

Behavioral Processes in Long-Lag Intervention Studies

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Abstract

We argue that psychologists who conduct experiments with long lags between the manipulation and the outcome measure should pay more attention to behavioral processes that intervene between the manipulation and the outcome measure. Neglect of such processes, we contend, stems from psychology's long tradition of short-lag lab experiments where there is little scope for intervening behavioral processes. Studying process in the lab invariably involves studying psychological processes, but in long-lag field experiments it is important to study causally relevant behavioral processes as well as psychological ones. To illustrate the roles that behavioral processes can play in long-lag experiments we examine field experiments motivated by three policy-relevant goals: prejudice reduction, health promotion, and educational achievement. In each of the experiments discussed we identify various behavioral pathways through which the manipulated psychological state could have produced the observed outcome. We argue that if psychologists conducting long-lag interventions posited a theory of change that linked manipulated psychological states to outcomes via behavioral pathways, the result would be richer theory and more practically useful research. Movement in this direction would also permit more opportunities for productive collaborations between psychologists and other social scientists interested in similar social problems.

Keywords

field experiments, interventions, behavioral mediation, theories of change, longitudinal studies

People's actions contribute importantly to their own welfare and that of the collectives to which they belong. For this reason, psychologists and other behavioral scientists often design interventions to modify behaviors that they believe are producing suboptimal outcomes for individuals or society (Thaler & Sunstein, 2008; Wilson, 2011). These interventions modify the individual's physical, social, or psychological environment in the hope that this will change their behavior and, in turn, the suboptimal outcomes they produce. A growing corpus of research finds that interventions taking as little as an hour can affect behavior and enhance the welfare of both self and others for extended periods of time, often over a year (Frey & Rogers, 2014; Walton, 2014). In most instances, the decision to undertake a long-lag intervention study is preceded by lab studies that show short-lag effects from similar manipulations. Moving from the lab to the field in these circumstances makes sense: A laboratory effect lasting an hour provides less credible evidence of the social welfare benefits of a particular intervention than an effect lasting months or years.

Researchers understandably are gratified when a manipulation found to "work" in the lab also appears to do so in the field. It is also understandable that they would be drawn to the parsimonious inference that the manipulation "works" in the same way in the short-lag lab and long-lag field contexts. However, such a presumption may not always be justified, as time elapsed is rarely the only psychologically relevant input that varies across the two cases. In particular, a *behavioral* chain can exist between the manipulation and the outcome measure in the field, whereas typically laboratory participants have no opportunity to take actions that might mediate between the intervention and the outcome. Indeed, when mediation is examined in the laboratory, it almost always involves a psychological process presumed to be induced by the intervention that then causes a change in the

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dependent measure. Examples of such processes include the content or accessibility of people's beliefs (e.g., Burnette, Boyle, Von Epps, Pollack, & Finkel, 2013; Devine, Forscher, Austin, & Cox, 2012) and the contents or intensity of their affective states (e.g., Mattarella-Micke, Mateo, Kozak, Foster, & Beilock, 2011; White, Abu-Rayya, Blive, & Faulkner, 2015). The relevant question in these studies is this: Was the manipulation's impact on the behavioral dependent measure mediated by the predicted psychological change in participants?¹

Behavioral Pathways in Long-Lag Interventions

There generally will be multiple possible behavioral paths through which a modified psychological state could produce a long-lag effect (Rogers & Frey, 2014). Not knowing the path or paths through which the state had its effect leaves us with an incomplete understanding of the intervention's workings and compromises our ability to implement or improve the intervention (Ludwig, Kling, & Mullainathan, 2011). Consider an experiment that presents participants with a persuasive communication designed to increase their concern and knowledge about skin cancer. Assume that message conveyed both the dangers of skin cancer and the role that sun exposure played in its development. Assume further that the effectiveness of the communication was established through assessment of both the participants' beliefs about the danger of skin cancer and their behavioral intention to wear sun-protective clothing when in the sun. Encouraged by this laboratory results assume that the government decides to deploy a public service announcement (PSA) with the same content. Following a lag of multiple years, the effectiveness of the intervention is assessed by comparing rates of skin cancer in the area in which the PSA aired with appropriate control groups (see Wakefield, Loken, & Hornik, 2010).

Were this long-lag intervention to be successful it would seem farfetched to think that any psychological state produced by the intervention (e.g., fear of, or increased knowledge about, skin cancer) directly influenced the outcome measure (skin cancer). It would seem much more plausible that the psychological state produced by the intervention led to one or more behaviors that reduced the likelihood of developing skin cancer. Among the most important modifiable behaviors contributing to skin cancer are time spent in the sun and tanning salons, the quantity of sunscreen applied, and the amount of sun-protective clothing worn (Saraiya et al., 2004).

Recall that the short-lag version of the previously described skin cancer intervention found an increase in participants' behavioral intentions to wear sun-protective clothing. Can we assume that this was the behavioral

pathway through which the long-lag version of the intervention reduced skin cancer? Not necessarily. It may have been but the relevant behavioral pathway could also have been spending less time in the sun, or wearing more sunscreen when in the sun, or some combination of these actions. A full understanding of how the intervention worked, therefore, would entail knowing the particular behavioral pathways that it affected (see Fig. 1). Unfortunately, too few long-lag interventions either employ designs that can diagnose the possibility of behavioral mediation or include measures that could identify the form that such processes might take.

Examples of Long-Lag Interventions

We develop our analysis of behavioral processes by considering long-lag interventions in three domains: the reduction of prejudice, the enhancement of well-being, and the enhancement of academic achievement.²

Reducing prejudice

Finding an intervention that reduces prejudice (outgroup antipathy) has motivated countless lab experiments, though surprisingly few field experiments (Lemmer & Wagner, 2015; Paluck, 2016; Paluck & Green, 2009). One question that naturally arises when lab interventions do successfully reduce prejudice is whether the reduction in prejudice persists once participants leave the lab? Let us assume a researcher does find evidence of long-lag prejudice reduction, say a year, following a lab or field intervention. What would we know about how the intervention "worked"? The finding that people expressed less antipathy toward a negatively evaluated outgroup a year after a one-time intervention is impressive but it would leave us unclear as to why the short-lag positive feelings produced by the intervention were still present a year later. One possibility is that the intervention, analogous to the removal of a cataract, instantly and enduringly changed the way the participants saw their social world. By this account, the provenance of those more positive feelings toward the outgroup observed immediately after the intervention and those observed a year later is the same. Another possibility, however, is that although the intervention instantly changed the way that the participants saw their social world these perceptions would not have endured had they not led to behaviors that sustained them. By this latter account, the intervention worked by producing ephemeral short-term liking that initiated subsequent downstream behaviors that, in turn, led to long-term liking. Stated differently, the prejudice-reduction intervention may have been the distal cause of the observed long-lag effect, but it was downstream behavioral actions that were the proximal cause.

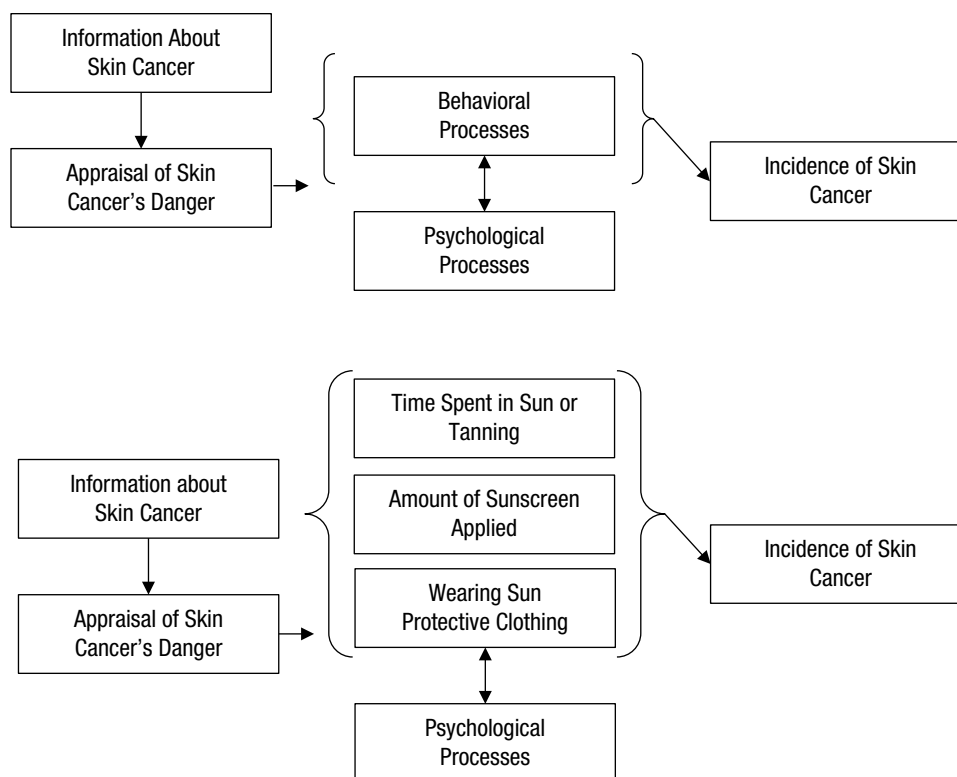


Fig. 1. Part one illustrates the skin cancer example with the behavioral processes unexamined. Part two illustrates this example with specification of potential behavioral processes. Although all three inputs may be altered, it is also possible that only one or two inputs shift as a result of the intervention.

To develop our argument, consider an experiment in which White college students were confronted with the fact that they ranked the value of freedom for themselves higher than the value of equality for others (Rokeach, 1971). The hypothesis was that the psychological discomfort stemming from this prompting would motivate the students to affirm their commitment to equality by showing increased commitment to the cause of minority groups. The study found evidence of psychological dissatisfaction following the manipulation and also evidence that this dissatisfaction led to greater support for minority groups. Specifically, it found that those White students who experienced value confrontation were more likely than those who did not to (a) respond positively to a mailed invitation to join the NAACP and (b) enroll in a course in ethnic studies during the next year.

Were Rokeach to have conducted a more typical lab version of this study, he would have confronted participants with their conflicting values and assessed their subsequent willingness to join the NAACP and take a course in ethnic studies within the initial lab session. As it was, he collected his dependent measures 15 to 17 months after the value confrontation had occurred. That an effect of the intervention was observed 17 months after its introduction is impressive. But how should we interpret

its enduringness? Should we assume that the psychological dissatisfaction induced by the intervention endured and was causally implicated to the same extent, and in the same way, in instances of increased outgroup sympathy observed 17 months later as it was in instances of such behavior observed in the initial session? Not necessarily, as it is also possible that the initial psychological satisfaction only persisted long enough to induce actions that themselves contributed to and reinforced outgroup sympathy. Indeed, the decision to join the NAACP and enroll in an ethnic studies course, while causally influenced by the intervention, might also have, in turn, causally influenced some other downstream measures that went unassessed. In other words, Rokeach's manipulation may have induced psychological dissatisfaction, and initial instances of increased outgroup sympathy may have derived from this psychological state, but instances of increased outgroup sympathy observed long after the intervention need not require that this psychological state persisted.

Despite the high praise they garner, there are few long-lag studies of prejudice reduction (cf. Broockman & Kalla, 2016; S. W. Cook, 1971; Green & Wong, 2009; Maholtra & Liyanage, 2005). Moreover, the few such studies that do exist are generally silent on the possibility that

particular behaviors maintained the influence of the intervention during the lag period. For example, it remains an unexplored possibility whether the increased racial tolerance found among students a month after a positive interracial experience in a 3-week wilderness camp (Green & Wong, 2009) had its roots solely in the camp experience or also in post-camp behaviors that the camp experience initiated. If the latter were true, it would not diminish the impressiveness of the intervention but it would enhance our understanding of it.

Short-lag interventions designed to reduce prejudice rarely depend on participants' motivation to reduce prejudice. For example, interventions that put people in close contact with members of groups they negatively evaluate do not assume that the targets necessarily are motivated to be unprejudiced. It is also conceivable that long-lag interventions could occur in the absence of any goal to reduce prejudice, but the number of potential behavioral mediators is greater if the targets are motivated to reduce prejudice. This was the case with Rokeach's White college students who were driven by the discomfort he induced in them through the value conflict procedure to show more support for African Americans.

Devine and colleagues (2012) provide another example of how an experimentally induced goal of reducing prejudice facilitates long-lag intervention success. These researchers revealed to White college students via the Implicit Associations Test (IAT) that they had racial biases. They then provided students with a "bias education and training program" that emphasized strategies they could use to reduce their implicit bias. These included cognitive strategies such as counterstereotypic images, individuation, perspective taking, and behavioral strategies such as seeking opportunities for contact with African Americans. Those in the experimental group showed reduced bias, particularly if they scored high on a measure of motivation to avoid discrimination. But having the explicit goal of not discriminating against others was not sufficient to reduce bias. It was also necessary to know how to accomplish this. We know this because a group of participants who did not receive the strategies did not show diminished bias on subsequent IATs.

The Devine et al. (2012) study makes two important points. First, it shows explicitly that it was what participants did after they left the laboratory that mediated the long-lag effect the intervention had on their level of prejudice. Second, it shows that those strategies that mediated the effect were not intuitively obvious to participants and needed to be pointed out to them for participants to achieve their goal of bias reduction. Whatever motivational effects interventions have, their long-term success often depends on the targets' understanding of how to convert that motivation into productive behaviors. We return to this issue later.

A final example of a long-lag prejudice reduction intervention is provided by a study that brought together Israeli and Palestinian teenagers for a three-week camp (Schroeder & Risen, 2016). The researchers predicted not only that the camp experience would produce a long-term reduction in intergroup hostility but that this reduction would occur via the post-camp continuation of friendships formed among individuals from different groups. This prediction comported with considerable correlational research that suggested that outgroup friendship formation is an important mediator of the positive effects of intergroup contact (Levin, van Laar, & Sidanius, 2003; Pettigrew & Tropp, 2011). Schroeder and Risen (2016) measured whether or not participants made an outgroup friend at the camp and examined whether this predicted outgroup attitudes up to 3 years later. Indeed, gaining even one out-group friend during the camp was associated with more positive outgroup feelings after the camp ended. What makes this study distinctive is that it did not simply show that an intervention could have a long-lag effect but specified and measured a pathway through which this could take place (the existence of additional outgroup friends once the camp ended).

Increasing health and well-being

Psychologically based interventions designed to improve people's health or well-being generally are long lag, as it is difficult to achieve success on these metrics in the time afforded by laboratory experiments. To begin our discussion of this category of interventions, we contrast two long-lag intervention studies that leveraged psychological manipulations to produce weight loss in women (Axsom & Cooper, 1985; Logel & Cohen, 2012). In both cases women who wished to lose weight were recruited for a research study by means of advertisements.

In Axsom and Cooper's (1985) dissonance theory-inspired intervention, the women were exposed to either a high or low effort "therapy" manipulation. This was achieved in the context of a cover story that suggested that increasing neurophysiological arousal raises emotional sensitivity in a way that can lead to weight loss. Participants in both conditions participated in 5 one-hour sessions over 3 weeks. They also both participated in cognitive tasks during the session with those in the *high effort* condition being designed to be much more difficult and demanding than those in the *low effort* condition. Six months after the final session, participants returned to the laboratory to have their weight recorded and answer survey questions. Consistent with predictions, those in the *high effort* condition lost more weight (8.5 pounds on average) than those in the *low effort* condition. The researchers attributed the success of those in the *high*

effort therapy condition to the fact that they “came to view the goal of losing weight with more zeal and fervor.”

That the arousal of dissonance increased participants’ commitment to the goal of weight loss is certainly plausible but we are still left wondering how this goal was executed. Research shows that the two main modifiable factors contributing to obesity are exercise and caloric intake (Sen, Mennemeyer, & Gary, 2011). It seems likely, therefore, that the weight loss in Axsom and Cooper’s participants occurred because they either consumed fewer calories, exercised more, or both. Unfortunately, we cannot tell which on the basis of the measures assessed. We also cannot tell, if they did achieve their weight loss more through one pathway than another, what exact form that took? Did they join a weight loss club or gym, stock up on low calorie food, make a public commitment to losing weight, and so on? This is important because it is possible that the behavioral pathway taken may have played an important causal role in the weight loss process. What persisted, in other words, may not have been the dissonance arousal itself, or even the dissonance-reducing goal to lose weight, but rather the residual effects of the behaviors that these motivational states initiated. Thus, the greater “zeal and fervor to lose weight” among the *high effort* participants might have led them to undertake certain actions that themselves contributed to weight loss, even if their zeal and fervor to lose weight waned. By understanding which pathway influenced weight loss interventionists would be better positioned to amplify these effects via channel factors (Lewin, 1946), such as offering discounted gym memberships.

The strategy behind Logel and Cohen’s (2012) intervention was not to increase participants’ motivation to lose weight but to decrease the psychological impediments they experienced in executing their motivation to lose weight. Specifically, they employed a self-affirmation intervention to reduce the stress experienced by the participants. A self-affirmation intervention provides individuals with the opportunity to reflect on values that are personally relevant to them, with the presumed consequence that this will buffer them from threat or stress (Cohen & Sherman, 2014; Sherman & Cohen, 2006). Logel and Cohen (2012) predicted that the protective function of the self-affirmation intervention would enable the women to better exercise the self-control necessary to execute their goal to lose weight. In the researchers’ words “focusing people on higher values rather than immediate impulses” helps participants “maintain self-control in difficult situations” and “buffers them against life stressors.” That self-affirmed participants showed lower BMI scores 8 weeks after the session, and were on average almost 3.5 pounds lighter, indicates that the intervention was successful.

But how did the intervention “work”? The authors’ claim that the treatment was effective because it lowered participants’ arousal thereby increasing their self-control suggests that the psychological state produced by the intervention persisted over the course of the 8 weeks. It certainly is possible that the affirmed state persisted in participants over the course of the experiment but this is not necessary for the participants to have evidenced increased exercise or reduced caloric intake over that period. It is sufficient that the treatment induced them to take actions that themselves increased their exercise or decreased their caloric intake. Without more information about what went on in the interval between the intervention and the measuring of the outcome we cannot tell.

In a well-known field study (Langer & Rodin, 1976; Rodin & Langer, 1977), elderly nursing home residents were encouraged to increase daily choices so as to increase their feelings of control and responsibility over their lives. The hypothesis was that the decline in health, alertness, and activity that generally occurs among the aged in nursing home settings could be slowed or reversed by increasing their perceived control over their lives. To test this hypothesis, the hospital administrator gave a talk to residents that, depending on the condition, emphasized their responsibility for themselves, or the staff’s responsibility for them. To bolster the communication, residents in the experimental group were offered plants to care for, whereas residents in the comparison group were given plants that were tended to by the staff. In reality, the choices and potential for responsibility that the treatment condition identified were options that were already available; the administrator simply stated their possibility more explicitly.

The results indicated that residents in the treatment group increased their participation in the activities of the home, including attending movies, socializing with staff and friends, and participating in contests. Most impressive, health ratings by nurses and physicians collected 18 months later were more positive. So, how did the intervention work? Did the feelings of responsibility and choice it engendered persist for 18 months or did these feelings merely endure long enough to initiate a set of health-benefitting behaviors that then persisted? We cannot tell.

What we can say is that some of the measures assessed by the researchers, as was the case in Rokeach’s study discussed earlier, possibly could have mediated others. For example, it seems possible that the nursing home residents’ greater attendance at movies, involvement with staff and friends, and participation in contests following the intervention themselves could have produced effects on the residents that contributed importantly to the more positive assessment they received from the medical staff 2 years later. This would not render the medical staff’s

assessment 18 months later any less impressive: It remains striking that giving residents responsibility can cause them to take actions that increase their well-being. It would make the causal story less clear, however, if it were discovered that the study's long-lag effects did not depend on the persistence of residents' experimentally induced feelings of enhanced perceived control over their environment. The question would then become what did it depend on? To answer that, we would need to know if, and in what ways, those in the treatment and control conditions behaved differently in the interval between the intervention and the outcome measure.

Academic achievement

The quest to improve academic achievement has long motivated social psychologists (Spitzer & Aronson, 2015). One of the most common forms of social psychological intervention in this domain involves changing the beliefs or mind-sets of students about factors that affect achievement behaviors (Dweck, 2007; Walton, 2014; Walton & Cohen, 2011; Wilson, 2011). The hypothesis that guides this category of interventions is that replacing one belief or mind-set (e.g., the belief that "intelligence is fixed"), with another that is more compatible with proachievement behaviors (e.g., the belief that "intelligence can grow"), will improve achievement outcomes. In most instances, long-lag interventions of this type are preceded by considerable evidence of the short-lag efficacy of the intervention in question. Evidence for the effectiveness of brief social psychological interventions on educational outcomes is impressive, especially with respect to closing racial and gender gaps among high school and college students (see Spitzer & Aronson, 2015). But how do these interventions work? How does a single brief intervention or even multiple such interventions designed to change students' mind-sets improve their grades?

In the typical mind-set intervention there is an outcome measure, such as GPA, and some measure of "construal," assessed directly through questionnaires or less directly through diaries. These construal measures are collected at various times between the intervention and the long-lag measures and are sometimes analyzed as potential mediators. Rarely, however, do researchers attempt to systematically link the interventions or the psychological states produced by them to behaviors known to contribute to academic achievement such as study time, class attendance, or out-of-class time spent with teachers. Occasionally, researchers do collect measures of behaviors that could have an independent causal impact on the outcome measure such as using available academic resources (Stephens, Hamedani, & Destin, 2014; Yeager et al., 2016), befriending other students in their academic program (Walton, Logel, Peach, Spencer,

& Zanna, 2015; Yeager et al., 2016), living on campus, joining extracurricular activities, and developing mentor relationships (Yeager et al., 2016).

In summary, we believe that researchers should be more open to the possibility that although it was the psychological state induced by their manipulation that initiated the process of change, other behaviors sustained and possibly even increased it. For example, it may have been an affirmed self, a reinforced sense of belonging, or a view of intelligence as mutable that initiated behaviors that facilitated academic improvement in particular mind-set interventions but these behaviors may subsequently have themselves contributed to the improved academic performance of the students. Similarly, while it may have been a psychological process produced by intergroup contact (e.g., diminished intergroup anxiety, decategorization) that facilitated behaviors that reduced prejudice, these behaviors might, in turn, have contributed to the observed prejudice reduction.

Why Is It Important to Observe the Behavior During the Lag?

Our analysis suggests that observing what participants in long-lag interventions do between the intervention and the outcome measure can yield several benefits for both the development of psychological theory and the application of psychological research to practice.

Identifying the behavioral path(s) leading to the problem

In recent years many communities in the United States and other countries have debated the merits of imposing a tax on drinks high in sugar as a means of reducing childhood obesity. Those supporting the so-called "soda tax" believe that a key contributor to childhood obesity is caloric intake, with soda being an important source of that intake. In contrast, opponents of the tax often deny that soda consumption, or caloric intake generally, plays a big role in obesity, and point instead to a lack of sufficient exercise, the other main modifiable contributor to obesity. This debate illustrates the importance of establishing the relative power of modifiable inputs contributing to childhood obesity when deciding on the appropriate intervention. If caloric intake is the behavior pathway most responsible for childhood obesity, then a soda tax or some other means of reducing caloric intake makes sense. If this is not the key pathway, then it doesn't.

As another example of a health-related gap that researchers have sought to understand and close, consider college drinking. Much concern has been expressed about the high consumption of alcohol among many college students, and many interventions have been launched

to reduce problematic drinking (Carey, Scott-Sheldon, Carey, & DeMartini, 2007). But what do we know about the drinking behavior of those college students whose consumption rates are much higher than others? Obviously they drink more but what pathways to alcohol consumption are most responsible for producing this gap? To address this question, a sensible first step is to identify the venues in which most college drinking occurs: at fraternity parties, in conjunction with athletic events, in residence halls, in off-campus housing, and in bars adjacent to campus (Gruenfeld, Johnson, Light, & Saltz, 2003). As a second step, we might consider how the consumption rates of heavy drinkers and moderate drinkers differ across these venues. Are some venues more responsible than others for the difference? Once we know the answers to these questions we are in a better position to design an intervention. For example, armed with the knowledge that a large portion of dangerous college drinking occurs at off-campus bars, Saltz, Paschall, McGaffigan, and Nygaard (2010) designed a successful intervention to reduce drinking specifically in this venue. Datta and Mullainthan (2012) refer to those key pathways identified as “behavioral stress points.”

Knowing the role played by different behavioral pathways will also be important in understanding racial, gender and demographic gaps. For example, which of the pathways contributing to academic achievement are implicated in the lower performance of first-generation than continuing generation college students? And are these pathways the same or different than those implicated in the lower performance of African American than White college students? We contend that whenever society and policy analysts wish to close gaps between groups of people it is important that they first determine if these gaps are more dependent on some potential pathways than others. Datta and Mullainthan (2012) refer to this process as “behavioral mapping.”

Identifying the behavioral path(s) leading to the solution

A fuller understanding of the paths that contribute to an undesirable gap in outcomes can help researchers and policymakers identify the appropriate intervention. For example, the obesity intervention one would design is likely to be different depending on the extent to which the variation in obesity rates was due to caloric intake versus exercise. Identifying relevant behavioral paths will yield not only a better understanding of the psychology underlying the gap but a better prospect of designing a robust and generalizable means of reducing it. An example of an intervention that benefited from first identifying a behavioral pathway difference between groups was that undertaken by Stephens and her colleagues (2014;

Stephens, Townsend, Hamedani, Destin, & Manzo, 2015) to reduce the achievement gap between first-generation and continuing generation college students. Previous research on this gap had revealed that lower achieving first generation college students were less like to use beneficial campus academic resources (e.g., tutoring opportunities, office hours) than their continuing generation counterparts (Kim & Sax, 2009). Consequently, the intervention that Stephens and her colleagues implemented (successfully) targeted this particular pathway. Specifically, it emphasized and encouraged engagement in what the researchers termed “help-seeking” behaviors. Not only did their intervention close the gap in the pursuit of this pathway but their analysis indicated it was this pathway that mediated the closing of the gap in end-of-the-year GPA.

When different interventions work through similar versus dissimilar pathways

One important implication of the fact that psychologically induced behavioral change can occur through more than one behavioral pathway pertains to the comparison of different interventions. Recall the earlier hypothetical example of a PSA that reduced skin cancer. Knowing which behavioral pathway(s) mediated the impact of the PSA would be very useful when comparing the impact of the PSA intervention with that of another successful intervention targeting skin cancer. If both interventions reduced skin cancer solely by reducing time spent in the sun there would be a strong case for proceeding only with the one that a cost benefit analysis, taking into account the size of the effect and the financial cost, showed to be superior. If they achieved their impact through different pathways, however, it may make more sense to deploy both. Thus, even if an intervention designed to reduce skin cancer by increasing the wearing of protective clothing in the sun was less effective than one designed to reduce time spent in the sun it may make sense to use both if they complemented one another.

There is another reason why the evaluation of different interventions should involve comparing not only their effectiveness but the pathways through which their success occurred. Consider the finding that when interventions have implemented two different mind-set manipulations there typically is no boost from their combination (Good, Aronson, & Inzlicht, 2003; Paunesku et al., 2015). For example, manipulations of students' sense of purpose and growth mind-set each alone has a positive effect on academic achievement but their combined effect was no greater than their individual effect (Paunesku et al., 2015). One interpretation offered for

this finding is that the presentation of the combined interventions resulted in a “half dose” of each (Paunesku et al., 2015). This is certainly possible but it is also possible that the effects of the interventions worked through similar pathways and thus tended to be redundant with one another rather than complementing one another. Without a fuller understanding of the extent to which different pathways are affected by the two manipulations we cannot assess this possibility. If we did know how the two threats affected each of the relevant pathways, and how each intervention affected the relevant pathways, it would be possible to more appropriately target and effectively combine interventions.

It is important to emphasize that, in referring to different pathways, we refer not to different psychological pathways but to different behavioral pathways. That different mind-set manipulations affect different psychological pathways (i.e., beliefs; Walton et al., 2012) does not tell us whether these different psychological pathways do or do not lead to different behavioral pathways (e.g., studying more versus attending office hours more frequently).

How Did We Get Here?

The social psychologist's natural milieu has been the laboratory. The advantages and disadvantages of the lab in comparison to the field are well-known (Brewer, 2000; McGuire, 1973; Paluck & Cialdini, 2014). When social psychologists move from the lab to the field they commonly feel they are accepting a trade-off: greater “noise” in exchange for greater external validity. The potential for noise only increases as the temporal lag between the treatment and the dependent measure increases. For this reason, few social psychologists would be surprised that a treatment effect that they had reliably demonstrated in the lab did not replicate in a long-lag field experiment (e.g., Hulleman & Cordray, 2009). Conversely, social psychologists are understandably excited when such an effect does replicate. But greater noise and complexity produces more than a reduced likelihood that an effect will occur; it also can increase the number of ways, or behavioral channels through which, an effect can occur.

When a behavior is assessed in the lab it generally serves as a dependent measure rather than a mediator. It is a rare lab study that examines the possibility that a manipulation initiates a behavioral chain (cf. S. W. Cook, 1971). This certainly could be done in the lab, but it is not part of the canonical lab study template. What is allowed for, indeed increasingly required by the lab study template, is that the researcher identify the psychological process that mediated any effect observed on a behavioral dependent measure. Current publication standards virtually require that laboratory studies measure both the

psychological state presumed to be affected by the manipulation and some behavior or the intention of some behavior presumed to be affected by that modified psychological state (Quiñones-Vidal, Lopez-García, Peñaraña-Ortega, & Tortosa-Gil, 2004). Sometimes the psychological mediating state is assessed through manipulation rather than measurement (Spencer, Zanna, & Fong, 2005).

The approach described above has been imported with insufficient modification, we contend, to the long-lag field study. This has resulted in researchers being insufficiently open to the possibility that their manipulation could have initiated a series of behaviors that contribute themselves to the outcome observed. That is, researchers too often fail to consider that their manipulation may have short-lag effects on behaviors that then require no further push from the manipulated psychological state to produce downstream effects. They seek evidence that a manipulation has changed an outcome but too rarely evidence of the possibility that downstream behaviors contributed to that change. Those attempting to identify the impact of variables through correlational studies are more likely to measure variables that might play mediating roles, including behaviors. Research on the role of contact in prejudice reduction is a case in point. Studies that manipulate intergroup contact and look at its effects tend to focus on far fewer potential mediators than correlational studies that attempt to assess the impact of contact (Pettigrew, 1998).

This does not mean that social psychological interventionists have no insight into what the mediating behaviors might be. Indeed, were researchers asked to select a dependent measure other than an outcome measure (e.g., GPA, weight, incidence of skin cancer) they almost assuredly would choose a behavior that represented one of the outcomes' input variables (e.g., class attendance, exercise, time spent in sun). The irony, however, is that when they can measure the outcome of the process rather than simply an input to it, they tend to disregard input variables as though they were second-best dependent measures rather than important process measures. The approach of interventions that have their roots in the lab can be contrasted to the approach taken by those whose roots are not experimental lab studies but econometric studies that identified the causal contributors to outcome variables (see List, 2011). These interventions are more likely to target behavioral inputs or at least measure them along with a distal outcome.

Interventions designed to reduce people's energy consumption provide an instructive example of the type of intervention we advocate. One such common intervention, inspired by social psychological research though generally conducted by researchers with economics credentials, provides homeowners with evidence of their neighbors' energy usage based on the assumption that

social comparison information can motivate people to reduce their consumption (Allcott, 2011; Miller & Prentice, 2016). These interventions focus on outcome measures such as kilowatt hours consumed over long-periods as well as on behaviors that prior econometric research has identified as causally relevant input behaviors, such as investments in energy-saving capital stock, default settings of thermostats, and so on (Allcott & Rogers, 2014). Collecting a wider range of measures provides a much fuller understanding of the behaviors that give rise to the long-lag impact of these interventions. This leaves us in a better position to decide how to deploy the interventions and how to combine them with other interventions. We suspect that the reason energy interventionists have looked more closely at upstream pathways that mediate aggregate outcomes of energy usage is that for them the relevant prior work was not only lab research that showed the impact of social norms but econometric research that identified important behavioral inputs to the energy consumption measures.

Those cases where psychologists do target behavioral pathways in their interventions tend to be in domains where the most relevant prior work also is correlational field research rather than laboratory experimental work. One example is provided by parental interventions designed to improve children's well-being. In one illustrative intervention project, Cowan, Cowan, and Knox (2010) reported a series of long-lag interventions designed to improve children's adjustment to school. These interventions were preceded not by research on the short-lag effectiveness of the intervention in question but on research that identified modifiable behaviors that contributed to children's adjustment, such as father involvement and quality of parent child interaction. Given this, it is not surprising that this research looked at not only the intervention's long-lag impact on the child's adjustment (teacher assessments at age 6) but its shorter-lag impact on the previously identified causal inputs. The distressed married couples' interventions designed by Gottman and his colleagues (Babcock, Gottman, Ryan, & Gottman, 2013; Gottman, 2011) have followed a similar protocol. These interventions sought to improve the relationships of distressed couples by targeting behaviors (e.g., destructive conflict and the expression of affection) that previous research had found behaviorally differentiated distressed and nondistressed married couples.

How Do We Move Forward?

We believe that before undertaking an intervention, researchers should posit a *theory of change* that not only identifies the psychological states that the manipulation is anticipated to induce but specifies how the modified psychological state will influence behavior necessary to

produce outcome change (Brest, 2010; Clark, 2004; Funnell & Rogers, 2011; Weiss, 1998). The accounts offered by those conducting long-lag interventions rarely meet this level of specificity. For example, in explaining how the dissonance arousal created by their intervention produced weight loss among their participants, the closest Axson and Cooper (1985) came to offering a *theory of change* was to say that their treatment induced a "zeal and fervor to lose weight." This account describes the psychological state produced by the intervention but it does not specify the behavioral pathway(s) through which this state was to produce weight loss. Those conducting growth mind-set interventions have been similarly nonspecific about the behavioral inputs their interventions change. When mind-set researchers do talk about the impact of their interventions they tend to focus on general perceptions, motivational states, and actions. As an example, consider Cohen and Garcia's (2014) claim that the reason replacing a disruptive mind-set with a beneficial one is effective is because it "influences how students perceived motivationally challenging circumstances so as to support their sense of personal integrity of competence, belongingness and purpose" (p. 15). This level of theorizing speaks to the general behavioral impact of beneficial mind-sets but not to the specific impact they have on education outcomes. It leaves unanswered, for example, which of the particular pathways that increase GPA an increased sense of "personal integrity" affects and whether these pathways are the same as, or different than, those affected by as an increased sense of "belongingness" and "purpose"?

Yeager and Walton (2011) make the interesting suggestion that the intervention field would benefit from the creation of a new class of professional they term "psychological engineers" who could advise on the psychological dynamics occurring in the intervention contexts. We applaud this suggestion, but if it were implemented we would recommend that all newly minted psychological engineers also become experts on the links between psychological processes and behavioral pathways. Identifying the psychological processes that initiate behavioral chains is important but so is identifying the impact of those behavioral chains themselves.

To be fair, few intervention researchers would deny that the psychological states their manipulations produce have downstream behavioral effects that influence the measured outcome. This is certainly true of mind-set researchers (Walton, 2014). But even when mind-set researchers invoke downstream behaviors it generally is for the purpose of showing how these behaviors reinforce and amplify the psychological state the intervention induced. These so-called recursive processes quite possibly are important but they are not the only means by which the long-lag effects could occur. Even if a mind-set

change did not persist, the actions it initiated could have produced long-lag effects. Intervention-induced actions such as contacting faculty outside of class (Stephens et al., 2014) and making friends with one's classmates (Walton et al., 2015) may reinforce the psychological state produced by the intervention, but it is not necessary that they do so for these behaviors to serve as behavioral pathways to improved academic outcomes. Similarly, a greater sense of control and responsibility may have induced Langer and Rodin's (1976) nursing home residents to seek more social connections in their institutions, but the role these connections played in the increased well-being and longevity observed may not have been to sustain the psychological state produced by the intervention but rather to initiate causally influential behavioral pathways.

In addition to more clearly specifying behavioral pathways of change, researchers should measure behavioral inputs to see if they have been affected. Consider the fact that mind-set research done in academic contexts often finds that interventions are most effective, and sometimes only effective, if they are provided at the beginning of the school year (Cohen, Garcia, Purdie-Vaughns, Apfel, & Brzustoski, 2009; J. E. Cook, Purdie-Vaughns, Garcia, & Cohen, 2012). One benefit of taking more measures of possible behavioral pathways in these studies might be the discovery of particular behavioral pathways or channels that are only available at that time. Another benefit of collecting additional measures is that they could be used in mediation analyses. But even if one is not convinced of the merits of such analyses for causal identification in interventions (Bullock, Green, & Ha, 2010), there can be value in their collection (Green, Ha, & Bullock, 2010). For one thing, when an intervention fails, and it is estimated that approximately 85% of interventions do fail (Granger & Maynard, 2015), collecting additional measures can offer insight into the reasons for that failure. These measures can inform researchers not just whether the intervention had the expected psychological effect, but in which of the identified behavioral pathways disruptions occurred.

Before undertaking an intervention, researchers also should assess participants' understanding of the behavioral levers that affect the targeted outcome or explain why the hypothesized change in behavior does not require such knowledge. The *theory of change* guiding many interventions presume the target's knowledge of the relevant behavioral levers and in these circumstances it would seem wise to ensure that this is the case before undertaking the intervention (e.g., Devine et al., 2012). One way of doing this is to educate participants about the relevant behavioral inputs. This is common in interventions designed to reduce energy conservation. A manipulation may motivate people to reduce their water

consumption, but if it is not accompanied by information about how to do this, its ultimate effect may be limited (Dolan & Metcalfe, 2013).

One high-profile intervention strategy in the educational space that illustrates the importance of this variable is that involving *conditional cash transfers* (Fryer, 2011). These interventions attempt to increase student performance by making financial incentives conditional on improved performance. The results of these interventions are not always consistent or clear, but what does seem clear is that making the cash contingent on an increase in the performance criteria is not as effective as making it contingent on an increase in an input variable. For example, whereas offering seventh graders cash for increasing their English grades in one study did not have any effect, offering them cash for reading books did increase their English grades (Fryer, 2011). Follow-up research found that the students who were the focus of this intervention were not able to identify central inputs in the production of higher grades: For example, they never mentioned studying as something they could do to increase their chance of getting the money offered for higher grades. Indeed, one might predict that a critical difference between those high versus low on performance measures in academic and health contexts is knowledge of the relevant behavioral inputs.

The fact that many mind-set interventions have proven effective with populations similar to those targeted by Fryer is worth pondering as it suggests a couple of different possibilities. One possibility is that the students in the former populations did have knowledge of the relevant behavioral inputs and so were able to take the appropriate steps to improve their grades once their mind-set was modified. A second possibility is that these populations do not have knowledge of the relevant inputs but that this is not required for mind-set changes to affect them positively. Ideally, a theory of change would take a position on these two possibilities that subsequent research could then adjudicate between.

We suspect that the failure to address the relation between the mind-set manipulation and those downstream behaviors that contribute to educational achievement is one reason that mind-set researchers face claims that their interventions seem like magic (Yeager & Walton, 2011). The sense in which these interventions seem like magic is that researchers provide students with a brief mind-set manipulation and then "presto!"—an increase in GPA occurs 2 years later. If researchers tied the mind-set change more directly to behavioral mediators (e.g., studying more), then the process would be demystified to a large extent. It does not require positing magic to understand how increased studying could lead to an increased GPA so all that needs to be explained is how the manipulation leads to increased studying. The demystification of

placebo effects provides an instructive analogy. Showing that taking an inert sugar pill can eliminate someone's pain will strike many as an act of magic, but it will strike many fewer as such when it is explained how taking a sugar pill under the illusion that it is medicine can actually affect the brain's release of pain-reducing endorphins.

Despite our admonition that interventionists better specify theoretically and empirically the behavioral pathways operative in their interventionist, we readily acknowledge the difficulty of this task. Identifying behavioral mechanisms in interventions can be fraught with problems (e.g., Green et al., 2010). For example, measuring processes during the intervention can interrupt critical psychological states and measuring processes after the fact requires strong faith in the accuracy of participants' recall (Spencer et al., 2005). Accordingly, our goal here is not to place greater burden on the already substantial undertaking of designing and implementing a psychological intervention, but rather to draw attention to a means by which such interventions could have greater impact on theory and policy. Even when measuring the behavioral pathway is not feasible, specifying the theory of change explicitly places future researchers in a better position to test and extend our understanding of the hypothesized behavioral pathways. Foundations supporting intervention-focused nonprofits have made a strong case that requiring practitioners to specify the theory of change underlying their interventions has yielded great benefit even in cases where measurement is not feasible (Brest, 2010; William and Flora Hewlett Foundation, 2012). At the very least, by incorporating a detailed theory of change into long-lag interventions, psychologists can use their speculations about specific behavioral processes as scaffolding for new theory.

Conclusion

Psychologists' role in efforts to ameliorate pressing social problems is ever increasing and for that our discipline justifiably can be proud and gratified. The argument we make in this article, however, is that to fully realize our potential contribution to intervention research we have to expand our conceptual and empirical focus. We must recognize that in moving from the lab to the field, and as a consequence from short-lag to long-lag effects, it is necessary for us to devote more attention to behavioral processes that intervene between the psychological processes that our interventions induce and the outcomes they produce. It is not simply the case that we should think of the lab as the context in which we test the theoretical basis of a psychological process and the field as the context in which we apply that process. We need to recognize that the theoretical basis of the process, because it involves downstream behaviors, can be

different as well (Kentthirajah & Walton, 2015). This will require researchers to devote more theoretical attention to what goes on during long gaps and more empirical attention to testing these assumptions.

A greater theoretical and empirical focus on behavioral pathways intervening between psychological states and outcomes measures also likely will have the additional benefit of increasing the prospects for collaboration between psychologists and researchers from other disciplines. For example, psychologists may believe that implementing a mind-set intervention will be more effective in promoting academic achievement than offering students cash incentives but dialogue between the two camps will be more productive if they can agree on the behavioral inputs that need to be altered to produce higher academic achievement and state how they believe their respective manipulations will affect these inputs. For all these reasons, we look forward to researchers treating the interval between the intervention and the outcome measure in long-lag studies less as a "black box" and more as a "white box."

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Notes

1. When we use the term *behavioral mediation* in the context of interventions, we are referring to mediation conceptually (as synonymous with process) rather than statistically. For a discussion of different means of establishing theoretical mediation with and without statistical mediation, see Spencer et al. (2005).
2. Although our analysis also applies in large measure to those long-duration interventions in which the treatment is repeated or reinstated (e.g., Dal Cin, MacDonald, Fong, Zanna, & Elton-Marshall, 2006), we restrict our discussion here to interventions in which the treatment is introduced only once.

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