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:

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 electro-chemical 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

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[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 eye-blink, 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

edition could not have existed in a methods handbook when I started graduate school, which, although a long time ago in the life of an individual, isn't that long in the life of a science. This is deep and fast progress and it makes the romance a worthwhile one.

I should say that the issue of the dominance of theory is not unique to our field. I learned that it was similarly experienced and discussed in other sciences, including physics. As just one example, the mathematical physicist Freeman Dyson (2015) points out that Thomas Kuhn's *The Structure of Scientific Revolutions* (1970), a book that every graduate student reads and relishes, did the disservice of elevating the status of theory above all else in science. And I would add that philosophers of science more generally have contributed to theory elevation, with philosophers of science like Karl Popper (1959) further reinforcing theory fixation. Their brilliant writing focused on concept-based revolutions leading to the misperception that those are the important moments in science. In fact, I recently stumbled across this ascription, wrong-headed in my view: "Methodological skills are needed to be an ordinary researcher, but theoretical skills are needed to be an extraordinary researcher!" (Bhattacherjee, 2012).

Instead, Dyson's view, and one that I'm sympathetic to, is that revolutions in methods (he says "tools") are more commonplace than revolutions in theory and that these method revolutions are more likely to produce both advances in science and salubrious effects on society. At the time he wrote, Dyson counted six major concept-based revolutions in the physical sciences (he includes Freud among them) to twenty tool-based revolutions (Dyson, 2015). In Dyson's (2000) book, *The Sun, the Genome, and the Internet*, examples of three obvious technologies are the focus – solar energy, genetic engineering, and worldwide connectivity. Each, he argues, has ushered in numerous scientific discoveries while also aiding to achieve a more just and equitable society.

Our methods and technology do include ones that are common to other sciences, most obviously the computer, and we do use advances made by other sciences for our own purposes – such as genetic analyses, brain imaging, or, recently as in my own work, machine learning algorithms to analyze large language corpora and chatbots. But methods don't need to cost hundreds of dollars per hour of subject time or require the beneficence of large corporations making available trained data sets. The humble methods of psychology – the simple experiment that introduces a manipulation and seeks to understand subjective experience by asking what people think and feel and why they think and feel the way they do – have the potential to inform and educate and change a society for the better. A good example appeared in the midst of the COVID pandemic when Van Bavel and colleagues (2020) published a review of existing knowledge on topics such as threat perception, emotion and risk taking, social norms, and inequality. Much of the research reviewed consisted of well-replicated studies about how human beings think and feel and behave in circumstances that had the features of a worldwide health disaster, with just one sure-fire path to saving lives (other than a vaccine) – a change in mental representation. What this paper's subtext revealed is that our field's intense focus on methods of research has allowed us to take the simplest thing, such as how a question is posed, and show that its formulation is not simple at all. Want to change people's mind about protecting the environment by skipping the need for fresh linen every day in a hotel? Just ask the question in the right, counterintuitive, way (Goldstein & Cialdini, 2008).

Let's go back to discoveries we were dwelling on, as basic as solar power and vaccines. The discovery of such technologies is not the work of our science. But the use of these technologies by humans who must accept them, for change to take place, falls to us to discover. We study what people know, believe, prefer, and value. We know

about the mechanisms of thought and feeling that allow humans to accept or reject technologies like solar power and vaccines. During this pandemic we did see a flourishing of irrational, superstitious beliefs about vaccines and even the simple wearing of a mask. More than a few people believed that vaccines came with the threat of the government or a corporation inserting a chip into one's body (the mechanism of how that would be done via a syringe was not fully thought through), or that masks interfered with God's natural breathing system. It was ultimately not the availability of masks or vaccines, but rather false beliefs about them, spread by social influence, that cost thousands more lives than necessary (Anderson, 2022).

I mention this only to signal the critical role that methods of research in PSP can play. Physical and biological technologies like solar power or vaccines are of little use unless mind and behavior co-operate. To this I'll add that solutions to these issues of social influence and persuasion are even more important in freer, more democratic societies where we believe that government should wisely and consciously keep itself at arm's length from the choices that individuals must make for themselves. In China, millions of people in a city will dutifully line up to take a vaccine when ordered; we have the harder job of persuasion at the level of individuals and communities with free access to social media.

Fortunately for you, the methods of research in this volume showcase, among other delights, the paradigms and procedures for studying false beliefs and resistance to persuasion, as well as dyadic relationships and small-group interaction, the interaction of genes and culture, the neural basis of thought and feeling, the beauty of the simple question that if posed well draws out the richness of subjective experience. Take the time to marvel at them for what they do because even with or without any electronic component these tools

can, in Dyson's terms, be the basis of leading us to a better world measured via any utilitarian metric such as the proportion of human pleasure versus suffering.

Among the positive advances I see today is greater interconnection across subfields within psychology and collaborations between psychology and other disciplines. These have grown considerably, and our work is now meaningfully getting integrated into the standard journals of other fields. My own last few papers have appeared in the proceedings of AI conferences whose names I had never heard of, and I do not believe I am unique. There are innumerable journals in which you can publish today, and I mean legitimate innumerable journals, not the predatory journals that flood your inbox with daily invitations, even demands, that you immediately submit any half-baked paper to them. The proliferation of even legitimate journals does have its challenges, but this expansion of outlets, including interdisciplinary journals, is a clear sign of advance – progress that can be traced to an increase in the number and diversity of research methods we have garnered and made our own. Yes, judging the quality of work without the convenient seal of approval of an established journal is harder, but it is overall a superior state of affairs that you can place your empirical work in more than three journals (which was roughly the number in social psychology, at least, when I entered graduate school). This is undeniable progress, even though today's generation must grapple with umpteen new challenges that surely accompany such growth, diversification, and interconnections.

By the time you get to reading these volumes, you're already hooked on scientific discovery. Whether you become an academic or not, you will have been transformed by your study of these research methods. Among what they will give you is the knowledge to chose the right tool

for your own research and the confidence that when you are confronted with a mess of data, you will know how to go about bringing order to it. That's significant power to have and to hold on to for an entire career. But I hope that you will see that, even more than this, the study of research methods gives you something more intrinsically important and best captured by the title of compiled writings of the physicist Richard Feynman, *The Pleasure of Finding Things Out*, first published in 1999. Studying these methods of research will bring you the many joys of finding things out.

I would like to end on a personal note, a more detailed description of which is available elsewhere (Banaji, 2022). I didn't enter graduate school in social psychology with any background in that field. I had taken some psychology before I arrived from India to attend graduate school at Ohio State, but it was mostly psychophysics and learning theory. I had never stepped foot in anything resembling a research lab, although I had done one experiment on family members using a memory drum! My chance encounter with social psychology came when I bought five volumes of the *Handbook of Social Psychology* (1968), edited by Gardner Lindzey and Elliot Aronson, at a railway station in India, having bargained down the price to a dollar a volume. On the long train ride from school in New Delhi to my home in Secunderabad, I read the first volume. But what I was able to grasp was just one chapter (the rest were on theories that made no sense to me whatsoever). The chapter that held my complete attention and amazement was by Elliot Aronson, and it was the chapter on research methods in social psychology. The descriptions of methods, the dramas that made up the "experiments," were fascinating. They were mind-blowing. I could not believe that there existed, somewhere in the world, far from my own, a tribe of people who had the nerve to do *this* – to measure these most complex mental and

social processes under controlled conditions that I had assumed were reserved for the study of the physical world. When I got off that train, I knew with a rare conviction what I had to do, even though the chances of success were slim. A chapter on research methods is the reason I became a social psychologist (Elliot knows), and if that's not the romance of a lifetime, I don't know what is.

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