

CHAPTER ONE

Scratch an Itch with a Brick
Why We Do Research

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What do mosquitoes, bricks, and research have in common? Both mosquitoes and research motivate us by bugging us, and both bricks and research build things. But bricks and mosquitoes? Let’s see. . . .

Start with the first pair, mosquitoes and research: Both make us itch. Most relevant here, we do research to scratch a mental itch. This is not trivial. Research is challenging; indeed, some would say that personality and social psychology are the really hard sciences, so this handbook provides guidance in doing them right and managing the setbacks. With so much grief (data can be so uncooperative, and reviewers almost always are), you have to have a real itch to do the science, to persist. If research is so tough, we as scientists have to be compelled, have to really want to do it. This chapter explores why and how we bother, brick by brick. So in effect, we are scratching the research itch with a research brick.

When researchers explain how they got involved with particular lifelong projects, they usually answer with some version of, “What really bugged me was this. . . .” Gaps, mysteries, and inconsistencies all drive regular people as much as researchers. Witness the popularity of mystery series, Sudoku puzzles, and suspense genres. People are wired to detect discrepancies and want to resolve them. One prime way to start a program of research is precisely to mind the cognitive gap. That is, scientists especially notice theoretical discrepancies, empirical inconsistencies, missing links in evidence, counterintuitive patterns, and all manner of knowledge that just does not fit (Fiske, 2004a). Noticing discrepancies could be indexed by the still, small buzz at the back of the mind, which interrupts the flow of reading, listening, watching, and synthesizing science. Focusing on the discrepancies is the first step to noticing an unsolved problem. If the discrepancy

matters to scientists (for reasons we explore next), they itch to resolve it. And we scratch it by building science, laying the bricks.

This chapter argues that we do research partly to represent our own new perspective on what’s missing and what needs to be done. We do this gap-filling empirically, not just theoretically, because we are a science that does not separate theory and research as much as, for example, theoretical and applied physics or economics. Hence, social and personality scientists mostly do not entertain theoretical contributions without empirical evidence; we are not satisfied until we do the research. As we will see, another separate and not as noble, but very human, motivation for research is that, for those in the field, research is pragmatic in several respects, as people forward their careers. But the most important reasons are intellectual and scientific, so the chapter turns to those first.

REPRESENT NEW PERSPECTIVES

Researchers make discoveries; we create new knowledge. What we bring to our work is our own unique perspective, whether intellectual, personal, identity-based, or even ideological. Some are more conventional sources of science than others, but all form parts of the picture; let’s examine each in turn.

Intellectual Puzzles

If science starts with an itch, a discrepancy, or a discontent, we build or use a theory to test explanations. We may detect gaps in existing theory, and this is the platonic ideal for science, as many chapters in this volume illustrate.

Alternatively, researchers may pit two theories against each other, sometimes supporting one to the exclusion of the other, but more often determining the conditions under which each is true. For example, in close relationships research, one might pit attachment theories (Shaver & Mikulincer, 2010) against interdependence theories (Rusbult, Drigotas, & Verette, 1994), but in fact both can operate simultaneously, one at an individual-difference level and the other at a situational level. Still, to the extent that two theories make distinct predictions, the suspense often captures a researcher's (and a reader's) imagination.

Some researchers commit to a meta-perspective, such as evolutionary or functional explanations, and apply them to the problem at hand, building support for that perspective. For example, an evolutionary approach might argue that people mistrust outgroups because it has often been adaptive to stick with your own kind (Neuberg & Cottrell, 2008), and specific research questions follow from these principles.

Another intellectual strategy borrows a neighboring field's theories and methods, applying them to social and personality phenomena. For example, social cognition research originally began by applying nonsocial models of attention, memory, and inference to social settings, discovering where common principles did and did not apply (see Fiske & Taylor, 2013 for more specific examples). For instance, attention is captured by novel social stimuli, just as by novel nonsocial stimuli (Taylor & Fiske, 1978; McArthur & Post, 1977). However, attention is also captured by information about another's intention (Jones & Davis, 1965), so uniquely social principles sometimes apply to other people, versus things, as objects of perception. So, borrowing from an adjoining field can illuminate what is unique about personality and social approaches.

Still another intellectual strategy of research ideas is going back in time to the earliest psychological writings. Some reread Aristotle (e.g., regarding social animals; Aronson, 2004); some like the French National Archives (e.g., regarding emotion theory; Zajonc, 1985). Myself, I like William James (Fiske, 1992).

Scientists also construct theories from scratch, sometimes going from the top down with a metaphor that seems to capture an important reality, such as depicting willpower as a muscle that can get fatigued (Baumeister & Alquist, 2009). Sometimes theories follow from the bottom up, beginning with data, where a systematic program of research consistently yields

particular patterns that demand a systematic explanation. For example, neural responses to face perception suggest that trustworthiness is the first and primary dimension that emerges, and theory then describes why that might be the case (Oosterhof & Todorov, 2008). All these then are intellectual motivators of research.

Personal Experiences

We don't often admit this outside the family, but psychological scientists do often get ideas from personal experience. We are after all part of our own subject matter. Informal sources of formal theory are legitimate, as long as the informal insights are then stated in a systematic and testable form (Fiske, 2004b). Not all theory has to be expressed in mathematical form – indeed, in social and personality psychology, most is not – but it does have to be logical, parsimonious, and falsifiable, unlike common sense. That is, even theory that derives from personal experience has to be accountable to empirical tests.

Being keenly interested in human behavior gives us an advantage in drawing ideas from experience. As trained social observers, we notice behavioral patterns that others miss. Indeed, McGuire (1973) exhorted graduate students to observe the real, not just what others have said or what the sanitized data say.

Within this approach, the trick is, as Lee Ross puts it, to “run the anecdote” (personal communication, October 12, 2011). If a story, a hunch, or even fiction seems to capture an important human truth, social and personality psychologists can design studies that simulate that phenomenon, to see if it survives the transition from imagination to a reality that replicates reliably. This volume provides instructions for how to do exactly that.

One caveat: New investigators sometimes fall into the trap of doing me-search – that is, studying their own thorny psychological issues, their own in-group's preoccupations, or some intense idiosyncratic experience. The problem here is that, although highly motivated, one may not be the most objective judge of an issue that is too close to home. At worst, one may be too invested in a certain result, and equally bad, one might have no insight at all. At a minimum, the motivational biases we investigate might also bias our interpretation of our results (Kahneman, 2011). At best, one has some relevant insights and an open mind about whether these testable ideas produce interpretable data. Only then is one really ready to

learn something scientifically new and reliable, as a result of personal experience.

Group Identities

Many of us go into social psychology because it focuses on the variance explained by situations, and situations can be changed, to benefit people’s well-being. If you think a social problem is caused by context, that is potentially a social policy issue, but if you think the social problem has genetic causes, that does not lend itself to easy societal solutions. One important social issue in today’s multicultural, globalizing world is intergroup relations – by the author’s estimates from conference talk titles, representing the preoccupation of about a quarter to a third of social psychology. As our field itself becomes more heterogeneous, more of us are thinking about various phenomena related to ethnic, racial, cultural, gender, sexual, age, disability, and other diverse identities.

On the principle of “nothing about us without us,” many of the researchers studying these issues come from the affected groups. This presents both opportunities and challenges. The opportunities come in our field’s chance finally to represent the underrepresented. Prejudice research, for example, has gone from merely studying the perpetrators to studying the targets, and target-perpetrator interaction (e.g., Richeson & Shelton, 2007), enriching the science of intergroup interaction, as well as the broader field, with new more widely applicable insights and methods.

The group-identity research faces challenges parallel to the me-search challenges, in what might be viewed as we-search. Besides the perils of lacking objectivity, one is also accountable to a larger identity group, whom one certainly does not wish to alienate with findings that might cast the group in a poor light. This issue arises even more for outsiders studying issues relevant to traditionally oppressed groups, for example, men studying gender and white people studying black experience. Ultimately, membership is not required to conduct good group-related science, but insights do derive from lived experience, and collaboration is one solution to keeping identity-relevant research both sensitive to politics and respectful to lived experience. However, even in these cross-identity collaborations, one must consider whether foregrounding one colleague gains credibility with one audience (e.g., subordinates), and foregrounding the other gains credibility with another audience (e.g., dominants). Peter Glick and I considered this issue in our ambivalent sexism research (e.g., Glick & Fiske,

1996), deciding for this reason, among others (including who ultimately did more work), to foreground the male member of our collaborative team.

Worldview Defense

Even more fraught but also honestly inspiring is research conducted to examine one’s own worldview, whether religious, political, or moral. But ideology and science make uncomfortable bedfellows, so this is an enterprise to enter only with both eyes wide open. One has to go into it with the goal of testing cherished assumptions and being willing to find them wanting. For example, liberals and conservatives emphasize distinct moral bases of judgment (Haidt, 2007), and the role of each may unsettle both ends of the spectrum. The inquiry is permissible if one agrees to play by the rules of science. Fortunately, reviewers and editors keep us honest, with no axe-grinding permitted in the ideal case.

Comment

Sources of ideas are as varied as scientists, and we can cluster these sources in various ways. For example, in a classic exhortation to the field, McGuire (1973) listed creative sources as including: paradoxical incident, analogy, hypothetico-deductive method, functional analysis, rules of thumb, conflicting results, accounting for exceptions, and straightening out complex relationships. I do not disagree, and the interested reader is referred to that earlier account.

WHY RUN THE STUDY?

All these sources of inspiration are good, but why do research and not just theory? In our field, other scientists will mostly ignore your armchair ideas unless you arrive with evidence in hand. We are trained to be skeptics because ideas are easy; evidence is harder, so it is more precious. What is more, this is science, and when we joined up, we agreed to adhere to the epistemological rule-book. But we also do the studies because research is fun. Let’s have a closer look at these motivations to walk the talk, going beyond ideas to research.

Because This Is Science

We do the research because this is science, not theater, law, or car repair. Our rules of evidence appear throughout this volume. When we join a graduate

program, we sign on to the scientific norms current in the community of scholars. Reliable evidence that meets shared standards is the coin of the realm.

Social and behavioral sciences might just be, as noted, the truly “hard” sciences, for a variety of reasons. First is measurement: Human reactions are difficult to record because most depend on human observers, whether self-reports on Likert scales or coders of nonverbal behavior, and humans are notoriously unreliable (D. W. Fiske, 1971). As observers of self and other, people are both biased (e.g., prefer to accentuate the positive) and prone to random error (e.g., variable over time, place, modality). Granted, we can use measurements that avoid the human reporter (e.g., reaction time, physiological measures), but these still entail a human judge. Even astronomy recognizes the “personal equation” in observing heavenly bodies (Schaffer, 1988), as apart from human ones. But the celestial stars ultimately submit to more exact measures than the human ones, so finding results in our science – despite the bias, despite the error – is really hard.

Science is all about discovery. Face it, we’re geeks; we like making measures, analyzing data, learning stuff. All this is a quest for truth and maybe even wisdom (Brickman, 1980).

Hitting the Sweet Spot Is Fun

The most exciting science finds phenomena of important everyday interest but connects to old problems for social and personality psychology, which allows well-grounded theory, not just flash-in-the-pan findings popular today but gone tomorrow. Hitting the sweet spot that includes both everyday interest and scientific advance is tricky but fun.

Some advice comes from Stanley Schachter, who reportedly urged his students to craft subtle, seemingly small independent variables that create large, undeniable effects on important dependent variables. For example, handing people a hot rather than iced coffee makes them more generous to strangers (Williams & Bargh, 2008); the warmth variable dates back to early childhood experiences of comfort and safety close to caretakers. What a nifty finding. As another example, making people think about professors makes them better at Trivial Pursuit (Dijksterhuis & van Knippenberg, 1998). This uses everyday materials to make an original point about the power of priming (Bargh & Chartrand, Chapter 13 in this volume).

To hit the sweet spot, another social psychologist, Robert Abelson, counseled young researchers to

capture the spirit of the times but before others notice it. Watching trends to get ahead of the curve allows a researcher to anticipate what the field will find interesting next. One does not want to jump on the bandwagon, but rather to drive it, or better yet, to design it. Creating clever, realistic, innovative procedures, which also meet all the criteria of methodological rigor and theory relevance, is indeed fun and motivating. Hitting the sweet spot should make you feel like shouting, “Woo-hoo!”

Solving the Puzzle Is Satisfying

Just as we are bugged by discrepancies and gaps, we like cognitive closure, especially when we have to think a bit to get there. Solving the puzzle is satisfying. Indeed, George Mandler’s (1982) theory of aesthetic pleasure posits that people most prefer small discrepancies easily resolved. Musical themes and variations do this. Crossword puzzles do this, if one hits the right level of difficulty. It follows the Goldilocks principle: Not too hard to resolve, not too easy, but moderately challenging seems to work best. One can recognize the right level of difficulty when one notices that time has passed without one being aware of it. Becoming optimally absorbed in the process of puzzle-solving creates the feeling of “flow,” which combines both challenge and skill, resulting in total involvement and complete concentration (Csikszentmihalyi & LeFevre, 1989). This happens more often at work than at leisure, and it makes many of us feel lucky to be paid for what we enjoy most.

Our contributions to the field also are satisfying because they fit previous work, making notable progress, adding to human knowledge, a brick at a time. Both resolving discrepancies and filling the gaps create the “Aha!” experience that keeps problem-solvers going.

Being Right Is Fun

Besides the “woo-hoo” and “aha” experiences, many scientists relish the “gotcha” moment, when they are right about a contested issue. Fun as it is to win a competition, scientists must absolutely fight fair – that is, hurling data, not insults. We all agree to abide by publicly replicable results, although of course interpreting them can remain contentious. In general, in my opinion, picking on other people’s results does not usually make the most impact, especially if it is nitpicking. Sometimes, of course, identifying a confounding issue in the established paradigm can release

a flood of useful research. Today’s methodological side effect can be tomorrow’s main effect of interest. This can create cumulative science.

Choosing and framing are essential here. Choose battles carefully: Is the end-result of winning worth making enemies? And if people are challenging your data, try to be a good sport. We are obliged to share our data and any unpublished details of our methods; we must strive to view the challenge as advancing science, to respond vigorously but respectfully to the challengers, who may just improve your work. Keep in mind that they would not be pursuing your findings if they did not consider them important.

If your view ultimately prevails, do not gloat. Apply all the rules of being a good competitive player who respects the other team. These are your colleagues for life, after all. Still, we cannot help rooting for our favored interpretation.

Telling Good Stories Is Entertaining

Good storytellers attract an audience, and our studies are our stories, as witnessed by the popularity of our field with science reporters, best-selling authors, and media moguls. Social and personality psychology can be entertaining, as when our research creates nuggets to share. Although a good science story may sometimes enliven dinner conversation, reciting factoids is probably not a good pick-up strategy. (One might earn the nickname PsycInfo®.) But with a light touch, and presented to the right audience, one might also hear an admiring “Wow!”

Promoting Evidence Is Important

Society needs science. As Daniel Patrick Moynihan reportedly said, “Everyone’s entitled to [his] own opinion, not [his] own facts.” Science can inform policy, and if taxpayers foot the bill for our science, we owe them some facts.

What is more, many of us went into the field to try to improve the human condition. We want to identify principles and possibly specific interventions that enhance people’s lives. The current federal emphasis on translational research reflects this priority. Our science can improve – or at least inform – social policy. And this too is satisfying.

SIDESHOWS: PRAGMATIC REASONS FOR RESEARCH

“Is this too idealistic?” you might shrug. We do not just do research because it is exciting, useful, and fun,

but also because we have committed to it as a career. Let’s acknowledge some practical motivations.

Publish or Perish

We do research partly to get a job. Even if we are hired to teach certain classes, covering certain areas, we are promoted for research published in refereed journals, preferably high-impact ones. Quality, not just quantity, counts here. For example, many tenure, promotion, and award committees consult the h-index (Harzing, 2007), which calculates an author’s number of citations relative to the number of total publications, thereby balancing quality and quantity. Journals can also be evaluated this way, to calculate their impact factor, although many journals now use sheer number of downloads, as well as citations, to gauge their status. These indices all tend to converge, which is reassuring for measurement reliability and validity.

Collaborate

Some of the more people-oriented among us do research partly for the rewards of collaboration. When we team up to do science, synergy arrives, joy happens, and companionship shares the inevitable tribulations of the research enterprise. In my humble opinion, cooperation is conducive to good science.

From these teams, we develop networks to connect for friendship and consultation through a career’s lifetime. Interdisciplinary collaborative research in particular often creates the leading edge in science; ideas catch fire when fields rub up against each other, creating the future networks of our sciences. The more social and behavioral scientists learn about the strength of weak ties and the importance of support systems, the more we should seek these linkages in our professional lives. Joint research is one way to do this.

Get Rich (or at Least Get Funding)

Researchers have many intrinsic reasons to seek research funding, not least because it enables them to get their work done. Many schools also emphasize funding as a criterion for promotion because national panels of colleagues have endorsed your research plans. On the pragmatic side, one must have done research to get funding to do research; that is, one must establish a track record. This prior research not only adds credibility, but it also organizes the next steps.

An underappreciated aspect of grant writing is that, even if unfunded, grant proposals organize research. Spending thoughtful effort on a program of research helps one prioritize and manage the ensuing studies, even in the midst of a busy, distracted semester, when the big-picture perspective tends to recede.

Teach

We also do research, among other pragmatic reasons, to inform and motivate our teaching. Contrary to popular belief, teaching and research complement each other. In teaching ratings, research productivity correlates with the professor’s rated knowledge, commitment, enthusiasm, and organization (Feldman, 1987). Admittedly, research does not correlate with rated time spent on teaching, there being only so many hours in a day. But students are evidently energized by a teacher who researches.

Serve

Research has unexpected links to service, as well. Our universities want to be famous for the research we do, because quality attracts quality and excitement is contagious, promoting our institutions, who after all write the paychecks. Sometimes we do research to serve populations we cherish (see politics, earlier in the chapter). Sometimes we do research to serve moral causes (also see earlier discussion) or to promote the general health and well-being of humanity.

Be Zen

Researchers rarely consider themselves to be on a spiritual quest, but an often salutary side effect of doing research is being humbled. Data prove us wrong. Students intimidate us with their creativity and hard work. Reviewers deceive us about the quality of our papers. Peers scoop us and do a better job. The field takes our ideas beyond even our own delusional hopes. Or more often, the field ignores our best ideas (Arkin, 2011). We do not go into research to have these humbling experiences, but they have their own advantages, as just noted.

Most of all, we do research to follow our bliss. We do it because we love it.

CONCLUSION: WHY WE DO RESEARCH

Research enables us to represent new perspectives, to test our ideas and make them empirically accountable.

Along the way, we also discover pragmatic reasons for doing research. The process and our investment in it are knowable and manageable. Besides, we can scratch an itch by laying a brick. Read on, and catch the urge.

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