The Psychology Research Handbook: A Guide for Graduate Students and Research Assistants

The Research Script:
One Researcher's View

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Chapter 33: The Research Script: One Researcher's View

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 a great deal of wisdom about how you should go about planning and executing a research project. It is probably clear to you by now that 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 rest. Rather than offering you advice about what I think you should do in some generic sense, I will offer my personal observations based on my own research experiences.

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 (or resist change) and how these attitudes guide our 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 some acquaintances who have selected cancer research or a more rare disease because they or a member of their family has been affected by it. Although I don't know if I am particularly good at social influence 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 long-standing interest in these questions because I think that if you are studying something you are truly interested in, you'll be more successful. You'll have better insights when you frame your research questions; and perhaps more important, 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 200 research articles 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 actually do in our research activities, I am unlikely to be able to tell you why we do what we do with any degree of accuracy. This is because people in general are typically 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 snazzy 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 (hopefully) 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

In my view, deciding what to study is the single most important step in the research process and the one (unfortunately) on which there is the least formal 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 vs. ANOVA), the threats to the validity of a study (e.g., internal vs. 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 when and for whom each result occurs.

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 looking for some research experience, you are best off scouring faculty Web pages at your university to look for a faculty member whose substantive interests most closely match your own. The same holds true for graduate students shopping for a graduate program and an initial advisor. The more genuine curiosity and interest in the fundamental research question that you can bring to a project, the better. Selecting a faculty advisor 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 advisor, 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 four fundamental types of contribution that are likely to be valued. I discuss each in turn.

(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, 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. Thus, if people were asked about their social security numbers and then asked to estimate the length of the Nile River, people with large social security numbers (e.g., 999–99–9999) would estimate the length of the river to be longer than people with low social security numbers (e.g., 111-11-1111; see Wilson, Houston, Etling, & Brekke, 1996). Thinking about your social security number should have no effect on your estimate of the Nile's length, but it does. This is not rational. Similarly, Johnson and Tversky (1983) showed that the momentary emotions people were experiencing could influence 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 of the frequency of negative events. 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 at 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 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 of us.

Although it was clear that bodily signals from others could influence our judgments, could our own bodily signals influence our own judgments—even if these bodily signals were actually irrelevant to our judgments? We wanted to know, so 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. That is, we told the participants that we were trying to scientifically simulate the movements they might make when jogging, dancing, and so forth. 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 agreed with it more than those told to shake their heads. Thus, when the message favored raising tuition, nodders said they favored higher tuition than shakers, but when the message favored lowering tuition, nodders favored lower tuition than shakers. This was one of the early studies showing that a person’s own bodily responses could influence his or her judgments.

(2) Extend or Clarify an Effect

In the studies I described above, the primary contribution of the research was to document a new effect. That is, prior to the research, we did not know that an irrelevant number, your mood, or your own head movements could affect your judgments. Once an effect is uncovered, people [p. 468 ↓] may wonder how general it is. That is, in any given study, an effect is typically shown with one particular manipulation, for
one particular participant population, and for one kind of judgment. Thus, in medical research, a particular drug might be tried out 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 vs. 20 mg) on different cancers (e.g., lung) and in different populations (e.g., women). 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, you might wonder, for example, if head movements can affect judgments other than monetary ones (i.e., tuition). Thus, some researchers wondered if head movements could affect people's evaluation 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 made or the particular population 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 other bodily movements could make a difference. The answer to this question appears to be a resounding "yes," as subsequent studies have shown that people will like things more if their face is put into a smiling rather than a frowning posture when they are evaluating it (Strack, Martin, & Stepper, 1988) and if they are making the bodily movements associated with pulling something toward them rather than pushing it away (Cacioppo, Priester, & Berntson, 1993). Other possible bodily responses and potential biases remain to be researched.

In our own lab, we were interested in extending the work on emotion and judgment. Recall that Johnson and Tversky (1983) had shown that experiencing a positive or a negative emotion could influence the frequency estimates for events. In the original research, the emotions of happiness and sadness were used, but these are only two of the several fundamental emotions possible (e.g., Ekman & Friesen, 1971). For example, sadness is a negative emotion, but so too are anger and fear. Given that the emotions are distinct, perhaps different emotions of the same valence would have different links to judgment. In particular, Dave DeSteno, Duane Wegener, Derek Rucker, and I
wondered if sad and angry states would increase the likelihood of all negative events, or whether there would be some greater specificity in the effects of the emotions. Thus, in some of our research (see DeSteno, Petty, Wegener, & Rucker, 2000), we had people who were placed in a sad or an angry state estimate the frequency of negative events that had a sad (e.g., loss of a loved one) or an angry (e.g., someone taking your parking place) tone to them. We found that there was a matching of emotion to the tone of the event, such that people made to feel angry were more likely to overestimate the likelihood of angering rather than sad events, but sad individuals were likely to do the opposite. Thus, this work extended or clarified prior work on emotion and judgment because the earlier work treated all negative emotions as having similar effects (i.e., increasing estimates of negative events). Our research was able to build on this earlier finding and show that greater specificity in predictions was possible.

When extending or clarifying a previous finding, others are likely to find it of greater interest if the extension is to important judgments, behaviors, stimuli, and population groups rather than to unimportant ones. Furthermore, extensions that are somewhat surprising or unexpected will likely be valued more than extensions that are somewhat obvious (e.g., if an effect works for 30-year-olds, why wouldn't it work for 40-year-olds?). As one example of a surprising extension of an effect, consider recent work on ostracism or social exclusion (e.g., Williams, Case, & Govan, 2003). It may not be very surprising to you that it is very aversive to be excluded or shunned by others; however, it may be much more surprising to know that who does the ostracizing doesn't matter much if at all. In an extensive program of research on this phenomenon, Kip Williams and his colleagues have shown that the effect is so robust that it is difficult to find qualifications to it. Thus, it doesn't matter if you are ostracized by liked or disliked others—it is equally unpleasant. Furthermore, in one study, college students felt as bad when they were excluded from a game by a “dumb” computer as when they were excluded from the game by living, breathing humans (Zadro, Williams, & Richardson, 2004).

It is important to note 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 to the effect. That is, the effect may not work in the population in which you aim to extend the effect, or it may not work for your new manipulation, or for your new measure—although you had initially expected that it would. In this case, if your research procedures are sound,
you would have uncovered a *moderator* of the effect, a variable that modifies the basic effect. This strategy is described after I first describe mediation.

(3) Demonstrate Mediation of an Established Effect

In the strategy just described, an investigator starts with an established effect (e.g., emotion affects judgment) and attempts to extend or clarify it in some way. 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, 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, another research strategy is to take an established relationship between variables and try to explain why the variable produces the effect it does.

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 process and see if it is responsible. 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). These days, many seem to think that demonstrating mediation is the only, or the best, strategy for testing one’s theory; however, another valuable approach to theory testing 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. 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* and the latter is known as the *moderator approach*. 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. But, this initial research did not address *why* head nodding had its effects. The goal was just 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, it may be that people who are nodding their heads are in a better mood than people who shake their heads, and this positive mood generalizes 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 & Cacioppo, 1986).

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., mood, thoughts, and 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 people who were shaking their heads. This is a necessary first step in trying to tease apart the mechanisms. The next step is to see if head nodding affected any of the proposed mechanisms. If classical conditioning of a good mood is responsible for the effect of head nodding on attitudes, then people who were nodding should report a more positive mood 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 head 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. When people had confidence in the mostly favorable thoughts that they generated to the message, they were more persuaded than when they had doubt in these favorable thoughts (i.e., when shaking their heads). That is, people used their thoughts in forming their judgments when they had confidence in their thoughts, but they did not use their thoughts when they were held with doubt.

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 possible mediating mechanisms. Thus, it is quite useful to try and determine what the mechanism is (e.g., why head nodding affects judgment or why accessible numerical anchors affect 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 showing that this mediator
could account for the effect better than alternative mediators that were plausible prior to our research.

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 is a plausible mediator of 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 of the head-nodding effect. 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. Nevertheless, [p. 471 ↓] 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 perhaps they did not provide evidence of mediation. 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. Moderation is an alternative means by which you can compare different theories or explanations of 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 others.
This moderation strategy is a favorite of mine and is one that we have used in a large number of studies. For simplicity, let's return to the head-nodding effect one more time. How can our preferred thought confidence explanation be compared with the others using a moderator approach rather than a mediator approach? The key is to think of some third variable that should make a difference according to the thought confidence approach but that would not make a difference (or might 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. 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 relying on one’s positive thoughts should increase agreement. But, what if the person listening to the message were thinking negative thoughts to the proposal? Here, nodding would increase confidence in these negative thoughts and thereby reduce agreement. According to the other theories, because one’s thoughts to the message have nothing to do with the effect (e.g., the positive mood from head nodding simply becomes attached to the message position), a manipulation of thought valence (whether the thoughts are positive or negative) should not matter. 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.

According to most of the theoretical accounts of head nodding, the direction of the thoughts should not matter, but it should matter greatly according to the confidence
theory. So, to compare these theories, we had participants read a message that either contained very cogent and compelling arguments that were pretested to elicit mostly favorable thoughts (e.g., let’s raise tuition at the university so that we can reduce class size) or rather weak and specious arguments that were pretested to elicit mostly unfavorable thoughts (e.g., let’s raise tuition so that we can plant exotic tulips rather than common ones on campus; see Briñol & Petty, 2003).

Figure 33.1 Effects of Head Nodding and Argument Quality on Attitudes

Then, the participants were instructed to nod or shake their heads while listening to the message and report their attitudes toward the proposal. Figure 33.1 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, 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, 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 pattern of results that all
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 occurs (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 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 initial findings, at other times it can be quite fruitful to follow up on findings initially reported by others. This was the case in some recent work we have done on what is called the prime-to-behavior effect. The basic idea in prime-to-behavior studies (see Dijksterhuis & Bargh, 2001) is that when various traits, stereotypes, or motives are subtly activated, people will often behave in accordance with the activated mental contents even if they are completely irrelevant to them. For example, in one famous study, the investigators wanted to see if college students could be primed to act more elderly (Bargh, Chen, & Burrows, 1996). In this research, undergraduates were asked to make sentences with sets of words that were provided. In the “elderly” prime condition, some of the words in the scrambled sentences they were given were associated with the elderly (e.g., grey, Florida, retired), whereas in the control condition, they were not. Following the sentence generation task, the students were timed as to how long they took to walk down the hallway toward the exit elevator. The key finding was that students primed with the elderly walked more slowly than those not primed. That is, the primed individuals acted in a manner consistent with the stereotype of the elderly. In other studies, people primed with the elderly became more forgetful (Levy, 1996) and became more conservative in their personal attitudes (Kawakami, Dovidio, & Dijksterhuis, 2003). These prime-to-behavior effects are quite robust, but it is not at all clear why they occur (see Wheeler & Petty, 2001).
Consider two possible accounts that have been put forward. One account is a social tuning explanation, which says that people adopt the traits of primed social categories because it is evolutionarily adaptive to fit in with others (e.g., Kawakami et al., 2003). That is, because people generally like others who are similar to themselves, if people automatically adopt some of the traits and characteristics of others, they will presumably get along better with them. So, if you walked into a room of elderly people and you adopted some of their traits and attitudes, they would presumably like you better than if you did not, and this motivation to fit in could be behind the prime-to-behavior phenomena. Another explanation which Christian Wheeler, Ken DeMarree, and I have called the active-self view (Wheeler, DeMarree, & Petty, 2005), says that when traits are subtly primed, they become confused with one’s own traits. According to this view, you are not trying to fit in with others, but you actually see yourself as having some elderly characteristics and act this way for that reason.

How can these two theories be teased apart? Both theories mostly make the same predictions about what attitudes or behaviors a person would adopt as a result of priming, but they differ dramatically about the motivation underlying the behaviors. One theory says that behavior is enacted because people want to fit in with the activated group (e.g., the elderly), whereas the other says that they are doing it as a reflection of who they are. We employed a moderation approach to distinguish these explanations. It turns out that there is an individual difference measure that taps directly into these divergent underlying motivations. The self-monitoring scale (Snyder, 1974) is a personality measure that separates individuals into those whose behavior is guided by fitting in with others (called high self-monitors) versus those whose behavior is motivated by acting on their inner feelings and traits (called low self-monitors).

It is important that the two different prime-to-behavior theories make opposite predictions about which group should show stronger prime-to-behavior effects. If the motivation underlying the effects is a motive to fit in, then high self-monitors should show the effects more clearly; however, if the motivation is based on a desire to act as oneself, then low self-monitors should show the stronger effects. In a series of studies, DeMarree, Wheeler, and I assessed our participants’ self-monitoring tendencies, primed various stereotypes, and measured relevant behaviors. Across these studies, we consistently found that it was the low self-monitors who showed the only significant prime-to-behavior effects (see DeMarree, Wheeler, & Petty, in press, for details).
showing this particular pattern of moderation, we were able to provide support for the active-self view of prime-to-behavior effects. Thus, whether you are following up on an empirical relationship that you uncovered initially or one discovered initially by others, a fruitful research strategy is to use moderator variables to limit the effect and thereby provide evidence for a particular conceptual account of the effect.

**Moderation versus Mediation**

When should you use a mediational approach, and when should you use one based on moderator variables? Sometimes a mediation approach might be preferable to a moderation approach, and sometimes the opposite is the case. Which is better can depend on how easy or practical it is to assess the plausible mediators and to ascertain, assess, and/or manipulate the relevant moderators. In the example on prime-to-behavior effects, we thought it was easier to use a moderation approach rather than try to determine if a prime activated a particular motive, which in turn drove the behavior. In other contexts, however, it may be more sensible to try the mediator approach. 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 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 one 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).
Checking the Idea Out: A Literature Review and other Methods

In the previous section, 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 are you checking the idea out? First and foremost, you want to find out if the idea is interesting to others and whether your idea seems plausible and important or not. Although the idea may appeal to you, others may find the idea to be wacky, or it may fly in the face of conventional wisdom. 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 the counterintuitive nature of the idea. Of course, what is counterintuitive to one person may seem mundane to the next. 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, however, and resubmitted the paper, 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 already accepted 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 to it) is already out there in the literature. Journal editors and reviewers especially value new or 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 uniqueness 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 during a shower. 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 her or his research at a conference or colloquium. There may be no better way to find out what is current and exciting in the field than to attend a major conference in your area. Studies are presented at conferences 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 merely read about research 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 appears 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 advisor) 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 out ideas 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 checking out your ideas with others, valuable collaborative relationships might be formed.

[p. 475 ↓ ] I had the incredibly good fortune of going to graduate school where there was an enviable collection of generous and expert faculty along with two very talented fellow students who became early collaborators and subsequently went on to become quite famous psychologists: John Cacioppo (now at the University of Chicago and renowned for his contributions to social neuroscience) and Gary Wells (now at Iowa State University and widely recognized for his contributions to psychology and law). Gary and I collaborated on several of my first projects, including the initial head-nodding study mentioned above. John and I have worked together on over 50 published papers and even more individual studies. My collaborations with John greatly influenced
the way I think about human nature and set the tone for subsequent successful collaborations. Short-term collaborations (for a project or two) are mostly based on mutual interest in a topic or perhaps some formal advisor-advisee relationship. Successful long-term collaborations also involve mutual interests but are more like a social relationship (e.g., a marriage) in that they require trust, similar work habits, and it sure helps if you really like the person as well! 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, as they can combine the best of intellectual stimulation with warm comradery. I am highly fortunate to have been the beneficiary of several such long-term collaborative relationships, and they are the part of my career that I find most rewarding.

Early in one's career, one is often advised to avoid too much collaboration or 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, you can accomplish this 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 work in which they are involved. So, if your collaborations are genuine (i.e., all parties are contributing intellectually), are leading to high-quality work, and are fun, then stick with them.

Carrying Out the Research

Once you have determined that the 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 methods. 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. Perhaps after reading all you can, however, you find that there are no prior methods or that these methods are too cost prohibitive or impractical. Thus, you might have to come up with something on your own. If this is
one of your early research projects, it is at this point that you might abandon the idea unless you can come up with something practical. At a minimum, your new methods might require piloting unless your procedures have such obvious validity that this seems unnecessary (e.g., there are not too many ways to manipulate head nodding).

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 I enjoy the most. 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 is most likely 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 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 important to know if the effect we uncovered is due to chance. 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'll 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.
If you have discovered a direction of effect that is not the one you anticipated, it is important to remember what my first advisor in graduate school, Robert Cialdini, always said: *The data never lie.* That is, assuming the effect is not due to chance, the unexpected effect was produced for a reason. Maybe your theory is right, but your research operations do not represent the concepts you intended, but 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 as interesting as, or even more interesting than, what you 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**

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 of your results often occurs first with informal discussions and presentations to colleagues. Making an oral report, 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 other issues and relevant literature that you might not have considered. Over the years, however, 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 idea 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 report of your research. My preference in writing research is always 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. If you do not write down the methods relatively quickly, you may forget the details. When you are just starting research, this may seem impossible. How can you forget something you’ve spent so much time thinking about and preparing? But once you’ve conducted a few studies, the essential details of the methods might become lost. You can end up with a pile of questionnaires, with little recollection of the order in which you administered them, or a big computer data file, with no record of whether this was the study in which you ran the male or the female participants. Mark and label everything clearly. There is no easier time to do this than while you are collecting the data.

[p. 477 ↓] Once the Methods and Results sections for each of the relevant studies are prepared, it is time to write the Introduction. The Introduction is probably the most important part of the paper. By the time your readers have read your Introduction, they need to believe that the key question or questions you are addressing are of sufficient importance and interest to publish.¹ How should the Introduction be written? Daryl Bem (2004) has persuasively 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 data support my conceptualization. The evidence in a criminal case (and in the psychological lab) is collected in all sorts of ways in a particular sequence. A good prosecutor would never think of presenting the data to the jury in exactly the order in which it was collected. Yet when I was a journal editor, I would see papers written by (inexperienced) authors who would present their studies in exactly the order in which the studies were conducted. 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. A prosecutor and a researcher should never make up evidence or be deceptive about the manner in which the evidence was collected, but it is perfectly appropriate to present the information in the order that tells your story the best (see also Bem, 2004).

In the days when research reports consisted of a single study, there was no issue about the order of presentation. Today, when the best journals require at least two or more experiments (with good reason, I think), the order of presentation is a very important issue to consider. Often in a research report, you will begin with a study that reports a
basic effect that forms the meat of the paper. Subsequent studies will deal with issues such as showing the generalizability or limitations of the effect or providing evidence of mediation or moderation of the effect that is consistent with your preferred explanation of the effect and inconsistent with alternative accounts. In some cases, when we have conducted five or six studies on a phenomenon, I sit down with our research team and try putting an Introduction together in different ways. Here is how we would tell the story if the studies were ordered in this way. Here is how we would tell the story if the studies were presented in a different way. After playing with different possibilities, we settle on one and begin writing the Introduction. Sometimes, in the course of writing, the sequence that we decided on fails, and we try another one. The process goes on until the story is presented in the most clear and compelling way.

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, but will not, the same is true of your research ideas. Write them 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. I recently had the experience of uncovering a folder of research ideas that was over 10 years old. Some of these ideas didn't make any sense to me now, or seemed silly; others were well past their prime. But a few still seemed worthy of investigation.

• 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. Very early in my career when I was preparing my dissertation project, I had planned a series of relatively simple studies on a fairly narrow topic (the mediators of the persistence of persuasion over time). When I went to my advisor, Tim Brock, he said my
dissertation topic was fine, but that what I really needed to do was to come up with a new theory. I had never contemplated proposing a theory, as that 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, so I swallowed the bait and tried to come up with something. The point is that I never would have thought of doing this 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 careers you can have, and you can be successful in many of them. 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 goodly 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!

**Note**

1. Unfortunately, some readers may not look at your Introduction first, as they will want to know what you did and found, unbiased by your story (e.g., see Oleson & Arkin, Chapter 4, this volume).

**Exercises**

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 to you is the most interesting. Assuming the authors present a theory to explain the key effect reported in the article, try to 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 discussions in this chapter indicate what each of them is about. I would recommend paying special attention, however, to the following two readings. These two best exemplify the contextualist framework emphasized in this chapter.


**References**


Handbook of research methods in social and personality psychology (pp. 412–450). New York: Cambridge University Press.


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