

Collaborations in Psychology: What Works, What Doesn't:

Review of

Collaboration in Psychological Science: Behind the Sciences

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Many years ago, when I was still an undergraduate, I conducted a research project on an item called "data sufficiency" from the Mathematical portion of the SAT. The item presented a mathematical problem with two pieces of data, Statement 1 and Statement 2. One was then told to indicate whether:

- (A) Statement (1) ALONE is sufficient, but statement (2) alone is not sufficient.
- (B) Statement (2) ALONE is sufficient, but statement (1) alone is not sufficient.
- (C) BOTH statements TOGETHER are sufficient, but NEITHER statement ALONE is sufficient.
- (D) EACH statement ALONE is sufficient.
- (E) Statements (1) and (2) TOGETHER are NOT sufficient.

(from <http://www.mba.com/us/the-gmat-exam/gmat-exam-format-timing/quantitative/sample-data-sufficiency-question.aspx>)

Here is a simplified example:

What was the cost *per apple* of the 8 apples Jessie bought?

1. Jessie handed the clerk \$10.
2. Jessie received \$2 in change.

The correct answer is C. (The apples cost \$1 apiece, which one can figure out by combining the pieces of information in 1 and 2.)

I was interested in whether part scoring would increase the validity of the test. The answer was “no” and that was the end of that project. I therefore am gratified that, 45 years later, I finally can do something with the data-sufficiency item, although not what I had hoped to do 45 years ago.

What one learns from *Collaboration in Psychological Science* is that when it comes to collaboration, the best (although certainly not only) answer, substituting “collaborator” for “statement,” is “C.” If the answer is either A or B, one of the collaborators does not really need a collaboration and may end up feeling like the other collaborator is useless or even a social loafer. The one who feels that he or she alone could produce the work may even end up resenting the other. The problem with answer D is that neither collaborator needs the other, so the collaboration does not, in the words of Hatfield and Berscheid, end up with 1+1 being any more than 2, and perhaps it will be less. If the answer is E, the collaborators are likely to be frustrated, as even their collaboration is insufficient to produce a viable product. So one hopes for C, because in this case the collaborators together produce a piece of work neither could have produced on his or her own.

The book is in many respects an elaboration on answer C, detailing to many ways in which collaborators enriched not only each other’s research, but also each other’s lives. Many of the collaborators not only produced work together, but also became lifelong friends.

The book is divided into five parts: (I) an introduction: behind the scenes, (II) collaboration with psychological science, (III) collaboration and interdisciplinarity, (IV)

collaboration with institutional and community partners, and (V) conclusion: best practices in collaboration in psychological science. In all, there are 21 individual chapters, plus the first and last parts, which are not assigned chapter numbers. The book also has a foreword by Peter Salovey, a distinguished social psychologist and president of Yale University.

The book presents a taxonomy of collaborations in terms of individuals within and outside psychological science, and individuals versus organizations of various kinds. However, there is another kind of taxonomy of collaborations between and among individuals that might be relevant, at least from a social-psychological point of view.

A 2x2 taxonomy, which I am proposing for the first time here, is between short-term and long-term collaborations, on the one hand, and between people of equal and of unequal status, on the other (see Table 1). The challenges are different for the different kinds of collaborations, as I have found in my own experiences with the four kinds of collaborations.

Type 1 collaborations: Short-term collaborations between investigators of equal status. The typical short-term collaboration between people of equal status is when a team of individuals, sometimes from the same institution but other times from different ones, is formed to address a specific scientific problem or set of problems. The collaborators could be within a network or a project grant, where the people are brought together for a specific purpose, often because they have different forms of expertise. Such collaborations are becoming increasingly common because of the recent tendency of funding agencies to fund multidisciplinary collaborative ventures with co-PIs having differing kinds of expertise. Such collaborations are not represented to any great extent in the book, but are increasingly important in science. Unlike the collaborations represented in the book, the individuals often have somewhat minimal prior

interactions and are getting together for a particular purpose. The grants under which they work are often only for a few years, so the collaborators face considerable challenges in learning to work together and to blend their various forms of expertise. Perhaps another book at a future time will discuss the increasingly common challenges of such collaborations.

I once was involved in a very short-term collaboration with Dr. Kenneth Kidd, a prominent geneticist and, like myself at the time, a full professor at Yale (Sternberg, Grigorenko, & Kidd, 2005). I had never worked with Kidd before and would never work with him again, except on a derivative article. I was interested in the question of socially-defined race and intelligence, and neither my longer term collaborator (Grigorenko) nor I had sufficient familiarity with the topic to be able to do the paper ourselves. The collaboration with Kidd produced what I thought at the time and still think was an excellent paper debunking many myths about race and intelligence, such as that racial differences in IQ can be accounted for largely by genetic factors. It would not have been possible without this short-term collaboration with a professor of roughly comparable status as a full professor.

Type 2 collaborations: Long-term collaborations between investigators of equal status. Many of the collaborations discussed in the book are of the second kind—investigators of roughly equal status over a long term. Indeed, much of the book is devoted to a discussion of such collaborations. Sometimes collaborations that start off short-term become long-term, but the large majority never do. This kind of collaboration is discussed extensively in the book. I have little to add beyond what is in the book.

I have had a very long-term collaboration with Li-fang Zhang (e.g., Zhang & Sternberg, 1998), who although she began working with me when she was just starting out as a faculty

member, has been a full professor for quite a while during our collaboration. Early in my career I was very interested in doing research on thinking styles (e.g., Sternberg, 1988), but as the years moved on, my interests moved also moved on. Zhang, however, started working with me on my theory or styles and maintained an active interest in styles as an area of empirical research. Through this collaboration, I was able to continue to work in this area because she pushed things along, in a way I never would have had I not collaborated with her.

Type 3 collaborations: Short-term collaborations between investigators of unequal status. The most typical collaborations of these kinds are between teachers and students, or occasionally between senior and junior faculty members. There are some such collaborations discussed in the book, such as between Shelley Taylor and Susan Fiske and between Richard Zweigenhaft and Bill Domhoff. But much more could be said about such collaborations (see, for more information, Sternberg, 2016).

I have had scores of collaborations of this type. Indeed, my career would be nowhere close to wherever it is, were it not for the many student collaborators I have had over the years. Like my adviser, Gordon Bower, I often let my students lead me. For example, I was not interested in learning disabilities, but one of my students, Louise Spear-Swerling, decided that she wished to devote much of her career studying learning disabilities. As a result, we did a number of joint papers (e.g., Spear-Swerling & Sternberg, 1994) in an area in which I never would have done any more than dabble had Spear-Swerling, when she was my graduate student, not led me to do serious scholarship in the area.

Type 4 collaborations: Long-term collaborations between investigators of unequal status. Such collaborations are common in the hard sciences, where a professor may have one

or more research scientists working in his or her lab. The research scientist is an individual who, for whatever reason, has not entered the tenure track but rather is supported on grants, usually with the professor as principal investigator. Such collaborations are challenging to the junior person because, no matter what he or she contributes to the research over a period of years, the research scientist stays in a lower status position relative to the professor (unless he or she enters the tenure track). These collaborations can go on for entire careers. They are not well represented in this book.

I perhaps have not had any lifetime Type 4 collaborations, but one of my best collaborators in my career was Elena Grigorenko (e.g., Sternberg et al., 2001), who for many years was a research scientist in my lab group when I was a professor at Yale. (She went on to become a faculty member at Yale and then the holder of an endowed chair at the University of Houston.) During the years she was a research scientist, we published scores of papers together, often with other coauthors. Much of the work we did overseas was possible only because she was willing to do so much of the traveling! Another great collaboration was with Janet Davidson, who started off as a research associate and later became a graduate student (e.g., Davidson & Sternberg, 2003; Sternberg & Davidson, 2003). The collaborations were invaluable to me and, I would hope, to them as well.

All of the chapters in this book are interesting, something one can say of few edited books. There was one remark in Steven Sherman's chapter that particularly impressed me, however. In academia, he notes, when candidates are hired and promoted, emphasis is placed on the candidates' having a "program" of research. Sherman defies the *Zeitgeist* by asking why we are so hooked on programmatic research. He points out how some of his own best

contributions have been non-programmatic. It occurred to me that the same would apply to my own research. One of my most cited papers, one on love (Sternberg, 1986), is, I believe, one of my best contributions to the field, but I never formed a full-fledged program of research on love. Should we discourage scholars from doing one-offs or two-offs or three-offs that, despite being non-programmatic, may nevertheless be important contributions? I think Sherman is on to something here.

The book has so many positive features it is hard to list them all. Here are five:

1. Most of the names are highly recognizable individuals of great stature in the field of social psychology. The book provides an opportunity to peer into the minds and the interactions of some of the greatest living social psychologists, including but certainly not limited to Shelley Taylor and Susan Fiske, Elaine Hatfield and Ellen Berscheid, Richard Nisbett and Lee Ross, and Hazel Markus and Shinobu Kitayama. (This is just a sampling of distinguished contributors: I just do not have space them all!)
2. One can learn about what has worked well in particular collaborations and what has not worked as well. For example, some collaborators have had to adjust to different paces of research work, different styles of research, and distances between collaborators that would have discouraged all but the boldest.
3. The book can be read at the beach (or the ski lodge). The tone is informal, inviting the reader in and providing any reader with insights he or she never would attain even if the reader were able to read all of the collaborators' previously published work.

4. The book reviews different kinds of collaborations, including collaboration within psychological science, interdisciplinary collaboration, and so forth.
5. The introduction and conclusion are worth the price of the book alone, providing deep insights into the nature of collaboration in psychological science.

The reader should be aware of three possibly limiting features of the books.

1. Although the title of the book refers to “psychological science,” the contributors are almost all primarily identified with social psychology. One will not learn of some of the great collaborations in other subfields of psychological science, such as cognitive psychology (e.g., Tversky and Kahneman) or developmental psychology (e.g., Caspi and Moffitt).
2. Although some of the authors discuss failed collaborations (e.g., Zweigenhaft discusses collaborators who have political axes to grind), there is much more in the book about ingredients of successful collaborations than about ingredients of failed ones. And the failed ones are the ones we all have to guard against.
3. There is more discussion of the benefits than of the costs of collaboration. There are exceptions: For example, Sherman notes how collaboration can be risky for getting hired and for being promoted to tenure, and Fiske and Taylor discuss how collaboration cost Fiske tenure in her first job. (I suspect that institution deeply regrets its decision now!) But generally, the emphasis is on the pluses rather than the minuses of collaboration.

In the final chapter, Borgida and Zweigenhaft summarize what they believe are the main lessons in the book: “share the data,” “beware of social loafing,” “be crystal

clear how who does what,” “establish the order of authorship early,” “reward collaboration,” “maintain open lines of communication,” “use collaboration as a social support,” and “value mentoring as a form of collaboration.” I think these are important lessons, but they do not fully capture all of the dynamics that come out in the individual chapters. So I would supplement their list with some characteristics of successful collaborations I see emerging from the book. In successful collaborations, the authors

- Trust each other
- Learn from each other
- Communicate well with each other
- Are honest, sometimes brutally so, with each other
- Motivate each other to reach greater personal as well as professional success
- Respect each other
- Like each other
- Emotionally as well as intellectually support each other
- Tolerate setbacks in the collaboration rather than giving up on it as a result
- Tolerate each other’s (sometimes annoying) idiosyncrasies
- Commit themselves fully to the success of the collaboration
- Each contribute something that the other could not do—as in the data-sufficiency problem, they are jointly necessary for the success of a project neither could do nearly as well on his or her own.

There are three caveats I would add that perhaps do not always come out forcefully in the book.

First, although collaboration can be wonderful, it also can be dreadful. Some of the authors point this out, for example, with respect to collaborative projects that never got finished. But one should remember that in the course of a career with large numbers of collaborations, one is likely to encounter some collaborations that seem to have come straight out of hell. One may encounter collaborators who steal data, who insist on an order of authorship they do not deserve, who incorrectly analyze data, who falsify data, and who are so unpleasant personally that one wishes never to have met them.

Second, individuals can have extremely distinguished careers individually without collaborations. Albert Bandura and Dean Simonton occur to me as psychologists who have produced marvelously successful single-authored works, but there are many others. So one should not feel badly if collaboration does not prove to be one's preferred working style, at the same time that one remembers that collaboration with students is considered an important part of many college and university jobs.

Third, one should be aware of strings attached to collaborations. In my experience, there are three types of strings.

The first kind of risky collaboration that I have experienced was with people in another university whose view on our collaboration was very different from the view of our group. The PI from the other university was someone whose expertise in our mutual field was certainly comparable to my own. Unfortunately, our views on the project and what it was supposed to accomplish were totally different and throughout the project we experienced a tug of war between the two PIs and their teams, with each group pulling in a different direction. The

project was probably less than the sum of its parts because of the tension between the two groups in the two universities.

The second kind of collaboration of which to be aware is when one signs up to collaborate with an industrial or other business partner. Businesses sometimes fund and then collaborate in research. But they also sometimes have ulterior motives in funding the research and appreciate only results that support rather than undermine the potential future sales (and legal position) of their products. Beware of collaborations that require confidentiality agreements, preapproval of a research paper by the funder, or that are funded by organizations whose research seems always or almost always to support rather than undermine the alleged safety or viability of their products. I once tried to work with a very well-known technology company. But the company put so many restrictions on publication that my university was not willing to sign the contract and I would not have signed on even if the university had. Basically, the company only wanted publication of findings favorable to their products.

The third kind of risky collaboration is one that involves government officials who are political appointees. I once had a fabulous contract that emanated from very high-level political appointees (outside the United States). The contract seemed like a terrific opportunity to change education in the country. I would be collaborating with government officials, university professors, and school teachers alike. Unfortunately for my project and others like it, a new government came into power and terminated all projects funded by the previous government.

In sum, this is a terrific book that I would recommend highly to any psychological scientist. Anyone who does collaborations—and that means almost all of us—will profit from

reading this book. And all of us will learn not only about collaborations, but also about how great scientists think within and even outside of those collaborations.

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Table 1

Four Types of Collaborations

Statuses/Length of Collaboration	Short-term	Long-term
Equal Status	Type 1	Type 2
Unequal Status	Type 3	Type 4