets and assessing when, how, and to what extent those targets elicit behavior change (**Figure 1**, Path B). The third step tests different intervention strategies to determine how target engagement, the desired change in targets, can be maximized (**Figure 1**, Path C). Findings from studies following Paths B and C provide researchers with a firm foundation from which to pursue the final step, full tests or randomized controlled trials (RCTs) to determine whether an intervention strategy or a set of strategies changes behavior via their effects on specified targets (Path D). The EM approach can be contrasted with traditional efficacy trials (Path X), which are principally concerned with whether, not how, interventions promote health behavior change and often either fail to obtain target assays or omit analyses of target engagement or target validation.

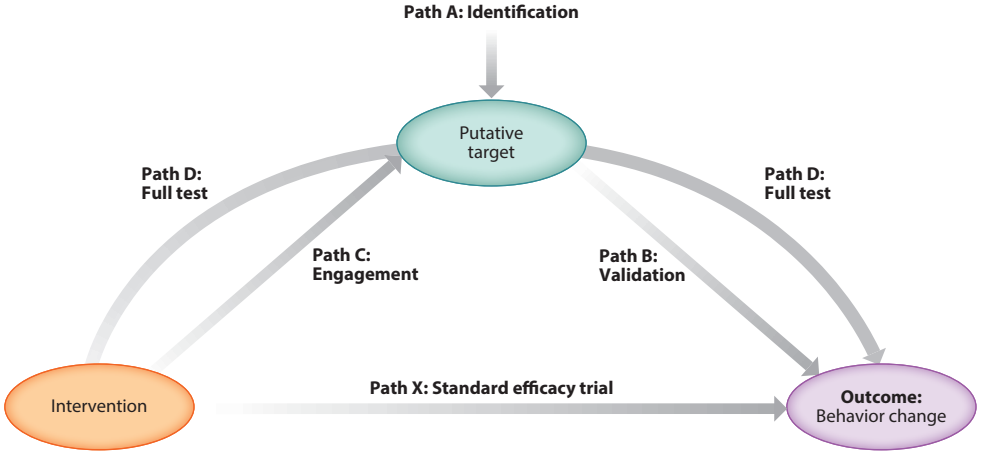

Figure 1

The experimental medicine (EM) approach to health behavior change. The EM approach specifies that research on health behavior change should proceed along four paths. Path A identifies putative targets, which are modifiable factors that may cause the behavior. Path B validates those targets by developing assays (i.e., measures) and testing the extent to which change in behavior accrues from manipulating the targets. Path C assesses the impact of different manipulations on the extent to which the target changes to discover how best to engage the target. Path D tests whether an intervention changes behavior because the intervention engaged the target and engaging the target changed the behavior. Whereas standard efficacy trials (Path X) often test only whether an intervention changes a behavior, the EM approach allows researchers to test both whether and why an intervention is effective. Figure adapted from Riddle et al. (2015).

Consider medication adherence as an example. Decades of research have established that rates of adherence to prescribed medication are suboptimal; however, interventions to improve adherence have been largely unsuccessful (e.g., Nieuwlaat et al. 2014). Interviews and surveys indicate that forgetting is the reason offered by most patients for failing to take their medication (e.g., Khatib et al. 2014). An EM approach to this problem might begin by identifying remembering to take one's medication as the target. The development of target assays (i.e., measures of remembering) might involve assessing whether patients have specified a particular opportunity for taking their medication each day (Gollwitzer & Sheeran 2006) or measuring the accessibility of this opportunity and the strength of its association with taking one's medication using a lexical decision task (Webb & Sheeran 2008). To validate this target, the researchers could test whether manipulating remembering (e.g., by mentally rehearsing the link between the relevant opportunity and taking one's medication) leads to improved adherence (Path B). Testing target engagement (Path C) involves assessing the impact of different strategies designed to enhance remembering to take one's medication. For instance, one could compare a pill box, a daily text message reminder, and the formation of an implementation intention [an if-then plan that specifies if this (opportunity) then this (response)] (Gollwitzer 1999). The data generated by these tests of Paths B and C enable researchers to undertake an RCT that tests, for example, the impact of implementation intentions on medication adherence via increased accessibility of the opportunity and stronger links between the opportunity and the behavior (Path D). A standard efficacy trial approach (Path X), by contrast, would make the leap from identifying forgetting as the reason for nonadherence to testing the effects of an intervention strategy (e.g., a text-based reminder system) on rates of adherence without testing the specific steps underlying any impact of the intervention. Thus, crucial information is not obtained or reported. For instance, if the intervention is unsuccessful, it cannot be determined whether this failure is due to the validity of the target (e.g., motivation to avoid side effects rather than forgetting explains nonadherence in this sample) or the strategy used to engage the target (e.g., the intervention did not increase the accessibility of the opportunity or strengthen the opportunity–behavior association). Furthermore, even if the intervention is successful, the investigators will have little information about why the intervention was effective and thus have limited guidance for subsequent efforts to refine or disseminate this intervention.

By emphasizing mechanisms of change, the EM approach unites basic research on behavior change with applied research that tests interventions to change health behaviors in clinical or field settings. Basic research is integrated into the larger process of studying health behavior change. Applied research focuses on Path D rather than Path X and can thus contribute rigorous tests of basic mechanistic processes that explain why interventions prove effective (or not). The EM approach promises that the production of knowledge about what targets to adopt and how they are best engaged will become more cumulative and efficient: Lessons can be learned from tests of Paths B, C, and D that cannot be learned from standard efficacy trials. Moreover, knowledge gained from tests in one domain can be applied to other health behaviors (e.g., strategies that improve remembering to take one's medication could also prove valuable in promoting flu vaccination or cancer screening) (Klein et al. 2016).

The EM approach to research on health behavior change underlies research frameworks such as the National Institutes of Health (NIH) stage model (Onken et al. 2014) and the NIH Science of Behavior Change initiative (SOBC) (Riddle et al. 2015) and is consistent with the Obesity-Related Behavioral Intervention Trials (ORBIT) model for developing behavioral treatments (Czajkowski et al. 2015), the United Kingdom's National Institute for Health and Care Excellence (2007) guidelines for behavior change, and methods such as the Multiphase Optimizations Strategy (MOST) (Collins et al. 2005, 2011) and Sequential Multiple Assignment Randomized Trial designs (SMART) (Lei et al. 2012). The EM approach and these other approaches are each characterized

by their responsiveness to the theoretical and practical challenges that underlie the development of effective intervention strategies. They engage with the practical challenges posed by the question of what intervention strategy to use for which person at what time or context and do so through a programmatic emphasis on experimentation. These approaches also recognize that our theories must provide more precise specifications than most current theories do. If we are to develop interventions that can effectively address the major challenges in public health, then our theories need to specify which mechanism of action to engage, what manipulations can elicit changes in that mechanism of action, and for whom and under what conditions these mechanisms and manipulations do and do not work.

The EM approach offers a useful lens through which to view the current state of research on health behavior change. In particular, EM invites reflection upon the progress of research concerning each of the paths specified in **Figure 1** and profitable directions for future work. In relation to Path A, we ask what targets are identified by contemporary health behavior theories and whether theories specify the parameters of target effects (i.e., what targets should be valid for different kinds of behaviors, samples, and contexts). We also discuss how targets are characteristically measured and offer evidence explaining why experiments are needed for target validation (Path B). We discuss the role of taxonomies of behavior change techniques (BCTs) and new trial designs in improving what can be learned from standard efficacy trials. Competitive tests of target engagement (Path C) and full tests (Path D) appear to be infrequent, and we discuss the implications of this dearth both for establishing targets and techniques and for developing new ones. The key message of the EM approach is that theoretical and empirical work that links change techniques to targets and targets to behavioral outcomes can move the discipline from observation to intervention and thus advance public health in domains such as adherence, tobacco use, alcohol abuse, vaccination, and suicide.

TARGETS SPECIFIED BY HEALTH BEHAVIOR THEORIES

Multiple Overlapping Theories and Targets

Health behaviors are "overt behavioral patterns, actions or habits that relate to health maintenance, to health restoration and to health improvement" (Gochman 1997, p. 3). Health behavior theories are a family of psychological models that have been used to understand and predict health behaviors (see the sidebar Health Behavior Theories). Numerous overlapping theories of health behavior have been developed or coopted since the inception of the health belief model (Rosenstock 1966). These theories include but are not limited to protection motivation theory (Rogers 1983), the extended parallel process model (Witte 1992), social cognitive theory (e.g., Bandura 1998), the transtheoretical model (Prochaska et al. 1992), the theory of reasoned action (Fishbein & Ajzen 1975), the theory of planned behavior (Ajzen 1991), and the prototype/willingness model (Gibbons et al. 1998) (for overviews of these theories, see Avishai-Yitshak & Sheeran 2017, Conner & Norman 2015, Glanz et al. 2008, Salovey et al. 1998). Typically, new theories are developed by adding new constructs to those specified by previous theories (for definitions of these constructs, see the appendix). For instance, protection motivation theory extends the health belief model by including (*a*) fear as a component of threat appraisal, alongside perceived risk and perceived severity of disease, and (*b*) self-efficacy as a component of coping appraisal, alongside beliefs about costs and benefits of action. Similarly, the theory of reasoned action has been extended by the inclusion of perceived behavioral control as an additional predictor of intentions and behavior (to form the theory of planned behavior) or by the inclusion of social prototypes and behavioral willingness (to form the prototype/willingness model). Thus, theoretical development has primarily involved

HEALTH BEHAVIOR THEORIES

The sheer volume of theories relevant to health behavior change presents a formidable challenge for testing and integrating new targets. For example, Michie et al. (2014) identified 83 theories comprising more than 1,000 constructs. Listed below are 17 frequently cited health behavior theories that represent both classic and contemporary research.

Classic Theories

- Health belief model (Rosenstock 1966)
- Protection motivation theory (Rogers 1983)
- Extended parallel process model (Witte 1992)
- Social cognitive theory (e.g., Bandura 1998)
- Transtheoretical model (Prochaska et al. 1992)
- Theory of planned behavior (Ajzen 1991)
- Control theory (e.g., Carver & Scheier 1982, Powers 1973)

Theories Developed During the 1990s

- Information-motivation-behavioral skills model (Fisher & Fisher 1992),
- Implementation intentions theory (Gollwitzer 1993, 1999)
- Health action process approach (e.g., Schwarzer 2008)
- Prototype/willingness model (Gibbons et al. 1998)

Recent Developments

- Temporal self-regulation theory (Hall & Fong 2007)
- Theories concerned with implicit processes, including Hofmann et al.'s (2008) adaptation of the reflective-impulsive model (Strack & Deutsch 2004) to health behaviors, Borland's (2013) CEOS theory, habit theory (Wood & Neal 2007), the nudge framework (Thaler & Sunstein 2008), and Papies' (2016) model of health goal priming

The targets specified by these theories are defined in the appendix Targets Specified by Health Behavior Theories, and the overlap among the theories and targets is illustrated in **Table 1.**

expanding the number of constructs in a theory rather than identifying when and how those constructs are most influential.

Piecemeal theoretical development has been accompanied by the use of a wide variety of terms to characterize essentially the same construct. Evaluation of the likely outcomes of performing health behaviors is termed attitude in the theories of reasoned action and planned behavior, outcome expectancies in the health action process approach, costs and benefits in the health belief model, and pros and cons in the transtheoretical model, and it is captured by the concepts of response efficacy and response costs in protection motivation theory. These different terms are also associated with distinct measures even though there appears to be no difference in their predictive validity (Sheeran et al. 2016).

The use of multiple terms for the same construct belies a different problem, the failure of health behavior theories to embrace important distinctions within constructs. For instance, the distinction between cognitive beliefs (concerning the instrumental consequences of behavior) and affective beliefs (concerning the emotional consequences of performing a behavior) as components

Table 1 Overlap among targets specified by major health behavior theories

Putative targets	Health behavior theories												
	Extended parallel process model	Health belief model	Protection motivation theory	Transtheoretical model	Theory of planned behavior	Social cognitive theory	Control theories	Information-motivation-behavior skills	Health action process approach	Prototype/willingness model	Temporal self-regulation theory	Implementation intentions theory	Implicit process theories
Cognitions about health threat													
Risk perception	✓	✓	✓					✓	✓				
Perceived severity	✓	✓	✓					✓					
Fear or worry	✓		✓										
Cognitions about focal behavior													
Intention	✓		✓		✓	✓		✓	✓	✓	✓	✓	✓
Willingness										✓			
Attitude	✓	✓	✓	✓	✓	✓		✓	✓	✓	✓		✓
Norms					✓	✓		✓	✓	✓			
Self-efficacy	✓		✓	✓	✓	✓		✓	✓	✓	✓	✓	
Social prototypes										✓			

(*Continued*)

Table 1 *(Continued)*

Putative targets	Health behavior theories												
	Extended parallel process model	Health belief model	Protection motivation theory	Transtheoretical model	Theory of planned behavior	Social cognitive theory	Control theories	Information-motivation-behavior skills	Health action process approach	Prototype/ willingness model	Temporal self-regulation theory	Implementation intentions theory	Implicit process theories
Volitional factors													
If-then plans									✓			✓	
Behavioral skills								✓					
Processes of change				✓									
Progress monitoring							✓						
Executive function											✓		✓
Implicit cognition													
Attentional bias													✓
Implicit attitudes													✓
Approach bias													✓
Implicit goals or norms													✓
Habits											✓		

of attitude is supported by principal component analyses of survey items (e.g., Crites et al. 1994) and cognitive paradigms (Trafimow & Sheeran 1998). Primary studies (e.g., Lawton et al. 2009) and meta-analysis (e.g., Rhodes et al. 2009) indicate that affective beliefs are stronger predictors of health-related intentions and behavior than are cognitive beliefs. However, current health behavior theories do not formally specify affective attitude as a distinct target for intervention (but see Conner & Sparks 2015, Kiviniemi et al. 2007). Similarly, there is a long-standing distinction between injunctive norms (beliefs about what other people think one should do) and descriptive norms (beliefs about what other people themselves do) (see Deutsch & Gerard 1955), but most health behavior theories focus on either injunctive norms (e.g., the theory of planned behavior) or descriptive norms (e.g., the prototype/willingness model), and no theory discusses both types of norms. Several theories (e.g., social cognitive theory) appear to construe norms as mere social consequences (e.g., doing X will lead to approval from others), although evidence suggests that beliefs about social consequences do not capture the impact of norms (e.g., Trafimow et al. 2010).

Decisions about whether constructs are equivalent or distinct encompass larger questions about how to update health behavior theories with new constructs and what evidence is needed to warrant theoretical development. Although theories have developed via the inclusion of new constructs (e.g., the prototype/willingness model), in several instances new constructs for which empirical support seems compelling have not been integrated into health behavior theories. One prominent example is habit (for a review, see Wood & Rünger 2015). Ouellette & Wood (1998) and Webb & Sheeran (2006) found that intentions were good predictors of infrequently performed behaviors (e.g., cancer screening) but observed that the predictive validity of intention was much weaker for behaviors that were performed repeatedly in stable contexts and could thus become habitual (e.g., exercise). This finding suggests that intention may not be the most appropriate target for interventions designed to change habitual behaviors but also indicates that habit formation could be a worthwhile target for interventions designed to promote behavioral maintenance (Rothman et al. 2015). However, despite decades of research on habit, temporal self-regulation theory (Hall & Fong 2007) is the only prominent model of health behavior that makes explicit mention of habit.

Table 1 presents a cross-tabulation of health behavior theories by the targets specified by those theories. Targets can be grouped into four broad categories: cognitions about the health threat (i.e., beliefs about the focal disease or condition), cognitions about the health behavior (i.e., beliefs about the focal behavior), volitional factors (i.e., factors that serve to bolster motivation or promote more effective translation of intentions into action), and implicit cognition (i.e., automatic responses to relevant stimuli). The overlap in the targets specified, especially by classic theories (see the sidebar Health Behavior Theories), is striking. Whether theories emphasize the role of cognitions about the health threat and the behavior (e.g., health belief model) or cognitions about the behavior only (e.g., social cognitive theory) appears to be an important distinction. Classic theories generally do not specify volitional or implicit factors as targets. However, many newer theories make little mention of the role of cognitions about the health threat or the behavior, so it is unclear whether the targets specified by those theories are designed to supplant or complement the targets specified by earlier models. Moreover, the potential for redundancy or synergy among volitional and implicit targets remains to be determined. For example, it is not yet clear whether there is redundancy among implicit targets such that attentional bias, implicit attitudes, and approach bias are not all needed to understand health behavior performance. Evidence of synergy across targets can be seen in studies that combine if-then plans with relevant behavioral skills (e.g., Achtziger et al. 2008, Sheeran et al. 2007), processes of change (e.g., Armitage 2009), and progress monitoring (Harkin et al. 2016) or use if-then plans to compensate for poor executive function (Hall et al. 2014). However, simultaneous tests of multiple targets—both within and between key categories of targets—are relatively rare and constitute an important avenue for future research. The field

offers many theories to guide our understanding of (and attempts to change) health behavior, yet there is both too much overlap (in terms of the targets that are identified) and too little overlap (in terms of accumulated knowledge about the relative importance of targets or how targets should be integrated) among theories. This state of affairs makes it difficult for an investigator to make informed choices about which targets to use in interventions.

The Parameters That Regulate Target Effects

Kurt Lewin's admonition that there is nothing more practical than a good theory relies on the existence of good theories (Rothman 2004, Rothman et al. 2013). What should a good theory be able to do? Although there is likely no single answer to this question, efforts to link theory and intervention provide insight into the questions that theories could be fashioned to address. Interventionists need to know what targets to engage, but that alone is not sufficient. Interventions are situated within a multidimensional context characterized by the behavior, the sample, and the setting. Thus, interventionists turn to theories to specify not only the targets for intervention but also the parameters that regulate target effects: For what behavior(s) and sample(s) and in what contexts (the psychological, physical, or social conditions) does engaging the target promote behavior change?

Classic health behavior theories identify a rich array of targets but provide limited guidance regarding the parameters that regulate the impact of targets on behavior. Constructs such as perceived risk, perceived severity, attitudes, norms, and self-efficacy are specified in theories as predictors—directly or indirectly—of behavioral performance irrespective of the focal behavior, sample, or setting. Although theorists would agree that these contextual factors affect the impact of targets and interventionists are advised to conduct initial assessments or focus groups to ascertain the applicability of specific targets, theories have not evolved to provide a priori guidance regarding the relative impact of specific targets (Rothman 2009).

Some theories have begun to offer greater specificity. For example, the prototype/willingness model proposes that behavioral willingness is a stronger predictor of behavior than is behavioral intention when the behavior involves risk (e.g., alcohol or drug use) and among younger people and those having less experience with the behavior (Gibbons et al. 2004). In contrast, behavioral intention is a stronger predictor of behavior than is behavioral willingness when the focus is on protective behaviors (e.g., physical activity) and among older or more experienced people (for a review, see Gerrard et al. 2008). Theories have also begun to delineate the conditions under which explicit (self-reported) attitudes have less of an influence on behavior. For example, according to Hofmann et al.'s (2008) dual process model, when people face impairments in self-control or working memory capacity (due to state or trait factors), the impact of explicit attitudes on health behavior is attenuated. Similarly, investigators have demonstrated that the formation of implementation intentions (i.e., if-then plans) is particularly effective when people are faced with a behavior that is difficult to perform or when they have chronic difficulty regulating their behavior (Gollwitzer & Sheeran 2006).

Theories have also begun to suggest that specific targets might be more influential at different times during the behavior change process. Rothman and colleagues (Rothman 2000, Rothman et al. 2011) have emphasized distinctions between the factors that underlie the initiation versus the maintenance of behavior change. For example, outcome expectations and self-efficacy are thought to be important determinants of behavioral initiation, whereas satisfaction with behavior change is thought to be an important determinant of behavioral maintenance (e.g., Baldwin et al. 2006, Hertel et al. 2008). Furthermore, people's dispositions for promotion or prevention affects their ability to successfully initiate and maintain behavior change (Fuglestad et al. 2008, 2013).

However, even with these advances, health behavior theories remain woefully underspecified. They provide remarkably few direct answers to the questions that emerge when researchers are faced with the challenge of intervening to change behavior: For which behaviors, samples, or settings will engaging a target be particularly effective, and perhaps even more important, under what conditions will target engagement not be effective? Given the scale and scope of empirical work in this area, why have these theories not evolved to answer these questions and address these needs? One important challenge may be the way in which theorists construe evidence of moderation or boundary conditions on effects (Rothman 2013). If a theoretical principle is thought to hold across behaviors, samples, or settings, then empirical evidence concerning boundary conditions could be considered a limitation; the failure to observe the predicted relation under certain conditions is construed as challenging the theory. Given this mind-set, investigators may focus their work on demonstrating that evidence of moderation is the exception rather than on trying to delineate the factors that mark the boundary conditions of the target.

What if investigators were to approach the same studies with an a priori commitment to discovering when or where the principle underlying their work may and may not operate? With this mind-set, obtaining evidence of moderation would not be construed as a limitation or a challenge but as an opportunity to improve the specificity and precision of the theory (McGuire 1989, Rothman & Salovey 2007). Moreover, because investigators would be prepared from the outset to delineate the target's boundary conditions, they would be more likely to make methodological decisions that improve the quality of the evidence generated. In particular, the moderator would be more likely to be measured with a valid and reliable instrument and at the right time within the study design. The investigator would also be more likely to capture the processes that underlie evidence of moderation, linking mediators and moderators both theoretically and statistically.

For example, gender might moderate the effect of an intervention strategy such that it works for women but not men either because gender moderates the effect of the intervention on the target (i.e., Path C) or because gender moderates the effect of the target on the behavioral outcome (i.e., Path B). These two outcomes have distinct implications for both theory and practice (Rothman 2013, Rothman & Baldwin 2012). In the first case, the evidence would indicate that the target may be appropriate for both men and women, but a different strategy is needed to engage that target for men. In the second case, the evidence would indicate that the strategy is able to affect change in the target equally well for men and women, but the target is not an important determinant of the behavior for men. The accumulation of empirical evidence that can distinguish between these two explanations would, over time, provide the evidence base necessary to enhance the specification of our theories. The fact that this approach to moderation has not been fully embraced may help to explain why our theories remain underspecified despite the accumulation of an enormous body of empirical work (Noar & Zimmerman 2005, Weinstein & Rothman 2005).

Target Measurement

An essential precursor to identifying promising targets for interventions to promote behavior change is having reliable and valid target assays. Importantly, the EM approach encourages the development of assays that capture the target at different levels of analysis (e.g., self-report, behavioral, neural, and physiological). Most of the targets specified by health behavior theories are measured using self-report questionnaires. Self-reports have considerable advantages (e.g., ease of use, capacity to survey large numbers of participants) but also well-known limitations (e.g., memory, accessibility, social desirability, and self-presentational bias; for a review, see Podsakoff et al. 2003). Concerns about self-report measures, combined with technological advances in measurement over the past 20 years, highlight the need for and potential to develop new and more

reliable target assays. Sensor and wireless technologies allow researchers to capture affective states (e.g., using voice pitch), exposure to social norms (e.g., using online recordings of social interactions), actual rather than perceived barriers (e.g., using GPS codes to determine whether the built environment facilitates physical activity), self-regulatory resources (e.g., using grip strength), attention to health information (e.g., using unobtrusive eye tracking), and health behavior itself (e.g., using accelerometers to measure physical activity). Immersive virtual reality environments (Persky 2011) can capture numerous targets in the context in which behavior takes place, increasing ecological validity. Such observational measures could serve not only to validate self-report assays but also to explain unique variance in health behavior. For example, Falk and colleagues (2010, 2011) observed that neural responses to persuasive messages explained substantial variance in health behavior change even after standard self-report assays were taken into account. Moreover, neural responses to different smoking cessation campaigns predicted the volume of calls to smoking quit lines at the population level, whereas self-reports concerning campaign effectiveness did not (Falk et al. 2012).

The increased ability to capture theoretically important targets with a variety of assays changes not only what can be measured but also when and how often targets can be measured. This progress is important considering that most tests of theories hinge on the measurement of constructs at a single, static moment in time. Researchers might measure participants' intentions to use a condom with a one-time survey—likely at a time when the behavior is not salient—and then use this measure to predict subsequent condom use. If intentions are measured more than once, however, it becomes possible to assay the temporal stability of intention, which is an important determinant of whether or not intention gets translated into action (e.g., Sheeran & Abraham 2003). Fluctuations in cognitions are likely commonplace, as demonstrated by research on the empathy gap (Loewenstein 2005) showing that, when people are in a cold (e.g., satiated) state, they have different intentions and self-efficacy than when they are in a hot (e.g., hungry) state (Nordgren et al. 2008). Moreover, fluctuations may characterize not only scores on the target assay but also how well or under what circumstances the target relates to behavior.

Research on health behavior change also departs from the EM approach by failing to standardize measurement of key targets. Clear medical guidelines exist for measuring blood pressure and concluding whether a single patient's blood pressure is high, low, or normal (Weber et al. 2014). There are no equivalent guidelines for the targets specified by health behavior theories. Fortunately, efforts are underway to address the need for consensual measures of health behavior constructs. The NIH has supported the funding and development of numerous portals that archive reliable, tested, and professionally vetted measures of many different constructs, including targets specified by health behavior theories. Examples include the Patient-Reported Outcomes Measurement Information System (PROMIS) (Carle et al. 2015), the Grid-Enabled Measures (GEM) project (Moser et al. 2011), and the Phenotype Measurement (PhenX) project (Hamilton et al. 2011). The NIH is also working on supporting tools that facilitate the development and use of shared definitions of constructs used across health behavior theories. Taking an EM approach requires thorough and rigorous efforts to assay targets. To this end, the field of health behavior needs to work toward the development of consensual and evidence-driven assays and take full advantage of the many new methods of obtaining target assays at different levels.

THE IMPACT OF TARGET ENGAGEMENT ON HEALTH BEHAVIORS

Evidence concerning the impact of target engagement (Path B) predominantly comes from observational studies that measure targets at one time point and measure health behavior(s) either at the same or a later time. Such correlational data largely support the predictive validity of

targets specified by health behavior theories. For example, several meta-analyses indicate that risk perceptions and perceived severity (e.g., Brewer et al. 2007, Milne et al. 2000), intentions and self-efficacy (e.g., McEachan et al. 2011), and implicit attitudes (e.g., Greenwald et al. 2009) each have average correlations of medium-to-large magnitudes with health behaviors. However, there are several reasons why correlational data can be misleading about how much behavior change will accrue from target engagement. The first reason is that the associations typically reported in meta-analyses are bivariate and do not take the influence of alternative targets into consideration. Thus, for example, it is unclear how much variance perceived severity explains in behavior once risk perceptions have been taken into account or how well self-efficacy predicts behavior over and above intention. Greenwald et al. (2009) observed that the average correlation between implicit attitude and behavior was $r_+ \approx 0.18$, controlling for explicit attitude. However, the choice of alternative targets likely plays a role. Explicit attitudes exhibit weaker correlations with health behaviors compared to intention and self-efficacy (e.g., McEachan et al. 2011), and the increment in variance explained by implicit attitude after associations with these targets have been considered remains unclear (Blanton et al. 2016). It is also unclear how large an increment in variance a target would have to explain for researchers to consider the target valid and thus worth engaging in a planned intervention (for a seminal analysis of the problems of drawing inferences from change in R^2, see Trafimow 2004).

The second reason is that past behavior is not typically taken into account. Bivariate correlations do not indicate whether targets predict changes in behavior. To gain insight into this issue, we reanalyzed data from McEachan et al.'s (2011) meta-analysis. The sample-weighted average correlations between intention and behavior, past behavior and intention, and past behavior and current behavior were each of medium-to-large magnitude ($r_+ = 0.40, 0.44$, and 0.48, respectively; minimum $N = 21{,}786$). Controlling for past behavior, however, the average correlation between intention and behavior change was only $r_+ = 0.21$, a small-to-medium effect size (Cohen 1992). It seems that the prediction of health behavior change is a good deal more modest than is the prediction of health behavior.

The third reason is that correlational designs cannot rule out the impact of unmeasured (third) variables and are therefore not fit for determining whether engaging a target changes health behaviors (Weinstein 2007). Only experiments permit causal inferences. Experiments have three defining features: (*a*) Participants are randomized to a treatment versus a control or comparison condition, (*b*) the treatment engages the target (i.e., engenders a difference between the treatment versus control condition on the target assay), and (*c*) behavior change or some other outcome is measured in the wake of the intervention (e.g., West et al. 2000). In a series of meta-analyses (Sheeran et al. 2014, 2016; Webb & Sheeran 2006), we leveraged these features of experiments to address the fundamental question that underlies the experimental medicine approach: How much change in behavior accrues from interventions that successfully engage key targets specified by health behavior theories? Findings showed that changing cognitions about the focal threat (risk perception, perceived severity) led to small or small-to-medium changes in health behaviors ($d_+ = 0.25$ and 0.34, respectively). Changing cognitions about the focal behavior also proved effective. Heightening intentions, attitudes, and social norms each had small-to-medium effects on behavior change ($d_+ = 0.36, 0.38$ and 0.36, respectively), whereas increasing self-efficacy had a medium effect ($d_+ = 0.47$). These findings indicate that designing interventions that engage risk perceptions, perceived severity, attitudes, social norms, intentions, or self-efficacy should be effective in promoting health behavior change.

How well do findings from correlational tests of these targets compare to the experimental results? We used Milne et al.'s (2000) meta-analysis of prospective correlational studies to represent the average correlations for risk perception and perceived severity and Sheeran et al.'s

(2016) meta-analysis of 18 previous meta-analyses to represent the average correlations between health behaviors and attitudes, social norms, and self-efficacy. The average correlation between intention and health behavior came from McEachan et al.'s (2011) quantitative review. All effect sizes were converted to Cohen's (1992) *d*-values for comparison purposes. Overall, correlational studies offered a poor estimate of the impact of targets on health behavior change observed in experimental studies. Correlational tests underestimated the impact of engaging perceived severity ($d_{correlational} = 0.14$ versus $d_{experimental} = 0.34$), slightly overestimated the impact of engaging self-efficacy ($d_{correlational} = 0.58$ versus $d_{experimental} = 0.47$), and substantially overestimated the impact of engaging attitude ($d_{correlational} = 0.70$ versus $d_{experimental} = 0.38$) and intention ($d_{correlational} = 0.87$ versus $d_{experimental} = 0.36$). Only in the case of risk perception and norms were the effect sizes from correlational and experimental tests of equivalent magnitude ($d_{correlational} = 0.24$ and 0.41 versus $d_{experimental} = 0.25$ and 0.36, respectively). These findings would seem to cast serious doubt on the value of correlational tests for intervention design: How well a target predicts health behavior does not indicate how much change in behavior accrues from engaging that target.

Evidence regarding the effects of several other targets identified in **Table 1** rests primarily on correlational tests. We could locate only a single experiment that engaged implicit attitudes and tested their effects on health behaviors (Hollands et al. 2011). Similarly, relatively few studies have systematically manipulated social prototypes (Gerrard et al. 2006, Rivis & Sheeran 2013) or processes of change (e.g., Armitage 2009). Behavioral skills from the information-motivation-behavioral skills model have been tested in several interventions (e.g., Chang et al. 2014), but it is difficult to disentangle the precise contribution of this target compared to the other targets included in the intervention (e.g., motivation, self-efficacy). Executive function (EF) and habits are expected to moderate the influence of other targets (e.g., intentions) and tend to be measured rather than manipulated in studies of health behavior. One intervention study involving working memory training observed reliable effects on subsequent alcohol consumption (Houben et al. 2011), but the magnitude and mechanisms of EF training effects in adults are the subjects of debate (e.g., Shipstead et al. 2012). Training programs to improve self-control, which EF subserves (Hofmann et al. 2012), do not appear to be effective (Inzlicht & Berkman 2015, Miles et al. 2016).

In contrast, evidence regarding the effects of implicit goals, approach bias, progress monitoring, and if-then plans rests predominantly or exclusively on experimental tests. There are compelling demonstrations of changes in eating behavior immediately following priming of health goals (e.g., Papies & Hamstra 2010, Papies & Veling 2013, Papies et al. 2014) and improvements in hand hygiene following priming of injunctive norms (King et al. 2016). The reliable associations between approach bias and risk behavior observed in correlational studies (e.g., Palfai & Ostafin 2003) have been complemented by inhibitory control training interventions to reduce this bias. A meta-analysis of RCTs reported a small-to-medium effect of training on health behaviors ($d_+ = 0.33$, Allom et al. 2016), although follow-up periods were generally short. Interventions that promote monitoring of goal progress (e.g., via food diaries, pedometers) are highly effective at increasing rates of progress monitoring ($d_+ = 1.98$) and lead to small-to-medium changes in health behaviors ($d_+ = 0.40$) (Harkin et al. 2016). Several meta-analyses indicate that if-then plan interventions are effective in promoting health behaviors (Adriaanse et al. 2011, Bélanger-Gravel et al. 2013, Gollwitzer & Sheeran 2006). If-then plans have proved effective in large-scale public health interventions (e.g., Neter et al. 2014) and, over extended periods, for consequential health behaviors such as pregnancy prevention (Martin et al. 2011) and reducing smoking uptake among adolescents (Conner & Higgins 2010). In sum, there are several reasons to question the adequacy of correlational tests of the impact of target engagement on health behaviors and to advocate for conducting experimental tests instead. Although many of the targets specified by health behavior have been validated by experiment, experimental tests are overdue for several other targets.

LEARNING FROM EFFICACY TRIALS

Numerous successful efficacy trials indicate that interventions are effective in promoting health behavior change (e.g., Diabetes Prev. Progr. Res. Group 2002, Greaves et al. 2011, Lemmens et al. 2008). For understandable reasons, efficacy trials focus on the intervention's ultimate impact on behavior, and it is often not feasible to obtain assays of targets that were engaged by the intervention. The absence of target assays poses a problem, however, as it is not possible to explain why interventions prove successful or unsuccessful; what can be learned from such trials is therefore limited. Two important responses to the issues raised by standard efficacy trials concern the development of taxonomies of BCTs and new approaches to trial designs.

Behavior Change Techniques

Several guidelines indicate that reports of behavior change interventions should specify such important features as the source, recipients, setting, duration, intensity, delivery mode, and fidelity of the intervention, as well as its content (e.g., Boutron et al. 2008, Moher et al. 2001, Davidson et al. 2003, Des Jarlais et al. 2004). Abraham & Michie (2008) pointed out that even though the contents of behavior change interventions constitute the programs' active ingredients, these contents are often poorly specified in the methods section of reports—the generic label behavioral counseling is probably the most notorious example. The first characterization of intervention content was in a meta-analysis of HIV-prevention interventions. Albarracín et al. (2005) identified ten techniques (such as information and attitudinal arguments) and tested whether the presence versus absence of these techniques was associated with changes in condom use. Importantly, this meta-analysis also tested whether techniques engaged respective targets (e.g., information changed knowledge, attitudinal arguments changed attitudes) and whether target engagement led to changes in condom use. Albarracín et al. (2005) were able to present decision trees that designated which techniques were effective for samples that differed in terms of gender, age, ethnicity, and risk status.

Of course, there are more than ten techniques that can be used to change health behaviors, and researchers are generally unable to trace the impact of BCTs through targets to behavioral outcomes in efficacy trials because target assays are not obtained. To formalize matters, Abraham & Michie (2008) offered a taxonomy of 26 BCTs that included clear definitions of each technique and evidence that these techniques could be identified reliably from intervention descriptions. Subsequent refinement and extension of the taxonomy has generated a hierarchical classification of 93 discrete BCTs (Michie et al. 2013).

The development of taxonomies of BCTs has been a boon to research on health behavior change. It has served to focus researchers' attention on the components of the intervention that engender change. BCT taxonomies have also given the field a common language with which to characterize intervention components and have thus facilitated the development, replication, and implementation of behavior change interventions in a manner that was not previously possible. Moreover, coding BCTs from reports of interventions has enabled researchers to compute associations between the use of particular techniques and the effect sizes obtained in RCTs (via metaregression and related techniques). In this way, researchers have been able to determine which BCTs are associated with a larger or smaller effect size and which are unrelated to effect size for various health behaviors (e.g., Bartlett et al. 2014, Dombrowski et al. 2012, Webb et al. 2010). BCT taxonomies thus enable informed comparisons between efficacy trials that involve complex intervention content and offer insights that go beyond the mere assessment of overall effectiveness.

However, efforts to relate BCTs to intervention outcomes face significant practical and conceptual challenges. At the practical level, extensive training is required to code interventions in terms of 93 discrete techniques, and reliability is modest ($\kappa < 0.70$) for approximately 20% of

these 93 BCTs (Abraham et al. 2015). Published descriptions of the BCTs used in interventions can also differ markedly from the BCTs coded from the original intervention manuals (Abraham & Michie 2008), which are often difficult to obtain (Abraham et al. 2014). Moreover, coding the BCTs used in the control conditions in addition to those used in the treatment conditions may be crucial for accurate interpretation of BCT associations. This is because the techniques used in the control condition (e.g., usual care) can explain substantial variance in effect sizes, and the BCTs deployed in the treatment condition may explain only a modest increment in the variance after techniques used in the control condition have been taken into account (de Bruin et al. 2010).

Conceptual issues must also be considered. Metaregressions of effect sizes on BCTs have revealed no significant associations between individual BCTs and intervention effectiveness (e.g., Hartmann-Boyce et al. 2014, Michie et al. 2009), which raises difficult questions about whether the coding of BCTs or the techniques themselves are causing the problem. Findings from metaregressions of effect sizes on BCTs can also be at odds with findings from meta-analyses of experimental trials that focus on particular techniques. For example, Michie et al.'s (2009) meta-analysis observed no reliable associations between implementation intentions and weight control behaviors (diet, physical activity), whereas meta-analyses of implementation intention interventions in relation to diet (Adriaanse et al. 2011) and physical activity (Bélanger-Gravel et al. 2013) observed reliable effects. Similar conflict is apparent in data regarding the effectiveness of progress monitoring (see Michie et al. 2009, Harkin et al. 2016). Analyzing the BCTs used in interventions can offer researchers who are planning trials valuable clues about which targets should be engaged and which procedures should be used to engage those targets. However, retrospective cataloging of intervention content and correlational tests of that content via metaregression cannot substitute for experimental studies to validate, engage, and offer full tests of targets.

New Approaches to Trials

Given the goal of specifying which intervention strategy to use for which person at what time or in what context, there is a clear need for approaches to RCTs that not only embrace the complexity of this goal but also strive to address it in an effective and efficient manner. Although there are numerous innovations in approaches to RCTs, we focus on two that are particularly synergistic with the aims of this review: multiphase optimization strategy (MOST) (Collins et al. 2005, 2011) and sequential multiple assignment randomized trial (SMART) designs (Lei et al. 2012). Both approaches demonstrate the ways in which experimentation can offer a more precise understanding of which intervention components should be used (i.e., what works) and under what conditions (i.e., when it works).

Interventions typically include many BCTs, making it difficult to discern the relative contribution that each BCT makes to the desired outcome. MOST provides a framework for assessing the relative contribution of discrete techniques by encouraging investigators to first specify the distinct factors that make up the intervention (consistent with the BCT approach discussed above) and then develop an experimental design that can assess the relative contribution of an identified set of factors on a specified outcome. Through the use of factorial or fractional factorial experiments (see Collins et al. 2005), investigators are able to test competing hypotheses about the relative impact of intervention strategies that are designed to either affect change in the same target (providing insights into which strategy is the most effective way to modify a target) or affect change in different targets (providing insights about which target pathway is the most effective means to modify behavior). Moreover, MOST is amenable to evaluating the effect of parameters of the context in which the intervention is delivered (e.g., neighborhood resources) that may not be specified

formally in theory. This should make it easier for investigators to articulate the conditions under which specific intervention targets should and should not be engaged.

Adaptive interventions can respond to the challenge of optimizing intervention strategies to match features of the individual, behavior, and context while also recognizing that the optimal strategy may change over time in response to changes in the person or the situation. Thus, adaptive interventions rely on decision rules that determine which intervention strategies to use at which time and in which sequence on the basis of prespecified factors (e.g., a person's response to an initial intervention strategy). SMART designs provide a framework for developing adaptive interventions by providing an opportunity to rerandomize participants to a subsequent intervention strategy in response to predetermined indicators (Almirall et al. 2014, Lei et al. 2012). Although SMART designs have been used primarily in domains in which response to an initial treatment strategy is poor (e.g., substance use), these designs can address nearly any question regarding the ongoing tailoring or targeting of intervention strategies. For example, all treatment participants could be randomized to an if-then plan intervention (a treatment that requires modest resources). Participants who respond to the treatment could be randomized to no further treatment or to booster if-then plans. Participants who do not respond to the treatment might need a more resource-intensive treatment (e.g., a motivational intervention plus if-then plans). In this way, participants receive the treatment they need, but there are savings in terms of intervention resources and participants' time that would not have been achieved if all participants had received the combined motivation plus if-then planning intervention at the outset. SMART designs thus provide a framework for examining whether there are advantages to engaging specific targets in a particular sequence and have the potential to stimulate significant transformations in current health behavior theories.

COMPETITIVE TESTS OF TARGET ENGAGEMENT

Effective Techniques in Search of Targets

Often, a technique is found to promote behavior change in traditional efficacy trials but the relevant target or mechanism of action linking the technique to behavior change remains unclear. Two prominent techniques in research on health behavior change that exhibit this feature are the question-behavior effect (QBE) and self-affirmation. The QBE is the phenomenon whereby questions about a target behavior (e.g., behavioral intentions) promote performance of that behavior relative to participants who were not asked questions. The QBE has been observed in relation to various health behaviors (e.g., vaccination uptake, cancer screening), and meta-analyses indicate that the effect is robust, albeit of small magnitude (Rodrigues et al. 2015, Wood et al. 2016). Self-affirmation (Steele 1988) involves reflecting upon important values, attributes, or social relations and is usually induced via a writing exercise. Self-affirmation was found by two recent meta-analyses (Epton et al. 2015, Sweeney & Moyer 2015) to reduce defensive resistance to health-risk communications and promote health-related intentions and behavior. Thus, the QBE and self-affirmation are relatively well-established BCTs.

However, we do not always know why or when these techniques are effective. The most likely targets underlying the QBE are attitude accessibility (questioning activates participants' underlying attitudes and so makes it more likely that those attitudes will be translated into action) and dissonance reduction (questioning causes participants to strive to act consistently with their expressed views). However, it is currently unclear whether either, neither, or both of these targets are responsible for the QBE (Wood et al. 2016). The list of possible targets underlying self-affirmation effects is even longer and includes reduced attentional bias, enhanced self-regulatory resources,

higher-level temporal construal, improved judgmental confidence, greater feelings of vulnerability, and increased self- or other-directed positive affect (Cohen & Sherman 2014). A better understanding of the conditions under which each of these mechanisms operates will facilitate the productive use of self-affirmation in interventions.

The EM approach suggests that a useful starting point could be competitive tests of target engagement, e.g., studies to compare the impact of QBE or self-affirmation manipulations on their various possible targets (Path C). These studies could, in turn, be followed by a program of research to validate the respective targets identified by the first set of studies (Path B). Competitive tests of target engagement could have both conceptual and practical benefits by explaining the variability in the impact of these techniques on health behaviors. For example, Godin et al. (2008) found that the QBE increased blood donation over one year, whereas a similar study of blood donation by van Dongen et al. (2012) observed no reliable QBE. Similarly, Peterson et al. (2012) found that a self-affirmation intervention increased physical activity among patients following a percutaneous coronary procedure, whereas an equivalent intervention proved ineffective among patients with asthma (Mancuso et al. 2012). In other studies, self-affirmation has been observed to promote behavior change without influencing cognitive responses to the message (e.g., intentions, Wileman et al. 2014) or even to reduce behavior change (e.g., Good et al. 2015). The EM approach suggests that research designed to identify, validate, and engage the relevant target(s) would better advance the field than studies repeatedly testing the behavioral impact of the QBE and self-affirmation. Other BCTs (e.g., use of incentives, mindfulness or self-compassion training, altering plates or utensils) could also benefit from this approach.

Targets in Search of Effective Techniques

In contrast, there is relatively little research on the most effective and efficient ways to engage several important targets that have been shown to elicit change in health behaviors (e.g., risk perception, intentions, self-efficacy). The need to specify techniques that effectively engage such targets presents both an empirical and a theoretical challenge. At the empirical level, competitive tests of target engagement are clearly warranted. An important review by Ashford and colleagues (2010) of self-efficacy for physical activity undertook detailed coding of the techniques used in randomized trials to increase self-efficacy and examined which techniques led to the largest improvements. Their findings showed that vicarious experience and feedback on both past performance and others' performance were the most effective techniques, whereas verbal persuasion, barrier identification, and problem solving actually reduced self-efficacy. Although this work represents a valuable starting point, we cannot rely on retrospective coding and meta-analysis to offer competitive tests. A sustained program of experimental studies that explicitly compare different techniques for engaging targets specified by health behavior theories is needed.

The theoretical challenge is to develop conceptual models of target engagement. Health behavior theories specify what targets should be engaged by interventions but generally have little to say about how these targets can best be engaged. The next generation of health behavior theories should focus on how best to engage targets. Research on health behavior change has long relied on intuition or pilot studies to determine how to engage targets. This approach is insufficient. Theory development and competitive empirical tests need to go hand in hand in order to forge a science of target engagement. Because the amount of change in the target forms the dependent variable in this work, researchers, reviewers, and editors need to recognize the value of research that is explicitly concerned with target change. The promise of the EM approach is that the study of target engagement is a vital part of behavior change research.

MOVING FROM OBSERVATION TO INTERVENTION

EM offers a roadmap for research on health behavior change that could help move the field from observation to intervention. Many researchers already recognize the limitations of standard efficacy trials and see value in full tests that assess whether intervention components engage targets and whether target engagement changes health behaviors. EM shares with other approaches the ideal that full tests should be used to evaluate behavior change interventions, and it also challenges health researchers to examine basic mechanistic processes as part of the trial (Path D). Like these other approaches, EM indicates that different programs of research are needed to provide the foundation upon which full tests can build. Basic research that identifies, measures, and validates targets (Paths A and B) can be the starting point for other programs of research. The techniques used to validate targets in basic research programs may be neither feasible nor acceptable outside of the laboratory, however, and thus research that tests the best ways to maximize target engagement (Path C) is an essential contribution. The EM approach also encourages researchers to look beyond their favorite theory or the behavior at issue and to view the science of behavior change in terms of targets—their identification, assays, validation, and engagement.

The identification and measurement of new targets are likely to come not only from theories but also from advances in technology. Many important determinants of behavior may have simply not found their way into current theories because of difficulties in measuring those targets in the past. Affective processes are a case in point. Whereas self-reports can alter the affect that one is trying to measure (Kassam & Mendes 2013), facial and vocal recognition software may offer unobtrusive insight into what participants are feeling and how that changes over time and in response to new information. Collecting intensive within-person data may offer new insights into when and how targets fluctuate and lead to refinements of our theories. Relatedly, the fact that techniques such as the QBE have proven effective in changing behavior even though respective targets are not precisely identified underlines the role of both rigorous empirical tests and deeper conceptual analysis in identifying new targets.

The EM approach challenges researchers concerned with health behavior change to strive for greater precision and specificity in the research questions we address (Paths A–D), the ways in which we address them (via experimentation, using assays at different levels), and the theories we use. Precise theories of health behavior change will specify whether cognitions about the health threat, cognitions about the focal behavior, volitional factors, and implicit cognition are all needed to understand behavior change and when or under what circumstances these different targets are or are not influential. Precise theories will also be able to specify the dosage of behavior change interventions: how much a target needs to change to generate behavior change (whether there is a linear dose-response relation or a threshold that must be met) and how much engagement is needed to create that change in the target (e.g., one or more face-to-face sessions versus a pencil-and-paper exercise). Courage will be needed to develop precise theories, as precision puts theories at grave danger of refutation (Meehl 1978). The encouragement offered by the EM approach is the promise of substantial progress in research on health behavior change that could meet mandates to improve public health.

APPENDIX: TARGETS SPECIFIED BY HEALTH BEHAVIOR THEORIES

Below we define 23 targets specified by health behavior theories.

- Risk perception is a person's judgment concerning the likelihood of getting a disease or illness (e.g., How likely is it that you will get HIV/AIDS?).

- Perceived severity is a person's belief concerning the seriousness of a disease or illness (e.g., How serious would it be if you got HIV/AIDS?).
- Fear is a negative emotion caused by the possibility of getting a disease or illness (e.g., How afraid are you of getting HIV/AIDS?).
- Threat appraisal is a person's overall estimate of how dangerous a disease or illness is and the output of risk perception, perceived severity, and fear.
- Intention is a person's self-instruction to act in a particular manner (e.g., I intend to use a condom if I have sex with someone new).
- Willingness is a person's inclination to perform an unhealthy behavior given conducive circumstances (e.g., If your partner was extremely attractive and you were using another form of contraception, how likely is it that you would have sex without using a condom?).
- Attitude is a person's overall evaluation of the consequences of performing a behavior (e.g., How good or bad would it be to use a condom if you have sex with a new partner?).
- Norms can be either injunctive or descriptive. An injunctive norm is a person's belief about whether significant others think that he or she should behave in a particular manner (e.g., Most people who are important to me think that I should use a condom if I have sex with someone new). A descriptive norm is a person's belief about how significant others themselves behave (e.g., Most people who are important to me use a condom if they have sex with someone new).
- Self-efficacy is a person's confidence in his or her ability to perform a behavior (e.g., How confident are you that you can use a condom if you have sex with someone new?).
- Perceived behavioral control (PBC) is a person's beliefs about how easy or difficult it is to perform a behavior (e.g., How easy or difficult would it be for you to use a condom if you have sex with someone new?). Although PBC includes both self-efficacy and beliefs about the controllability of the behavior, beliefs about controllability do not predict behavior over and above self-efficacy (e.g., Conner et al. 2016), and PBC and self-efficacy are effectively synonymous.
- Coping appraisal is a person's overall estimate of his or her ability to prevent or manage a disease or illness; the output of self-efficacy and beliefs about whether a recommended behavior is effective in preventing the disease or illness (i.e., response efficacy); and the costs of engaging in the behavior (i.e., response costs). Both response efficacy and response costs can be seen as components of attitude.
- Social prototypes are a person's images of someone who typically engages or does not engage in a particular behavior. Measures of social prototypes involve both judgments of similarity (e.g., How similar are you to the type of person who uses a condom during sex with a new partner?) and evaluations (e.g., How likeable or dislikable is the type of person who uses a condom during sex with a new partner?).
- If-then plans specify cognitive or behavioral responses to good opportunities to act or obstacles that could prevent action and make it more likely that people successfully realize their intentions to perform health behaviors. The plans are so-named because they have the format if (opportunity/obstacle)-then (response).
- Behavioral skills are a person's actual and perceived abilities to undertake the various behaviors relevant to reaching one's goal. In the context of condom use with a new partner, relevant skills include buying, storing, and carrying a condom and negotiating condom use with a sexual partner.
- Processes of change are strategies that a person can use to change behavior. The transtheoretical model specifies ten processes of change comprising both experiential (e.g., developing awareness, using self-reappraisal) and behavioral (e.g., reorganizing one's environment, engaging in substitute activities) strategies.

- Progress monitoring is the practice of comparing one's current standing relative to one's goal. Both features of a behavior (frequency, intensity, duration) and outcomes of a behavior (e.g., weight loss) can be compared.
- Executive function is a set of cognitive abilities that serve to keep thoughts, feelings, and behavior in line with the person's goals. Executive function has three components: response inhibition (overriding unwanted responses), mental flexibility (switching from one rule to another), and working memory (holding relevant information in mind and using it effectively).
- Attentional bias is the extent to which cues related to unhealthy substances (e.g., tobacco, alcohol) capture and hold a person's attention. Attentional bias is typically measured by modifications of the Stroop color-naming task or the visual dot probe task.
- Implicit attitude is a person's automatic affective reactions triggered by a stimulus. Implicit attitudes are typically measured by reaction time tasks such as the implicit association test.
- Approach bias is a person's automatic action tendency to move toward a desired stimulus (e.g., chocolate, tobacco). Approach bias is typically measured using a joystick task that measures how quickly participants move toward versus away from relevant stimuli.
- Implicit goals or norms are desired outcomes or behavioral standards that are primed by features of the context without the person realizing the impact of the priming stimulus on behavior. For example, a poster for a low-fat recipe primed participants to consume fewer snacks (Papies & Hamstra 2010), and placing an image of eyes over a sanitizing dispenser increased rates of hand hygiene among people entering a hospital (King et al. 2016).
- Habit is a person's automatic tendency to repeat a well-practiced behavior in response to cues that led to performance of the behavior in the past. The cues that trigger habit performance can include particular times, places, settings, or people or previous actions in a sequence.

The theories that specify these targets are outlined in the sidebar Health Behavior Theories, and the overlap among the theories and targets is illustrated in **Table 1**.

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