

DIALOGUE

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Welcome to the Fall 2012 Issue of Dialogue

Charles Stangor, University of Maryland
& Hart Blanton, University of Connecticut



Dear SPSP Members,

We are thrilled to bring you the Fall 2012 issue of SPSP Dialogue, the official newsletter of the Society for Personality and Social Psychology. Dialogue reports on current developments in the field and on activities of the Society. Dialogue is a forum for diverse perspectives on issues relating to personality and social psychology. Dialogue publishes contemporary, newsworthy, and potentially controversial articles in a variety of areas of interest to society members.

Please note that some material that was formerly printed in Dialogue, such as a summary of the Executive Committee and other reports, will now be available to logged-in members at SPSP.org, under the Main tab on the menu titled "EC Reports."

Dialogue publishes in the Spring and the Fall of each year. Our goal is to provide you with a mix of enjoyable reading, society news, and scientific dialogue.

We hope that you will find this issue's articles on scientific data integrity and sharing particularly informative. Also, we are very pleased to be able to publish the return of Paula Niedenthal's *Academix* to Dialogue, following a long hiatus abroad. We continue our tradition of publishing a Travel Section. The goal of this section is to report on the work experiences of social and personality psychologists "living abroad" (in this case, outside of social psychology training programs). In this issue, we hear from Kerry A. Reynolds who is working at RAND, a nonprofit research institute that seeks to improve public policy through research and analysis.

Continued on p. 2

If you are reading this document online, you can access the SPSP article discussion area by clicking the icon by each article's title.



Dialogue is the official newsletter of the Society for Personality and Social Psychology. It seeks to promote current developments in the field and activities of the Society. The opinions expressed in Dialogue are the views of the authors and not necessarily the views of SPSP or of the editors.

Please note that Dialogue can be read online, or downloaded for convenient portable reading.

The Dialogue invites submissions and ideas for topics from the membership broadly. Please send us your thoughts, comments, ideas, and particularly your lighter side! We want to hear from you!

All Dialogue articles in this issue are open for comments from SPSP members on the SPSP website, SPSP.org.

The opinions expressed in Dialogue are not the official views of SPSP or necessarily of the editors.

Links to previous issues of the Dialogue, in PDF format, can be found at <https://www.spasp.org/?page=Dialogue>

We hope to continue to bring you diverse and interesting issues over the next years of our term.

Open Letter to the SPSP Membership Regarding Ethical Conduct of Research



Patricia Devine, University of Wisconsin

Dear SPSP Members,

Every now and again, our field gets tested. This is one of those times. In a matter of a few short months, three cases of suspected fraud in the data collecting and reporting process in our field have come to light. In the wake of these cases, there has been a great deal of discussion concerning not only what to make of these specific cases but also about the pressures and practices of our field more generally. Outright fabrication of data is obviously wrong. But what about the “gray areas” of eliminating cases, conditions, or variables? And, in a period of growing suspicion, will people become more cautious and concerned about sharing their data and stimuli? What are the consequences for how our science is viewed both inside and outside the field? These are just a few of the many questions we are all talking about in the aftermath of the cases of suspected and, in some instances, confirmed cases of fraud.

Fabricating data and other forms of fraud threaten the core foundation of any field of scientific endeavor – trust and integrity. Indeed, if we can’t trust the integrity of the process, of the attendant data, and of the authors who produce reports to present to the scientific community, then we have nothing. Once this trust is betrayed, it’s hard to get it back. Such events are effectively black marks on our field – damaging our collective reputation – and we need to decide as a collective how to respond to such threats.

Some will say that our field is in crisis. To that characterization I would add out of every crisis comes opportunity. In my conversations with colleagues throughout the field, I have learned that the discussions they are having with their students about the specific cases and the various questions outlined above have been productive and have afforded opportunities to discuss explicitly a variety of issues that we may have implicitly taken for granted were effectively communicated through our training (i.e., we must uphold the strictest standards of ethical conduct in our research). Students are asking more and better questions about their data and how to analyze them. People are talking about what our goals are as individual scientists (i.e., promoting our science or promoting ourselves). Though prompted by unfortunate circumstances these conversations are good to have and will serve us well in the long run. Indeed, I’m both impressed and inspired by the quality and the depth of conversations we are having and they will serve as the basis for us to move forward with confidence amid the fraud cases.

Some have wondered what our organization is doing to address these issues and others have wondered what they can do as individuals. The Society has, indeed, been working on these issues but the work has been behind the scenes. We would like to share with the community, however, what SPSP has done and will be doing to address the issues. I then offer a few reflections on what you can do as individuals.

What SPSP Is Doing

Soon after the fraud allegations were made in the Stapel case, Todd Heatherton, in his role as President of SPSP, appointed a Task Force on Responsible Conduct. The charge of the Task Force was to examine ethical conduct within the field, including what can be done to uncover misconduct, how the field can be more confident about the veracity of collected data, how training within the field can enhance ethical behavior, and how we can

generally promote social and personality psychology as a credible scientific endeavor. The Task Force was chaired by Jenny Crocker and included members of the SPSP Executive Committee as well as representatives from a number of other organizations (e.g., APA, APS, SESP, FABBS, SPSSI, SAN, and ARP). The Task Force met at the SPSP meeting in San Diego in January of 2012 and produced a report that outlined a variety of ways we could take positive steps to ensure the integrity of our science. You can read through the report and the Task Force recommendations at the following link:

http://spsp.site-ym.com/resource/resmgr/files/task_force_on_responsible_co.pdf

The SPSP Executive Committee has devoted a considerable amount of time to discussing these issues and how the organization could play a positive role in addressing the concerns. One of the many ideas we generated was to have a symposium at our meeting in January in New Orleans devoted to some of the questions and issues that seem most pressing for society members. This symposium would provide a forum for our community to come together to discuss the issues and to explore how to best protect our science against temptations of fraud and ensure the integrity of our science.

And, the Society's efforts do not end there. SPSP is taking initiative to develop new workshops, policies, and standards for responsible conduct in research. The challenges to the field create new opportunities for SPSP to assume professional leadership.

What You Can Do

Our ability to uphold strict standards of ethical conduct is only as good as the efforts made by the members of our community. The single most important thing you can do is to adhere to these standards. If you have questions or uncertainties, ask questions. Make discussions of ethical behavior part of the everyday discussion in your lab. By virtue of how you conduct your science, become a role model for others. The threats to our field's integrity have made many people uneasy. In some cases, people seem overly suspicious of the validity of others' findings. In other cases they have become wary when others ask for their data for reanalysis, with the implicit notion that someone might not trust the integrity of one's science. Against this last concern, a wise colleague recently commented that if you haven't done anything wrong, you don't have anything to worry about. Thinking about it this way should put us at ease and allow us to move forward with confidence that our science is strong and that our community, with a very small number of exceptions, has integrity.

The SPSP Executive Committee will continue to work on these issues and we will do our best to keep the membership informed of our efforts.

Yours sincerely,

Patricia G. Devine

President, SPSP, on behalf of the SPSP Executive Committee

Todd Heatherton (Past President)
David Funder (President Elect)
Monica Biernat (Secretary Treasurer)
Randy Larsen (Member at Large)
Wendi Gardner (Member at Large)
Sam Gosling (Member at Large)
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SCIENCE

A 21 Word Solution

Joe Simmons (University of Pennsylvania), Leif Nelson (University of California, Berkeley), and Uri Simonsohn (University of Pennsylvania)

About a year ago we published “[False-Positive Psychology](#),” in which we argued that scientific journals should require authors to disclose how data were collected and analyzed. Here we propose a simple 21-word statement that achieves this, and then reflect on reservations people have expressed regarding disclosure.

“False-Positive” has received much more attention than we ever expected, but this attention has not yet materialized into concrete action designed to reduce the share of false-positives in the literature. We write this article hoping to move the discussion towards action.

1. If you are not [p-hacking](#) and you know it, clap your hands.

Many support our call for transparency, and agree that researchers should fully disclose details of data collection and analysis. Many do not agree. What follows is a message for the former; we begin by preaching to the choir.

Choir: There is no need to wait for everyone to catch up with your desire for a more transparent science. If *you* did not *p-hack* a finding, *say it*, and your results will be evaluated with the greater confidence they deserve.

If you determined sample size in advance, *say it*.

If you did not drop any variables, *say it*.

If you did not drop any conditions, *say it*.

These 21 words in a Methods section can *say it* succinctly:

“We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study.”

When needed, supplemental materials can be used to ensure the 21 words are accurate.

When sample size is not determined in advance, one could write:

“We added 50 observations after analyzing the first 100”.

A small but energetic choir may get the entire congregation to sing along.

2. Big-picture skeptics

Three big-picture concerns have been raised:

- a) Trust: Requirements assume or create lack of trust among peers
- b) Policing: Enforcement turns journals into the police (not Sting’s)
- c) Effectiveness: Disclosure won’t do anything

Our reactions:

a) Trust.

For trust to exist, people must agree on what it is they are trusting. We cannot “trust” our colleagues to run and report their studies “properly” if there is no shared understanding of what “properly” is.

There is no shared understanding of what “properly” is.

Some think dropping conditions is fine, others do not. Some think collecting 10 subjects at a time is fine, others do not. Some think dropping measures is fine, others do not. And so on.

Note how disclosure depends on trust.

If Faith says she collected only one dependent variable we will believe her.

But what if she does not say anything? What should we think?

Because many forms of *p*-hacking are common, currently acceptable, and often encouraged by journals, we should wonder, in the absence of disclosure, whether Faith actually collected more measures.

Note how lack of disclosure fosters *distrust*.

Asking authors to disclose does not take trust out of our scholarly exchanges. It creates a framework for trust to meaningfully exist.

b) Policing.

At coffee shops we can freely ask, “Hey, is this 1% or 2% milk?”

Our aspiration is that our journals achieve this coffee-shop-grade level of transparency, so that we can freely ask when reading papers, “Hey, is this a 1 or 2 dependent variable study?”

The Starbucks barista does not pull out a milk-fat-assessment instrument when asked. He just reads the carton. He trusts the carton to be truthful. Journals, similarly, would merely ask authors to indicate the *p*-hack content in their research and trust them to be truthful. No policing, just asking.

Figure 1. Levels of Transparency



Note: pictures taken at a Peet’s coffee, not Starbucks.

c) Effectiveness.

If you think about it, the only way in which disclosure does not reduce the negative impact of *p*-hacking – the likelihood that false-positives will be published in our journals – is if authors *lie* when they disclose. They explicitly state their study had three conditions when in fact it had five. We don’t think that is very likely to happen, so we think disclosure is very likely to work.

Those who disagree must not trust psychologists as much as we do.

3. Small-picture skeptics.

Two small-picture concerns that have been raised are:

- a) Red-tape: Disclosure requirements add excessive regulation to the publication process
- b) Exceptions: What if once in a while disclosure is impractical?

Our reactions:

a) Red-Tape.

The disclosure requirements we proposed in “False-Positive” fit in half a column of *Psychological Science* (see Table 2 in p. 1362). The say it succinctly version we proposed above has but 21 words. Both can be implemented in supplemental materials, keeping the length of published content unchanged. Contrast that amount of red-tape with the over 400 pages of the APA style guide most journals require complying with when submitting.

Figure 2: Number of words required for transparency versus stylish writing.



Nevertheless, if push came to shove, we could maintain the existing amount of red-tape constant by substituting some arguably less vital aspects of style requirements from the APA guide with those we have propose here. For example, we could eliminate the following 23 words in the “Metrication” chapter of the *Publication Manual* to make space for our proposed 21 words enhancing the credibility of a paper’s findings: “**Spacing**. Never use a space between a prefix and a base unit. Examples: kg, kilogram [...] Do not use a period after a symbol.” (p. 130)

b) Exceptions

Debates about change tend to focus on exceptions: “What if this happens?” “What if that happens?” Exceptions, fortunately, are exceptional. They are atypical.

If we accept that the status quo is not perfect (and in that there appears to be little contention), changes need not be assessed in terms of their perfection, but merely in terms of their improvement. A good policy can then be enacted as a default, and waivers be granted for exceptions.

In the early 1900s the United States passed “pure food” regulations that required manufactures to disclose what was in the food they were selling. The practical concerns manufacturers expressed towards transparency in food then mirror those that researchers express towards transparency in research today.

Not unlike objections to require sample size to be $n > 20$, for example, ice-cream manufacturers objected to the arbitrary threshold of 16% of buttery fat to qualify as ice-cream.¹

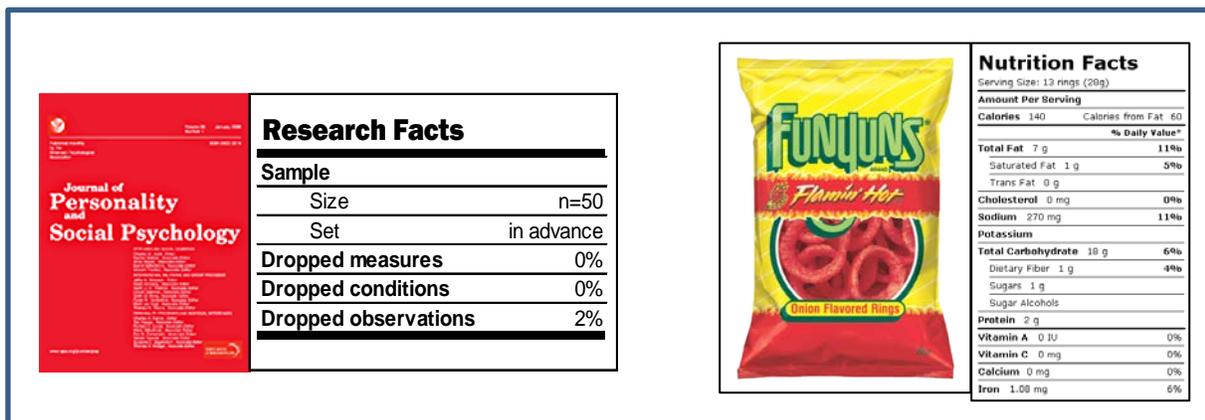
¹ “Do not want to make alcohol conspicuous,” *New York Times*, September 21st, 1906, pp.5

Not unlike objections that it is easier to comply with disclosure for researchers in some fields than in others, sardines importers complained that labeling the exact type of oil in their cans was harder on them than on domestic producers, etc.²

Fortunately for all of us today, the Federal Government knew better than to let the perfect be enemy of the good. If only perfect solutions were implemented, we would still be drafting the Pure Food Act of 1906.

We hope that editors will emulate the pragmatic politicians of the 1900s, deciding to implement disclosure requirements in our journals before a perfect solution with no detractors is arrived at. In the meantime, those of us who realize transparency is a necessary condition for evidence to be scientific can start adding 21 words to our papers.

Figure 3. One of these labels is not mandatory



What Can We Do to Reduce Scientific Misconduct?

Wolfgang Stroebe (Utrecht University and University of Groningen), Tom Postmes (University of Groningen) and Russell Spears (University of Groningen)

The news that the highly respected social psychologist Diederik Stapel had committed large-scale scientific fraud came as a wake-up call to our scientific community. Even though there had been major fraud cases before in physics (e.g., Jan Hendrik Schön, 2002) and in medicine (e.g., Darsee, 1981; Wakefield, 2004; Woo Suk Hwang, 2006), most of us had never considered that such major fraud would happen in our midst. After all, the fraud of Karen Ruggiero (2001) was comparatively minor, resulting in only two retractions of scientific articles, and Marc Hauser (2002) was a biologist by training and thus not really a proper psychologist, even if he did hold a position in psychology (for information, see Stroebe, Postmes & Spears, in press). Finally, the case of Sir Cyril Burt and his invented twin data happened long before most of us were born.

The international press had a field day and lambasted psychology, suggesting that the Stapel case “exposes deep flaws in the way science is done in [...] psychology” (Carey, 2011). Journalists also wondered why we did not discover the fraud earlier (e.g., Campbell, 2011) and frankly, we asked ourselves the same question (Stroebe et al., in press). We had always assumed that science is self-correcting in that findings that are based on fraudulent research will be discovered either in the peer review process or through (failed) replications (Broad & Wade, 1982; Goodstein, 2012). As Crocker and Cooper (2011) recently asked: “Scientists generally trust that fabrication will be

² “Grocers complain of Wiley,” *New York Times*, November 20th, 1904, pp.7

uncovered when other scientists cannot replicate (and therefore fail to validate) findings. In this particular case, however, reliance on replication did not work. Why?”

In order to see how other frauds were identified, we began to study reports of fraud cases. There is abundant material (e.g., Case summaries from the Annual Reports of the Office of Research Integrity http://ori.hhs.gov/case_summary), but official reports typically do not disclose how the fraud had been discovered. For this information we had to rely on newspaper reports. This limited our sample to 40 of the more spectacular cases. As we reported in an article to appear in “Perspectives on Psychological Science”, we found to our surprise that hardly any of these cases had been discovered during the review process or as a result of failed replications (Stroebe et al., in press).

In retrospect, one can think of good reasons for this. Reviewers evaluate a manuscript in terms of whether hypotheses are clearly derived from theory, whether the research is sound, whether alternative explanations are ruled out, etc. Because they have to rely on information provided by the authors, fraud may be difficult to detect. However, our research also shows that in several of the cases we studied, the fraudsters were sloppy and left clear signs of wrongdoing that could (perhaps should) have raised reviewers’ suspicion (Stroebe et al., in press). This suggests that in general, reviewers are not always sufficiently alert to the possibility of fraud.

With regard to replications, a frequently deplored problem is that journals typically do not accept replications for publication. As a result, they are rarely done and if they are done, they do not become known to the scientific community. However, as we will discuss below, there are also other reasons why replications are not always very effective means for the identification of fraudulent research.

Incentives for Fraud

We all know that the way to achieve success in our discipline is to publish in high impact journals. And to get accepted in these journals, one not only has to develop hypotheses that are novel and interesting, but ideally the predictions should be supported unambiguously by the data. Obviously, researchers who fabricate or falsify their data have an advantage here. It is therefore not surprising that journal impact measures correlate positively with number of retracted articles (Fang & Casadevall, 2011). However, this system not only rewards fraudsters, it also rewards deserving researchers. Furthermore, the system is shaped by market forces. As long as there are more good researchers than there are jobs, departments will be selective and as long as there are more manuscripts than there is publication space, editors will be too. Thus, we reasoned that there was little likelihood for this system to change.

Strategies For Fraud Reduction

Instead of trying to change the system, we decided to focus on making improvements to the process of conducting research. In this section, we discuss five potential strategies of fraud reduction. Some of these will be quite familiar (i.e., courses on research ethics, increasing the accessibility of data sets, facilitating replications) but others are less so (i.e., strengthening the position of whistleblowers, instituting research audits).

Research ethics. Many have pointed out that the field needs clear standards and clear procedures to deal with suspicions about research or researchers. Indeed, our research suggests that universities have often been reluctant to investigate fraud cases or, if they investigated them, to make their findings public. This hinders us in combatting this problem. Institutions may also need to devote more attention to research ethics. A course on research ethics should therefore be part of every graduate program (and although Stapel himself also taught such a course, this is no reason not to). In such courses one should discuss the obvious rules and good practices (e.g., if data of participants are eliminated, accepted rules have to be followed and this has to be reported in the article), as well as the grayer areas of research practice, such as failure to report null findings.

But we believe that the development of a macro-level infrastructure for dealing with fraud (rules and procedures) should be complemented by a consistent micro-level commitment to maintaining the highest standards of research integrity in our everyday research practices. Research on fraud points to the strong influence that the immediate social environment’s norms and practices have on one’s ethical conduct. Accordingly, the micro-level maintenance of ethical standards by the local research group should be the most impactful way of guaranteeing that standards are upheld. Our research confirms that fraud is most often flagged up by insiders, aware that

something is amiss. Extending this, the local level is also the best place to ensure that ethical research practices are promoted. In some sense, this is also a heartening conclusion: we can take matters into our own hands.

Strengthening the position of whistleblowers. As in the Stapel fraud case, reports by whistleblowers are by far the most frequent way in which fraud is discovered. These whistleblowers are mostly research collaborators, who have inside knowledge of the research practice in their laboratory. They are often PhD students or postdoctoral researchers. It is therefore important that graduate students are not only taught proper research practices but are also informed that fraud does happen and what should be done when it is suspected. There should also be assigned people of trust at each department to whom people can turn in cases of suspicion. And there should be clear protocols stating how such discussions should be handled. Furthermore, the anonymity of whistleblowers must be safeguarded whenever possible. Being a whistleblower often has negative effects on people's careers—we need to ensure that the reputation and careers of whistleblowers do not suffer, both for individual whistleblowers and for the institutions who decide to self-investigate.

Increasing the accessibility of data sets. Even though the APA rules clearly specify that authors should share research data with others on request (e.g. American Psychological Association, 2010), authors are often reluctant to do so. For example, Wicherts, Bakker and Molenaar (2011), who contacted the corresponding authors of 141 articles published in psychology journals, reported that most authors failed to send their data. One of the most widely accepted strategies of fraud detection is the creation of a publicly accessible repository of the data of published studies, which would at least discourage the most egregious example of fraud based on obviously dubious or even nonexistent data. However, this still leaves the possibility that researchers could “massage” their data (e.g., by omitting participants, who did not respond in line with hypotheses). At least for studies conducted by computer, this could be prevented, if all research institutions stored data of studies in read only files and keep these data for a decade or more. With studies using written questionnaires, these could be scanned and also stored electronically.

The public availability of data sets would also facilitate the application of statistical methods designed to expose scientific fraud. Such methods have been used by Simonsohn (2012) in identifying problems in the articles of social psychologists Smeesters and Sanna. Both resigned as a result of these accusations. In the case of Smeesters, a university investigation committee concluded that the findings reported in three of his articles were “probably the result of data selection by Smeesters” (Erasmus University Rotterdam, 2012). The case of Sanna was investigated by a committee at the University of North Carolina, where he had worked when he published the suspected research (before he moved to the University of Michigan). The findings of the committee were not made public. However, Sanna resigned his position at the University of Michigan and withdrew three of his published articles (Yong, 2012). Other methods of statistical fraud detection have been suggested by Diekmann (e.g., 2007). While the development of such methods is certainly an extremely promising way to identify fraudulent research, it still needs to be clarified how well such methods discriminate between fraudulent and non-fraudulent research. Also, public availability of data would have little effect, unless there was some probability of the data being scrutinized and reanalyzed. As we will discuss later, one way of assuring this would be through the institution of random audits being conducted by research institutions.

Facilitating replications. Another widely accepted strategy of fraud detection is to encourage replications of studies. For example, Crocker and Cooper (2011) argued: “Despite the need for reproducible results to drive progress, studies that replicate or fail to replicate others’ findings are almost impossible to publish in top scientific journals. This disincentive means fraud can go undetected, which was the case with Stapel”. And similarly, Chambers and Sumner (2012) write: “Replication is our best friend because it keeps us honest. In science, false results have a short (albeit potentially damaging) lifespan because regardless of how they come about, other scientists won’t be able to reproduce them. On the other hand, true results will be replicated time and time again by different scientists.” Mummendey (2012, p. 7) goes even further and suggests: “Scientific journals could expand their already high standards of the peer review system by adding the requirement for a thorough external replication. Authors submit their manuscript together with their data. Once the publication has been approved by a preliminary group of reviewers, the editors invite suitable experts to attempt a replication of the results. After this has been accomplished, both the original manuscript and the replication study are published together.”

Our perspective, however, is that this trust in the power of replications is somewhat idealistic and even misguided. First, purely in practical terms there is the problem of the doubling of resources needed to conduct publishable research (and such resources may not be easily or equally available to all in these difficult economic times). Second, we have numerous examples in the psychological literature, where “true” results repeatedly failed to be replicated. [The more ancient among us will still remember the controversy surrounding the Festinger and Carlsmith (1959) results, which could only occasionally be replicated, until it was discovered that freedom of choice and negativity of consequences were essential for the effect to emerge.] Since there are always numerous reasons for a given finding not to be replicated, failure to replicate cannot be seen as a reliable indicator of fraud. Furthermore, since due to their high productivity fraudsters are often highly respected in their field, even blatant failures to replicate their findings might not arouse suspicion. Finally, even successful replication cannot be seen as indication that the original result was *not* fraudulent. Since fraudsters are typically careful in suggesting plausible hypotheses, it is quite possible that these hypotheses might have been supported by an empirical study had the fraudster cared to conduct it. In the case of Stapel, one of the committees examining his publications has suggested that some of his PhD research was fraudulent (Keulemans 2012), but these findings have been replicated on occasion, at least conceptually.

Clearly, information about multiple failures to replicate a study is important because it suggests that a given finding is not very reliable or stable. Furthermore, the indication that different findings of a particular researcher or research group cannot be replicated might signal that there could be a problem. For example, the failures to replicate research by the physicist Jan Hendrik Schön motivated his colleagues to have a close look at his publications (Reich, 2009). This led to the discovery that he had published similar performance curves for different devices and ultimately to the discovery of his fraud (Reich, 2009). Therefore the recent creation of a website, where researchers can upload and view results of replication attempts in experimental psychology is a useful initiative (PsychFileDrawer.org, 2012). However, it is difficult to decide on the basis of a brief summary how, or how well a given study was done. Furthermore, the researchers who report their replications to this web site should be required (rather than merely advised) to download their data.

We are somewhat less convinced of the usefulness of the Open Science Collaboration and their plan to replicate all studies published in three journals during a given year (<http://www.openscienceframework.org/>). Although the initiators of this collaboration emphasize that they do not target fraud per se, but hope to check the extent to which psychological research can be replicated, one can doubt whether the information that a certain percentage of studies did not replicate will justify the enormous investment in research time and resources that this task requires. Furthermore, since social psychological research is more sensitive to social and other context factors than is research in psychophysics, it is very likely that the project will find that social psychological research is less replicable.

Instituting research audits. Another frequent method through which fraud was identified in our sample of cases was through research audits. During such an audit, researchers have to disclose all the material used in a given study and typically their data are reanalyzed. Such research audits are frequently conducted in medicine, but mostly when there is already suspicion of research fraud. However, to be successful in fraud prevention, audits should be conducted on a random basis and not only once there has been reason for serious suspicion. Although this seems impracticable, we know of at least one research institution, where such random audits are being practiced <http://www.emgo.nl/kc/Audit/1%20Internal%20Project%20Audit%20Procedure.html>

Such audits would not only discover outright fraud, they would also discourage behaviors in the grey areas between good practice and scientific misconduct. As part of such an audit all members of a research group would be interviewed and unusual research practices could be identified. For example, Stapel claimed to have done field research (e.g., Stapel & Lindenberg, 2011). Since it is implausible that senior researchers collect such data themselves, a research assistant would have been involved in real data collection (assuming this occurred), who could have been interviewed in an audit. (The cases of Sir Cyril Burt and also Karen Ruggiero, where such assistants appeared not even to exist, might have brought these frauds to light earlier had they been audited). Since only a small proportion of research projects could be audited in this way, the probability that one of Stapel’s projects would have been audited does not seem all that great. However, given that he was an active inventor/researcher at three research institutions, the chance is not negligible. Furthermore, since his fraud would most likely have

been discovered in such an audit, the knowledge that such audits are being conducted might have discouraged his behavior.

Conclusions

Even though the prevalence of research fraud is likely to be low [most estimates put it around 1% to 2% (Stroebe et al., in press)], scientists have a particular responsibility to society and it is understandable that reports of research fraud are greeted with a public outcry. Although the Stapel case hardly exposed deep flaws in the way we conduct our science, it clearly demonstrated that any trust-based system, as science is, is open to exploitation. We therefore need to look at our procedures and check whether they can be tightened. Any system can be improved, and lessons can be learned from the Stapel case as well as from the many other cases of fraud. And the major lesson to be learnt is that the assumption that science is self-correcting and that findings based on falsification will eventually be discovered and rejected is an illusion.

We have been criticized for drawing this conclusion by colleagues who have argued that such claims will cause people to lose trust in science. In our opinion, the trust in science is undermined by cases of research fraud and not by analyses of underlying causes of fraud (although we would add that exposing cases of fraud should help us to rebuild trust in the long run). The Stapel fraud was a wake-up call that motivated social psychology to scrutinize their research practices. And although there is much good in Social Psychology and although the discipline has been very successful in recent years, the case threw light on some procedural weakness (which our research suggests can be found in our own discipline as well as many others) that need to be addressed and fixed. If we use the case as a learning experience rather than deciding to return to “business as usual”, something good will have come out of this painful episode.

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What Is Wrong With Social Psychology?

Gregory Mitchell (School of Law, University of Virginia)



Those weary of discussions of Hauser, Stapel, Sanna, and *p*-hacking may be relieved to discover that my title does not refer to the recent revelations of manufactured data and other questionable research practices by social psychologists to generate statistically significant results (e.g., John, Loewenstein & Prelec, 2012; Simonsohn, 2012). My title is based on two other troubling facts that have received less attention on the blogs, in the popular press, and in our journals.

First is my recent finding that social psychology fared much worse than other psychological subfields in a comparison of results in the laboratory and the field (Mitchell, 2012). This replication and extension of Anderson, Lindsay and Bushman (1999) collected 82 meta-analyses in which effects in the laboratory were compared to effects in the field (e.g., were the effects of alcohol on behavior the same in a "bar lab" as in a real bar?) and examined the correlation, relative magnitude, and constancy of effect direction for 217 pairs of effects obtained from these meta-analyses for a wide range of phenomena from many psychological subfields. I found that industrial-organizational psychology performed remarkably well in the field ($r = .89$ for paired lab and field effects), and the magnitude of effects were similar in the lab and field; laboratory studies from personality psychology also held up well in the field ($r = .83$), but there were considerably fewer paired effects for this subfield than for I-O and social psychology. Social psychology performed much worse: over 20% of effects from social psychology laboratories changed signs in the field, the correlation of lab and field results was much lower ($r = .53$ if we exclude an outlier pair of effects), and the relative magnitude of effects differed greatly between the lab and field. Social

psychology laboratories often produce not only externally invalid results but positively misleading claims about the nature of relations among observed variables.

Second is the woefully low level of data sharing by social psychologists (Blanton et al., 2009; Wicherts et al., 2006, 2011). For instance, I (along with others) requested data from 42 manuscripts reporting correlations between IATs and criterion measures, and the results were not pretty: we received 17 datasets and promises that four datasets would be shared after publication of the results (to date none of the results have been published despite several of the datasets being several years old); nine datasets were not available for various reasons (e.g., computer failures with apparently no backup of the data, inability to locate where the data is stored or the student who collected the data, failure to archive the data when making a move between institutions); researchers explicitly refused to share data for five published studies; we received indications of possible sharing of data sometime in the future for two datasets held by one author; we received no response to multiple requests for five datasets. In one case, I engaged in an extended negotiation to try to obtain one dataset, but the authors used statements in the current *APA Publication Manual* and *APA Ethics Code*—that authors and requesters should come to a written agreement on the terms of the data sharing (see APA, 2010, pp. 12-13; *APA Ethics Code Standard 8.14*, available at <http://www.apa.org/ethics/code/index.aspx?item=11>)—as grounds for placing many conditions on the sharing of their data. Ultimately, the authors refused to share their data despite repeated specific statements by me of the analyses that were planned and their purposes, despite my agreeing to get their permission before performing any other analyses, despite my agreeing to share all results with them before submitting the results anywhere, and despite my offering to submit any disputes that might arise regarding the data to some neutral party for adjudication (they said their data would be made public at some point, but to my knowledge that has not yet occurred). Other authors I contacted used the *APA Ethics Code* and *Publication Manual* to limit what could be done with their data, but these other authors did not ultimately use the standard as a complete shield against sharing their data. My experience confirms Wichert and Bakker's (2009) concern that the current APA standard and statements on data sharing inhibit rather than promote data sharing.

These two facts reveal pervasive problems that I believe have a common cause: reactions of rational actors to a system that rewards splashy studies published quickly and imposes no costs for failing to follow the scientific norm of data sharing or for failing to produce generalizable results. Designing and carrying out a field study or following representative design principles in the laboratory (Dhimi, Hertwig & Hoffrage, 2004) can be extremely time- and labor-intensive. It is much easier to embrace “psychological realism” as a design principle (Wilson, Aronson & Carlsmith, 2010, p. 57) and then assume that the psychological processes activated by short, simple, contrived, one-shot interactions with strangers or imaginary persons are the same as (or not meaningfully differently from) the processes invoked outside the lab that motivated the study in the first place. Unfortunately, as my recent comparison of laboratory and field results showed, that assumption may often be false.³ Likewise, cleaning up a dataset so that every variable label and transformation is clear to an outsider and documenting every step in the research and analysis process consumes precious time that could be spent producing work publishable in *Psychological Science* (and worthy of mention in APS's grandiose press releases or perhaps a *New Yorker* article); sharing data, on the other hand, may reveal fraud (see Blanton & Mitchell, 2011), selective reporting of dependent measures (see Blanton & Mitchell, 2011; Fiedler, 2011), neglect of outliers (see Blanton et al., 2009), analytical mistakes (see Wicherts, Bakker & Molenaar, 2011), or simple errors such as transposing digits or reversing the direction of a correlation. The reward to the individual researcher for sharing data is, at best, a warm glow and a feeling of relief when no problems are found by the requester. Of course, science depends on self-correction

³ Wilson and colleagues (2010) argue for both psychological realism, which they define as activating the same or similar processes in the lab as those activated in everyday life for the phenomena under study, *and* experimental realism, or engaging participants with experimental settings and interactions that have as much impact as everyday settings and interactions do. It could well be that inattention to experimental realism, as opposed to false assumptions about the activation of the same psychological processes inside and outside the lab, accounts for some of the failed replications, and there are other possible explanations as well. Whatever the cause, my findings support the view that external validity presents an empirical question and should not be assumed, particularly with respect to findings from social psychology laboratories.

through collective efforts to move toward more reliable findings and theories, and data sharing is a crucial piece of the self-correction process.⁴

The solution starts with journal editors and editorial boards. Journals that regularly demand multiple studies for publication should impose the requirement that one of the studies be a field study or a study otherwise devoted to demonstrating the robustness and generalizability of a result. Editors of other “basic research” journals should recognize that internal and external validity go hand-in-hand and should begin encouraging field tests of questionable or counter-intuitive findings: replication in the field demonstrates both generalizability and the internal validity of causal conclusions in the lab. At a minimum, authors should be required to show how they developed the stimuli and response variables for their experiments: were they the result of a “convenience sample” or the product of a systematic examination and sampling of conditions from the target environment and tasks? The use of unrepresentative stimuli and tasks should not be a bar to publication, but their external validity limits should be made explicit and the theoretical or practical reasons for use of unrepresentative conditions should be provided.

Many journals now have a data-sharing requirement as a condition of publication, but journals leave enforcement of this requirement to requesters, whose requests are regularly ignored or denied. A simple solution would be a requirement of data archiving on acceptance of an article (and the APA should substitute that expectation for its current statement on data sharing). Economical means for centralized archiving now exist (e.g., the Dataverse Network), and such archiving would have the added advantage of saving authors from misplacing their data, losing it through a computer malfunction, or having to learn how to reply to an e-mail, problems that have commonly plagued the authors to whom I have made data requests.

The Open Science Collaboration (<http://openscienceframework.org/>) begun by Brian Nosek is an important step toward making social psychology a better member of the scientific community because the project may motivate some researchers to document their research carefully and make their data publicly available. But this project will not address the external invalidity of social psychology and will not provide a systemic solution to the data-sharing problem given the voluntary nature of the archiving component of the project.⁵ Replication in the field and the sharing of data are both crucial to the development of reliable scientific theory; social psychology journals need to impose strict requirements that ensure both occur.

⁴ The responses I have received to data requests suggest that such requests (or at least those by this particular requester) are seen as the work of scavengers, free riders, or politically motivated ne'er do wells who are determined to make effects disappear, which one author told me we all know can be made to happen. Another author advised me that doing my own studies would be more useful than “playing with people's data (presumably to arrive at null effects).” (This author ultimately shared his data, and we found that the published article contained a clear error that was acknowledged by the author in correspondence but that has still not been the subject of a published correction almost two years after notice of the error.) Yet another set of authors told us that they would share their data if they deemed our purposes of sufficient scientific value (they did ultimately share the data after further correspondence, including our objection to the authors' exercising a veto power based on their judgments of scientific value). Many embrace data sharing in principle, but one's commitment to the norm may wane once one is on the receiving end of a data request.

⁵ It is in the very research area where Dr. Nosek has established himself that I have researchers now invoking APA data-sharing guidelines as a means of limiting access to data, and I had to defer a request to Dr. Nosek for published data because of the rate of compensation that Dr. Nosek sought for the time he would have had to spend to get a very large dataset into a sharable condition. Dr. Nosek has not yet posted the requested data to Dataverse, as he has done for several of his datasets, and I have not renewed my request for this data since Dr. Nosek began the Open Science Collaboration; perhaps the price for organizing this dataset so that it can be shared would not be as high now. My point is that absent an archiving requirement as a condition of publication, many researchers will not take the time needed to place data into a sharable form due to the labor costs associated with doing so.

Postscript. A few days after finishing this comment, the SPSP’s Task Force on Responsible Conduct published on the web its report and recommendation on “how we can generally promote social and personality psychology as a credible scientific endeavor” (http://spsp.site-ym.com/resource/resmgr/files/task_force_on_responsible_co.pdf). The report states that “[d]ata sharing advances science, and should be encouraged but not (yet) mandated,” with three reasons listed for the hortatory as opposed to mandatory stance: “too much resistance at this stage,” “[w]e need some guidelines, examples of data sharing agreements,” and “[l]ogistical issues to be solved.”

The first reason—too much resistance—is in fact a reason for making data archiving mandatory immediately because it is further evidence that voluntary data sharing does not work. Some psychologists apparently doubt the scientific value of data sharing or are unwilling to incur the costs of data sharing. These researchers cannot be counted on to share their data absent unless data archiving is a condition of publication. Until the costs of not sharing data outweigh the costs of sharing data, we should expect low levels of compliance with the data-sharing norm. Barring publication for those who refuse to archive their data is one simple way to alter the cost-benefit calculus in favor of greater data access.

The logistical issues listed by the Task Force—need for data to be interpretable and permanently available, participant privacy, credit for the original researcher—do not pose serious hurdles to a data-archiving requirement. Sophisticated data archiving technology already exists (with guidance on how to use it) at the Dataverse Network and ICPSR, users must register with the sites to access the data or must access the data through member institutions, data archiving is subject to IRB requirements, and uses of archived data are subject to IRB requirements.⁶ Use of the archived data can be made contingent on acknowledgment of the originator of the data (existing norms found within the APA code already require that credit be given to the original gatherer of the data). Studies in which authors present data collected by others as data they themselves collected should be subject to retraction, with an explicit notice of that the author wrongly passed off data collected by another as the product of one’s own efforts. Such behavior is a type of plagiarism that should subject the plagiarist to serious sanctions. In sum, technological, institutional, legal and ethical solutions to the logistical concerns raised by the Task Force already exist.

With the Task Force report, we see the idea of data-sharing agreements becoming an impediment to the field as a whole moving to data archiving. We need to step back and ask why so much emphasis is being placed on data-sharing agreements. What legitimate functions might such contracts serve, and what illegitimate functions might they serve?

It is not clear that such contracts will serve any legitimate function that could not be achieved through other means. Any research, whether involving data collected by oneself or another, must satisfy institutional review board requirements, making superfluous any agreement that the original researcher and the second researcher might make on participant protection. Most importantly, data should not be archived in a way that permits participant identity to be discovered. If it truly is impossible to archive any of the data without revealing identity, then a limited exception could be made for such data, but it should still be subject to inspection by others to confirm the claims made about the data (e.g., confirmation that participants whose behavior was videotaped and coded did in fact act in the ways reported by the original researchers). Existing norms found within the APA code require that credit be given to the original gatherer of the data; as noted above, falsely portraying oneself as the data gatherer would be a serious scientific offense that would be easily detected by the original data gatherer and should result in serious reputational and publication costs due to the plagiarism. One might imagine data-sharing agreements having some positive effects, such as making clear data attribution requirements and IRB restrictions, but it is hard to imagine any beneficial terms that could not be built into a mandatory data-archiving system put in place by journals.

⁶ Rather than have Sage or the Social Psychology Network create a data-archiving site from scratch, a move that would further segregate rather than unite the social sciences, psychology should join the larger research community and begin archiving its data on the Dataverse Network or at ICPSR.

The abstract possibility of positive benefits of data-sharing agreements should be weighed against the concrete negative effects of requiring that a data requester enter into a contract with the original researcher as a condition of data sharing. Requiring such agreements gives those holding the data veto power over data sharing that can be achieved by insisting on unreasonable terms, and, even with well-intentioned parties on both sides, this requirement imposes an unnecessary transaction cost. Furthermore, the APA's position that the original data gatherer and the data requester should agree on all of the analyses to be performed on the data enables original researchers to gain knowledge that can be used to further manipulate the data before sharing it or to determine that the risks of sharing and the revelation of weaknesses or fraud outweigh the risks of not sharing.⁷

By endorsing the idea that researchers should enter into data-sharing agreements, the Task Force effectively endorses giving data gatherers a strong power to exclude others from accessing and using data. Social psychological data (at least that data that enters the scientific realm through publication or that is produced at research institutions) should be seen as a public good with originators of the data being given a limited term to exclude others from the data so that the original researchers have a first chance to analyze and publish the data. After that limited term, the data should be publicly available to other researchers. This limited right to exclude acknowledges the initial efforts of the first researchers to study individual participants, but it also recognizes that individual participants were the real sources of the data and that the information these individuals provided should not be held hostage by an original researcher who turns out to be too lazy or disorganized to analyze and publish the data, who failed to get statistically significant results, or whose pet theory was not supported by the data.

Finally, the Task Force report asserts that data sharing should not be legislated and states that "replication [in contrast to data sharing] is the key to uncovering false positives and other problems." Were strict replications more common in social and personality psychology, then this statement would make more sense. Conceptual replications, which build on earlier studies and findings, may provide evidence suggestive of false positives or other problems, but it is more likely such research will lead to a proliferation of moderator and mediator variables or parameters rather than outright rejection of earlier work. Field replications hold out greater promise because researchers have less control to stage manage a result and because there are persons outside the lab who can observe what is done, but it bears noting that many of Diederik Stapel's fraudulent datasets were supposedly collected in field studies--yet his deceptions lasted for years. Replication of all kinds should be encouraged, but they will not be a salve for all that ails social psychology, and the hope of more replication in the future should not be a substitute for mandatory data archiving now.

The unexamined assumptions and weak recommendations found in the report of the SPSP Task Force on Responsible Conduct in Research will likely reinforce the acceptability of *not* sharing data rather than promote social psychology as a credible scientific endeavor. Certainly some datasets will present delicate matters that justify exceptions to a rule of mandatory archiving of data, but no good argument against mandatory data archiving as the default rule, from which limited exceptions can be made by journal editors, is found in the Task Force's report (nor have I seen or heard good arguments elsewhere despite having many discussions about the topic, including on NSF review panels as the NSF moves to impose stricter data-sharing requirements on grant recipients). The current system of voluntary data sharing is broken. If we continue on with this system, we should expect continued low levels of data sharing and increasing levels of disrespect from the other sciences.

⁷ The requirement found within the APA manual that authors must only share data to permit confirmation of published analyses and results (see APA, 2010, p. 12) can easily be used to justify deleting unreported variables from a dataset that is to be shared with another researcher. As Fiedler (2011) discusses, the selective reporting of dependent measures is potentially a very serious problem within psychology that reaches well beyond the voodoo correlations of social neuroscience. Any mandatory data archiving rule should specify that all data collected should be preserved and archived, with explanations or notes provided on why some variables should not be utilized or should be used for only limited purposes.

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A Call to Share Your Data

Social Psychology Network Executive Director¹ and Advisory Board²



"Like it or not, the time is rapidly approaching when social psychologists will be expected or required to make their data freely available to other scientists."

–Jennifer Crocker, *SPSP Dialogue* (Spring 2012, p. 12)

When it comes to published research, psychology is much like other sciences: Its journals archive a cumulative body of peer-reviewed work that builds over time. Yet unlike scientists in many other fields, psychologists rarely

archive the data on which their published conclusions are based. For the most part, data files are treated as having only temporary value—private records, computer files, and documents that investigators store for a few years and then dispose of, much like old tax returns or musty newsletters.

This view of data is changing, however. In the words of British Cabinet Minister Francis Maude, data have become "the 21st century's new raw material" (Open Data White Paper, 2012, p. 5). Earlier this year, the U.S. government announced a \$200 million "big data" initiative, and data-sharing research networks have even been called "the 21st century microscope" (Higginbotham, 2011; Office of Science and Technology Policy, 2012).

After centuries of science, why the sudden focus on data sharing? Clearly, one factor is that data sharing tends to be less expensive than data gathering—a way to stretch research dollars during tough economic times. But an equally important reason is that information technology has finally matured and been adopted widely enough to make large-scale data sharing feasible.

This evolution is apparent in the case of Social Psychology Network (SocialPsychology.org), an educational and scientific network with nearly 2,000 profile holders from 50 countries. SPN was founded in 1996 and began receiving National Science Foundation support in 1999, but it took the Network until this year to develop a system for archiving and sharing data.

Data Archiving Made Easy

All SPN profile holders now have 10 gigabytes of space to upload nearly any type of file, and they can link uploaded files with publications or courses listed in their profile. For instance, profile holders can post and share datasets, codebooks, user manuals, rating forms, and supplemental material linked to specific journal articles. Individual files can be up to 300 megabytes in size, and each file is assigned a unique permalink that authors can use when citing a dataset or other uploaded documents.

The archiving process takes roughly 10-15 minutes per data file (much less time than most archiving sites) and requires the following information:

1. Title of the research project
2. Principal investigator(s)
3. Geographic location(s) where the data were collected
4. Start and end date of data collection
5. Type of data (e.g., numeric/quantitative)
6. Data format (e.g., SAS)
7. Type of sampling (e.g., random sample)
8. Mode of data collection (e.g., laboratory experiment)
9. Whether data collection received IRB/Ethics Board approval
10. Field of study
11. Abstract of research report
12. Keywords related to the research topic

Once this information is submitted, a permalink is generated and the data file becomes instantly accessible. For details on the uploading process, please see this brief video tutorial:

<http://www.socialpsychology.org/videos/files-tour>

(Note: Researchers without an SPN profile can obtain one at no cost by completing the form at SocialPsychology.org/profile. Currently, profile holders must have a psychology-related doctorate, but SPN hopes to offer member pages and file uploading privileges to students and others in the future. In the meantime, students working collaboratively with a supervisor might speak with that person about uploading data to the supervisor's SPN profile.)

Why Share Your Data?

A May 6, 2011, memo from the American Psychological Association Task Force on Data Sharing and Data Linking ("Recommendations on data sharing and data linking") summarized several key reasons for data sharing. According to the Task Force (p. 4):

"There are many ethically sound and scientifically compelling reasons for sharing data. Sharing data within the larger scientific enterprise enables replication for verifying empirical findings; promotes aggregation for the purposes of knowledge synthesis, hypothesis generation and testing, programmatic decision-making, and determining the generalizability of particular findings; opens up the data for analysis with new, more powerful or integrative techniques than available at time of collection; and encourages a culture of openness and accountability in scientific research."

The Task Force also pointed out that data sharing plans are often required by funding institutions (for examples, see the "Data Sharing Policies" sidebar).

Finally, there's one other reason why it's important to publicly share data: doing so has the potential to deter data fraud. This reason is especially important in light of several high profile cases of alleged data fabrication by Diederik Stapel, Dirk Smeesters, Larry Sanna, and others.

Indeed, if these cases have taught us anything, it's that tainted data are like second-hand smoke; they harm the whole community, not just the individual. To ensure the health of the field, we need to stop treating data management as an individual matter and start treating it as a "public health" issue that requires institutional and policy-level changes among the full gamut of stakeholders: scholarly journals, professional societies, funding agencies, employers, and of course, members of the research community.

In its memo, the APA Task Force on Data Sharing and Data Linking acknowledged that "The process of sharing data can be cumbersome and labor intensive," and it suggested that one way to create the necessary culture change in psychology would be "to ease the burden for data sharing by making it easier to upload and store data" (p. 5). This is precisely the spirit in which Social Psychology Network has developed an easy way for researchers to archive their data.

We therefore ask all colleagues reading this essay to give a few minutes of their time and begin the joint work of making it normative to archive data. To begin, simply watch the video tutorial mentioned earlier, create an SPN profile if you don't already have one, and upload data from a study you've recently published. In a matter of minutes, you'll be setting an example for others and helping bring our field into alignment with other disciplines that have long regarded data sharing as standard practice.

Notes

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Data Sharing Policies

Here are just a few data sharing policies adopted by professional societies, research journals, and funding agencies:

"When data are published in a peer-reviewed journal, authors should deposit associated data in a suitable publicly accessible repository, when available. This includes nucleic acid and protein sequence data, expression data, neuroimaging data, and other data types."

Society for Neuroscience Guidelines: Authors of Scientific Communications (Section 1.11)

"Sociologists share data and pertinent documentation as a regular practice. Sociologists make their data available after completion of the project or its major publications, except where proprietary agreements with employers, contractors, or clients preclude such accessibility or when it is impossible to share data and protect the confidentiality of the data or the anonymity of research participants (e.g., raw field notes or detailed information from ethnographic interviews)."

American Sociological Association. (1999, 2008). *Code of Ethics and Policies and Procedures of the ASA Committee on Professional Ethics* (13.05: Data Sharing). Washington, DC: Author.

"All data necessary to understand, assess, and extend the conclusions of the manuscript must be available to any reader... *Science* supports the efforts of databases that aggregate published data for the use of the scientific community. Therefore, appropriate data sets... must be deposited in an approved database, and an accession number or a specific access address must be included in the published paper."

Science: General Information for Authors, Data and Materials Availability (American Association for the Advancement of Science)

"Authors of quantitative or experimental articles are expected to address the issue of data availability. You must normally indicate both where (online) you will deposit the information that is necessary to reproduce the numerical results and when that information will be posted (such as "on publication" or "by [definite date]"). You should be prepared, when posting, to provide not only the data used in the analysis but also the syntax files, specialized software, and any other information necessary to reproduce the numerical results in the manuscript."

American Political Science Review: Submission Guidelines, General Considerations

"Investigators are expected to share with other researchers, at no more than incremental cost and within a reasonable time, the primary data, samples, physical collections and other supporting materials created or gathered in the course of work under NSF grants."

[Dissemination and Sharing of Research Results: NSF Data Sharing Policy](#) (National Science Foundation)

"Proposals submitted or due on or after January 18, 2011, must include a supplementary document of no more than two pages labeled "Data Management Plan." This supplementary document should describe how the proposal will conform to NSF policy on the dissemination and sharing of research results."

Dissemination and Sharing of Research Results: NSF Data Management Plan Requirements (National Science Foundation)

"Data sharing strengthens our collective capacity to meet scientific standards of openness by providing opportunities for further analysis, replication, verification and refinement of research findings. These opportunities enhance the development of fields of research and support the potential for cross-directorate activity. In addition, the greater availability of research data will contribute to improved training for graduate and undergraduate students, and make possible significant economies of scale through the secondary analysis of extant data. Finally, researchers have a special obligation to scientific openness and accountability when the research is publicly funded."

NSF Social and Economic Sciences Data Archiving Policy (National Science Foundation)

"Data sharing is essential for expedited translation of research results into knowledge, products, and procedures to improve human health. The NIH endorses the sharing of final research data to serve these and other important scientific goals. The NIH expects and supports the timely release and sharing of final research data from NIH-supported studies for use by other researchers. Starting with the October 1, 2003 receipt date, investigators submitting an NIH application seeking \$500,000 or more in direct costs in any single year are expected to include a plan for data sharing or state why data sharing is not possible."

National Institutes of Health. (2003, February 26).

Final NIH Statement on Sharing Research Data (Notice NOT-OD-03-032)

Ideological Diversity in Social and Personality Psychology: Questions and Answers



Yoel Inbar and Joris Lammers (Tilburg University)

Recently, we published an article regarding the political ideology of social-personality psychologists and its consequences (Inbar & Lammers, 2012). We described two surveys of professional social-personality psychologists in which we found that 1) though there is a predominance of political liberals in social-personality psychology, there is also more ideological diversity than people think, at least for economic and foreign-policy domains; 2) those who hold views conflicting with the perceived norm in the field—i.e., those who describe themselves as moderate or conservative—feel a hostile climate towards their views and are reluctant to express them to their colleagues for fear of negative professional consequences; 3) they are likely right to do so, as substantial numbers of respondents (although never a majority) say that they would discriminate against conservative colleagues professionally—and the more liberal respondents are, the more likely they are to say they would discriminate.

People had strong reactions to our article—in the published commentaries, in blog posts, on the SPSP e-mail list, and in emails to us. These comments raised many worthwhile questions, and we'd like to thank everyone who took the time to read and respond to our article. Here, we will try to answer some of the most frequent and important questions. We hope that doing so will clarify why we did these studies, what we think the results mean, and what we believe should happen next.

1. Who are you and why did you do these studies?

We are politically liberal social psychologists. One of us is a registered Democrat and volunteered for Barack Obama in 2008, the other is a member of the Dutch Green Party. We were trained as social psychologists, and we work in the same social psychology department. Admittedly, one of us has been accused of political bias before—albeit by conservatives, who did not appreciate his work on the relationship between disgust sensitivity and conservative ideology (e.g., Inbar, Pizarro, & Bloom, 2009).

We did these studies because we were curious and followed the data where it led us. We realize that publishing these studies may have short-term negative consequences for our field, but we believe that the results we describe are important and true, and that this warrants publishing them despite the fact that they may lead to bad publicity. Furthermore, as we'll explain a bit later, we hope that the long-term consequences for the scientific integrity and public credibility of social-personality psychology will be positive.

2. Isn't it a problem that you didn't ask people whether they'd discriminate against political liberals? Maybe people simply dislike agenda-driven research.

In a perfect world we would have asked the discrimination questions with liberal targets as well, but one of our concerns was maximizing response rate. In order to do so we needed the survey to be as short as possible, and that is why these questions were not included. However, we also believe that in a field with strong liberal norms it's unlikely that people would discriminate against political liberals (i.e., the in-group), or against work espousing a politically liberal (i.e., normative) perspective. Here's one example consistent with this belief: In the latest issue of *Social Justice Research*—a journal where social psychologists are very well-represented as editors and contributors—the editors-in-chief published a journal “mission statement” in which they called for more attention to “Marxist-oriented critiques of distributive justice theories” and (separately) for contributors to “conduct research exposing such incidents [of injustice], reveal social-structural mechanisms perpetuating injustices, and act as whistle blowers” (Törnblom & Kazemi, 2011). To be clear, we like this journal, our friends and colleagues publish there, and we largely agree with its political aims. We also don't think that *Social Justice Research* is representative of most social-personality psychology journals. Our point is simply that there *is* a place for agenda-driven research in psychology—as long as the agenda is agreeable to political liberals.

Finally, in the General Discussion of our paper, we give some other reasons to think a dislike of conservatives in particular is responsible for people's answers to the discrimination questions, including the finding that self-reported likelihood of discriminating is higher among more liberal respondents.

3. Isn't it possible that when people said they'd discriminate against conservatives, they meant that they'd do so unintentionally? After all, social psychologists know a lot about implicit biases—maybe they're just applying this research to themselves.

It's possible, but not likely. We asked people for comments at the end of the study, and almost no one mentioned this interpretation of the questions (one person referred to “possibly unintentionally” holding conservatives to a higher standard). Conversely, almost everyone who commented on the discrimination questions gave reasons or justifications for their answers that showed they were interpreting those questions as “what would you deliberately decide to do” (for example, “I would not be attracted to do a symposium with someone who is known to be conservative because this is to a large extent an interpersonal situation”). We believe that if people had been interpreting the questions as asking about unintentional and undesired bias, they would have been quite motivated to tell us so, for fear of being seen as explicitly endorsing attitudes that they in fact disavowed. Finally, of course, on either interpretation of the questions the answers are bad news for conservatives in social-personality psychology.

4. What about sampling issues? Isn't it a problem that you recruited your respondents from the SPSP e-mail list? Might a particular group have been more likely to respond, thus skewing the results?

Any sampling method may have problems with selective responding. Even random-digit dial surveys (the “gold standard” of survey research) can be biased by some groups being more likely to answer the phone or not, to hang up immediately or not, to have land-lines or not, and so on. We make the assumption that the SPSP listserv membership is roughly representative of our population of interest (professional social-personality psychologists)

and that there are no systematic differences between responders and non-responders on dimensions that would substantially bias the results. In the paper, we do our best to test these assumptions (by comparing the demographics of our sample and the entire SPSP membership) and do not find evidence that our respondents differ demographically from all SPSP members. We believe that it's difficult to come up with a selective response account that explains the lack of demographic differences between our sample and SPSP members and also accounts for the entire pattern of results. That's not to say that such an account is impossible, just unlikely.

5. Isn't it possible that conservatives just don't find social psychology appealing and opt out of the field (or never enter it)?

That may well happen; there is research showing that self-selection partly explains liberals' over-representation in academia (Gross & Fosse, 2012). However, clearly self-selection isn't the whole story. In our survey, conservatives (and moderates) told us that they felt inhibited from discussing their politics openly for fear of negative consequences. And a substantial minority of liberal respondents said that they would indeed discriminate against conservatives. This suggests that although some conservatives may opt out of psychology, others are being pushed out the door. This, to us, seems like a problem.

6. Do we really want conservatives in social-personality psychology? They are anti-scientific and believe things that are untrue. I certainly wouldn't want a creationist as a colleague!

We agree that some socially conservative beliefs, especially those that are religiously based, conflict with what most mainstream scientists believe to be true. However, as we point out in the paper, there are also many conservative beliefs that do not have an obvious truth value (for example that abortion is wrong, or that economic inequality is not in and of itself objectionable). More broadly, this sort of response seems to us to be an example of the hostile environment that our more conservative respondents describe. Consistent with what research on intergroup social perception would lead us to expect (e.g., Park & Rothbart, 1982), conservatives are seen as a stereotyped and homogenous outgroup, instead of as individuals holding a varied and nuanced set of political and moral beliefs.

Furthermore, as we report in the paper even those who describe themselves as politically *moderate* encounter a more hostile climate compared to liberals. In two analyses not reported in the paper, we checked whether more conservative economic and foreign-policy ideology predicted experiencing a more hostile climate, *even controlling for social conservatism*. In both cases the relationships were positive and highly significant ($p < .001$). Thus, it's not just social conservatives who encounter a hostile climate in social-personality psychology (and in fact, very few of our respondents described themselves as socially conservative at all). Political moderates and those who hold more conservative economic and foreign-policy views encounter a hostile climate as well. Surely this isn't fair to them, or good for social-personality psychology as a scientific discipline.

7. Let's say you're right about all of this. What should I do?

If you are part of the liberal majority, we believe that there are four things that you can easily do:

First, *don't presume your audience is uniformly liberal*. As our surveys show, this isn't true—over 30% of Survey 1's respondents described themselves as moderate or conservative on economic and foreign-policy issues. Nonetheless, in papers, conference presentations, and casual conversations, many social-personality psychologists assume that their audience consists entirely of political liberals. Making this assumption may mean that you are unintentionally alienating many of your listeners.

Second, *be especially careful around students*. There is an obvious power imbalance between students and faculty. Faculty must be careful not to take advantage of this imbalance to push their political beliefs on their students, even if only unintentionally. One post-doc who took our survey described being insulted publicly by a senior colleague for having voted Republican. Most of us realize that this is not acceptable, but we may not be as aware of the more subtle ways in which we are communicating what the "correct" political beliefs are. When talking to students, faculty should be mindful that students' political beliefs may differ from their own, and should work to avoid creating an environment where students feel excluded or intimidated because of their politics.

Third, *take conservative beliefs seriously*. Simply dismissing conservative beliefs as the product of ignorance, religious fanaticism, or stupidity is itself lazy and ignorant. Liberal social-personality psychologists need not be less critical of political ideas they disagree with, but they should criticize respectfully, remain open to discussion, and strive to avoid letting their political beliefs interfere with treating others professionally. It may also help to read serious publications that take a more conservative perspective. Perhaps you will disagree with what you read, but it will be educational.

Fourth, *practice tolerance*. We often need to interact with people we disagree with (most of us can probably think of at least one family member who meets this description). Generally, we manage to do this: If we disagree, we can disagree respectfully; if we find we are unable to disagree respectfully, we can avoid certain hot-button topics. Most working people manage to do this in their professional lives, as most professions are nowhere near as ideologically homogenous as psychology is. If so many people manage to tolerate those who disagree with them—if we ourselves are able to do so in many areas of life—is it so much to ask that we do it in our professional roles as well?

Conclusion: Benefits of greater ideological inclusivity

We believe that greater inclusivity will yield substantial long-term benefits for social-personality psychology. First and most important, we believe ideological diversity will lead to better science: It will highlight new research questions that we are currently overlooking and should make it less difficult to publish work that contradicts values important to political liberals (e.g., McCauley, Jussim, & Lee, 1995). Second, our scientific credibility depends on not being seen as ideological warriors by the public (see Tetlock, 1994). It is worrisome that the reaction to our research among many conservatives was “well, obviously.” If we are seen as ideologues instead of scientists, we will only convince those who already agree with us.

Finally, greater ideological inclusivity may also have personal benefits. Political discussion can be informative and educational, but only if it is more than an echo chamber of the like-minded—research shows that this is a recipe for ever-greater extremism and polarization (Myers & Lamm, 1975). As scientists, we also know that intellectual openness, freedom to dissent, and vigorous but respectful debate are our best weapons against complacency and error. Let’s make use of them now.

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Lights, Camera, Action (Research)!

G. Daniel Lassiter (Ohio University)



I was in the later stages of my doctoral training at the University of Virginia when I happened upon a photograph in *Time* magazine of a video-recorded police interrogation in which only the front of the suspect could be seen; the interrogator was not visible due to the position of the camera. (A short time later, I learned that this camera position was common for recording interrogations.) With the literature on social cognition fresh in my aspiring scholar's mind, I could not help but ponder the phenomenon of *illusory causation*—what stands out in our visual field is judged to be more causal or influential even if objectively such a conclusion is unjustified—and its implication for how observers of the video noted above might evaluate the voluntariness of the suspect's incriminating statements. Could the well intentioned desire to record the events transpiring in a police interrogation inadvertently introduce what I came to call the *camera perspective bias*? That is, would the mere visual prominence of suspects in video-recorded interrogations increase the tendency for observers to conclude that incriminating statements made by suspects were largely voluntary rather than a result of pressure on the part of less visually conspicuous interrogators, notwithstanding the true state of affairs?

After nearly 30 years of programmatic research on this question (where did the time go!), the answer is clearly, yes! I never imagined I would be working on this same issue for so long, but in the early days colleagues with whom I discussed the research always had questions that I could not answer with the initial data that were collected. Would observers who felt more accountable for their judgments (as actual jurors presumably would) continue to be affected by camera perspective? Would observers who deliberated the issues before making a judgment be less susceptible to the bias? Would having observers experience an entire trial context make a difference? Could the effect be replicated with a nonstudent population? Would it manifest with dichotomous as well as rating scale measures? Would the bias survive if observers were directly admonished not to let the camera angle influence them? Did it matter whether this admonishment was delivered before or after viewing of the video? Could the legal expertise of criminal trial judges or the real-life experience of veteran police interrogators make them immune to the biasing effect of camera perspective? Empirically answering these and other more theoretically focused questions (e.g., can it be demonstrated that visual attention is indeed a mediator of the camera perspective bias, and is the bias more a result of perceptual processes—how information is initially registered—or later conceptual processes—how registered information is elaborated, interpreted, or remembered?) has taken up the better part of my academic career, but good science always takes time and perseverance. Besides, it has been great fun working on these issues over the years with a number of talented graduate and undergraduate students and overcoming the obstacles that inevitably arise with any scholarly endeavor. As Trinity said to Neo in the film, *The Matrix*, “[i]t's the question that drives us...” (always wanted to use this quote!)

Sticking with this topic as a main focus of my research agenda has had its professional pluses and minuses over the years. For approximately the first two decades, there were probably more minuses than pluses. Doing incremental science was not the norm in the field (still isn't, but may be a bit more acceptable today); the purported path to academic success and notoriety was to constantly break new conceptual ground. Spending time on demarcating the boundaries, limitations, and real-world generalizability of a phenomenon was better suited for undergraduate honor's theses than first-rate scholarship. (I purposely don't include masters theses as well because faculty colleagues who sat on masters committees of several of my students chided me for bringing them yet another study on video-recorded interrogations. I believe it was good-natured ribbing, but the underlying point was clear—can you please do something different or more exciting!). But I made up my mind that I wanted to know just how robust and generalizable the camera perspective bias was even if I was the only one who really cared one way or the other, and that I was reasonably prepared for any professional “slings and arrows” that decision might bring. (A few others in the U.S. and in countries like Sweden and Korea were sufficiently intrigued to conduct their own research on the phenomenon and their results provide valuable independent and cross-cultural replications of the camera perspective bias.)

It is the plusses that have been the real surprise. I had no way of knowing when the research began that advances in DNA technology in the latter part of the 1980s would become instrumental in helping identify individuals who were wrongfully convicted and incarcerated by our system of jurisprudence. Disturbingly, the number of such individuals continues to grow; however, this has allowed for an examination of the causes of these miscarriages of

justice. It turns out that false confessions or false guilty pleas contributed to these lamentable errors in approximately 25% of the cases so far examined. Law enforcement officers, prosecutors, defense attorneys, state and federal trial court judges, reviewing court judges, legal scholars, social scientists, political leaders, and the public at large appear largely to share a common belief that video recording of interrogations, widely practiced, will greatly rectify the false-confession problem and the subsequent wrongful convictions to which false confessions all but inevitably lead. Because of this development there is a great desire among legal policy makers and practitioner's for research-based knowledge relevant to how best to implement the video-recording procedure. In contrast to most academics, these folks, not surprisingly, want to know how generalizable a scientific finding is—in this instance, the issue boils down to whether compelling evidence exists that the camera perspective bias translates to an authentic legal context—before basing their best practice recommendations on it. So, all those “boring” studies that looked at boundary conditions and emphasized external validity are now making a difference. Together they seem to have been persuasive in getting a typically skeptical legal community to seriously consider the merits of the research and its relevance to their decision making with regard to establishing appropriate video-recording guidelines.

I remember being asked during the first decade of the research program by the occasional interested journalist about when I expected the research to impact policy? I replied that it would likely be well into the future if at all, and I honestly thought that was the way it would go. But in the early 1990s I received word from a government official in New Zealand that a new nation-wide policy on video recording had gone into effect there and that based on our research it specifically required the camera to take an equal-focus perspective (profiles of the suspect and interrogator[s] both visible), which our early studies had shown tended to minimize bias. This was truly unexpected and I was both flabbergasted and thrilled that research in which I was involved could actually have this kind of real-world influence. Since then I have learned that the research has helped shape policy in parts of the U.S. and Canada. I'm most proud of the fact that in 2009, North Carolina, my birth state, was the first to pass legislation explicitly requiring an equal-focus camera perspective when video recording police interrogations (Go Tarheels!).

The social/cognitive literature continues to be a rich source for new questions and concerns about the video-recording practice that extend beyond the issue of camera perspective. In one line of investigation, my collaborator, Jennifer Ratcliff, and I have proposed that even an equal-focus camera perspective might not provide adequate protection for minority suspects. Drawing on research demonstrating that minority (in comparison to majority) group members receive more attention from majority group observers, we hypothesized that an equal-focus videotape that depicts a suspect who is a member of a minority group (e.g., an African American or a Chinese American) and an interrogator who is a member of the majority group (i.e., a Caucasian) may inadvertently introduce what we dubbed a *racial salience bias*—the tendency for majority group observers to attend more to minority suspects when the latter are paired with Caucasian interrogators, which, in turn, triggers illusory-causation-induced prejudicial judgments of the minority suspects. In this case, it is not the camera perspective that is directing disproportionate attention to the suspect, it is the fact that the suspect (but not the interrogator) is a minority group member and thus stands out more to majority group (i.e., Caucasian) observers. Three initial experiments demonstrate the racial salience bias is potentially a legitimate concern for the criminal justice system. A new NSF grant will facilitate continued exploration of this effect, including tests of possible strategies for minimizing it.

The social/cognitive literature also offers insights regarding possible ways to improve evaluations of video-recorded interrogations. Inspired by *construal level theory*, Shannon Pinegar and I have recently examined whether inducing an abstract or concrete level of mental construal prior to viewing video-recorded interrogations promotes more accurate assessments of a confession's reliability. Previous findings indicate that a concrete level of mental construal (said to involve narrow and individuating processing) produces a more detailed initial perception of an event and greater sensitivity to contextual factors that may constrain an observed other's behavior than does an abstract level of mental construal (said to involve broad and global processing). Therefore we hypothesized that a concrete construal level will better assist observers in detecting subtle coercive influences that may occur during an interrogation, allowing them to more accurately assess whether any incriminating statements that occur are indeed reliable evidence of guilt. Very preliminary results are encouraging and with the help of the aforementioned funding, Shannon and I will continue to pursue this promising line of investigation.

It looks like I will be hearing groans from fellow masters committee members for some time to come!

FUNDING

News from the Social Psychology Program at NSF



Rosanna E. Guadagno and Sally S. Dickerson, National Science Foundation

Greetings Social Psychology Community! We are writing with an update from the National Science Foundation. Thanks to the editors of Dialog for allowing us to share this news with you.

Staffing Changes

NSF would like to thank Dr. Chuck Stangor for his service as the temporary steward of the Social Psychology program. NSF now has two new Program Directors for Social Psychology, Dr. Sally Dickerson (sdickers@nsf.gov) and Dr. Rosanna Guadagno (rguadagn@nsf.gov). Dr. Dickerson rotates in from UC-Irvine and Dr. Guadagno joins NSF from the University of Alabama. Please do not hesitate to contact us with your funding questions – we are happy to serve the Social Psychology Community!

Most Recently Funded Grants

We received 163 grant submissions in FY2012. Of those, 18% were funded. Below is a list of investigators funded after our last panel session. Congratulations to the newly funded investigators!

Funded Primarily by the Social Psychology Program:

- Jason Clark, University of Iowa, & Luther Barden, Howard University: “Collaborative Research: Stereotype Validation and Intellectual Performance”
- Joseph Cesario, Michigan State University, “Automatic Responses as Situated Computational Processes”
- Amy Halberstadt, North Carolina State University, “A Dynamic Multidimensional Examination of Parental Socialization of Children’s Emotion Understanding and Social Competence in Middle Childhood” (grant supplement)
- Kent Harber, Rutgers University, Newark, “Psychosocial Resources and the Language of Interracial Feedback”
- Thomas Holtgraves, Ball State University, “The Role of Interpersonal Processes in the Interpretation of Uncertainty Terms”
- Mark Landau, University of Kansas, “Examining How Exposure to Metaphorical Framing Influences Attitudes”
- G. Daniel Lassiter, Ohio University, “Video-recorded Interrogations: Beyond Camera Perspective”
- Debbie Ma, California State University, Northridge, “RUI: The Role of Facial Physiognomy in Stereotypic Trait Inference”
- Saul Miller, University of Kentucky, & Joshua Ackerman, Massachusetts Institute of Technology, “Collaborative Research: Grounding the Behavioral Immune System in Mental and Physiological Processes”
- Michael Poulin, SUNY Buffalo, “Threat Reduction as a Novel Mechanism Linking Empathy and Prosocial Behavior”
- Marjorie Rhodes, New York University, “The Development of Social Essentialism”
- Diana Sanchez, Rutgers University, New Brunswick, “Exposure to Biracial Americans and Changes in Essentialist Beliefs”
- Valerie Taylor, Spelman College, “Deflecting Ingroup Members’ Stereotype-Confirming Behavior: Factors that Facilitate High Performance and Positive”
- Daryl Wout, John Jay College of Criminal Justice, CUNY, “Creating a Diverse Society that Works: Investigating the Role of Social Identity Threat in Interracial Interactions”

Co-Funded by the Social Psychology Program:

- Eli Berman, University of California, San Diego, “Transforming Security Research Workshop” (Primarily funded by Decision, Risk & Management Sciences)
- Mary Campbell, University of Iowa, “Health Care Providers and Patient Interactions” (Primarily funded by Sociology)
- Fiery Cushman, Brown University, “Investigating the functional match between punishment and learning” (Primarily funded by Law & Social Sciences)
- James Druckman, Northwestern University, “Time-Sharing Experiments for the Social Sciences (TESS): Proposal for Renewed Support, 2012-2015” (Primarily funded by Political Science)
- Emily Martin, New York University, “The Recovery of Introspection: An Ethnography and History of Experimental Psychology” (Primarily funded by Cultural Anthropology)

Co-Funded INSPIRE Grants:

- John Jost, New York University, “INSPIRE: Computer Learning of Dynamical Systems to Investigate Cognitive and Motivational Effects of Social Media Use on Political Participation”
- Ali Minai, University of Cincinnati, “INSPIRE: The Hunting of the Spark: A Systematic Study of Natural Creativity in Human Networks”

Budget

We expect that the government will be operating under a continuing resolution for some time and how that might impact NSF is unclear. Rest assured, we are doing everything we can to fund as much Social Psychology research as our budget will allow. Keep your eyes out for interdisciplinary calls that involve Social Psychology, such as sustainability. At NSF, the future is bright for our field!

NSF Funding Opportunities

Keep your eye on the Social Psychology webpage

(http://www.nsf.gov/funding/pgm_summ.jsp?pims_id=5712&org=BCS&from=home) for news about the program itself. In addition there are opportunities and news items that cross-cut the Foundation so you should also monitor NSF homepage www.nsf.gov.

INSPIRE

INSPIRE stands for Integrated NSF Support Promoting Interdisciplinary Research and Education. This program addresses complex and urgent cross-disciplinary problems within the scientific community. Owing to this interdisciplinary nature, contributions to the funded proposals come from programs throughout NSF. The maximum budget for an INSPIRE grant in FY12 was \$1 million dollars. It is anticipated that the INSPIRE program will be repeated in FY2013.

For more information on the INSPIRE Program, visit this webpage:

http://www.nsf.gov/news/news_summ.jsp?cntn_id=124898&org=OIA&from=news

RUI

RUI stands for Research in Undergraduate Institutions. These proposals have same target dates as are submitted to the Social Psychology program and are due at our standard January and July dates. Both two and four year institutions qualify for this type of award, as do some doctoral granting institutions provided they “award no more than an average of 10 Ph.D. and/or D.Sc. degrees per year in all disciplines that NSF supports, averaged over 2 to 5 years preceding proposal submission.” The intent of this award is to support high quality research conducted at institutions that are primarily undergraduate. If you are interested in this opportunity, check with your sponsored programs office to see if your institution is eligible.

For more information, see: http://www.nsf.gov/publications/pub_summ.jsp?ods_key=nsf00144

CAREER

The Faculty Early Career Development program (CAREER) is a prestigious Foundation-wide program that provides support to junior faculty who excel at the roles of teacher and scholar as evidenced by outstanding research, excellence in education and the integration of the two. NSF welcomes submission of CAREER proposals from junior faculty members at all CAREER-eligible organizations. The next deadline is July 24, 2013.

For more information, see: http://www.nsf.gov/funding/pgm_summ.jsp?pims_id=503214

Other Relevant Funding Opportunities

Many other opportunities for research support exist within NSF. We would be pleased to assist you in discussing your projects and finding it the proper ‘home.’ Keep in mind that there are other funding programs outside of NSF that may be of interest to our community. Information on one such opportunity appears below.

Minerva

The Minerva program topics and corresponding subtopics illustrate social science questions of specific interest to the DoD. Disciplinary approaches of interest include but are not limited to anthropology, cognitive science, demography, economics, geography, history, political science, psychology, sociology, and computational sciences. Interdisciplinary approaches are encouraged. The next deadline is 12/12/12.

New Minerva BAA for FY13: at <http://www.onr.navy.mil/Contracts-Grants/Funding-Opportunities/Broad-Agency-Announcements.aspx>

Thank You Again!

We are delighted to serve our community as representatives of the National Science Foundation. Please let us know what we can do to help you and keep on the lookout for emails from us with new program solicitations.

GRADUATE STUDENT PERSPECTIVES

What Good is a PhD? Putting it in Perspective



Paul Conway (The University of Western Ontario)

What good is a PhD? I know that many have asked themselves this question—often using a sardonic tone. Whether you are a graduate student, post-doc, young faculty, or an experienced researcher, chances are you are familiar with the various tribulations facing today’s graduate students: Tenure-track positions are shrinking, part-time faculty jobs are increasing, and many of us know someone who has had a rough couple of years on the job market. Yet, graduate school is not easy! Balancing classes, research, writing, teaching, and hopefully a semblance of a life is no small feat, and expensive to boot. It can be alluring to slip into cynicism and question whether a PhD is really worth it. Resist this urge! There is much value in a PhD if you take the right perspective.

Historically, jobs have not existed until there was a need for them—this is an under-appreciated insight from our sister discipline of anthropology. Thousands of years ago, there was only one career available: hunting and gathering. The beginnings of permanent human settlements around 10,000 years ago brought about increasing specialization: Career options expanded to include farming, blacksmithing, and trading. Increasing wealth made communities targets for brigands, so a professional warrior class emerged, along with professional bureaucrats (after all, *someone* needed to oversee—and extract taxes from—all the activity in the community). The advent of bureaucracy demanded important new skills like reading, writing, and managing basic information (e.g., tracking the number of cows and sheep in the community).

As history unfolded and human settlements expanded in size and complexity, human activity became untethered from the immediate community—soon there were far-flung empires to manage, religious messages to spread, and international businesses to oversee. The massive increase in the complexity of human interaction demanded new understandings of politics, language, geography, and mathematics, all of which required ever-increasing levels of sophistication in understanding, organising, and creating information. The first universities were founded at places like Oxford and Heidelberg to train specialists in these disciplines.

As the world moved through the Enlightenment, advances in chemistry, medicine, and engineering facilitated ever-larger urban populations and ever-increasing industrialization. Although great strides in human knowledge had propelled us from humble bands of hunter-gatherers to urban, globetrotting industrialists, the realities of modern living presented whole new sets of challenges that escaped the purview of traditional ‘hard’ sciences: The 1800s saw waves of unrest and popular uprising across much of the world, followed by revolutions in several of the largest countries in the 1900s, not to mention a Great Depression, two World Wars, social upheaval, and a Cold War. Grappling with these challenges required new insight into the nature of human beings themselves—the purview of the social sciences.

A century and a half ago, as it was becoming more and more apparent that the world’s problems require a sophisticated understanding of human behavior, psychology finally emerged as a distinct discipline of scientific inquiry. Since then, the world has not slowed down for one moment, and the array of troubles facing humanity continues to expand: Challenges ranging from climate change to economic instability to terrorism dominate the Internet age, and we are rapidly approaching revolutions in biotechnology and nanotechnology that have the potential to change human experience beyond fathoming. Adjusting to all of these challenges is a matter of greatest import, and will require above all changes in human behavior. Therefore, the science of psychology has more to offer the world than ever.

Sometimes the scramble of academic life lends itself to a focus on the concrete—writing that grant, publishing that paper, teaching that class—rather than the abstract big picture of *why* we are all doing this. Yes, psychology in 2012 is a radically specialized discipline; yes, it is unlikely that you will be well-read outside your immediate specialization; yes, it can seem as though the things we do are tenuously connected at best to the broader world we live in. Yet, if you take the long view, you will see that psychology emerged as the need for it became apparent, and that need is only increasing. Bear in mind the scientific goals of understanding, prediction, and control. Without basic psychological research, we would never have the understanding needed to predict and exert control over the potential disasters waiting around the next corner of history.

Finally, consider your unique position to contribute to this legacy of knowledge: if you were born thousands of years ago your job options would have been drastically limited. Even in the modern world, only 6.7% of the over 8 billion people on earth have a college degree of any kind (Barro & Lee, 2010)! It is a happy accident of history that you find yourself in the 21st Century, living in one of the few places wealthy and stable enough to afford you the opportunity to probe the secrets of the human mind, yet plagued by ills that desperately require your input. What a rare privilege! Make good use of it—the rest of the world is counting on you.

References

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ENTERTAINMENT

Paula Niedenthal: *Academix*



ACADEMIX: CONFERENCE TIME SLOTS



TRAVEL

RANDom Thoughts: An Insider's Perspective on Being a Social Psychologist at RAND

Kerry A. Reynolds, Behavioral/Social Scientist (RAND)



When I tell social psychology colleagues that I work at RAND, I usually get a raised eyebrow in response, quickly followed by a series of questions. Colleagues are by turns suspicious, curious, intrigued and surprised that a social psychologist can find fulfilling work outside the world of academia. When they learn that the bulk of my work week consists of research, and that I really enjoy my work, they are often genuinely surprised. Inevitably, the conversation turns to the questions “What is RAND?” “What kind of research do you do?” and “Are you happy there?” Though I can’t speak for everyone at RAND, or even all psychologists at RAND, I hope that I can provide some insight on what it’s like to work here.

First, a little bit of background on RAND as an organization. Approximately 1,700 people from more than 50 countries work at RAND, representing diversity in work experience; academic training; political and ideological outlook; and race, gender, and ethnicity. More than 1,000 of these staff members are research professionals whose collective expertise spans nearly every academic field and profession—from economics and behavioral science to medicine and engineering. In fact, psychologists are the second largest disciplinary group next to economists at RAND with 50 psychology Ph.D.s, many of whom are trained specifically as social psychologists. With expertise from academia, government, and industry, RAND researchers combine theory with real-world experience to find solutions to today’s difficult, sensitive, and important problems. Our research is supported through a wide variety of sources, including government agencies like the National Institutes of Health (NIH), National Science Foundation (NSF), Agency for Healthcare Research and Quality (AHRQ), the U.S. Department of Defense, Army and Air Force, as well as foundations, other nonprofit organizations, and private-sector firms. As an organization, RAND is dedicated to high-quality and objective research and analysis (www.rand.org).

One of the main differences between the philosophy of work at RAND and traditional psychology departments is the relative emphasis on the application of theory as opposed to the genesis of theory. At RAND, theory is critical not for its intellectual value alone, but for its ability to inform the search for a solution to a real-world problem. Sometimes our application of theories suggests important refinements and limitations to the external validity of theories, but such findings are typically not the focus of what we do. Because my research at RAND is almost always problem-focused, I typically get to see the findings translated into action. For example, I’m currently working on a project that has improved screening for depression and connection to resources for new moms. This community project will continue to assist these women after our research project has been completed. Another current project is focused on identifying the unmet needs of cancer survivors and will be used to generate relevant programs and services. Almost all of the projects I work on have the potential to make a positive difference in the world (sooner as opposed to later) – which is one of the very gratifying parts of my job at RAND.

In addition to a focus on more applied work, there are also lots of other differences between RAND and academic settings, and depending on your perspective, these could be good things or bad things. RAND isn’t for everyone, but it can be a great fit for those looking for something a little different from a typical tenure track position in a psychology department. I’ve tried to categorize some of the main points in the sections below.

All Research, All the Time. Although RAND’s graduate school (which confers more Ph.D.’s in policy analysis than any other institution) as well as affiliated universities provide opportunities for RAND researchers to teach, there are no teaching requirements at RAND. There are also few administrative requirements (although again, opportunities exist for those with interest), which means that research staff at RAND spend most of their time doing research. If you love doing research, this can be an amazing opportunity to focus your efforts on the activities you like best. Of course, that also means that you need to keep a full calendar of ongoing research projects. I like to think of the RAND environment as ‘soft money with support.’ Almost all of RAND research is externally funded. Technically, it’s up to you to bring in work or join projects that fill your calendar and meet your personal and professional goals. However, there are built-in supports to assist staff in making connections with others, as well as managerial and financial supports to assist people should they find themselves with less coverage than desirable. The downside to needing to keep a full calendar of projects is that it is sometimes

necessary to accept work further from your area of interest. However, these “stretch” projects often provide opportunities to work with new colleagues, broaden your skills or expertise, or apply the skills you have in a different substantive area, which can provide exciting opportunities while working on the project and can open the door to other new opportunities down the road. As I describe below, this leads to another difference between RAND and academia, which is an emphasis on breadth as well as depth.

Breadth vs. Depth. In the academic world, researchers are often highly specialized in their area of study. While there are certainly researchers at RAND who focus their work on only one narrow area, it is typical for researchers to work on multiple projects that span a variety of topic areas, especially early in their careers when they are more dependent on others for coverage opportunities. Most researchers eventually develop areas of substantive expertise for which they develop national reputations, but some focus instead on applying a specialized skill or method to a variety of projects with differing subject matter. Nonetheless, almost all researchers at RAND are willing and excited to engage in new opportunities simply because they find an idea interesting or believe it is important. For researchers who are interested in working on multiple topics, this provides an opportunity that is rarely available in the academic world. Moreover, the cross-fertilization that takes place when people work across different areas invariably helps them bring new perspectives and insights to the work in which they specialize.

Pathways to Success. In most research-oriented academic psychology departments, it’s “publish or perish,” with the number of high-quality, high-impact journal articles produced providing a metric of success and an indicator of the likelihood of achieving tenure. At RAND, this is also a potential pathway to success, but it’s not the *only* pathway to success – other paths also exist. Researchers at RAND work on a variety of projects – some are funded by grants, others are funded by contracts or even private sector clients (though RAND will not do proprietary work and always insists on the right to independently publish findings of its research). Grant work often lends itself to publishing research findings in academic journals, but contract work typically culminates in a report and/or other final product, many of which are peer reviewed and publicly available, (e.g., a technical report or Congressional briefing) that addresses the specific needs of policymakers, clients, or government officials. Success in either of these domains is considered valid and important, and is weighed in the promotion process. Many RAND researchers also participate in other professional research activities that are typical for our colleagues at academic institutions, such as presenting papers at national or international conferences, reviewing articles for journal and conference, serving on journal editorial boards, and serving on study sections or review panels for organizations like NIH and NSF.

Multidisciplinary Environment. RAND doesn’t have departments organized by discipline in the same way that academic institutions do. There are opportunities to interact with others from a similar disciplinary background (e.g., psychology speaker series), but most of our work happens in multidisciplinary teams. This type of work can be both exciting and challenging. When solving big-picture, real-world problems, it makes a lot of sense to convene expert opinions from a variety of fields. However, there are challenges to conducting work this way: different substantive areas have very different ways of framing problems, use different methodologies, and have different standards for interpreting data. Fortunately, when working toward a common goal, these various perspectives are eventually integrated or the team reaches a consensus on how to proceed. The end result is typically a more broadly applicable, implementable solution than could be reached by one discipline alone. As an individual, working in a multidisciplinary team can be daunting—you might be the only representative on the team from your discipline (How can I speak for all of psychology?). There are two keys to successfully navigating these situations: (1) Knowing what you know, and (2) Knowing what you don’t know. In other words, it would be impossible to know everything that’s ever been done in psychology; no one would reasonably expect this of you. But as a psychologist, you have familiarity with many relevant perspectives and theories that can assist in helping a multidisciplinary team to achieve a balanced view of an issue. And, if you know what might be relevant, but don’t know much about it, there may be an opportunity to spend some time learning about the area so that you can help the team move forward. The breadth of expertise at RAND also means that there are other psychologists you can enlist to give input when you need assistance.

Resources. RAND is a place focused on getting research done, so the organization’s infrastructure provides an incredible number of resources to make the process easier for researchers. We have amazing budget staff (who do everything from assisting with assembly of proposal budgets to helping you re-budget during the middle of a project if necessary), research support staff (including research assistants and project associates, who typically

have master's degrees and lots of prior experience), statistical support (e.g., statistician consulting services), communications analysts (who can assist with writing and editing), research programmers, web developers, a publications department, a survey research group, administrative support, and more. All of this support means that you can work efficiently toward the goals of your research without spending precious time and energy setting things up. The downside is that having these resources available is expensive. Without tuition from students to help offset overhead costs, our overhead rate is rather high compared to universities, which can sometimes make it difficult to pursue certain types of opportunities.

Family-friendliness & Work-life balance. I've attended several presentations by successful academic women about how to have a family while maintaining your career trajectory, and I have always left these talks feeling depressed. It always seemed to me that even if you could manage to have a child (probably only one) and keep yourself on track for tenure in a research-oriented department, that there would still be the lingering suspicion among your colleagues that you weren't really dedicated to your career if you chose that path. I also worried that I wouldn't be able to devote the time or attention I felt were important to being an involved parent. As a woman who values both my career and family, this felt like an impossible situation. I recognize that this perspective may not accurately reflect all departments or all academic institutions, but I definitely felt discouraged. At RAND, I have never felt that I have to choose between a successful career and a family, which makes me incredibly happy. In fact, later this fall, we'll be welcoming our second child into our family. While it's true that it's not always easy to balance career and family, I have never, ever sensed even a whisper of disapproval from my colleagues. In fact, many of my male *and* female colleagues at RAND are in dual-career couples and have children (often more than one). This atmosphere of acceptance has helped me to thrive, not just in my career (I was recently promoted), but also in my personal life. If I need to solicit the insight of another career mother with young children, I need only open the door of my office and walk down the hall.

In sum, RAND is an interesting place for a psychologist to work, especially for those who enjoy applied work, don't mind broadening their area of research, and are excited by the possibility of working in multi-disciplinary teams. RAND has major offices in a variety of places across the country (Santa Monica, CA; Washington, DC; Pittsburgh, PA; and Boston, MA) as well as smaller offices in a variety of locations around the world, meaning that there are plenty of options to find the right fit.

To learn more about RAND, including employment opportunities, visit www.rand.org. For more information on the wide variety of research topics studied at RAND, click on the "Research Areas" tab, where you can see a sampling of recent work in our main categories of focus, which include areas like *Health, Education, Law, Science and Technology*, and many others.

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MEDIA

Ignore the Content and Celebrate the Styles

Charles Stangor, University of Maryland



Social psychologists study some pretty arcane topics, and understanding the use of "style words" in the English language seems to fall into that category. But James Pennebaker's new book, *The Secret Life of Pronouns: What Our Words Say About Us* (Bloomsbury) takes us on a travelogue into the remarkable world of language, and provides a wide set of data showing how our pronouns, articles, conjunctions, negations, and quantifiers reliably reveal our inner personalities and predict our behaviors – from academic achievement to suicide.

Pennebaker's "travelogue" ranges widely, stopping along the way to consider more traditional social psychological topics such as detecting deception, close relationships, and the influence of sex, age, race, social class, and power differences on language use.

But Pennebaker's journey ranges beyond social psychology's usual domains, including analyses of language in speeches, plays, poetry, movies, among airline pilots and co-pilots, and in everyday conversation, Beatles lyrics, Wikipedia, and even the correspondence between Freud and Jung.

This research is timely, as the databases of available writing samples are exploding. Pennebaker gets his voluminous data from a variety of sources, including the Nixon White House tapes (or what remains of them), email conversations, instant messages, Wikipedia, and of course Twitter. If you Tweet regularly you can get an analysis of your own personality here: <http://www.analyzewords.com>

Pennebaker argues that word use is "associated with almost every dimension of social psychology that I had ever studied," and he provides countless examples of these. He focuses in large part on the 20 "stealth" or "style" words that make up over 30 percent of text ("I, the, and, to, a, of, that, in, it, my, is, you was, for, have, with, he, me, on, but).

Consider your own emails: You'll find, for instance, that when you are writing to superiors you will use fewer I-words, more you-words, and more we-words, in comparison to when you are writing to subordinates.

Pennebaker spends some time addressing how languages vary in their use of function verbs, and gets into the nitty-gritty of the problems of linguistic analyses (Does the word "we" signal connections with others or the tendency to deflect self to a broader subject?)

The statistical analyses of words helps us put some more intuitive but less valid claims to rest, for instance the conservative idea that President Obama is a selfish president because he uses "I" words a lot (statistically he's lower than average).

Pennebaker's writing style is engaging, but not particularly colloquial. He is primarily a scholar, and this is a scholarly book. He carefully documents the contributions of each of the students that he worked with, something that I greatly appreciated.

In the end, there are some clear disappointments, but these are not so much due to Pennebaker, but rather with the nature of the data he's collected. For one, the use of functions words is not causal of these outcomes he studies – only reflective. And we usually don't know if language is predicting the outcomes better than any of many other variables we might think of.

Perhaps Pennebaker is right, that the little words mean more than the big ones do, but in some ways he hasn't completely convinced me. Will Josephine be a good friend, roommate or marriage partner? I guess I'd prefer to know how often she says "Jesus," "Shotgun," "Dirt bike," "Expressionism," or "Latte" rather than how many time she says "I" or "you." But then again, I could be completely wrong about that.

Book Information:

Pennebaker, James W (2011-08-23). *The Secret Life of Pronouns: What Our Words Say About Us* (Kindle Location 2191). Bloomsbury Publishing Plc. Kindle Edition.