Introduction

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Welcome to the third edition of the *Handbook of Research Methods in Social and Personality Psychology*. The first two editions of this handbook – published in 2000 and 2014 – have played an important role in widening access to and utilization of cutting-edge methods in the field. Useful as these volumes have been, the science of personality/social psychology never sleeps when it comes to developing new and improved research methods. And so herewith we present a third edition, designed to capture some of the most influential and promising new methodological advances in our field.

This edition covers both traditional methodological topics that have seen advances in recent years and novel approaches of recent vintage. There are, of course, many other topics that could have been included. We've chosen content that we believe will be most relevant to the largest proportion of new scholars in the field. This is not to suggest that topics omitted from this edition are less important – in fact, with the help of Cambridge University Press and the consent of the authors, we've made several of the still timely chapters from the second edition freely available on their website (address needed). Like the current chapters, these should be considered essential elements in a survey of the field's methodological state of the art and might well be incorporated into a methods-course syllabus.

Methods are the bread and butter of science: more than just defining what is possible, they suggest what can be learned. It goes without saying that much of what appears in journals today asks questions that could not have been imagined, much less investigated, by earlier researchers. Tony Greenwald's (2012, 99) observation that there is "a much greater frequency of Nobel science awards for contributions to method than for contributions to theory" still rings true. Methods open the door to questions, and questions make it possible to advance theory. Our fondest wish is that this book will inspire you, the researcher, to come up with and pursue new and better questions.

How Can I Toggle between Advancing Theory and Using New Methods?

The methods in this handbook are not intended to replace theories, but rather to provide you with a guide to optimally test them. To this end, most chapters toggle between theory and method, presenting them as two sides of the same coin. As Greenwald notes, the history of psychology is full of such examples of theories advancing with the advent of new methods, and vice versa. For example, relationships scientists were able to develop and test complex theories of how partners influence one another over time once advancements were made in multilevel modeling generally, and dyadic analysis more specifically. But those advances happened out of necessity, once scholars realized the importance of handling issues such as nonindependence of data properly; without doing so, it was easy to make mistakes in drawing conclusions from the data. As another example, advances in psychophysiologic measurement have enabled researchers to move beyond the lab to the field to collect cardiovascular data using watches and phone apps; questions such as how basic stressors impact people's health across multiple social contexts and in multiple countries can now be studied in a more ecologically valid manner. And with these tools being widely available, more and more researchers can now think even bigger about the contexts in which they can study physiological processes – from the classroom to the operating room.

Parts I and II of the handbook introduce readers to basic issues in the science and design of research. These might be considered "what you need to know before starting." Parts III and IV introduce the reader to new methodological and statistical procedures while considering the role of theory development in their application. As you read these sections, we urge you to ask yourself, "Will this be the best method available to test my question? Will this analysis strategy allow me to ask what I need to ask to advance my theory? What will this method allow me to learn that I couldn't have done with other methods?" It can be tempting, especially in the early stages of becoming a scholar, to lean into sexy new methods, to let your research topic be guided by a trendy methodological approach that you're excited about. Yes, knowing about new methods opens up the possibilities of what can be tested, but leading with a good theoretical question, especially one that may lead to useful applications, is good practice. Methodological approaches change all of the time, but, to quote Lewin's well-known dictum, there's nothing so practical as a good theory.

In This Era of Rapid Change, How Can I Possibly Keep Up?

One challenge all researchers face is that methodological and statistical advances are happening at a rapid, and perhaps expanding, pace. We three editors struggled to create a handbook that is both cutting-edge in the approaches covered, and also likely to stand the test of time. To these ends, our goal is to expose the reader to as many methods as possible, while also covering the "tried and true" principles of methodological skill building. For example, since the prior edition of this handbook, the age of massive data began – a point made clearly in Kosinski's chapter in this handbook. Scholars can now test big questions with huge multinational samples, such as how emotions spread throughout social networks, who we are attracted to, and what gives rise to misinformation contagion. As we were finalizing this handbook, the advent of generative AI happened. AI tools will shape not only the types of question we can ask as scientists, but also how we go about developing materials, collecting and analyzing data, and even writing up our scientific findings. Issues of ethics will surely come up as these tools develop. And with these new methods, advances in statistical procedures continue. Machine learning and computational modeling – topics covered in Part IV – allow scholars to examine dynamic processes that unfold naturally over time. Using "data-driven" statistical approaches such as these can help circumvent some of the ethical issues around questionable research practices that have entered the zeitgeist since the prior edition of this handbook. On top of these changes, most analyses these days are conducted using packages in R, Python, MPlus, and SPSS, many of which can be run with little knowledge of what is going on "under the hood." It can be all too easy to run code without knowing what it does; fighting this temptation is essential as you develop your methodological and analytical skills.

In short, there's a lot you can do without knowing the classic issues of psychometrics and study design – basic things like principles of construct validity and scale construction. We think the opposite is true: the basic building blocks covered in Parts I and II of this handbook are essential to learn during this time of constant change. This knowledge will anchor your understanding of methodological developments, and give you a more critical eye when encountering something new.

How Should You Read This Handbook?

When deciding how to organize this handbook, we adopted the perspective of a new researcher – someone with little experience conducting or running studies, who is in the early stages of formal graduate-level education. This book is designed to guide the reader through that process – from the earliest stages of thinking about research, when foundational issues of ethics and replication are top of mind, through the design, implementation, and analysis stages. The handbook deliberately starts out broad, addressing big-picture issues relevant to all new scholars interested in the study of personality and social psychology, regardless of topic. The chapters in the middle – Parts III and IV – are more bespoke, addressing specific methodological and statistical procedures. Here, our goal is to expose the reader to cutting-edge methods and statistical procedures that have the potential to dramatically shape the types of question you can ask, and how you go about testing them. We aimed for as much breadth as possible, given that most graduate programs offer expertise in only a handful of areas.

The first part of the handbook – foundational issues in psychological science – covers basic issues around ethics in conducting research, replication, how to conduct team science, and how to conduct research that captures multicultural and multinational samples. We recommend reading this part first. It requires no prior knowledge or expertise in conducting experimental research.

The second part of the book – basic methodological considerations – covers the basics of design and validity – beginning with essential concepts of validity and experimentation, then moving to quasi-experimental design and field methods, which take the researcher out of the lab and into uncharted (real-world) territory.

The third part of the handbook assumes a basic knowledge of experimental method and design, along with consideration of ethics and replication. Here, the authors of the chapters provide a deep dive into several popular methodological approaches at the forefront of cutting-edge research. These chapters need not be read in order (as we suggest with the first two parts), but rather can be turned to as the researcher begins to think through different methodological options.

The fourth and final part is aimed at the reader who has designed a study and collected the data, and is now ready for data analysis. Like Part III, we opted for breadth in presenting multiple data-analytic approaches, all of which consider the role of theory in data analysis. We advise novice researchers to read these chapters when planning their research, and not just after the data are in hand; knowing what your analyses will need, and being aware of what they can do, may dictate how you ask your questions.

Concluding Remarks

One of the most exciting times in a researcher's career is the early stage, when ideas are free-flowing, the topics available for study seem boundless, and the prospects for contributing to knowledge are animating. Our goal in editing this handbook has been to help the reader maintain that sense of excitement, all while introducing them to new concepts in a hands-on way that is not meant to intimidate, but rather to create opportunities and to inspire. And if you don't learn it all in one pass, that's okay. In fact, it's expected. As scholars who've been at it for a while, the three of us still become aware of new methods often. The learning never ends!

Reference

Greenwald, A. G. (2012). There Is Nothing So Theoretical as a Good Method. *Perspectives on Psychological Science*, 7, 99–108.

1 The Romance of Research Methods

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romance, /rōˈmans,ˈrōˌmans/

n. a quality or feeling of mystery, excitement, and remoteness from everyday life

(Lexico)

n. an attraction or appeal to the emotions e.g., *the romance of the sea*

(Merriam-Webster)

This handbook on research methods contains knowledge on a dazzling range of methods of research that are available to you and me today. But the principles of scientific discovery that underlie these methods are, of course, quite old and shared by all sciences. Epistemological inquiry – that is, an understanding of how knowledge may be acquired and what constitutes a claim to knowledge – connects those of us across time and place fortunate enough to engage in the production of knowledge. I find romance in these connections: between us and those who came before with the same hope and desire to make new knowledge, and between us as psychologists and the same adventurers across all of science today.

Because we stand on the shoulders of giants, the research methods that you and I use every day can be traced, at a deep level, at least to Aristotle's teachings of logic and systematic observation, to Archimedes' dare about a lever long enough, to Francis Bacon's *Novum Organum* that pushed out dogma and ushered in the modern idea of the scientific method. As we delve into the nitty-gritty that gives each of the sophisticated methods in this handbook its unique power, we would do well to remember, even if only in passing, that we are part of a line of people joined not by blood, religion, or language but by a commitment to a distinctive way of understanding the world – in the case of our science, an unexampled way of understanding the mental and social world of humans. And that way, because of the methods by which we do our work, has a remarkable quality – of revealing the nature of reality *independently of whatever we may think or feel about it*.

Science (in fact just its most daring feature – the methods of research) is to my mind the great romance in the history of ideas. The term "romance" has had many different meanings across time and contexts; in just the one domain of early modern fiction, a plethora of very different uses can be found (Lee, 2014). I hope it is obvious that my usage has nothing to do with romance as in romantic love or, even by extension, with "falling in love with science." Rather, I began this essay with two less common definitions of romance that I wish to signal. For clarity, "romance" as used in this essay on the methods of research (of all things!) is meant to capture some amalgam of the following:

* I gratefully acknowledge comments from Visty Banaji, R. Bhaskar, Tessa Charlesworth, and Kirsten Morehouse. I thank all three editors for suggesting improvements. I dedicate this chapter to the first graduate student who dared to select me as his Ph.D. adviser in spite of many better options, the incomparable Curtis Hardin. Many ideas in this essay were formed in the process of doing the work we did together, and especially through our meandering conversations about the philosophy of science.

a profound attraction to the scientific method based on uncontainable curiosity; feelings of anticipation, elation, and wonder about what may be learned; and a set of daily experiences that sharpen the senses and create the compulsion for even greater understanding.

Scientists are rebels. By nature, or by training, it is our wont to be dissatisfied with things as they are, and to believe that a truer view of reality is within grasp. But also, to be questioning the way in which we go about this understanding, for it is persistent discontent with the quality of existing epistemic instruments and what entirely new inventions could reveal that is obligatory for progress – even when it means having to discard an instrument that we ourselves invented in favor of a superior one. All this places you and me in a minute minority of humans of all those who have ever existed who not only were deeply curious about the social world (there are many such people) but who also were committed to expressing this curiosity in the oddest possible way – in a manner that is set up to prove our strongest beliefs wrong. If that's not romance, I don't know what is.

How we go about our work has many components, but the one that signifies great romance to me is the daily cycle of developing the skill with a *method*, adapting it for our purpose, improving it, and placing it back into action in the testing ground. To many, this continuous cycle without end might be disconcerting, or at least perplexing (an aunt of mine, when I told her what I did, said, "But what will you do, dear Mahzarin, once you know the answer?"). But by the time you've gotten to a stage that has led you to this handbook, you know that you've opted into such a unique tribe – people who rely on methods and instruments brand-new and ages-old that are independent of you the individual and what you think and believe. Instead, you have committed to a way of knowing that is deeply dependent on shared understanding and a consensus about how to go about adjudicating a claim to knowledge. The romance of research methods as embodied in this handbook is the culmination of a history of the hard-won idea that the truth can be known not by the most compelling divination, not by the most sophisticated armchair theorizing, but rather by the labor of empirical investigation that rests on *methods* for doing our work that we built with our own minds and hands with the predominant purpose of keeping us honest.

A life spent doing this, without concern about the fruits of the work, would be, I assure you, an entirely satisfying and worthwhile avocation. But if you are fortunate, the methods described in this handbook will lead you to challenge received wisdom effectively enough that you will enter that group of giants on whose shoulders others will stand. Although giant status is not likely for any one of us, these research methods render such an outcome possible, and at the very least they allow us to be links in this great chain of original knowledge production. Is there, I ask, a more romantic idea than that?

To describe what we do, I have used the word "chasing," at least since Eddie van Halen, who when asked to reflect on his guitar virtuosity, said innocently, "I was just chasing sounds." To chase sounds is hard and we envy those who chase them and bring them back for us to hear. It's the same with you and research. Personality and social psychology (PSP) is the science of understanding humans as individual selves and as social beings. It is hard to chase what you are after as a social and personality psychologist. Nothing that you are studying that brings you to this handbook on methods of research is ingenerate in the sense of inborn or innate; nor is the process intuitive. Nothing you do at any point during this "chase" has a clearly marked path or an end point. Francis Bacon's idea of an imposed inductive process is *not* intuitive. It is an acquired taste that most people in the world have simply not had the opportunity or inclination to develop. Don't be dismayed when people don't understand why what you do and how you do it makes little sense to them, as long as there is a community, small as it necessarily will be, that understands.

As I say to my friends and audiences in the world of space exploration, what we study in PSP is not rocket science. It is a whole lot harder. Why? Because what we study isn't something that we can place at the other end of a telescope, such as a planet, or under a microscope, such as a platelet. Those are sufficiently challenging fields of inquiry that they garner billions of dollars in investment. But what we study (and what should garner billions of dollars in investment) are things that can never be seen directly, as they have no physical form at the level at which *we* want to understand them. Sure, thoughts and feelings have a basis in electrochemical activity, but that's not the level of analysis that we have opted into. We are, as psychologists, interested in mental representation: perception and memory, the process of inference, the thoughts and feelings inside a person's mind, unfolding in complex social contexts. That's what we are chasing; that's our unique path as social and personality psychologists, and that's among the hardest stuff to chase and bring back for others to see. To understand the difficulty of what we do can be brought home to us by the wisdom in a comment attributed to the physicist Murray Gell-Mann: "Think how hard physics would be if particles could think."

Methods of research, once we have developed deep expertise in them, need not be easily understood by everybody. When I taught at Yale, a colleague in another area of psychology would always ask students defending their dissertation a question that would go something like this: Why would your grandmother care about this? Or, about a particularly arcane aspect of the method, he would ask, How would you explain this to your grandmother? Of course, I am not opposed to striving to make our work as accessible as possible to the broadest group interested in learning about it. But it had become customary for me to intervene in these moments and ask, Why should we care if Suzie's grandmother cares about this work or not? Why should that be a requirement of the work of a Ph.D. thesis? If we believe that our methods of research – abstract, esoteric research methods explicitly designed to overcome the limits of lay inference – are honed over years to achieve a sophistication that can be accessed only after deep study, why should we expect grandparents to understand any of this? Think of your work this way: of course, you should strive to make what you do and how you do it as transparent as possible, your thinking and writing as lucid as possible. But also remember, your work is rooted in technically sophisticated methods that have been developed by generations of scientists to ultimately have a place in this handbook. These methods make possible the strongest inferences that are attainable today (even though, if we are fortunate, they will be outpaced by new developments). You are now a part of this tribe of experts. Your grandparent is not.

In your day-to-day life as a wielder of the methods of research, you will often ask yourself how best you can track these invisible entities called *thoughts* and *feelings* that underlie complex aspects of personality and social psychology. You have at your disposal both dry and wet methods: behavior is the dry stuff; neural activity and physiology are the wet stuff. Both types of data are indirect proxies of what we wish we know because, as I've said, our ultimate task is to understand the mind as it reveals and shapes personality, and ourselves as social beings.[1] As if this effort were not already hard enough, psychology faces an

[1] I had assumed that to be a psychologist is to be interested in the mind – of humans and other species. But I realized in the process of writing this chapter that this is not a widely shared view and that psychologists consider themselves to be interested in "behavior" at a more primary, or the same, level as the mind. To me, behavior and brain/physiology are two ways in which we know how to reach the mind, which is why we are *psych*ologists, not behaviorologists. I am going to stay with my view that behavior is only of interest in that it teaches us something about the (human) mind, which remains my primary and ultimate interest. I should also add that I consider an eyeblink, a keypress in response to a stimulus, and a police officer stopping and searching a citizen to all be equally worthy of the label "behavior." For this reason, we should express our puzzlement if, when presenting, say, reaction time or survey data, we hear the question "Did you also include a measure of behavior," even though we know that what the questioner likely means by "behavior" is a more ecologically realistic behavior.

additional and unique problem among the sciences. As the comedian Emo Philips captured it, "I used to think that the brain was the most wonderful organ in my body. Then I realized who was telling me this." Or, to use William James's more scholarly and familiar phrasing of the same problem, the *knower* is also the *known*.

And therein lies a unique problem of inference, that the thing being understood is also the thing doing the understanding. Because of this, so much greater is the need for us to work out established methods of research; so much greater is our need to build protections into our methods that will keep us honest. Among these should be the goal of developing methods that can be shared across laboratories, methods whose components are transparent, methods whose data from experiments are available for anybody to analyze. There is no alternative to a diversity of scientists using the same method to examine the same questions. I recall a conference my colleagues and I organized with NSF support many years ago when laptops were newly invented (our family's early-adopter laptop took up a whole extra seat on an airplane), where the goal of the conference was for participants to bring their computerized experimental procedures for each other to try out (new in those days), and for us to be actual subjects in each other's experiments, something that the study of implicit cognition uniquely offered. Those experiences revealed in a unique way where we were united in our understanding, where differences were arising from, the extra little instruction some of us were using that others were not, and technical nuances that could account for disparities in the data – but also that the processes we were tapping into were reliable and robust enough that certain variations did not seem to matter at all. I have never enjoyed a small-group conference more. I came back with direct knowledge of what my colleagues were doing at a level that would simply not have been available by reading a report. I am impressed with the changes in journal policies today, especially the removal of word limits for methods and results sections of empirical reports that are available with full details in supplemental materials.

The more accepted and routine a research procedure is, the more invisible it will become to the expert, and the less particular aspects of it will appear even to warrant mention. But our intuitions can only be checked if we can hand over versions of our procedures to other labs with PIs who have expertise and shared assumptions but whose intuitions differ from our own. Fortunately, we are more respectful today of meeting the dual goals of greater sharing of procedures/materials and greater transparency, both of which are encouraged by the open-science movement. Of course, this is not to say that every newcomer is limited to using only the methods that exist. In my own career, I focused disproportionately on a method that did not exist when I entered the field. To understand it and hone it has been among the most intellectually fulfilling experiences of my life.

The great romance of research methods is that we believe that the mind can be measured. I loved "*Attitudes Can Be Measured*," an early paper by L. L. Thurstone (1928), who invented the method of equal-appearing intervals for attitude measurement. If you read the paper, and especially the footnotes, it will be clear to you that Thurstone is bristling at the orthodoxy of his time, in particular the view – that things called attitudes, beliefs, and opinions simply could not be measured. To us, almost a century later, the idea that attitudes can be measured is so patently obvious that we cannot fathom the need for a paper defending that obvious premise. But we have our own challenges. When the first studies of automatic or implicit attitudes emerged, the response was also one of disbelief; we were so familiar with one type of attitude measure that other, new forms could not be easily accommodated, and were even threatening. I was led to pay homage to Thurstone with an essay titled "*Implicit Attitudes Can Be Measured*" (Banaji, 2001). New methods have the power to render even familiar ideas into strange ones. For a moment, in the early years, this was true of implicit cognition. The data that the new methods spat out were indeed surprising, even challenging. The standard paradigm could not accommodate them. But a community of methodologists was devoted to understanding that strangeness, and today we can say that applications of new methods to the study of implicit cognition has given us a more clear view of the nature of attitudes and beliefs, conscious and less conscious.

Whatever your topic of study, if you have chosen a sufficiently difficult problem to tackle, the path will be littered with obstacles. On some days you will feel akin to Hercules completing the ten labors. Part of the reason for this is that you are not in a field where the problems worthy of study are more or less mapped out, and you, the new entrant, must pick one to work on. That's not for you as a member of the PSP tribe. You have chosen a field where the questions for study themselves are being discovered. You have before you a map with some territories vaguely identified, but, like the maps in the medieval world, the map of your discipline has entire regions unexplored – marked only with symbols that warn "Here be dragons." Of course, such adventures are scary for you will confront the limits of your own bladesmithing ability worthy of these dragons.

As if to study representations of the physical world such as the perception of complex scenes were not hard enough, we in PSP study thoughts and feelings about a totality of the self, called personality, a good chunk of which lies outside the person, called the social context. And as if this were not hard enough, we attempt to understand the tenuous processes that emerge at the intersection of two individuals called a relationship. And as if that were not hard enough, we try to understand what goes on in the head, as measured by behavior or physiology and brain activity, as we try to represent our views of our conspecifics and relations among them, not to mention entire collectives. Whoa.

Entering this greatest of romances requires, first and foremost, a deep recognition of the indispensability of strong methods of research, even though it would be so much easier to sit around a fire and chat about these things as an acceptable way of knowing, as our ancestors did from the beginning of time and until a second ago in evolutionary time, when the scientific method emerged and made conversations around campfires just that, conversations that need not contain any facts. What is thrilling to us is that Baconian demand that we simply cannot rely on our intuitive ability for correct observation and inference. Our field, more than any other, has been responsible for compelling demonstrations that human beings cannot be trusted to see others and even themselves as they truly are. Just looking at the classics in our field, we know this in a unique way: from the gobsmacking experiments of Stanley Milgram and those by Bibb Latane and John Darley that showed the ability of humans to behave in ways they would surely despise in another; from the self-deceptive rationalizations discovered by Festinger; from the decisions riddled by misattribution revealed by Schachter or so flat-out biased, that we were bug-eyed reading the results of Richard Nisbett and Lee Ross; from the short-termism documented by Walter Mischel; from the costs we impose on our relationships, as Elaine Hatfield and Ellen Berscheid showed; from Susan Fiske and Shelley Taylor's compendium of the limits and challenges of social cognition broadly and Patricia Devine's work on automatic race bias even in those who view themselves to be unbiased; and from Dan Gilbert and Tim Wilson's evidence that we are no good at knowing what our own future selves will desire. Our field, historically and today, offers the most incontrovertible evidence of why we must remain skeptical of human intuition and perhaps this is why we have this unique romance with the methods of research.

To some, methods of research are dry and boring. Instead, it is theory that makes their neurons pop. But without a deep connection to epistemic approaches, we risk missing the thrill of discovery in real time, in that moment when the method allows only you to see evidence for something previously unknown, when only you know that "it didn't have to be this way." To not immerse ourselves in the depths of the methods of research is to also miss out on the possibility of directly contributing to advancing them, no matter how small that advance. How often have you thought about method improvement as a part of your scientific goals? The pleasures can be deep and endless. So, as you study the approaches and methods from this handbook, consider developing an identity, not necessarily with a single method but with the enterprise of measurement itself. There's a chance that, if you do, you will experience an additional dimension of the romance inherent in science, that of creating and improving the very instruments of exploration and discovery.

If I sound Panglossian about research methods, let me add that I am, of course, aware that methods of research, by the fact that they are the creations of limited information processors called humans, can lead us to wrong answers. Our implementation will necessarily be imperfect, and our interpretations will be inaccurate. But it's the process of iterating through these imperfections, improving what we know and how we know it, and the failures, that makes for the romance. Among the most treasured experiences that can come from this investment is the possibility of seeing a beloved theoretical idea fizzle out. In my own case, I can say that because my ability to theorize is relatively weak compared to the strength of the methods I've been able to deploy, I have experienced that unique romance of learning that I was completely wrong in my expectations of a result innumerable times. The methods of research were so compelling that my mind was changed. Not because a theoretical idea hit me on the head as I sat under a tree, but because an analysis based on a method of research showed me what was true or untrue as I sat at my computer.

When I began graduate school in 1980, a program of research in my adviser's lab had been underway for several years on the sleeper effect – the result that a persuasive message produced greater attitude change over time rather than immediately. Not only was the interpretation of the sleeper effect based on a theoretical claim that the effect occurred because of a dissociation between the content of the message and the discounting cue (e.g., a low credibility source), but also the theoretical claim hovered over all discussions of experimental design and directly shaped the research procedures that were used. The brief history is that work, starting with the original report of the sleeper effect by Hovland, Lumsdaine, & Sheffield (1949), had resulted in dozens of experiments across different labs but without a clear understanding of when the effect appeared and when it did not. Around the time I was finishing my own dissertation, the lab I worked in published an interesting paper. The authors concluded that the effort to study the sleeper effect had been unnecessarily inefficient and meandering (Greenwald et al., 1986). They argued that the discovery of the conditions that produced the sleeper effect would have materialized more clearly and rapidly if the scientists hadn't been mired in (content-cue dissociation) theory of differential rates of fading in memory of the message relative to the discounting cue.

Although I wasn't involved in this research, watching the stumbling by seasoned research scientists indelibly shaped my own preparedness for research. There is, of course, built-in failure in the research process and these failures are critical to the path of discovery. But in retrospect, the sleeper effect's failures, the authors were claiming, were unnecessary failures. As an observer of that arc of a research program, that lesson had significant influence on my thinking. Specifically, it led me to the view that, of course, theory was deeply important, but how it drove research and specifically where in the research cycle it was free to have influence – and, more importantly, where it needs to be set aside – needed explicit recognition.

An alternative approach was to work toward simply producing an effect like the sleeper with the best methods available without the concern for theory testing, even a theory as cool as content-cue dissociation. Learning the history of the failure of producing a reliable sleeper effect (eventually successful) taught me not the unimportance of theory, which is a common misunderstanding of the conclusion reached by the authors of that paper but rather the placement of theory; to reduce the burden of theory in the design and testing phases of the research cycle. Setting theory aside in that moment may ultimately, even more effectively, advance theory. This way of thinking was a revelation to me, and I was even more persuaded by later arguments that framed it as a variation on the original Lewinian adage that "there is nothing more theoretical than a good method" (Greenwald, 2012).

Among the outcomes of this understanding that theory can get in the way of research progress was my sensitivity to a particular sentence that would often accompany letters of rejection of the early papers from my lab. As is often the case with normal science in the Kuhnian sense, experiments simply built on previous work. These were, in my opinion, competent studies, well executed, and indeed the reviewers and editor could not easily find any obvious fault that should lead to rejection. Yet a common final sentence of the rejection letter would read, "However, in the opinion of the reviewers and myself, your work does not sufficiently advance theory." Really? Every paper, even if it's not a theory paper, must significantly advance theory? In what universe is this a mark of a healthy science rapidly developing bodies of reliable knowledge I asked myself. So, after some thought, I took to making this clear in my cover letters at submission time, especially to the *Journal of Personality and Social Psychology*, the most egregious offender of this theory cop-out. Believing that it would be easier on both parties, I would make an offer in the cover letter: "This paper is a report of a set of empirical discoveries. This paper does not offer any notable advance to theory at this time. If explicit theory advancement is likely to be the basis of rejection, it would be efficient (for the journal and us) not to send this paper out for review." It was a surprise to me that no editor ever desk-rejected a paper on those grounds; it always went out for review. I also believe, although I cannot be certain, that because I elicited from the editor this implicit commitment (thank you, Bob Cialdini), the paper was less likely to be rejected on the grounds of "no advance to theory."

It took our field a while to come to terms with this way of thinking, and it still hasn't been fully achieved by any means. I recall my spouse, an engineer and computer scientist by training, asking me in the 1980s, "Why is a publication in your field treated as a reward rather than a report?" I had not thought that was the case until we compared notes across our fields. But the situation is far better today than it was four decades ago. It is my belief that the improvement comes from our methods having gained in strength and research procedures that are not unique to a single lab but utilized by multiple labs. So, if you are entering the field in this moment, it is indeed a better time to be doing research, at least in this regard. To understand this, just skim the table of contents of this tome and ask how many of these chapters could have existed forty-five years ago when I studied research methods for the first time. I count that about 60 percent of the chapters in Part I could not have existed then, and even the rest are massively more advanced today. In Part II I judge that 100 percent of the chapters in this