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Research

Minimally Sufficient Research

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ABSTRACT—*Psychology would be improved if researchers stopped using complicated designs, procedures, and statistical analyses for the sole reason that they are able to do so. The present article reviews some of the classic studies in psychology, all of which are breathtakingly simple. The notion of minimally sufficient research is suggested as an ideal worth following. More generally, questions should dictate research methods and statistical analyses, not vice versa.*

Less is more.
—Robert Browning (1855), *Andrea del Sarto* [The Faultless Painter]

The 1999 report by the American Psychological Association's Task Force on Statistical Inference contains an important recommendation regarding the use of statistics under the heading "Minimally Sufficient Analysis":

> The enormous variety of modern quantitative methods leaves researchers with the nontrivial task of matching analysis and design to the research question. Although complex designs and state-of-the-art methods are sometimes necessary to address research questions effectively, simpler classical approaches often can provide elegant and sufficient answers to important questions. Do not choose an analytic method to impress your readers or to deflect criticism. If the assumptions and strength of a simpler method are reasonable for your data and research problem, use it. Occam's razor applies to methods as well as to theories.
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> (Wilkinson & The Task Force on Statistical Inference, 1999, p. 598)

I ask all of my students to read this paper, and I call their attention in particular to the principle of a minimally sufficient analysis. But I also warn them that this principle, as reasonable as it is, often goes against the grain of current research practice. They should follow it at their own peril.

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We all have our own stories about an editorial process that led us on a tortured path through highly complex statistical analyses, back to the same point at which we would have arrived had we done a minimally sufficient analysis. My own story concerns a simple cross-sectional study that found support for the contention that A influenced C through its effect on B. My original analyses showed that the product-moment correlation between A and C was significant, and that it shrank essentially to zero when B was held constant in a partial correlation. But the reviewers were not happy and suggested that structural equation modeling (SEM) was a better way to make the point, never mind that A, B, and C were unifactorial constructs measured with excellent (and comparable) internal consistency.

I am an agreeable sort, so I set about to make this revision. Because I did not know how to do SEM, I enrolled in a lengthy faculty workshop on the topic, bought the relevant software, installed it on my computer, learned how to use it, and did the suggested analysis. This took 6 months of my time, not to mention numerous consultations with our local SEM expert. What I learned (drum roll) was that A influenced C through its effect on B. The paper was duly published (Peterson & Vaidya, 2001), and it has been cited essentially not at all except by me. I do not blame the use of SEM for the paper's lack of impact, but I do conclude that SEM did not turn this particular sow's ear into a silk purse, which illustrates the thesis of the present article.

I also confess that I was a bit proud of myself because I had added a new tool to my statistical repertoire, and perhaps that provides some insight about why maximally sufficient analyses are so popular. We conduct them because we can. Never mind that they may be unnecessary or, indeed, that they may even get in the way of other psychologists understanding what we did, if the maximal tool happens not to be in their repertoire.

Mind you, I am not a Luddite, and I am happy to agree that there are good and appropriate uses for SEM and other advanced statistics. I am certainly happy that SPSS-PC exists and that the analyses that I once did by hand can now be done by pointing and clicking. I have used the Internet to gather data, and I do not rule out my eventual use of fMRI if it will shed light on a theoretical matter of interest. In short, most of me as a researcher lives in the 21st century. But like Wilkinson and The Task Force on Statistical Inference (1999), I believe that our questions should dictate our methods, including how we analyze and

PERSPECTIVES on Psychological Science

report research findings as well as how we design studies in the first place. We should not automatically do the most complicated study imaginable and then use the most exotic statistical analyses available, simply because we can. We need scientific reasons to do so, and those are to be found in our theories, not in our methodological toolboxes.

The most important studies in psychology are breathtakingly simple. They are also really interesting, which is why they become and stay important. They are not important because they had complex designs. They are not important because they used maximally sufficient analyses. They are not important because they were reported in a 50+ page article or because they used the multiple-study format that has become the state of the art in premier outlets like the *Journal of Personality and Social Psychology*. They are important because they are interesting, and because methodological and statistical nonsense did not obscure the theoretical points they made. Studies are important when they show other researchers what is possible and how to do it, not because they make research daunting. In short, an important study exemplifies what I call minimally sufficient research.

Let me be concrete with some examples of important studies and how they embody the simplicity I am extolling. Most of us who are teachers are familiar with the ancillary text *Forty Studies That Changed Psychology* (Hock, 2006). The studies sketched in this book range across the different fields of psychology and were chosen because of their high impact as judged by their ongoing coverage in introductory psychology textbooks. There are other ways to identify the most important studies in psychology—like brute-force citations counts—but none would argue that the studies in this book are among the most important ever done in our field.

The common thread is their simplicity, in design and statistical analysis. Indeed, some were case studies and used no inferential statistics whatsoever (e.g., Freud's studies of patients with hysteria, Watson and Rayner's conditioning of Little Albert, LaPiere's traveling investigation of attitudes and actions, Skinner's superstitious pigeons, Harlow's forlorn monkeys, and Rosenhan's multiple case study of being sane in insane places). Even the original Milgram study of obedience in effect was a case study, inasmuch as he did not assign research participants to different conditions. (Of course, Milgam, 1974, conducted subsequent studies that were true experiments, systematically varying the parameters of his obedience paradigm, but it was the original demonstration of obedience that has had such a huge impact on psychology.)

Other studies of note were experiments, but always very simple ones with one or a few dependent variables: Asch's inquiry into conformity; Calhoun's study of the effects of crowding among rats; Festinger and Carlsmith's laboratory test of cognitive dissonance theory; Bandura, Ross, and Ross's Bobo doll study of modeling; Wolpe's investigation of systematic desensitization as a treatment of fears; Seligman and Maier's demonstration of learned helplessness in dogs; Langer and Rodin's field experiment with nursing home residents; Latané and Darley's investigation of unresponsive bystanders; Rosenthal and Jacobson's study of teacher expectations; and so on. Results of these experiments were typically analyzed with one-way analyses of variance (ANOVA) and pairwise comparisons. In each case, we have a minimally sufficient design and a minimally sufficient analysis.

Other high-impact studies used a correlational design—like Friedman and Rosenman's study of the Type A coronary-prone behavior pattern and Holmes and Rahe's study of stressful life events and disease. Results were analyzed with simple measures of association.

Still other high-impact studies introduced a theoretically driven method or measure, like Piaget's *méthode clinique*, Morgan and Murray's Thematic Apperception Test, Rorschach's inkblots, Kohlberg's moral dilemmas, Rotter's locus of control measure, and Bem's self-report androgyny scale. What is important about these methods and measures is that they were simple enough for other researchers to use and interesting enough that other researchers wanted to use them and obviously did. In many cases, these methods and measures have been criticized by subsequent researchers, but their importance remains, and the new and improved versions of these strategies are unmistakable descendents of the originals.

The Smith and Glass meta-analysis of psychotherapy outcome studies deserves special mention because it introduced to psychologists a new statistical method for aggregating research results. Does this extremely high-impact study contradict my premise that researchers should keep it simple? I think not, because the statistical technique was introduced without fanfare and explained in lucid fashion. The question of interest to Smith and Glass demanded a new way of looking at data, and their version of meta-analysis followed. And please note that their paper was only nine pages long! Would their study be published today? Would any of the papers mentioned here be published today? These are rhetorical questions without real answers, but they are still worth considering if we are trying to improve the field of psychology.

The skeptic might object that of course these classic studies in psychology embody minimally sufficient research because that was all that existed decades ago. If Piaget, Festinger, or Milgram had SEM computer programs or fMRI laboratories available to them, they might have used these strategies, and some of my arguments here would be specious. I have no definitive rebuttal, although I disagree. But my other arguments remain valid. Our questions should dictate our methods and analyses, not vice versa. We should not do research in a particular way simply because we can. We should not use a statistical technique simply because we can understand the software. We should not study introductory psychology students simply because we can. Surely, the reader noticed the diversity of the samples used in the high impact studies in our field.

Appreciative inquiry is an organizational change strategy that tells group members to examine what they do well and to then do more of it (Cooperrider & Srivastva, 1987). In the present case,

the group is psychologists, and what we do is research. The lesson of history is that what we do well is often very stark—less is more—and if we were to do less (and think more), psychology would be improved.

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