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## THE RESEARCH SCRIPT

### One Researcher's View

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If you are following this book in sequence, many of you have now read a bunch of chapters about the research process. These chapters contain considerable wisdom about how you should go about planning and executing a research project. Although there is considerable agreement within the community of science-oriented psychologists about what the appropriate steps are for conducting research, in this final chapter, the editors have asked me to talk about my personal experience in conducting research. Thus, this chapter is a bit different from the others.

Before going further, it may be useful to learn a little bit about me to set the background context for my remarks. I am a social psychologist who mostly studies how people's attitudes and beliefs are formed and changed and how these attitudes guide behavior. In the majority of our studies, we examine the basic underlying psychological processes involved in change, but this work is relevant to all sorts of applied contexts, such as changing people's beliefs in therapy, in an advertising context, in the courtroom, in electoral politics, and so forth.

It is sometimes said that most psychologists are particularly interested in studying what they are really good at or what are particular problems for them. Thus, in the realm of medicine, I have friends who have selected cancer research because a family member has been affected by it. Although I don't know if I am particularly good at persuasion or particularly poor, I do know that this is a topic that has interested me since I was on the debate team in high school (a long time ago . . . don't ask). That is, I have always wondered about certain things such as: Why do some people become radical liberals and others right-wing conservatives? What causes some people to like Coca-Cola over Pepsi? Why do some people have an abiding faith in God whereas others are atheists? I mention my interest in these questions because I think that if you are studying something you are truly interested in, you will be more successful. You'll have better insights when you frame your research questions, and perhaps more importantly, when your insights prove *wrong* (and they inevitably will), you will have the motivation and ability to come up with a new idea and try something else.

In trying to outline my thoughts for this chapter, I realized that although along with my collaborators and coauthors I have published over 400 research papers of one sort or another, I hardly ever think about the research process itself. Thus, I do not have a handy list of dos and don'ts ready to disseminate. Furthermore, the psychological research literature has convinced me that although I can probably do a pretty good job of telling you *what* we do in our research and *how* we do it, I am unlikely to be able to tell you *why* we research what we do with much accuracy. This is because people in general are not

very good at coming up with the correct reasons that underlie their behavior (Nisbett & Wilson, 1977). Thus, I may say I like Coca-Cola because of its taste, but it may really be that in a blind taste test I rate the flavor as rather low, and it is the fancy advertising that has gotten me to like the product. So too it may be that I think my latest research idea came from my astute observations of real life, but instead it came from a television show that I saw last week or a journal abstract I read a month ago. Nevertheless, with this caveat in mind, I will plow forward.

There are some aspects of the research script that appear to be universal. You must come up with a research question, collect some data and analyze it, and report the results to your collaborators and funding agency (if any), and—it is hoped—to the world in an archival journal. There are other aspects of the script that may vary from study to study and from person to person. For example, for some projects, we spend considerable time reading the relevant prior literature, but for other projects, we do not. Next, I will comment on each of the major steps in the research process, with particular attention to the first one.

## THE IDEA FOR THE PROJECT

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Deciding what to study may be the single most important step in the research process and the one (unfortunately) on which there is the least guidance. I would guess that over 90% of what is written about conducting research concerns the ways a study should be designed (e.g., should you use a within- or a between-subjects manipulation?), the appropriate ways to analyze the data (e.g., regression or ANOVA?), the threats to the validity of a study (e.g., internal versus external?), and how to write it up for publication (e.g., what should be included in a methods section?). Relatively little has been written about deciding what to investigate. One notable exception is William McGuire (1997), who has come up with dozens of ways to brainstorm something to research. His handy checklist—with suggestions such as “thinking about a deviant case” or “introspecting about the causes of one’s *own* behavior”—is definitely worth examining. McGuire is something of a hero of mine. Not only is he one of the most famous researchers on attitudes (see McGuire, 1985), but his overall orientation toward research fits mine quite well (Petty, 1997; Petty, Tormala, & Rucker, 2004). McGuire is a “contextualist” who believes that virtually *any* result is possible in psychological research. According to him, our job is to determine the *conditions* under which each result occurs (e.g., *when* and for *whom*?).

Within the various areas of psychology, there are thousands of possible research questions. How can we possibly decide what to investigate? This question may be approached differently at different stages of one’s career. As an undergraduate, your first study might be a class project where you attempt to replicate a study that has already been published. If you are looking for a research mentor for more independent experience, you should try scouring faculty webpages at your university to look for potential mentors whose substantive interests most closely match your own. The same holds true for graduate students shopping for a graduate program and an initial adviser. The more genuine curiosity and interest in the fundamental research question that *you* can bring to a project, the better. Selecting a faculty adviser will help narrow the domain of possible research questions to some topical area (e.g., attitude change, altruism, memory process, anxiety disorders, and so forth). But within any one of these areas there are still an unlimited set of possible research topics. After considering your own personal interests and those of your adviser, the next most important thing to consider is what kind of contribution the research might

make to the field. At the risk of oversimplification, we can consider five distinct types of contribution that you might aspire to make.

### (1) Discover a New Effect

Science is replete with exciting new discoveries: penicillin, nonstick coating for frying pans, the laws of behavioral reinforcement, and so forth. Thus, one way to make a contribution is to discover something new. Although it is statistically unlikely that you will be the one who uncovers the cure for cancer or the common cold, it is quite realistic to aim to discover a new relationship between psychological variables. The more pervasive the variables are in the world and the more surprising the relationship, the more newsworthy the finding will be. For example, consider the fact that economists have believed since the founding of their discipline that people are basically rational. That is, a fundamental economic assumption is that people carefully weigh the costs and benefits of an option before making a decision. In light of this, the psychological finding that people are sometimes influenced by seemingly irrational cues and heuristics was worthy of a Nobel Prize (see Kahneman, 2003). For example, Tversky and Kahneman (1974) showed that people's numerical estimates could be influenced by a salient and completely irrelevant number that was accessible. In one study (Ariely, Lowenstein, & Prelec, 2003), as a way of activating a relatively large or small number at random, business students were asked if they would buy an "average" bottle of wine for a dollar amount that was equal to the last two digits of their Social Security number (SSN). Regardless of whether they said yes or no, they were then asked what was the maximum they would pay for the wine. Students with the lowest last two SSN digits (e.g., 13) said they would pay about \$9, but those with the highest last two SSN digits (e.g., 98) said they would pay about \$28. Thinking about your Social Security number should have no effect on how much you would pay for wine, but it does. This is not rational. Similarly, Johnson and Tversky (1983) showed that the momentary emotions people were experiencing influenced their estimates of the frequency of events. Thus, people who were sad estimated the frequency of negative events (e.g., getting an F on a test) to be higher than people who were not sad, but people who were happy provided lower estimates. Again, this effect of experienced emotions is not rational (see Petty, Fabrigar, & Wegener, 2003, for a review of the effects of emotion on judgments).

Within this basic paradigm—looking for how seemingly irrelevant events could bias people's judgments—there are a host of potentially biasing factors that might be investigated. You just need to think of what some of these might be. In my own case, one of the first judgmental biases we investigated was provoked by a personal experience. I was in graduate school, and a fellow grad student, Gary Wells, and I had just started teaching. We both were struck with how the students' reactions to our lectures were important in determining how good we felt about our performance. If our students were smiling and nodding their heads, we clearly thought we had done a good job, but if they were frowning and shaking their heads, we felt as though we had not performed well. This seemed pretty rational, as bodily signals from others can surely convey their impressions to us.

But could our *own* bodily signals influence our own judgments—even if these bodily signals were actually irrelevant to our judgments? To examine this, in an initial study we asked undergraduates to put on headphones and either nod their heads up and down, or shake them from side to side, while they listened to a radio program (Wells & Petty, 1980). They thought they were doing this in order to test the quality of the headphones. While making these movements, they heard an editorial that was either about raising or lowering

the tuition at their university. After the message, we asked them what they thought the tuition should be. The primary result was that people who were told to nod their heads while listening to the message (whether it was for or against raising tuition) agreed with it more than those told to shake their heads. This was one of the early studies showing that a person's own bodily responses could influence their judgments and thus introduced a new effect into the psychological literature.

## (2) Extending or Generalizing an Effect

Prior to the studies just described, we did not know that an irrelevant number, or your mood, or your own head movements could affect your judgments. Once a new effect is uncovered, people may wonder how general it is. That is, in any given study, an effect is typically shown with just one particular manipulation, for one particular sample of people, for one kind of judgment, in one particular setting. For example, in medical research, a particular drug might be tried in a hospital with a particular form of cancer (e.g., liver) in a particular population (e.g., men over 50). If that research uncovers a new effect—the drug reduces cancer—then people might want to extend the research by trying different dosages of the drug (10 versus 20 mg) on different cancers (e.g., lung) and in different populations (e.g., women) and settings (e.g., a school clinic). Such “extending” research is well worth doing once an initial effect is uncovered.

The same is true in psychological research. Thus, after learning about the head-movements study just described, one extension question might be whether head movements can affect judgments other than monetary or academic ones (e.g., tuition). Thus, some researchers wondered if head movements could affect people's evaluations of consumer products and found that it could (Tom, Pettersen, Lau, Burton, & Cook, 1991). This extending research showed that the impact of head nodding on judgment didn't appear to depend on the particular kind of judgment (or sample, or period in time) used in the original research.

Instead of extending the head-nodding effect, *per se*, other researchers might wonder if head movements are a unique bodily response that can affect judgments or whether the head-movement effect could be extended to other bodily movements. The answer to this question appears to be a resounding yes. For example, subsequent researchers have found that putting people's faces into a smiling rather than a frowning posture can influence how funny they rate cartoons (Strack, Martin, & Stepper, 1988), and people evaluate an object more favorably if they are making the bodily movements associated with pulling the object toward them rather than pushing it away (Cacioppo, Priester, & Berntson, 1993). Many other bodily movements are capable of affecting a person's attitudes as well (see Briñol, Petty, & Wagner, 2011, for a review of how such “embodied cognition” works), and even more extensions to other bodily movements seem likely.

When extending a previous finding, others are likely to find it of greater interest if the extension is to important judgments, behaviors, and population groups than to unimportant ones. Furthermore, extensions that are somewhat surprising or unexpected will likely be more valued than extensions that are fairly obvious (e.g., if an effect works for 35-year-olds, why wouldn't it work for 36-year-olds?). Finally, extensions that really broaden the domain of the effect tend to be more valuable than extensions that are more minimal. For example, when Tom and colleagues (1991) extended the domain of topics to which our head-nodding effect applied from a controversial issue (student tuition) to more everyday

objects like consumer products, this was a relatively broad extension. However, if their stated goal had been to extend the effect from the midwestern college students used in the original study to the western college students used in their study, this extension would be more minor. Thus, when planning an extension study, consideration of the dimensions of the planned extension and their informativeness and importance is critical.

It is also worth noting that sometimes when you are aiming to show an *extension* of an effect (i.e., show its generality), you may end up showing a *limitation* of the effect instead. For example, the effect may not end up working in the new population to which you aimed to extend it. In this case, if you successfully produced the effect under the original conditions (e.g., for men), but not in your extension conditions (e.g., for women), you would have uncovered a *moderator* of the effect—a variable that modifies the basic effect (e.g., gender). This moderation approach to conducting research is described after I first describe mediation. But for now, I can say that finding a limitation that is severe (the effect only works for men but not women) will typically be more important than one that is minor (the effect works for everyone except those over age 100).

### (3) Demonstrate Mediation of an Established Effect

In the research strategy just described, an investigator starts with an established effect (e.g., head nodding affects judgment) and attempts to extend it in some way beyond the original manipulations, measures, population, or setting of the original research. Uncovering new associations among psychological variables and determining how widely applicable new effects are motivate a lot of research. But, in addition to uncovering *how* variables relate to each other and how general those effects are, psychologists are often interested in *why* variables have the impact that they do. In fact, this is what theory in psychology is all about—trying to explain why variables relate to each other. Thus, research that takes an established relationship between variables and then tries to explain why the variable produces the effect it does is another research technique.

There are two common strategies for theory testing. One is to specify the underlying mechanism responsible for your effect and then conduct research in which you try to *measure* this mechanism and see if it is a viable *mediator* of the effect. If the proposed mechanism can be measured, then you can see if the variable of interest affects the proposed mechanism and if this mechanism in turn affects your outcome (see Baron & Kenney, 1986). Some scholars seem to think that demonstrating mediation is the best way to test one's theory. However, another valuable approach is to make a unique prediction from your theory of why something works, and show that your theory can predict an outcome that others cannot (see Spencer, Zanna, & Fong, 2005). For example, you might predict uniquely from your theory that the effect should hold especially for certain kinds of people or in certain kinds of situations, but not in others. The former is known as the *mediational approach* (measuring the mechanism) and the latter is known as the *moderator approach* (manipulating something that should modify the effect according to one theory but not another). I address each of these strategies in turn, beginning with mediation.

Consider first the research on head nodding described above. A number of studies have shown that head nodding can affect a wide variety of judgments. However, this initial research did not address *why* head nodding had its effects. The goal of the initial head-nodding research was primarily to uncover the effect (Wells & Petty, 1980) or extend it to a new domain (e.g., Tom et al., 1991). Various existing theories in social psychology suggest a number of possible mechanisms by which variables such as head nodding could

affect judgments. I'll just consider a few of the more obvious ones. First, maybe people who are nodding their heads are in a better mood than people who are shaking their heads, and this positive mood transfers to the topic of the message by a process of classical conditioning (e.g., Staats & Staats, 1958). Or perhaps nodding one's head in a positive way facilitates access to positive thoughts and shaking one's head facilitates access to negative thoughts. The more positive thoughts one has to the message, the more one is likely to agree with it (see Petty, Schumann, Richman, & Strathman, 1993).

Pablo Briñol and I were interested in exploring another possibility based on the social signal of head nodding. That is, when other people are nodding their heads at you while you are speaking, you are likely to gain confidence in what you are saying based on this consensual social validation (Festinger, 1954). Likewise, if people are shaking their heads at you, you are likely to lose confidence in what you are saying. We wondered if it could be the case that nodding your own head while thinking could validate your thoughts by giving you greater confidence in them and that shaking would produce doubt in your own thoughts. The possibility that people could validate their own thoughts was something Pablo, Zak Tormala, and I called the *self-validation* effect (Petty, Briñol, & Tormala, 2002).

Each of the possible theories (classical conditioning of mood, biased thoughts, self-validation) of head nodding predicts that nodding your head up and down could increase agreement with a message, but the mechanism is different. In the mediational approach, the mechanisms are compared with each other by trying to *measure* the presumed mechanisms and seeing which is best at accounting for the results. In the relevant conditions of one study using this approach (Briñol & Petty, 2003), we gave college students in Madrid, Spain, a message containing very strong arguments in favor of a new requirement that students must carry personal identification cards for security purposes. The students were asked to nod or shake their heads while listening to the message. After the message, we assessed their attitudes toward the topic as well as several of the possible mediators of the effect (e.g., current mood, positive and negative thoughts, and rated thought confidence). First, like the previous head-nodding studies, this experiment found that people who were nodding their heads were more favorable toward the message proposal than were people who were shaking their heads. This is a necessary first step in trying to tease apart the mechanisms. The next step was to see if head nodding affected any of the proposed processes.

If a good mood is responsible for the effect of head nodding on attitudes, then people who were nodding their heads should report having more positive feelings than people who were shaking. But this did not occur. If nodding one's head leads people to have easier access to positive thoughts, then people who were nodding should have written more positive thoughts or a greater proportion of positive thoughts than people who were shaking. But again, this did not happen. Finally, to test the self-validation hypothesis we asked people how confident they were in the thoughts that they listed. In this case, we found that people who were nodding their heads reported more confidence in their thoughts than people who were shaking. This is a critical finding in establishing mediation. Furthermore, we found that when we controlled for the effect of heading nodding on thought confidence, the effect of head nodding on message attitudes disappeared. Thus, it appears that head nodding increased thought confidence relative to head shaking and the more confidence people had in their largely positive thoughts to the message, the more



persuaded they were. That is, people relied on their positive thoughts more in forming their judgments when they had more confidence in those thoughts.

As was the case with the link between head nodding and persuasion, it is generally true that for any given relationship between variables, there will be a number of plausible mediating mechanisms. Thus, it is quite useful to try and determine what the mechanism is (e.g., why head nodding affects judgment). You can be the first to provide evidence for a particular mediator of an effect that already exists in the literature (as we did with the head-nodding effect). Or you can provide evidence for a *new* mediator of an effect for which people believe the mediation is already known (e.g., Tormala, Petty, & Briñol, 2002). In the case of our head-nodding research, the effect itself was already established in the literature. What made our studies worth publishing was identifying a unique mediator of the effect—people's confidence in their thoughts—and also showing that this mediator could account for the effect better than plausible alternative mediators.

One potential problem with the mediational approach is that you are looking for one mediator to win out over others. Our positive results for thought confidence provide good evidence that this was a plausible mechanism for the head-nodding effect. But our absence of effects for mood and number of positive thoughts is less convincing evidence *against* these theories as possible determinants. This is because null effects are open to multiple interpretations. Perhaps we did not have a very good (reliable or valid) measure of mood or thoughts. Or perhaps the effect of head nodding on mood or thoughts is smaller than its effect on confidence, and thus we would need more participants (power) to detect effects. It is not necessarily the case that any given effect has only one mediator (Rucker, Preacher, Tormala, & Petty, 2011). Nevertheless, providing positive evidence for a new mediator of an established effect is a sensible research strategy combined with negative effects for other plausible mediators assessed with accepted methods. So, when you read the literature on some topic of interest, think about why this effect might have occurred. It may be the case that the authors have suggested one or more possible mediators as accounting for their effect, but did not test them, so you can be the first to do that. Or perhaps they provided evidence of mediation, but did not look at alternative mediators that you believe are possible. These provide avenues for new research (see Exercise 1).

#### **(4) Demonstrate Moderation of an Established Effect**

Another potential—and in some ways superior—strategy for theory testing research is to examine possible *moderators* of an effect in the literature. A moderation approach is sometimes used when people are looking for limitations of an effect (e.g., it works for men but not women). As noted earlier, sometimes these limitation effects are discovered in the process of trying to extend an effect to another population, setting, manipulation, or outcome. In this section we deal with moderation studies that aim to do more than extend or limit an effect (as in research strategy 1), but to use the approach to test a mechanism for why an effect occurs. That is, moderation is an alternative means to mediation by which you can compare different explanations of or theories for an effect. Consider the research on head nodding again. Imagine that proponents of the mood or biased-thinking theories were upset with our mediational research because elimination of their theories relied on null results. An alternative way to compare the mediators is to try and think of some implication of one theory that differs from the plausible other theories.

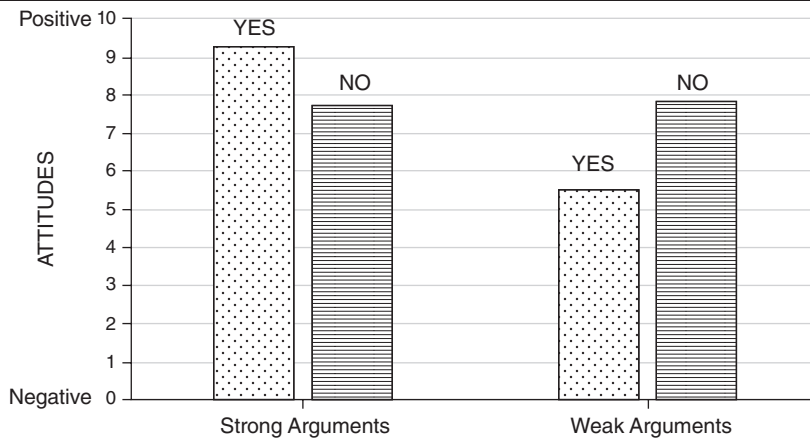
This moderation strategy is a favorite of mine and is one that we have used in a large number of papers. For simplicity, let's return to the head-nodding effect one more time.

How can our preferred thought confidence explanation be compared to the others using a moderator rather than a mediator approach? One way to distinguish the theories is to think of a variable that can be manipulated that should make a difference according to the thought confidence approach but that would not make a difference (or might even predict an opposite effect) from the competing approaches. One aspect of some of the explanations for the head-nodding effect is that they seem to *always* expect vertical (yes) movements to produce more favorable responses than horizontal (no) movements. If vertical movements put people in a good mood and this is simply associated with the message position via classical conditioning, it would always make the message position seem more desirable. Or, if vertical movements increased access to positive thoughts, this too should always make attitudes more favorable. Other possible theories also make this “main effect” prediction. For example, if people simply reason that if they are nodding, they must agree (a “self-perception” account; Bem, 1972), this too always expects nodding to produce more agreement than shaking.

According to the self-validation (thought confidence) account, however, nodding would not always produce more favorable attitudes than shaking. This is because nodding is proposed to enhance confidence in one’s thoughts. Therefore, if the thoughts to the message are favorable (as they generally would be if the arguments are strong), then nodding would produce more favorable attitudes because having confidence in and relying on one’s positive thoughts should increase agreement. But what if the person listening to the message was thinking negative thoughts to the proposal? Here, nodding would increase confidence in these negative thoughts and thereby *reduce* agreement. As just explained, however, according to the other theories, manipulating the valence (positive or negative) of the thoughts should not matter because head nodding is doing something outside of this induction (e.g., the positive mood from head nodding becomes directly attached to the message position). Thus, one moderator approach to comparing the confidence theory to the others is to have people nod or shake their heads to a message that elicits either mostly favorable thoughts or mostly unfavorable thoughts and see if the effect of head nodding is the same for each kind of message.

In a study using this moderation approach we had some participants read a message that contained very strong arguments for raising university tuition (e.g., let’s raise tuition so that we can reduce class size) that were pretested to elicit mostly positive thoughts. Other students read a message with very weak arguments to raise tuition (e.g., let’s raise tuition so we can plant exotic tulips rather than common ones on campus) that were pretested to elicit mostly negative thoughts (see Briñol & Petty, 2003). Then, the participants were instructed to nod or shake their heads while listening to the message over headphones and report their attitudes toward the proposal. Figure 37.1 presents the results. You can see that the effects of head nodding were quite different depending on whether the message presented strong or weak arguments. When the arguments were strong and thoughts were mostly positive (Figure 37.1, left), nodding produced more agreement than shaking—the result found in all of the prior research and expected by all of the theories. However, when the arguments were weak and thoughts were mostly negative (Figure 37.1, right), head nodding led to *less* agreement than shaking. This was exactly as the thought confidence explanation expected, but counter to the other theories. Because the self-validation hypothesis predicted a pattern of results that the other theories did not, it was not critical to measure the postulated mediating processes. Indeed, if your theory can predict a



**FIGURE 37.1** ■ Effects of Head Nodding and Argument Quality on Attitudes

Source: Data are from Briñol and Petty (2003).

pattern of results that all other plausible theories cannot, then you can provide very compelling evidence for your theory in the absence of mediational evidence.

In the case of the head-nodding research, we uncovered the initial phenomena in one study (Wells & Petty, 1980) and then many years later conducted mediator and moderator studies to examine a mechanism by which the effect occurred (Briñol & Petty, 2003). This is how research progresses in a long-term research program. Sometimes a research team uncovers a finding and then spends the next several years examining various mediators and/or moderators of the basic effect. Indeed, according to the contextualist framework, it is unlikely that thought confidence is the *only* mechanism by which head nodding can affect judgments, and future work will likely show that different mechanisms can be responsible for the effect in different situations (Petty & Cacioppo, 1986). Although sometimes researchers follow up on their own original findings, at other times it can be quite fruitful to follow up on findings initially reported by others. So, as with the mediational approach, when you read the literature on some effect of interest to you, consider why this effect might have occurred. If the original authors have not already tested their conceptualization with the moderation approach or if you have an alternative conceptualization, consider how the moderation approach could provide evidence for it.

### Moderation Versus Mediation

When should you use a mediational approach, and when should you use one based on moderator variables? Which approach is better can depend on how easy or practical it is to measure the plausible mediators and to ascertain, assess, and/or manipulate the relevant moderators. Sometimes it has taken years for researchers to figure out how to assess a postulated mediator. For example, research on cognitive dissonance theory (Festinger, 1957) took place over several decades, with many moderator studies being conducted before a team of researchers came up with measures to assess the uncomfortable feelings that dissonance was thought to invoke (Elliott & Devine, 1994). In other contexts, however, the mediator approach might be the most straightforward. For example, say that someone has a theory that engaging in exercise reduces heart attacks by reducing inflammation in the

blood passageways, whereas another investigator has the hypothesis that exercise works by reducing cholesterol levels. It would be relatively straightforward to assess these possible mediators in high- versus low-exercise groups and then determine which (if either) is the more plausible route by which exercise reduces heart attacks.

Finally, it is very important to note that although I have presented the mediator and moderator approaches as independent strategies, it is certainly possible and desirable to examine them together. For example, in a single study, you could both manipulate the moderator of interest and measure the mediators of interest. Such designs are capable of examining moderated mediation and mediated moderation (see Wegener & Fabrigar, 2000, for further discussion).

## (5) Conducting a Replication Study

All four of the previous approaches to research involve coming up with something new: a new effect, a new extension, or a previously undiscovered moderator or mediator. But no single study clearly establishes an effect or an extension or a moderator or mediator until that study is repeated by you or someone else, preferably the latter, often several times. A successful replication study always extends a previous result in at least some way (e.g., minimally to a different point in time or to a different sample of people). As noted earlier, such replication-extensions are typically of greater interest when the extension is to a very different population or setting or manipulation or measure than the original. Importantly, sometimes, researchers conduct replication studies not so much with the intent of extending (or limiting) an assumed real effect, but mostly to see if the original effect is actually a reliable one. As you may recall from your statistics class, when effects emerge and are statistically significant ( $p < .05$ ), there is still some probability that the effect was a chance finding.

Many of you have probably heard about the *replication crisis* in psychology (e.g., Pashler & Harris, 2012) and other disciplines (e.g., Ionnidis, 2005). The crisis resulted from the fact that when various labs have tried to replicate some previously published effects (e.g., Open Science Collaboration, 2015), in a surprising number of instances (exceeding 50% in some cases), the effect failed to occur. Why is this? To understand replication failure, we first need to consider two kinds of replications. In an *exact replication*, the replicator aims to repeat in as much precise detail as possible what was done in the original study. Thus, if one research team was studying the impact of frustration on aggression and had participants in the frustration condition try to solve an impossible (rather than easy) puzzle and then measured how much hot sauce the person delivered to a partner in each condition, the replicator would do exactly the same thing—use the same puzzle and hot sauce, though with a different sample of people. In a *conceptual replication*, the replicator tries to instantiate the *concepts* rather than the precise *methods* used. Thus, a replication of the frustration-aggression study might have participants receive negative (rather than positive) feedback after doing well on a task and then assess the aggressiveness of the written feedback left for the experimenter.

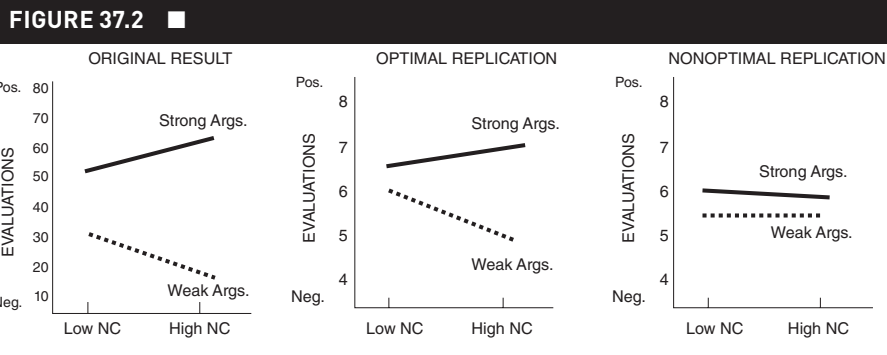
Although there are various arguments in favor one type of replication over the other (e.g., Strube & Strack, 2014), my view falls on the side of conceptual replications largely because the exact replication approach fits the physical sciences much better than it does psychology. First, in the physical sciences there tends to be a one-to-one match between the “concepts” of a study and the “operations.” Thus, if a chemist wants to replicate a study in which two parts of hydrogen are mixed with one part of oxygen to produce water, there

are not many ways to represent these variables. This is unlike the many ways to represent frustration and aggression. Why does this matter? In the water study, there is really only one thing to do: mix two parts of hydrogen with one part of oxygen and see if water results. It doesn't matter where you get these ingredients. Wherever you get your supply, you are likely to appropriately represent what is required to reproduce the result. In the aggression study, things are not so simple. It is certainly easy enough to use the same difficult and simple puzzle as in the original study and buy the same brand of hot sauce for the assessment. But the original researcher was likely not really interested in whether not solving difficult puzzles produces hot sauce delivery but rather whether frustration causes aggression. Thus, a replication researcher needs to make sure that if the original puzzles are used, they still cause the same degree of frustration as in the original sample and whether hot sauce delivery represents aggressive behavior in the new sample as well as in the original (see Fabrigar, Wegener, & Petty, 2020).

Imagine a replication researcher attempting to repeat the original study in a new country where the original difficult puzzle is one that is widely known and therefore not so difficult. Using the exact same frustration manipulation would not result in the same level of frustration as in the original study. Further imagine that in this country, hot sauce is a highly desired food item and sharing it is seen as gift giving. Using the exact same aggression measure is not sensible if you want to test the frustration-aggression hypothesis in this new country. In replicating psychology studies, it matters less if you do exactly the same thing than if you successfully instantiate the psychological variables. The aim of a replication study should not be to produce an exact methods replication but an *exact conceptual replication* where the concepts have the same meaning and range as in the original study (Petty, 2018). If the original research provided both manipulation and outcome checks, a replicator would know what targets have to be met, and the job of replication is easier.

So, in the physical sciences there is often a one-to-one relationship between concepts and operations (e.g., there is only one way to represent hydrogen). In psychology we have many ways to represent concepts (e.g., there are many ways to represent frustration). There is a second important way in which psychology studies differ from the physical sciences, and that is the relationship between the causal variables and the outcome variables. In the physical sciences, there is often a one-to-one relationship between them. That is, mixing  $H_2 + O$  produces water and nothing but water. And if you want to make water, you *must* combine  $H_2 + O$  and nothing else. In the frustration-aggression relationship, frustration can produce things other than aggression such as dislike of the person frustrating you. And there are many ways to enhance aggression other than frustration, such as increasing the ambient temperature (Anderson, 2001).

There are many implications of this many-to-many situation. For example, imagine that someone attempted to repeat the original frustration-aggression experiment in a lab room that was unusually hot. Because this high temperature might increase the overall level of aggressiveness even in the group that gets the nonfrustrating puzzle, no differences in aggression between the experimental and control groups might be seen. In any replication study, there are numerous contextual variables like this that could be operating that could obscure treatment differences (Van Bavel, Mende-Siedlecki, Brady, & Reinero, 2016). In any given replication, you might not know what these are, but you should be on the lookout for any plausible contextual features (e.g., heat) that might affect your outcome measure (e.g., aggression) in addition to your intended causal variable (e.g., frustration).



Source: Original Cacioppo, Petty, and Morris (1983) result (left panel) and replication by Luttrell, Petty, and Xu (2017) using optimal (middle panel) and nonoptimal (right panel) procedures (i.e., those employed by Ebersole et al., 2016).

I mention these two problems—(1) failing to appropriately represent the conceptual variables, and (2) introducing contextual differences that could obscure treatment effects—because both were present in a reported failure to replicate one of our own studies. It can be disappointing when other researchers claim that an effect you have introduced may not be real. Yet, when Ebersole and colleagues (2016) attempted to replicate one of our studies in which we showed that people who were relatively high versus low in their chronic motivation to think engaged in more thought about a persuasive message (Cacioppo, Petty, & Morris, 1983), they failed. In our original study, we measured motivation to think using the need for cognition (NC) scale (Cacioppo & Petty, 1982). To assess amount of thinking about the persuasion message, we varied the argument quality (AQ) of the rationale provided in the message randomly with some participants receiving a message with strong arguments and others with arguments. The message evaluations of those who are thinking more should greater differentiate the quality of the arguments (Petty & Cacioppo, 1986). Our key finding was that people who scored high in NC showed a larger distinction in the different arguments than those who scored lower on the scale (i.e., NC interacted with AQ in determining evaluations; see left panel of Figure 37.2). However, Ebersole et al. (2016) found no evidence for this interaction. This puzzled us because this interaction had been replicated several times previously (see Carpenter, 2015).

We believe that the Ebersole et al. replication effort fell prey to the two problems just noted (Petty & Cacioppo, 2016). The first problem was that the two key variables, NC and AQ, were not instantiated in a way that was as valid as the original. With respect to NC, the replication researchers used a six-item scale instead of the original 34-item scale. With respect to AQ, although the original study pretested the arguments to ensure that they were appropriately strong and weak, the replication researchers failed to do this. The second problem was that the replicators changed the context of the study in an important way. Specifically, their instructions to the participants implied that the position advocated, if adopted, would affect the participants directly. The original study had no such instruction. Including this instruction meant that the replicators were looking for NC to affect thinking in a context in which everyone (even those low in NC) were likely to be thinking since increasing the personal relevance of a message is a well-documented way to motivate thinking (Petty & Cacioppo, 1990). Thus, what the replicators were attempting was similar to testing the impact of frustration in a hot room where even the nonfrustrated were likely to be aggressive.

To see if these problems were responsible for the replication failure, two graduate students and I (Luttrell, Petty, & Xu, 2017) conducted a new study in which we compared the instructions and procedures used by the replicators with more optimal instantiations of the independent variables (i.e., full NC scale, pretested messages) and a more optimal context (i.e., with no implication of personal relevance of the advocacy). When we used the optimal procedures, we replicated the original finding (see middle panel of Figure 37.2), but when we replicated the replication procedures, we replicated their failure to find the interaction (see right panel of Figure 37.2). Before we were able to publish our results, the journal editor insisted that the initial replicators now replicate our optimal procedures to see if they could now produce the effect. As it turned out, when they used the optimal procedures, they too were able to produce the original effect whereas they did not produce the effect when they replicated their flawed procedures (Ebersole et al., 2017). The lesson here is that if you aim to replicate someone else's study, you should be sure your instantiations of the key variables match the original in their construct validity, and you use a context in which these variables are as free to operate as they were in the original work.

## CHECKING THE IDEA OUT: A LITERATURE REVIEW AND OTHER METHODS

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In the previous sections, I outlined some major categories of studies that you might conduct. Following the generation of your idea (e.g., I wonder if head nodding affects persuasion) and prior to conducting your study, it is quite common to check your idea out. Why? First, you want to find out if the idea is interesting to others (and not only you), and whether your idea seems plausible and important or not. If others don't like the idea or find it unimportant or implausible, this does not mean that the idea is bad, but it does mean that in writing about your idea, it would be wise to emphasize your awareness of its counterintuitive nature or perceived lack of importance. Of course, what is counterintuitive or unimportant to one person may not be to another. I even had the experience once of a journal editor telling us that a result we reported was so surprising that it was virtually impossible. We were asked to replicate the effect a few times using other manipulations. Once we replicated the effect and resubmitted the paper, however, the same journal editor now found the effect to be "obvious." Indeed, once we become familiar with something, we may think we knew it all along, so you may need to convince others that your idea is not obvious by carefully citing prior thinking about the subject matter.

Another important reason for checking your idea out is to find out if the idea (or something close) has already resulted in published research. Journal editors and reviewers especially value unique ideas, so you want to make sure that this characterizes your proposal. Or, if your idea relates to existing propositions, you want to be sure to indicate what the points of novelty and advancement are. The literature is already replete with the same basic idea masquerading under different names. You do not want to (deliberately) contribute to this.

There are several ways to check out your idea, but the two most common are asking others and scouring the psychological literature. Researchers vary as to whether they recommend reading the literature intently on a topic before or after you settle on a study idea. Some fear that reading too much literature beforehand might stifle creativity by boxing you in to established ways of thinking. Others fear that a failure to read the literature will lead you to waste your time planning something that will not be a significant

contribution. In reality, this is not an either/or decision. Your basic idea may just pop into your head when you wake up in the morning. Or it may be more obviously provoked by reading something in the newspaper or the scientific literature. A moderation or mediation idea likely comes from reading about an established effect or perhaps hearing someone talk about their research at a conference or a colloquium. There may be no better way to find out what is current and exciting in the field than to attend a conference in your area. Studies are presented at professional meetings years before they appear in the journals, and by learning what contemporary researchers are doing right now, you have a leg up on those who wait for it to appear in a journal. The worst feeling is to spend a year or two working on what you think is a great idea only to find a similar study appear in a journal just as you have finished writing the first draft of your paper. Attending conferences does not preclude this from happening (it has happened to me), but it does make it less likely.

In addition to reading the literature, you should check your idea out with others. Your fellow students and available faculty members (especially your academic adviser) are a critical source of feedback. At a minimum, they can tell you how your idea fits with their way of thinking about human behavior. Or, if they are experts in the domain, they may be able to give you a quick indication of how it fits with the literature. At this point in my career, I still find it highly valuable to check ideas out with others (mostly my graduate students or postdocs—present and former). In addition, they are checking their own unique ideas with me. In the process of sharing your ideas with others, valuable collaborative relationships might be formed.

I have had the incredible good fortune of going to graduate school where there was an enviable collection of generous and expert faculty along with two very talented fellow graduate students (John Cacioppo and Gary Wells) who became early collaborators and subsequently went on to become quite famous psychologists. Later in my career, collaborations with my own graduate students over an extended period of time have also influenced me greatly. Although I have had many fantastic students as collaborators, the two most prolific are Duane Wegener and Pablo Briñol, who in addition to our many collaborations, also developed into extraordinary award-winning scientists in their own right. There are a number of famous long-term collaborations in psychology (e.g., see Levine & Moreland, 2004), and when these collaborations are clicking, they are joyous relationships. Although extended collaborations can be incredibly fulfilling, short-term collaborations (for a project or two) can also be valuable when there is mutual interest on a particular topic. Successful long-term collaborations also involve shared interests but are more like a social relationship in that they require more trust, similar work habits, and it sure helps if you really like the person! Indeed, working with my favorite collaborators is the part of my career I find most rewarding.

Despite the value of collaboration, early in one's career, one is often advised to avoid too much work with a particular collaborator. The fear is that it may be difficult to parse the credit. (Is the work yours or your collaborator's?) Although it is certainly important to demonstrate your own independence in a scholarly career by not working *exclusively* with the same people on every project, this can usually be done without sacrificing any collaborations that you wish to continue (i.e., collaborations that are both enjoyable and productive). In my view, the primary way in which people evaluate fellow scientists is by the *quality* of the work in which they are involved. So, if your collaborations are genuine (i.e., all parties are contributing intellectually), leading to high-quality outcomes, and are fun, then stick with them!



## CARRYING OUT THE RESEARCH

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Once you have determined that your research idea seems sensible and worthy of pursuit, colleagues and the literature will also be helpful in guiding you toward any established ways of conducting the research. All else equal, if you can conduct your study using established manipulations and measures, you will be on safer ground than if you have to invent your own. This is primarily because if things do not turn out as intended, you won't know whether to attribute the problem to your conceptualization or to your unique new methodology. But as noted earlier, even if using established procedures, you still need to make sure that these methods are linked to the intended psychological constructs in your new sample and setting. What if after reading all you can, you find that there are no prior methods or that these methods are too cost-prohibitive or impractical or no longer reflect the constructs of interest? Then, you would have to come up with something on your own. If this is one of your early research projects, you should proceed with caution.

This book and many others provide much useful advice about conducting and analyzing your data, and I will not dwell on that here. But it may be important to note that if the data came out as you expected all of the time, this stage of the research enterprise would be relatively simple (and boring). In fact, if data always came out as you expected, it might be hard to get excited about collecting and analyzing the data, as you would already know what the results are going to be! But the data will not always come out as you expect. This is, in fact, one of the parts of research that is most interesting and challenging. When data do not fit your expectations, you have a puzzle to be solved. Why do the data differ from your expectations? In our own lab, seemingly uncooperative data have sometimes led to totally new lines of research.

If your procedures produce null effects for what was a sensible hypothesis, it may be the case that your procedures are at fault (e.g., unreliable measures or insufficiently strong manipulations, or too much psychological "noise" in the experiment). Or perhaps the procedures are fine, but the power of your study was insufficient to detect a significant effect. Much has been written about the statistical *significance* of results. Some authors argue that we should not worry much about whether our results are statistically significant or not because any two variables will likely be related if you have enough participants (e.g., see Schmidt, 1996). These authors argue that we should be more interested in the size of the effects that we uncover. Although there is undoubtedly too much emphasis placed on whether a statistical test produces a .06 or .05 *p* level, it is very important to know if the effect we uncovered is due to chance or not. Therefore, although I agree that any two conceptual variables of sufficient interest to motivate someone to conduct a study are likely to be related, it will not always be obvious what the *direction* of the relationship will or should be. Until the results can be deemed as not likely due to chance, you will not be sure what the direction of the effect is. In theory-testing research, establishing a clear direction of an effect is usually more important than the size of that effect. However, in applied research, the effect size question can be much more meaningful.

If you have discovered a direction of effect that is not the one you anticipated, it is important to remember what my first adviser in graduate school, Robert Cialdini, always said: *The data never lie*. That is, assuming the effect is not due to chance (which happens sometimes), the unexpected effect was produced for a reason. Maybe your theory is right, but your research operations do not represent the concepts you intended and represent some other concepts. If you believe that this is the case, the natural solution is to try a

different method that might better capture your conceptual ideas. But an alternative strategy is to try to figure out what conceptual variables were inadvertently tapped by your procedures and pursue this. The unexpected finding may be even more interesting than what you initially intended to study. Only you can ultimately decide if this *digression* is worthy of pursuit, but discussions with colleagues and a new reading of the literature can once again help you.

## REPORTING FINDINGS TO OTHERS

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The final stage in the research script involves reporting the research to others. I once knew a psychologist who claimed to conduct research projects simply to satisfy his own curiosity, with no intention of reporting the results, but this is rare. Reporting often occurs first with informal discussions and presentations to colleagues. Making an oral presentation, either formally at a conference or informally in a seminar or brown-bag series, forces you to try to explain what you did and why you did it. It allows others to ask questions and point out relevant literature or concerns that you might not have considered. Over the years, I have learned that the data almost always look better (stronger) in initial oral reports than they will ultimately look when written in manuscript form. This is because in an oral report you can easily gloss over imperfections or irregularities and focus your audience on the key methods and findings. One implication of this is that if you can't make a persuasive oral report from your data, then there may be little hope of turning it into a full-fledged publication. So if you think your results are promising, take any reasonable opportunity to make an oral presentation of your research to some audience.

If your research survives the oral report (i.e., no fatal flaws were uncovered), it is time to prepare a more formal write-up of your research. My preference in writing research is to prepare the Methods and Results sections first. In fact, I like to prepare the methods as soon as it is determined that there are some sensible results that might be worth reporting. Even better is to document the methods in writing *before* you conduct the study. If you do not write down the methods relatively quickly, you may forget important details. When you are just starting research, this may seem impossible: How can you forget something you have spent so much time thinking about and preparing? But once you have conducted a few studies, the essential details of the methods might become lost. Preregistering your study with your methodological details can be very useful if you are sure to follow your plan. You don't want to end up with a big data file with little recollection of the order in which you asked questions or the gender of the participants, etc. Mark and label everything clearly and carefully. There is no easier time to organize your materials and data files than before the data are even collected.

Once the Methods and Results sections for each of the relevant studies are prepared, it is time to write the Introduction. In many ways, the Introduction is probably the most important part of the paper. By the time your readers have read it, they need to believe that the key question or questions you are addressing are of sufficient importance and interest to publish. How should the Introduction be written? Daryl Bem (2004) has argued that your manuscript should tell a story. When writing the Introduction, I like to think of myself as a prosecutor in a criminal case. My job is to prove beyond a reasonable doubt that the studies I will report are critical (i.e., will be informative) for testing my conceptualization. The data in a criminal case (and in the psychological lab) are collected in all sorts of ways in a particular sequence. A good

prosecutor would never assume that the evidence should be presented to the jury in exactly the order in which it was collected. A prosecutor knows that even if the bloody knife was found before the eyewitness, maybe it makes sense to present the information in the opposite order. Of course, a prosecutor and a researcher should never make up evidence or be deceptive about the manner in which the evidence was collected or the hypotheses that led the data to be collected (e.g., Kerr, 1998). But it is perfectly appropriate to present the information in the order that allows readers to understand the research most clearly.

## CONCLUSION

In this concluding section, I will add just a few final words of advice about aspects of the research script:

1. *Write your ideas down the minute they occur to you.* Like the methods of your study that you think you will remember forever, the same is true of your research ideas. You might forget a good one! Write your ideas down in a reasonable form along with any ideas you have about *why* the idea might be important and to what literature you think the idea relates. You will not pursue all of the ideas that you have, but some of them may be useful later (others may seem nonsensible).
2. *Be bold.* You can begin thinking in terms of a simple relationship between variables or about one particular mediator or moderator of an effect, but as you progress, you may find that you have invented a whole new theory with multiple mediators and moderators. Early in my career, when I was preparing my dissertation, I had planned a series of simple studies on a fairly narrow topic. When I went to my adviser, Tim Brock, he said my topic was fine, but that what I really needed to do was to come up with a new theory. I had never contemplated this as it seemed like something one did much later in one's career. But a theory is nothing more than a series of smaller ideas strung together with some conceptual glue, specifying mediators and moderators. I never would have thought of developing a theory on my own, but it was a valuable lesson in trying to think on a larger scale. So, don't shy away from being grandiose.
3. *Have fun.* There are many possible careers in which you can be successful. If you choose a career that involves psychological research, do it because you are truly interested in human behavior, want to contribute to the building blocks of the great human puzzle, and greatly enjoy at least one or more aspects of the research process. As I noted earlier, the aspect of research that I have enjoyed the most is the amazing opportunity I have had to work with a good number of very bright and talented collaborators who are genuinely fantastic people and also cherished friends. My career would not be what it is without them. They know who I'm talking about even if I could not mention them all by name in this chapter!

## EXERCISE

Examine the titles of the articles in the last few issues of your favorite psychology journal. Read the abstracts of studies with appealing titles to find the one article that you find to be the most interesting. Assuming the authors present a theory to explain the key effect

reported in the article, try and think of a *different* reason why the key effect might have occurred. Think about how you might assess the alternative underlying processes in a mediational study. Then, think about what variable(s) might moderate the effect, according to your alternative theory, that would not make a difference (or would make a different difference) according to the prevailing theory.

## RECOMMENDED READINGS

The reference section below provides a wealth of possibilities for additional readings and the discussion above indicates what each of them is about. I recommend paying special attention, however, to the following two readings. These two best exemplify the process-oriented framework emphasized in this chapter.

McGuire, W. J. (1997). Creative hypothesis generating in psychology: Some useful heuristics. *Annual Review of Psychology*, *48*, 1–30.

Petty, R. E. (1997). The evolution of theory and research in social psychology: From single to multiple effect and process models. In C. McGarty & S. A. Haslam (Eds.), *The message of social psychology: Perspectives on mind in society* (pp. 268–290). Blackwell.



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