

# DIALOGUE

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Diane Quinn &  
Hart Blanton

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## Changes Came, Changes Coming: Dialogue Goes Online!

Hart Blanton & Diane Quinn, University of Connecticut

Three years ago, when the two of us started entertaining the idea of editing *Dialogue*, we interviewed some of our friends in the field to ask what they liked about the newsletter and what they thought might change. The one recurring gripe we heard – almost to a person – was a desire to see *Dialogue* move to an online format. “Why am I still getting it in my mailbox?” some would ask. “Why am I not getting it in my mailbox?” others with out-of-date address information would query. With what we perceived to be a broad consensus in a field known for its diversity of opinion, we submitted a proposal for moving *Dialogue* into the digital era as part of our application to take over the editorial posts of our newsletter. “Let’s kill the print edition and move *Dialogue* online,” our pithy proposal proposed.

We must have given the impression we had a strong vision for the newsletter, as our submission did earn us the opportunity to edit *Dialogue*. However, after we began our editorial terms and started attending the biannual meetings of the Executive Committee, we came to realize that we had been soliciting the opinions of a nonrandom sample of members. Yes, we had fallen prey to a selection effect. It turns out that we not only hang out in social groups that have an unhealthy faith in the power of 2 x 2 factorial designs to uncover the complexities of the universe (a limitation we already appreciated), we also select into cliques that like getting their news online. The world is a more complicated place than this, we learned. Some psychologists like correlational studies and think print news should be ... printed. Through our new roles as editors, we began to meet members of our society who were more than a little attached to the print version of *Dialogue*.

*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.



"I always look forward to it arriving in my mailbox," said a member who clearly had an up-to-date mailing address. "I like reading it on my flight to the convention," said another member. "It's the only publication I get that finds its way with me to the bathroom," opined an oversharing and eminent researcher in the field. Truth be known, as we became more involved in the workings of the publication – and got to do such things as choose the style and color of the new font – we too developed greater affection for the print version. And we will admit that it was with no small amount of pride that we saw our product appearing in our colleagues' mailboxes. Moreover, just this last year, on the last-leg flight to the convention (that last leg where everyone on board the plane is either a psychologist or a screaming baby), we could not help but notice that our version of the newsletter was also being carried by members onto the plane. We can only imagine the other places some of these copies might have been.

But there is no standing in the way of progress. A healthy science remembers history and rejects nostalgia, and there is little to no utility of producing hard copies of *Dialogue* (or most any publication, we think). Moreover, during the course of our three years of editing *Dialogue*, we have detected a change in attitudes on this issue. Even those who were sentimental about the print edition came to appreciate that an online version of the newsletter would better serve our members. Online, we can move more information to members with fewer production costs. We can also heighten the visibility and access of our newsletter to non-members. Further, with an online platform in place, our stories can wend along unanticipated paths created by social networking connections. It is only a matter of time before a *Dialogue* article goes "viral," we hope. If that possibility isn't reason enough to end the print edition, we also point that an online edition is "green" and for many who considered this, it was cause to kill the print edition. Sure, some members may resist this change as they can no longer receive a physical version of *Dialogue* they can take with them on their flights. But worry not. It is with great joy that we report that after months of negotiation, Jack Dovidio has agreed to buy all members a cell phone with a data plan. Through his charity, any member can now download the online version of *Dialogue* before the convention flight. (\*Disclaimer: The above is a joke. You can, however, read *Dialogue* on the plane with your iPad, except during takeoff and landing when all electronic devices must be turned off and stowed.)

### ***New Format, New Content***

With the move to a new format, we have had the opportunity to critically examine the role that *Dialogue* has played as the primary newsletter for the society and the role it can and should play now. One concern that has been given careful consideration is how this storied publication should coordinate with and around SPSP's new web presence ([www.spsp.org](http://www.spsp.org)). If you have not yet checked it out, we encourage you to take a tour of the rest of the pages at SPSP.org. There you will find useful links to SPSP's resources, including the executive committee reports, our newsroom (now staffed by our public information officer Lisa Munoz), member services, and other valuable resources created by our web co-editors, David Dunning and Don Forsyth. We think you will see that our new webpage is dynamic and versatile in ways the earlier version was not—it is designed to increase interaction among members, rather than to just present information. Check it out now and keep checking it out as it will be undergoing additional changes over the coming months and year.

As fortunate as it is to have this great new resource on the web for our society, its presence has forced us to identify the unique role that an online version of *Dialogue* might play and how this should differ from the roles being played by the new, multi-purpose webpage. After consulting in depth with society members and seeking guidance from the executive committee, we have determined that the move online and the resulting coordination between the newsletter and webpage has necessitated a facelift for *Dialogue*. In the past, the newsletter has served two primary purposes. One has been to inform and the other to engage. In its informing function, *Dialogue* has been the primary source allowing members to review minutes of the executive committee meetings, plot the progress of committees through the biannual chair reports and learn important details about our annual conference. In its engagement function, the editors of *Dialogue* have exercised their creative centers to find new ways to interest and entertain members. To this end they have recruited perspective pieces on recently published articles, written and solicited commentaries on the state of our field, published book reviews and gone to great lengths to produce no small number of crossword puzzles, anagram tasks, fake advice columns, and cartoons. And every now and again, *Dialogue* has also published satirical research articles that have been almost as good as their authors believe them to be.

In our new online presence, *Dialogue* will be dropping its first function to more vigorously pursue its second. More than any online newsletter can be, the revamped SPSP webpage is geared to be fast and agile – far more nimble than *Dialogue* ever was when it functioned as the primary news source for the society. The webpage can disseminate important information in a timely fashion, and when that information goes out of date, stale news can be taken down and replaced with fresher reports. In short, the webpage has pushed *Dialogue* aside as the primary vehicle for information dissemination in the society – as it should. The new webpage will also seek to engage members with entertaining news and columns but here as well, the webpage will be characterized by its more dynamic pace of information flow. To get a sense of what we mean by this, we encourage readers to take note of short, blog-like posts at [PSPConnections](#) and to review the stories in our [newsfeed](#). We think members who do this will find that the new webpage is far more useful than a print version of *Dialogue* ever could have been.

But what is left for *Dialogue*? Plenty, we think. As editors of this newsletter, we have embraced and even pushed for the changes in the webpage as they free us up to more vigorously explore the creative sides of *Dialogue*. As a biannual publication, *Dialogue* will now shift from being what we might term as a “newsletter” to being more of a “magazine.” This society publication will become the primary source for in-depth writing and reporting on the field. Look to future editions of *Dialogue* to focus greater efforts on member reporting. Look to *Dialogue* to cover debates in our field, with input and reactions from those involved in these debates. Look to us as a resource where graduate students and junior faculty can get advice from senior colleagues and where members can learn about important social policies that affect the future of our science. In short, look to *Dialogue* as a publication that you enjoy reading – and one that you can read, whether you’ve remembered to give the society your most recent mailing address or not. We think you will see a hint of where we are going in the current issue, but the metamorphosis has just begun.

### ***More Changes, Comin’***

One important way that the future online version of *Dialogue* will change is in its stewardship. It is with great regret that one of us (Hart) has to inform you that he is being asked to start pulling his editorial weight. This has to happen because his valued co-editor (Diane) has decided to step down. After three years co-editing *Dialogue* (and 2 years editing *PSPB*), Diane has decided it is time for her learn how to sabbatical like someone who knows how to sabbatical. She will be on academic leave next year, and instead of using her extra time to revamp and co-edit *Dialogue*, she plans to spend it learning such things as, how one sets the vacation message on a Gmail inbox and how to enjoy this thing called “free time” she hears others talk about. She might even use this opportunity to look up the term “hobby” in a dictionary, although that may be pushing it too far. Hart’s contract with *Dialogue* also ends this year and so he, technically, becomes a free agent. There are rumors he will stay, however. He currently is in contract negotiations with SPSP to continue as a co-editor and more reporting on this will follow. True, there are some reports that there might be an aggressive bid to get him to play ball for the *Society of Socially Experimental Psychologists*, the *Society for the Study of Psychologists with Issues* or possibly the *New York Yankees*. Whether with him or without him, a search committee has formed to locate new talent to help edit the next four years of *Dialogue*. This is an exciting opportunity, as the future of *Dialogue* need not look like its past, nor must it look like the issue you are viewing now. The next editors of *Dialogue* will have wide latitude to define a new magazine and its scope and mission. To this end, they will be assembling a larger group of other interested individuals who will help them fulfill the new “member engagement” mission of this new magazine. The *Dialogue* publication group will include a crack team of columnists, reporters and other semi-regular contributors, all of whom will work together to ensure that the new face of *Dialogue* will continue to engage both the new and the old faces in our field.

### **Dialogue Editor**

SPSP is seeking a new editor or editors for *Dialogue*, the newsletter of the society. *Dialogue* puts out two issues a year, and the new editor(s) could step in for the Fall issue, with assistance from the current team (Hart Blanton and Diane Quinn). The new editor(s) will play a fundamental role in shaping the content and format of the *Dialogue* as it moves from print format to an on-line format on the new SPSP website. Interested individuals should contact Monica Biernat at [Biernat@ku.edu](mailto:Biernat@ku.edu) by 3/15/12. Please provide a brief “vision” statement outlining goals for the content and/or appearance of the newsletter. Editors are provided a small stipend from the society, and typically serve a 4-year term.

## **SPECIAL ISSUE: PERSPECTIVES ON OUR DATA**

### **Unexpected Lessons on Scientific Ethics**

Camille Johnson, San Jose State University



In my business course, I give only one lecture on ethics. Like you, I always thought that my wannabe CEOs probably needed more than one lecture on ethics. After all, we all know how corrupt bankers and other business people are, especially compared to people like me – me and my fellow enlightened academics who toil in the pursuit of knowledge, illuminating the puzzles of human behavior. In contrast to the poor behavior of politicians and financiers, it was easy to believe that ethical failings (or prejudice or sexism) did not apply to my circle of colleagues.

But, of course, this fall I learned all too well that ethical scandals are not just some other field's problem, someone else's problem. I found myself yanked from my comfortable assumptions about my field, my discipline, and my friends, and thrown, if not in the middle of, then certainly in very near proximity to the epicenter of a scandal that has reverberated through our field. To say the least, it was horrifying and shocking to find that a friend and a mentor had committed wholesale fraud. Seeing events related to my life in the news, on science blogs, and even in the posts of friends of friends on Facebook, and having to explain the import of these events to my family, friends, and colleagues outside my field, was surreal. Each rumor, every article, and then the preliminary Levelt report hammered home the fact that, despite my prior beliefs, this *was* happening to me and that I could suffer some consequence. I described it as a feeling like someone or something had died, although I did not know if the feelings arose from the death of a friendship or the realization that the friendship had never really existed.

Not enough time has passed to provide true perspective on these “unfortunate events” (or the “Stapel debacle” as the clever flippantly call it). As a field, and as individual co-authors, we are waiting for the other shoe to drop and bracing for the next wave to hit. Only once the full scope of the fraud and the consequences for our personal careers, and the field as a whole, are revealed, can we begin to understand what has happened to us. But, there are some early lessons to be drawn.

Foremost, it has become apparent that ethics are not someone else's problem. There are rumblings in the field of data repositories and increased ethics training for graduate students, strident voices calling for a pound of flesh, but it feels like those voices miss the point. These solutions could too easily make the future of our field as an ethical discipline someone else's problem. Consider data repositories. In the age of m-Turk and on-line data collection, data are easily and quickly collected, and a careful and knowledgeable statistician could create reasonable data files out of thin air for submission to such repositories. In addition, repositories could become our version of airport security measures: the establishment of one very public deterrent to destructive events allowing us to relax despite very real threats that still exist. The fact that data are publically available might allow us to go about our business certain that if bad acts do occur, “someone” will find them. In fact, if inconsistencies in datafiles can be detected, who will be the person who seeks out and finds the flaws in these thousands of datasets? What would be the motivation to find these inconsistencies, and would that motivation be trusted? Would discovery lead to job offers or stature in the field? Tenure? We don't have time to closely read all the articles that are published in a given month; who has the time to analyze someone else's data? Assuming that someone does have enough interest to conduct a meta-analysis or other forensic analysis, what is that person supposed to do with this knowledge? Rumor apparently followed Diederik for years, but took over 10 years to come to fruition. We need to realize that ethical lapses may occur more frequently than we would like to admit, whether because of professional pressures or lack of moral character, and we need to create an alternative channel to whispering and gossip for addressing concerns about questionable behavior.

In telling my story over the past few months, I discovered that among the many emotions I was feeling, there was a hint of relief that I wasn't the person who had found him out. Of course, it would not have been me - I trusted him no more or less than I trust my current research partners in distant places and was not in a position to suspect his specific data collection stories. But, nonetheless, there was a breath of relief. What, specifically, would I have done? Who would I have told? Who would have believed me? While I received an excellent education in ethics and ethical practice as a graduate student, nothing in my training had truly prepared me to respond when I encountered a questionable practice. Instead, my experiences taught me that it was unlikely that I would ever

encounter a questionable practice so egregious as to require action, and that the consequences of questioning another psychologist could be as costly to me as to them. I believed that someone higher in status or power (an editor, a reviewer, an RTP committee) would eventually resolve the problem – that it was not my problem. Thus, it seems significant to me that it was the newest members of our field, the least socialized to our disciplinary norms, who called out the emperor with no clothes.

As a discipline, we first need to admit and recognize that we all could encounter questionable practices and that enforcing ethics is everyone's responsibility. Calling for more ethics training for graduate students suggests that the root of the problem lies in an inability to know right from wrong. For most people, I do not think that this is the problem. Rather, I think that we are not equipped, as a discipline or as individuals, to deal with the resolution of ethical dilemmas. Even when a lapse of ethics is obvious, students or faculty do not have the means to address these issues properly. For minor lapses, we have comforted ourselves in the knowledge that "everyone does it" or that science will correct itself. When we have more substantial concerns, we are like Darley and Latane's participants, sitting in a room filling with the smoke of whispers and gossip, but waiting for someone else to act. We have to teach ourselves and our students *what to do* when they have questions. We have to practice and become comfortable with questioning and being questioned. No matter how many data repositories we create, no matter how many obstacles to fraud we create, no matter how well we address the situational pressures that lead to fraud, fraud will happen again. There will always be people who believe that they can beat the system and avoid the fatal errors of previous charlatans. The greatest deterrents to such frauds and the best limits to the damage caused by these deceptions are to increase the social pressures and social presence of colleagues.

Second, we must realize that ethical dilemmas are dilemmas because they are not clear-cut situations. They frequently occur when there are two rights. The infamous Kohlberg scenario in which a husband steals medicine for his dying wife leads to debate because there is the rightness of stealing the medicine and the rightness of respecting the pharmacist's property. In our discipline, an analogous scenario might be when you have a graduate student going on the market with a relatively empty vita but an R&R at a top-tier journal. The additional study required for publication almost "works", but for a single problematic response. Elimination of that participant would result in the paper being published and substantially increase job prospects. Alone at the computer, there are two right answers. The first relates to your role as a mentor and personal obligation to launch your graduate student successfully. The second relates to your role as a scientist and the rightness of adhering to abstract principles. Whereas the wrong is abstract, the gain is concrete and immediate. In addition, it could easily be rationalized that the other four studies support the hypothesis, this is just a stupid thing that some anonymous reviewer asked for, and that everyone massages their data a little bit. It seems unlikely that anyone would be hurt by this change, but it is clear who would be hurt by *not* doing it. Moreover, this is not for personal gain, but in the service of another. Most of us would say that changing the data is completely unacceptable, but can also understand the temptation of making the change and that there may be real costs associated with adhering to ethical principles. But, we would hate to admit it. The effective practice of ethics requires that we admit the temptations that we encounter. Only by recognizing, acknowledging, and discussing these situations with our students and our colleagues, can we change the social pressures and psychologically prepare ourselves to intervene.

Personally, this has been an extremely trying time for me, yet my experience pales in comparison to the experiences of our Dutch colleagues who faced not only profound uncertainty, but also vicious media attention. Diederik was never my advisor, thus I was never fully dependent upon him nor was I ever in a position to be intimidated by him or to work with him on a daily basis. I know that he was an arrogant man and never sought to curry favor with others. Soon after I met him, I told him that ambition emanated from him. But I regarded him as a friend, and was grateful to him for the apparent help and guidance he provided to me over the years. In light of his actions, you might have expected from me anger and expletives. Please do not confuse my charity with forgiveness or approval. Compassion costs me nothing, and there is nothing that can be taken from him that will make me or his other co-authors whole. It would be too easy to make him into a monster and, by contrast, view ourselves as more virtuous than perhaps we really are. Instead, while our field should remedy the situational characteristics that make unethical behavior more likely and profitable, we should also embrace humility. We are all as human as Milgram's participants committing terrible acts because the experiment requires them, and as human as the

financiers and politicians in our desire to be successful. As we recognize Diederik's extraordinary weaknesses and errors, we must each accept our individual responsibility to nurture ethical practices.

**Resource:** Koocher, G.P, & Keith-Spiegel, P. (2010). Peers nip misconduct in the bud. *Nature* 466, 438-440. doi:10.1038/466438a

## Science: A True Story

Laura A. King, Editor, JPSP: PPID



*"...other respondents provided justifications which, although self-categorized as 'defensible,' were contentious (e.g., dropping dependent measures inconsistent with the hypothesis because doing so enabled a more coherent story to be told increasing the likelihood of publication)"* (John, Loewenstein, & Prelec, in press, p. 13; underlining added).

Making a research article a "good story" has been a goal for social psychologists at least since Bem (2004) instructed us on the nuances of writing a good empirical journal article. Having a coherent story to tell is valuable tool in scientific publishing. But the fraud committed by Diederick Stapel should cause us to contemplate what it is that qualifies as a good story in our science and how the value of a good story has potentially overshadowed another quite valuable tale, the true story of science. We were duped by Stapel but, as the interim report released by the committee investigating this scandal points out, a context existed that permitted this duping to continue long past the time when someone ought to have noticed something was awry:

*The data were too good to be true; the hypotheses were almost always confirmed...*

This is possibly the most precarious point of the entire data fraud. Scientific criticism and approach failed on all fronts in this respect. The falsification of hypotheses is a fundamental principle of science, but was hardly a part of the research culture surrounding Mr Stapel. **The only thing that counted was verification.** However, anyone with any research experience, certainly in this sector, will be aware that most hypotheses that people entertain do not survive. And if they do, the effect often vanishes with replication. The fact that Mr Stapel's hypotheses were always confirmed should have caused concern... (*Interim Report*, 2011; bold emphasis added)

From the perspective of the true story of science, the fact that all of Stapel's studies "worked" should have been a red flag. Yet, we have journals filled with similarly hued flags. Let's face it: Our version of a good story is an unusual one in the world of good stories. In our good story, unfortunate events very rarely happen. This story's brilliant narrator is phenomenally prescient, knowing, in advance, which dvs to include, which covariates to measure, which manipulation to use. Everything almost always works, otherwise, we might ask, "Why publish it?"

One thing that does not fit our good story is null results. Such lousy bumps on the way to a good story can be minimized if we use very small samples, have a list of dvs that includes the kitchen sink, and, following Bem's (2004) advice, thoroughly analyze the data prior to figuring out exactly what the study is about. These are important steps to avoid wasting the precious time that it takes to run a study. Yes, we know that these very steps maximize the chances of Type 1 error, but really, how bad is a research literature potentially littered with Type 1 error, compared to, say, wasting one's time with null results? Pretty bad, I'd say, when it sets up a context so readily gamed. Stapel knew the right (i.e., publishable) answer to every question in our field is the same:  $p < .05$ . What if the right answer to every question depended on the data? And what if, occasionally, social and personality psychologists were allowed to be wrong?

What if we redefined what a good story is, prioritizing truth at the expense of the appearance of omniscience? Imagine a less clever narrator, grappling with real data, more Ishmael than Ahab. Consider Melville's lesson: We can force the whale of data to prove us right every time, or we can survive (Note to my students: See, everything really is about *Moby Dick*). In their recent critique of the field, Simmons, Nelson, & Simonsohn (in press) suggest that reviewers and editors must become more tolerant of imperfections in papers and that researchers must begin to emphasize "transparency" over "tidiness."

As an editor, I have read many papers with the occasional null results. I've read, as well, reviews of those papers suggesting that such results should be removed because they are uninformative and confusing. My preference has been to let authors tell the true story of science rather than some imaginary story where no one ever makes a prediction that doesn't work out. Null results are utterly useless only from the standpoint of null hypothesis significance testing. We can learn from such results by using alternative analytic strategies that do not treat them as essentially uninformative, providing, of course, studies are sufficiently powered to provide evidence for and against null and alternative hypotheses. I have accepted papers for *JPSP* with nonsignificant effects strewn among the significant ones, and even at least one that eschewed significance testing altogether. As far as I can tell, the world has not ended.

Here's the thing: I think the truth is that we *don't* treat null results as uninformative. Rather, we assume they provide important feedback about our own methodological acumen (or lack thereof). Recently, LeBel & Peters (2011) described two important types of beliefs that influence common research practices, method-relevant beliefs and theory-relevant beliefs. While method-relevant beliefs are easily changed, theory relevant beliefs are more likely to be held with great fervor. Thus, if someone (say, Stapel) publishes a paper that presents studies supporting a particular conceptual argument (and didn't they all?) and one is unable to replicate these effects, the conclusion is not that the conceptual arguments are wrong but that the researcher must have made a methodological mistake along the way. LeBel and Peters were not focused on the Stapel case, but they might as well have been. I will admit to having something like the following conversation about a few different findings that are part of "what everyone knows" in social psychology:

Me: "You know, we have ever been able to get that effect in my lab."

Someone Else: "Oh, nobody ever does."

Talk about wasted time. Still, I confess that only recently have these conversations started to disturb me, particularly in light of the following from the committee report:

"'Too good to be true' was meant as a genuine compliment to his skill and creativity. Whereas all these excessively neat findings should have provoked thought, they were embraced. If other researchers had failed, that was assumed to be because of a lack of preparation, insight, or experimental skill." (*Interim Report*, 2011)

Call me crazy but it seems to me that, optimally, the true story of science would appear in, oh I don't know, scholarly articles and not in conversations as one queues up to buy a drink ticket at an SPSP social hour.

Sometimes the true story of science is a long one as well. Consider, again, the committee's interim report on Stapel's breach of scientific integrity:

*Insufficient disclosure and specification of the method; the experiments were too complex for the schools....*

Apparently neither the reviewers nor the editorial teams of journals delved into aspects of this kind. The lack of space in the articles themselves understandably prevents comprehensive coverage of these aspects of research, but questions should still have been raised about the actual course of events in the reported study. It is no unnecessary luxury for journals to demand a detailed report of the research procedures followed to be made available on Internet.

Somehow, the notoriously harsh reviewers in social psychology never questioned the fact that studies were conducted in ways that seem, logistically, impossible. Details were glossed over and apparently no one thought to ask how it was possible to carefully control, for instance, the temperature of water, in a school classroom. Should the good story of science be "bogged down" in such details? Another lesson from the Stapel scandal is that sometimes it must.

We live in a very strange world, where applicants for graduate training have the kinds of publication lists that used to be expected for job candidates, job candidates have tenurable vitas, and one has to remind oneself that the external letter one is writing is, in fact, for tenure and not promotion to full. How can we acknowledge (or even tell) the true story of science in this highly competitive context? Critiques of research practices mainly involve including much more information than most of us do (e.g., John et al., in press; Simmons, Nelson & Simonsohn, in press). Ironically, these critiques have appeared in *Psychological Science*, where the true story of science typically is

crammed into 1000 to 2500 words. (See Simmons, Nelson, & Simonsohn Table 3 to appreciate just how much longer a true account is than a fudged one).

Might there be room in the scholarly literature for the true, occasionally flawed, and sometimes long story of science? Online supplements surely provide some recourse. Still, it seems like what is needed is a particular type of journal, one that emphasizes theoretical impact and the presentation of programmatic series of studies. An emphasis on packages would, one hopes, alleviate the need for significant results in every single study and for every single test. Further, some of the studies might be acknowledged to be exploratory and others accurately presented as confirmatory. Exact (not conceptual) replication might be offered, especially for highly counterintuitive effects. This journal would have an outstanding editorial board of reviewers who might, with some editorial prodding, learn to live with imperfections and turn their amazing scholarly chops to the important goal of winnowing out the “too good to be true” from the “true.” Of course, such a journal would need to have sufficient page space to include the details of those studies.

If only we had a journal like that for personality and social psychology...Oh wait.

### Annotated References

Bem, D. J. (2004). Writing the empirical journal article. In J.M Darley, M.P. Zanna, & H.L. Roediger, (Eds). *The compleat academic: A career guide (2nd ed.)*.( pp. 185-219). Washington, D.C.: APA.

*In light of recent events, I am not sure it is possible to read this chapter in the same way ever again. I am not saying that everything Bem said was wrong. But one could argue that taking his advice perhaps even further than he intended (or selectively attending to only some of it) has gotten us to where we are today.*

*Interim Report Regarding the Breach of Scientific Integrity Committed by Prof. D.A. Stapel (October, 31, 2011).*  
<http://www.tilburguniversity.edu/nl/nieuws-en-agenda/commissie-levelt/interim-report.pdf>

*This one is in English and is well worth a read, if you can stomach it.*

John, L.K., Loewenstein, G., & Prelec, D. (in press). Measuring the prevalence of questionable research practices with incentives for truth telling. *Psychological Science*. Temporarily available at [www.psychologicalscience.org/redesiqn/wp-content/uploads/2011/11/john\\_manuscript.pdf](http://www.psychologicalscience.org/redesiqn/wp-content/uploads/2011/11/john_manuscript.pdf)

*BTW, I was a participant in this study.*

LeBel, E.P., & Peters, K. R. (2011). Fearing the future of empirical psychology: Bem’s (2011) evidence for Psi as a case study of deficiencies in modal research practice. *Review of General Psychology*, 15, 371-379.

*One of the perks of editing an APA journal is complimentary subscriptions to any APA publication. I know that “no one reads RGP” but seriously, folks, this one is worth a read. No, I am not saying we all have to become Bayesians, but we might all benefit if we redefined what “wasted time” really is.*

Simmons, J.P., Nelson, L. D., & Simonsohn, U. (in press). False positive psychology: Undisclosed flexibility in data collection and analyses allows presenting anything as significant. *Psychological Science*. DOI: 10.1177/0956797611417632

*They may not have gotten everything right, but this is still a great paper for conversation at your next journal club meeting.*

### Campaign for Real Data



Cheryl Kaiser, University of Washington

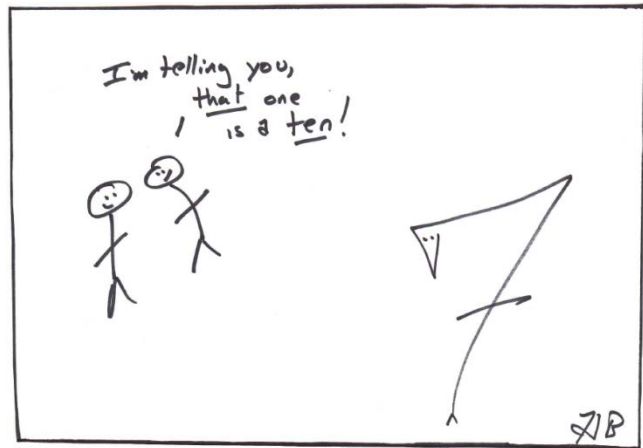
In 2004, Dove introduced the Campaign for Real Beauty, a marketing effort directed toward reducing the glamorization of an overly thin, largely unattainable, female body. The campaign addressed concerns that young girls were engaging in unhealthy and dangerous behaviors in an attempt to obtain an idealized body, and that when they failed to achieve this goal, they felt worthless. In the Campaign for Real Beauty, Dove launched a series



of ads for beauty products featuring women of all shapes and sizes, rather than models who typified the idealized body.

Like idealized bodies, hypothesis-confirming data can be beautiful, provocative, alluring, and on occasion, even sexy. Sometimes we luck out and our data resemble the image of Aphrodite, leaving us in a state of awe. However, when studying a species as complex as humans, our assumptions about behavior will often be wrong. For many of us, our real data can be downright ugly. Real data taunt us with their inexplicable patterns or their marginal, yet still nonsignificant, trends. Real data make our heads hurt. Although we might airbrush our data so they can join their prettier friends on the pages of our best journals (see the lively discussion of Simmons and colleagues' (2011) paper on researcher degrees of freedom), or bury them deeply in our files so that we need not be reminded of the ugliness we have created (see Greenwald, 1975, for problems with this approach), I want to argue here that there is real beauty in real data.

By selectively showcasing only our idealized data, we create the impression that only perfect data are scientifically generative and worthy of our gaze. There are at least two problems with this restricted image of data. First, by denying the beauty in real data, we miss opportunities to learn from these data. Sometimes "failed experiments" (especially "failures" that produce the same real data more than once) are silently screaming at us, demanding that we rethink our assumptions about human nature. When we embrace our real data, despite their lack of idealized beauty, we get closer to understanding the true nature of phenomena. For example, our real data can be the catalyst for identifying moderators that unlock null effects and can lead to studies that ultimately create novel directions in our field. By finding beauty in real data's indifference to our predictions, we can generate entirely new ways of understanding phenomena.



Second, by selectively sharing data that conform to idealized standards of beauty, we send a message to newcomers in the field that they too should value only idealized data, and aspire to one, often unattainable, standard of scientific beauty. When new graduate students see their own real data, they may feel ashamed, incompetent, and harbor a sense that they are not psychological scientists of worth, at least on an equal basis with others. If our idealized view of beautiful data gives students the message that they should toss research ideas aside due to the nature of their real data, the field misses out on the potential novel insights provided by students. People who are newer to the field bring fresh perspectives that faculty, who share a common socialization in the field, might miss or dismiss because of their shared background.

It can be difficult for real data to make their way into premier journals.<sup>1</sup> Nonetheless, there are ways we can more fully share our real data. Doing so can help counteract the false image that idealized data are the only worthy and beautiful data, and can encourage expansion of the image of scientific beauty. Further, sharing our real data can help us further understand them, and this can lead to more effectively packaging and ultimately publishing our real data. Below, I offer a few suggestions for how we might more fully embrace our real data.

When colleagues ask us to identify the most exciting recent finding in our lab, rather than replying with the finding that best typifies the idealized standards of beauty, we could share the ugliest, or euphemistically, the realest finding. We might get more out of these conversations as they could lead to new ideas that help us understand real data. Additionally, we could prominently display graphs highlighting our real data in our offices, so we can engage office visitors in conversations about our real data. The braver might include their real data in talks, hoping someone in the audience has ideas for how to harness the beauty hiding within the beast. We might also get real data into our papers, permitting them to tag along with their more conventionally beautiful data siblings who are the focus of the paper. If reviewers and editors are open to imperfections in otherwise interesting papers, we might create more opportunities to explore seemingly ugly, but potentially beautiful, ideas. We can also encourage our students to unconditionally love and accept their real data, even if their data fall short of idealized standards of

beauty. Finally, the dangerous behaviors that can occur while striving to obtain idealized data might just be lessened if we can do a better job embracing our real data and seeing its potential for beauty. When it comes to the scientific perils of adopting an idealized image of data, there is something to be said for keeping it real.

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<sup>1</sup> However, real data, including null effects, can make their way into our premier journals. This can occur when real data fly in the face of common assumptions (e.g., Meehl et al.'s (2007) null effect in *Science* showing that men and women express similar amounts of words each day). This can also occur when real data convincingly reinterpret accepted scientific wisdom (e.g., Gillig & Greenwald's, 1974, persistent JPSP paper examining the sleeper effect).

## Groundbreaking or Definitive? Journals Need to Pick One



Sanjay Srivastava, University of Oregon

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Do our top journals need to rethink their missions of publishing research that is both groundbreaking and definitive? And as a part of that, do they — and we scientists — need to reconsider how we engage with the press and the public?

2011 was bookended by two extraordinary events for social and personality psychologists — events that have produced a lot of uncomfortable scrutiny from both within and without the field. The year began with a paper on parapsychology (Bem, 2011) that presented what many scientists think are impossible results. And it ended with a high-profile case of a researcher, Diederik Stapel, who fabricated data and published perhaps dozens of fraudulent articles in peer-reviewed journals. Both of these incidents have led to a great deal of reflection and reevaluation of how we do science. This ongoing conversation has been taking place both online (for example Crocker, 2011; Roberts, 2011; Yarkoni, 2011) and in traditional journals (Simmons, Nelson, & Simonsohn, 2011). This has been an important discussion, and many of these proposals have focused on how researchers can improve their methods, or how editors and reviewers can better distinguish good and bad work.

But it is also vital to go beyond the practices of individual scientists and look critically at how we have structured our institutions. The institution I am particularly concerned about is journals. Journal publishers control the single largest incentive for most academic researchers and the major avenue for disseminating research to other scientists and to the broader public. As a result, they play a substantial role in how science gets done and ought to be the subject of continuing and close scrutiny.<sup>1</sup>

My concern here is with what I see as the impossible missions of some of our top journals. High-profile journals like *Science*, *Nature*, and *Psychological Science* try to be both **groundbreaking** and **definitive**. For example, *Science*, a publication of the American Association for the Advancement of Science, describes its mission as publishing findings that are both "novel" and "significantly advance scientific understanding" (AAAS, 2011). *Psychological Science*, published by the Association of Psychological Science, describes itself as both "cutting-edge" and publishing articles with interdisciplinary relevance and "general theoretical interest" (APS, 2011). I think it is worth reflecting on whether groundbreaking and definitive are compatible goals.

Groundbreaking means original, novel, new. Just like a literal groundbreaking means putting your shovel into as-yet undisturbed soil, groundbreaking research is work that presents ideas or findings that nobody has presented before. From the investigator's perspective, groundbreaking research is a first finding of its kind, something that is unprecedented. And from an audience's perspective, it is research that makes you say "wow".

But that is a long way from definitive. In fact, in some key ways groundbreaking is the opposite of definitive. There is a lot of hard work to be done between scooping that first shovelful of dirt and completing a stable foundation. And the same goes for science (with the crucial difference that in science, you're much more likely to discover along the way that you've started digging on a site that's impossible to build on). "Definitive" means that there is a sufficient body of evidence to accept some conclusion with a high degree of confidence. And by the time that body of evidence builds up, the idea is no longer groundbreaking.

How do we get from groundbreaking to definitive? The key phrase is **independent replication across multiple methods**. (I for one would be very happy if this phrase eclipsed "correlation does not imply causation" in popularity, but that's an argument for another time.) It is worth taking that phrase apart. *Replication* means that the study has been run again to determine whether the findings can be reproduced. *Independent* means that the replication has been conducted by a different set of researchers, who are less likely to share the biases, incentives, or errors of the original researchers. *Multiple methods* means that researchers reach the same underlying conclusion when they test it in different ways. (For example, if the conclusion is "suppressing emotion-expressive behavior causes difficulties getting close to others," it should be possible to obtain supporting evidence in both laboratory experiments and longitudinal field studies.)

So more and more, I have been coming to the view that groundbreaking and definitive are incompatible. Popular depictions of science in movies and journalism often conflate them into a "eureka" moment when the scientist (usually wearing a white lab coat and surrounded by test tubes) makes a big discovery that changes everything. In this stock narrative the only hard work after that first discovery moment — if there is any hard work at all — is to convince the world of the brilliant idea. In the real world, that first discovery is just the beginning (and more often than not it is the beginning of a road to a dead end). A single study reported in a single paper cannot be both the start and the finish of an idea. A journal could, in principle, publish a mix of both kinds of papers— some groundbreaking new findings, some conclusive reviews of bodies of evidence. But that's not what is usually happening. The way the high-profile journals carry out their missions, they expect most articles to do both.

None of this is to say that we don't need journals for brand-new, groundbreaking findings. Nor does it contradict the many good ideas that have been floated recently about how scientists and journals could improve rigor and reporting. But some part of the tension between groundbreaking and definitive is irreducible. As long as a journal pursues a strategy of publishing "wow" studies, it will inevitably contain more unreplicable findings and unsupportable conclusions than equally rigorous but more "boring" journals. Groundbreaking will always be higher-risk. And definitive will be the territory of journals that publish meta-analyses and reviews, like *Psychological Bulletin*, or to a lesser extent (because definitive is a matter of degree) long-form journals that publish multi-study investigations.

So back to the question that I posed at the top: should our journals — and we scientists — stop telling the world about our newest discoveries?

At the very least, findings that are new and exciting to specialists should not yet be presented to scientists in other disciplines or the broader public as settled facts. Most conclusions, even those in peer-reviewed papers in rigorous journals, should be regarded as tentative at best; but press releases and other public communication rarely convey that. Some journalists are catching on and becoming more critical of science journals, but in the new media landscape we cannot count on a few skeptical science journalists to be gatekeepers. We as individual scientists need to remain skeptical of our journals, and to communicate that skepticism better. Our standard response to a paper in *Science*, *Nature*, or *Psychological Science* should be "wow, that'll be really interesting if it replicates." And in our teaching and our engagement with the press and public, we need to make clear why that is the most enthusiastic response we can justify.

But beyond individual efforts, our institutions, especially our journal publishers and the professional associations that sponsor them, have a lot of power to change the conversation: by clarifying their missions, by tempering their

messages, and perhaps by bringing science to the press with less frequency but greater confidence. In the short term, the incentives work against them doing that. Who wants to be the publisher of the *Journal of Things that Might Be True*? But if 2011 has shown us anything, it is that in science, the facts eventually catch up with us. If we want to keep the trust of the public and of each other, we need to be mindful of that.

### Note

<sup>1</sup> Along these lines, I have argued elsewhere that journals and electronic databases need to do a better job of removing retracted articles from circulation, and that after a journal publishes a study it should be required to publish and track direct replication attempts, both to encourage such studies and to hold journals more responsible for what they publish.

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## Openness with Data: The Time Has Come

Jennifer Crocker, Ohio State University



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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. The very idea of being required to share their data strikes fear and outrage in the hearts of many researchers.

Two concerns typically arise when the topic is broached. First, psychologists fear that others will discover inadvertent errors or other problems in their data analyses, leading to public embarrassment or humiliation. This fear is reinforced by the current ethical principles of the American Psychological Association, which require that psychologists share their data for the purposes of verifying substantive claims through reanalysis. Those who obtain data for this purpose may not use it for any other purpose unless they obtain prior written agreement. In other words, by current ethical standards, the only reason we must share our data is so that others can check to see if we have done anything wrong. No wonder people drag their feet when they receive requests for their data.

Second, psychologists often fear that other scientists will use their data to write articles that they had intended to write themselves. It's bad enough to be scooped by independent research; it's terrible to think of being scooped with one's own data. Some data are expensive and time-consuming to collect, or involve samples of research

participants that are hard to access. Longitudinal data can take years, or decades, to collect. Why should another researcher get credit for studies based on my data, obtained through my own efforts, and often the result of grant proposals I wrote? Publication of original research is the coin of the realm. To be forced to give away one's data before one has completely milked it for publications seems downright unjust.

On the other hand, there are some very good reasons why social psychologists should, and will, share their data in the future. First, many of us may soon have no choice. Since 2003, NIH has instituted a data sharing policy for grants exceeding \$500,000 ([http://grants.nih.gov/grants/policy/data\\_sharing/](http://grants.nih.gov/grants/policy/data_sharing/)). Word from people in the know indicates that NIH plans to extend this policy to all NIH grants. Starting in 2011, NSF requires a full data management plan for all proposals (<http://www.nsf.gov/fga/dias/policy/dmp.jsp>). NIMH has convened scientific working groups to consider how electronic sharing of data could improve research and practice. These developments have clear implications for psychologists.

More important than changes in funding agency policies is the potential benefit to our science that results from sharing data. I recently chaired a task force on data sharing for APA's Publication and Communication Board. I must admit, I tried to get out of doing this, and began the task with the same fears and sense of injustice about the idea of being required to share my data as many others. However, the discussion among the task force members changed my mind.

Sharing data with other scientists can have tremendous benefits for our science. When data sets are available to other scientists, they can easily be used to test new hypotheses by other scientists, including graduate students and researchers at smaller institutions who lack the infrastructure to collect their own data. Data can more easily be synthesized for meta-analysis. The generalizability of particular findings across labs and samples can more easily be explored. When data are archived in a repository, they can be analyzed later with new, more powerful or integrative techniques than available at time of data collection. Finally, sharing data encourages a culture of openness and accountability in scientific research.

This last point is not something we should take lightly. The recent interim report by the Levelt Committee indicated that Diederik Stapel fabricated data for dozens of studies over about 17 years, with untold costs for the careers of young scientists, for our science, and for public trust in science. The fact that this fraud continued in our best journals for so many years suggests that something is not working in our field.

Openness and accountability achieved through sharing data will not completely solve the problem of data fabrication, but it can help. Recent criticisms of our science, such as [the article by Ben Carey in the New York Times](#), suggest that we must get in front of this problem, leading through example rather than dragging our heels as funding agencies and federal laws force us to change.

It seems to me that one shift in our culture that would encourage data sharing is recognition that collecting data is an important contribution to science. If data sets were considered citable contributions, then researchers could get credit in the form of a citation each time their data were used in a secondary analysis. Some researchers might find that their data sets are cited more than their articles. To be sure, it will take time to convince tenure and promotion committees to consider citations of data to be significant indicators of the impact of a scientist's work, but I believe this culture change can, and will, happen.

The APA P&C task force on data sharing developed a draft set of principles that could guide the move toward more data sharing. The task force recognizes the questions that the draft policy raises—where will data be deposited, will it be permanent, will it be interpretable, what about human subjects protections, who will have access, and how can people get credit for their data when its used by others? Implementation will surely be complicated, but I think we must begin to develop answers to these questions.

This is the moment for social psychology to take the lead on this issue. Doing so would both advance our science, and help re-establish our credibility as scientists. I hope all the social and personality psychology societies—SPSP, SESP, EASP, ARP—and social psychology journals, including JPSP, agree that we need to move in this direction, and begin thinking about how to implement it.

**[Draft Principles proposed by the APA P&C Board task force on data sharing, with commentary](#)**

1. APA believes that sharing data promotes science.
2. APA journals policy requires that, for articles published in APA journals, authors share the data on which the article is based.
3. It is the responsibility of the author to find and deposit data on a hosting site in usable, interpretable form.
4. The original author and the secondary user of the data both are responsible for protecting individual participants' privacy and confidentiality of the data.
5. The secondary user of data must acknowledge the original source of the data and may not transfer those data to any other individuals.

### Commentary

There are many compelling scientific reasons for sharing data. Sharing data within the larger scientific enterprise, promotes 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; allows aggregation for the purposes of knowledge synthesis, and encourages a culture of openness and accountability in scientific research.

All authors of articles published in APA journals should participate in data sharing activities as long as sharing and linking data do not violate the privacy rights or confidentiality of data on identifiable research participants. The responsibility for protecting the confidentiality of the data and the rights of research participants lies both with the original author and with any subsequent scientist using the data for new purposes (secondary user). Data that have more potential to reveal subject identity need additional security. Both the original author and the secondary user of the data are responsible for ensuring that the level of security protection of human rights is in place. Sharing of data must comply with federal and institutional guidelines. If an author knows prior to the publication of an article that it will not be possible to share the data on which the article is based, that situation should be disclosed to the journal editor prior to the publication of the article.

It is the responsibility of the author(s) to make data published in APA journals available to the scientific and academic community in usable, interpretable form. APA expects that authors preserve their data in a permanent archive so that their data can be available to scientists indefinitely.

Data should be archived at least at the level of detail used for analyses reported in the article. The archive should include metadata such as, but not limited to, code books, user manuals, and analysis procedures. The data archive should include the first data transformation, such as cortisol scores as compared to biological samples and diagnostic score as opposed to interview transcript or biological samples. Data sharing arrangements must comply with copyright restrictions, consent provided by participants, requirement of funding agencies, and rules promulgated by the employer of the holder of the data.

Secondary users must acknowledge the original source of the data and may not transfer the data to any other individuals. Sharing of data does not entitle the original author to authorship on articles generated by secondary users, nor should it preclude the possibility of authorship. Authorship of the original data must be cited in the methods section and in the reference list, with appropriate DOIs or URIs.

## **FASHION TRENDS**

### **The Cultural Approach in Social Psychology: How Far Have We Come and Where Shall We Go?**



Shinobu Kitayama, University of Michigan

At this year's SPSP meeting, the Career Contribution Award will go to Harry Triandis. Harry was a pioneer in the cultural approach in social psychology. His systematic research program on individualism and collectivism is exemplary and legendary (Triandis, 1995). He never stereotyped any nation or group. Instead, he defined these constructs as cultural syndromes that are composed of features that are probabilistically associated with the constructs. He and his students pioneered the cultural priming research (Traffimow, Triandis, & Goto, 1991; Triandis, 1999) nearly a full decade prior to the next priming work that was to appear in the culture literature

(Gardner, Gabriel, & Lee, 1999). The work by Harry has since become an important cornerstone of the contemporary cultural research in psychology. Above and beyond all substantive contributions Harry has made to the field, he has always provided support and encouragement to young researchers – that is, to almost everybody who has ever worked in the now-familiar field of cultural psychology. If there were any single social psychologist who was responsible for the achievement of the cultural approach over the last two decades, it would be Harry. This award, then, provides us with a precious moment of reflection about how far we have come, as well as where we shall go from here.

More than two decades ago, when I started my research career, culture was nearly completely ignored in social psychology. Much has been changed since then. Culture is now recognized as a critical element in understanding some basic social psychological processes. In this short essay, I would like to point out what the cultural approach in social psychology has accomplished over the last two decades and then to share with my fellow social psychologists some visions I have for the future of the field of social psychology in general.

### **Cultural Approach: What Have We Learned?**

The most important contribution of the cultural approach over the last two decades comes from the fact that researchers in this once-new sub-field have worked hard to show that culture is deeply ingrained into some of the basic social psychological processes (e.g., Markus & Kitayama, 1991; Triandis, 1995). Just to illustrate the point, let me consider one phenomenon that is at the heart of the social psychological literature in the last half century: dispositional bias in person perception.

Dispositional bias refers to a tendency to infer dispositional features of a person such as her traits and abilities upon observation of her behavior that is obviously constrained by the situation she faces. This bias is highly robust in Western cultures. Another name for it is the fundamental attribution error. Very early on, when I was still in graduate school (in the 1980s), one prominent social psychologist told me in passing while commenting on my growing interest in culture that if one could nail a cultural difference in something like the fundamental attribution error, then the cultural perspective could be real. An obvious implication back then was that culture would never have any such deep influence.

By this criterion, the cultural approach has earned its own success. Now a large number of researchers agree that dispositional bias may be grounded in a Western cultural view of the person as independent and, thus, as internally motivated. This cultural hypothesis implies that in non-Western cultures where an alternative, more socio-centric, collectivistic, or more interdependent view predominates, the bias should be much weaker. This in fact proved to be the case (Kitayama & Uskul, 2011, for a recent review). Recent work has gone further and shown that this cultural difference occurs even in early automatic stages of information processing (Na & Kitayama, 2011).

Dispositional bias is only one of many examples that can be cited here to illustrate the extent of cultural influence. Numerous studies in the last two decades have focused on East-West comparisons and showed consistent differences between the two broadly drawn cultural regions. The overall pattern of the results is consistent with the hypothesis that independence of the self is highly sanctioned in Western cultures, but it is interdependence of the self with others that is more strongly sanctioned in Eastern cultural contexts. I should note that the cultural difference described here is not limited to college undergraduates. Recently, we tested a large number of non-student adult Americans and Japanese in a wide age range, both male and female, from diverse educational backgrounds, and found that across all these subgroups, the expected cultural differences are sizable across 16 measures that are linked to independent vs. interdependent self as well as to the related cognitive dimension of analytic vs. holistic mode of thought (Kitayama et al., 2012).

### **Future Agendas**

Although the cultural approach has accomplished a lot, is there any future in it? Where shall we go from here to exploit new horizons of research? I would suggest several important agendas that would keep the cultural approach quite vibrant for the next generation of researchers (Kitayama & Uskul, 2011).

Identifying causally active elements of culture. An agenda of the foremost significance is to find out origins of cultural differences in psychological processes. Much of the work done in the past started with a premise that European Americans (or Asians) are independent (or interdependent) and then predicted that European Americans

(or Asians) should show features in attention, inference, attribution, emotion, dissonance ...etc. that are arguably independent (or interdependent). Likewise, the body of research on cultural priming uses a similar logic, namely, that priming of independence (or interdependence) should produce behaviors that are arguably independent (or interdependent) (Oyserman & Lee, 2008). This framework had a lot of heuristic value. In particular, it suggested what cognitive tasks or behavioral measures researchers might use to capture important cultural differences/priming effects. I think this framework was indispensable for establishing solid empirical foundations for the field. Ultimately, however, the logic is somewhat circular, and I believe that the field is now ready to move ahead and identify factors that are causally linked to independence and interdependence.

There is an increasing volume of work addressing this issue. Oishi and colleagues have demonstrated that residential mobility is linked to independence (Oishi, 2010). Expanding on earlier cross-cultural analysis by Berry (e.g., 1971), we show that farming is likely to encourage interdependence whereas herding is linked to independence (Uskul, Kitayama, & Nisbett, 2008). Another factor that is related, but distinct, and that is crucial in understanding what America is as a culture is its history of settlement in frontiers. Frontier settlement may foster independence (Kitayama, Conway, Pietromonaco, Park, & Plaut, 2010). Additionally, the field has recently brought Karl Marx back in to the debate by focusing on profound influences of social class (Stephens, Markus, & Townsend, 2007). Yet, another fascinating idea comes out of evolutionary psychology. It has been argued that historical risks of pathogen infection give rise to interdependence and collectivism (Schaller & Murray, 2011). Articulating these ecological, geographic, and social structural factors and establishing them as causal antecedents of independence or interdependence will be increasingly important.

Going beyond independence and interdependence. While cultural psychology has gained a lot of mileage by focusing on independence and interdependence, there remains an important question of whether there might be other important cultural dimensions. Gelfand has recently emphasized the significance of tightness vs. looseness dimension (Gelfand et al., 2011). Haidt has presented important differences in value profiles between liberals and conservatives in the US (Haidt & Graham, 2007). Adam Cohen (2009) has investigated religious denominations as a major influence. And Nisbett and Cohen (1996) demonstrated that we could not dismiss honor as archaic in accounting for some aspects of the mentality of contemporary Americans. Furthermore, hierarchical vs. egalitarian orientations may also prove to be an important issue in influencing how we process social information (e.g., Cheon et al., 2011). Investigating these and other cultural dimensions will remain another high priority of the field.

Cultural Neuroscience. One important recent extension of the cultural approach is cultural neuroscience. Very much like reaction time and memory measures did during the initial years of social cognition research, some neural methods such as functional magnetic resonance imaging (fMRI) and electroencephalogram (EEG) have enabled contemporary researchers to take a fresh look at old problems in psychology. These methods are now heavily used in investigating cognitive and social cognitive processes. Further, they have also been put in use increasingly more in investigating cultural influences (e.g., Kitayama & Park, 2010). For example, when people make self-reference judgments (e.g., "Am I honest?"), the medial prefrontal cortex (mPFC) is consistently activated. This effect, repeatedly documented in North America, can be reliably replicated in China. For Chinese, however, the same area is also recruited when the reference is made to one's mother as wekk (e.g., "Is she honest?") (Zhu, Zhang, Fan, & Han, 2010). The mother-reference effect does not occur among Westerners. It appears then that the self and the mother are "more connected" for Chinese than for Westerners at the level of brain representations. What we hypothesized more than two decades ago seems to hold even at the neural level.

While it may seem self-evident that if behaviors are different across cultures, underlying brain processes also differ, this point was not obvious at all just several years ago when some of us thought about the possibility of cultural neuroscience. Having been involved in several neuroscience projects myself since then, I am increasingly confident that culture influences behaviors by, first and foremost, changing relevant brain parameters and mechanisms. This means, among others, that cultural differences should be most clearly observed in the brain.

Gene x culture coevolution. Last, but not least, potential roles of genetics (frequency of different gene variants) and epigenetics (gene expression) in understanding cultural variations in mentality will begin to be one central focus of the next generation of researchers (Chiao & Blizinsky, 2010). The brain – with all psychological processes it supports – is a product that is grounded in a large number of genes. These genes are differentially activated and expressed as a function of a variety of factors including features of socio-cultural environment. If empirical work



can demonstrate certain brain functions of people in one culture are different from those of people in another, a next important analytic step would be to articulate how these differences might occur as a function of mutual influences between certain relevant genes and socio-cultural environment. I believe that by empirically addressing this issue with a large scale cross-disciplinary research, it will be possible to achieve a better understanding of the human mind as prepared by evolution, but fully realized through social and cultural processes. I see great potential here by way of integrating nature and nurture (Laland, Odling-Sme, & Myles, 2010).

### Concluding Reflection

Culture matters. Simple and self-evident as it might seem, it took the field two full decades to realize some fundamental truth involved in it. While some of my fellow social psychologists may still insist that a lot can be learned from empirical work on US undergraduates alone, an increasingly large number of them have begun to recognize, somewhat reluctantly, certain “weirdness” involved in the typical participant in social psychological studies. That is, they worry about the fact that nearly all of the participants come from Western, Educated, Industrial, Rich, and Democratic societies (Henrich, Heine, & Norenzayan, 2010). Clearly, the cultural approach has had a big impact on how social psychologists think about the very content domain they focus on.

I started this essay by noting my great appreciation and intellectual indebtedness to Harry Triandis as a pioneer and as a supporter of the cultural approach in social psychology. Since his pioneering work on culture, much has been accomplished. Obviously, however, much, much more has yet to be explored and uncovered. I hope Harry will read this little piece. And when he does, I am sure that he will respond by smiling and saying, “Oh fine, let’s do it!”

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## Nobody Studies Groups Anymore

Donelson R. Forsyth, University of Richmond



“Social psychology has always been ambivalent about the study of groups per se.”

-- E.E. Jones, 1985 (p. 77).

When Los Angeles Mayor Antonio Villaraigosa was asked about the level of gang activity in his city, he explained “I’m not a sociologist or an anthropologist, so I can’t share with you the root causes of gang violence that you see in urban areas” (Sims, 2007). He did not include “social psychologist” on his list of experts on gangs, because social psychologists don’t study gangs—in fact, social psychologists don’t even study groups anymore. That is why Lee Ross, Mark Lepper, and Andrew Ward (2010), in their chapter on history in the *Handbook of Social Psychology* concluded that (a) the study of groups used to be called “group dynamics” and (b) “there is still a relative paucity of work on groups per se” (2010, p. 4).

Their pronouncement leaves me wondering why I still subscribe to the APA/EPF journal *Group Dynamics*. I’m also wondering why, within the field of social psychology, there is a journal that focuses on relationships (*Personal Relationships*), social cognition (*Social Cognition*), influence (*Social Influence*), and the self (*Self and Identity*) but three that examine group-level processes (*Group Dynamics*, *Small Group Research*, and *Group Processes and Intergroup Relations*). And why is the 2010 *Encyclopedia of Group Processes & Intergroup Relations* edited by John M. Levine and Michael A. Hogg a 2 volume, 998 page, compendium of over 300 entries?

But Ross, Lepper, and Ward’s verdict is one that has been bandied about ever since the great Ivan Steiner asked “Whatever happened to the group in social psychology?” in his cleverly titled *Journal of Experimental Social*

*Psychology* paper in 1974. He lamented the golden age of group dynamics—the 1950s—with its studies of communication networks, leadership, group decision-making, and performance in groups. (Hard as it may seem to believe today, Leon Festinger’s 1955 Annual Review chapter was titled “Social Psychology and Group Processes.”)

Steiner’s dismal outlook has been repeated by many commentators in the intervening years. Gwen Wittenbaum and Richard Moreland (2008), themselves researchers who study groups, admit the field is nearly static. Richard Hackman and Nancy Katz (2010, p. 1208) explain “small group research has migrated to the periphery of the field”. Brooke Harrington and Gary Alan Fine (2000) similarly conclude that researchers in social psychology, both in the sociological and psychological traditions, “express little interest in small groups as an organizing principle of social life” (p. 313).

Yet, others express a more Panglossian perspective on groups. John Levine and Richard Moreland, in 1998, hope that “research on small groups is experiencing a renaissance within social psychology” (p. 448). In that same year Dominic Abrams and Michael Hogg wrote that “research in group and intergroup processes is being published at a disproportionately accelerating rate compared with the increase in social psychology as a whole” (p. 7).

One reason for this diversity in opinions regarding the health of groups research is ambiguity about the definition of a group. Levine and Moreland (2012), for example, don’t think dyads are groups, and so they exclude any studies using paradigms that involve two interacting individuals from their analyses. Never mind that the study might be testing some theoretical perspective pertaining to influence, social comparison, power, leadership, communication, or some other group-level process—dyads aren’t groups. Kipling Williams (2010), by the way, takes a different perspective, in part because he considers his work on ostracism—which often involves one person rejecting another person—to be groups research (so did the Annual Review of Psychology, which nested his review of Ostracism under the heading “Small Groups”). He probably also thought he was studying groups in his work on social loafing (which in many cases involved two people working to contribute to a shared resource).

A second reason for the differences in conclusions about the state of group dynamics as a field is ambiguity about what processes qualify as group processes and which ones don’t. Wittenbaum and Moreland (2008), for example, focus on five topics when they offer up their comprehensive review of the state of groups research: group composition, group structure, group performance, conflict in groups, and the ecology. They also add, grudgingly, intergroup processes, but exclude others: affiliation, aggression in groups, collective behavior (e.g., crowds, gangs, etc.), conformity, contagion, crowding, family dynamics, group formation, group development, group-based identity, groups and therapeutic change, inclusion/exclusion, justice, leadership, negotiation, obedience, ostracism, perceptions of groups (entitativity), power, social comparison, social identity, social network analysis, status and hierarchy, and teams. Some of these topics may not fall squarely into the realm of group research, but all explore processes that are relevant to understanding the behavior of individuals when in groups.

Perhaps the conclusion “interest in studying social processes within small groups has diminished over time” (Wittenbaum & Moreland, 2008, p. 187) is only reasonable when the list of groupy topics has been whittled down to a select (and, arguably, most boring) few. A more generous interpretation of the field’s rightful domain of interests yields a far more positive conclusion. For example, Georgina Randsley de Moura, Tirza Leader, Joseph Pelletier, and Dominic Abrams (2008) reviewed 90,827 articles pertaining to social psychological topics published between 1935 to 2007 in over 60 journals. They discovered that a healthy percentage of those papers, 16.5%—about 15,000—pertained to groups. When they examined annual publication rates they found evidence of a linear increase over time with a particularly dramatic increase from the 1990s onward attributable, in part, to the increased integration of groups with studies of social cognition. This increase was particularly pronounced when they focused on the leading journals within the field of social psychology. They went back, through the preceding 10 years, and located the 10 articles from each year with the highest impact as measured by Total Cites from Thomson’s ISI Web of Knowledge. Of the 881 top-ranked articles, fully 35.2% pertained to a group-level topic (which they defined, fairly conservatively, as pertaining to intergroup relations, social identity, stereotyping, stereotype threat, social influence, entitativity, group performance, group decision making or productivity, social dilemmas, leadership, structure or ecology of groups, power in groups, and conflict in groups). Although Randsley de Moura, Leader, Pelletier, and Abrams live on the same planet as Wittenbaum and Moreland, they conclude, “The progress of group processes and intergroup relations based research is steady and sure, both in terms of quantity and impact” (p. 591).

A final reason for the pronounced differences in opinions regarding the state of the field of group dynamics is the interdisciplinary interest in groups. No one discipline holds the exclusive rights to the study of groups. Scientists in such fields as anthropology, communication studies, education, engineering, fields devoted to mental health, political science, sociology, sports and recreation, the legal profession, and, of course, business, all study groups. When the work of scientists in these fields is recognized, then the actual level of interest in group-level processes can be more fully appreciated (Hackman & Katz, 2010; Sanna & Parks, 1997). Consider, for example, the study of teams—which, by the way, are groups. A search of the phrase *social cognition* yields a healthy 226,000 hits in Google Scholar. Search for the word *team*, in contrast, generates 3,730,000.

In sum, it is not clear that the study of groups is, or even ever was, moribund. In fact, the exact opposite may be the case. Ross et al. offer up a bleak assessment of the study of groups, but they do not mention the findings reported by F. D. Richard, Charles Bond, and Juli Stokes-Zoota in their 2003 meta-analysis of meta-analyses in social psychology. When they examined 100s of prior meta-analytic studies of various social psychological processes, they discovered that the average effect size in those studies was .21, a low to moderately strong effect. But, when they looked more closely across topics, they discovered that some relationships were particularly paltry, whereas others were more robust. Studies of the relationship between personality and behavior, for example, are often considered relatively unsubstantial by social psychologists, but as personality psychologists have maintained all along they were consistently stronger ( $r = .22$ ) than the relationships documented in studies of influence ( $r = .12$ ), attribution ( $r = .14$ ), and expectancies ( $r = .16$ ). And what one area of study has yielded the strongest support for predicted relationships between the variables specified in its theories? Leading the way, across all 18 topics identified by Richard and his colleagues: The scientific study of groups and their dynamics, with a mean  $r$  of .32.

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## **TRAVEL**

Editor's Note: Each issue, we invite submissions from "Social and Personality Psychologists Living Abroad" (i.e., outside of traditional social-personality training programs) to contribute to the *Travel Section*. In recent issues, we have heard from social-personality psychologists working in business schools and public health programs, at historically black colleges and at teaching institutions, and working both industry and government positions. This issue, we hear from Anepla Maitner, who is living abroad in the more traditional sense. She is faculty at American University of Sharjah, United Arab Emirates and she shares her experiences learning her new role as college faculty while also adapting to a new culture.

### **Not Completely WEIRD, Definitely Different: Opportunities and Challenges Associated With Working in Non-Western, Non-Eastern, Non-Democratic, International Environments**



Angela Maitner, Department of International Studies, American University of Sharjah, United Arab Emirates

By some standards, I have a diverse educational background. I earned my B.S. in psychology (with minors in anthropology and math) from Eckerd College, a small liberal arts college in St. Petersburg, FL. I completed my Ph.D. at the University of California, Santa Barbara before pursuing a postdoctoral fellowship at the University of Kent in Canterbury, England. I've attended both small college and large research university, and spent time in the U.S. and abroad. And yet in my training, as most of us, I was only exposed to the WEIRD (Western, educated, industrialized, rich, democratic; see Henrich, Heine, & Norenzayan, 2010), populations on which our science tends to be based.

Two years ago I accepted an assistant professorship at the American University of Sharjah, United Arab Emirates. AUS is an Emirati institution offering coeducational, liberal arts, American-style education. Our students represent 80 different nationalities with the largest proportion (19%) being Emirati (Jordan 12%; Egypt 8%; Syria 8%; Pakistan 7%; Palestine 7%, India 5%, Saudi Arabia 4%, Iran 4%, Iraq 4%, Other 22%).

When I told people about my decision to move to the UAE, reactions ranged from supportive to confused to blatantly disapproving. What united people in their reactions was their immediate desire to know why (on earth) I wanted to move there. I gave a standard answer about the opportunities that existed for someone who studied intergroup relations. Two years in, it doesn't matter why I decided to go - I probably don't have introspective awareness of my decision making process anyway. What matters is that I went. Here I'd like to share some of the insights that I've have in that time, outlining some of the realistic challenges faced by faculty working in non-Western (and non-Eastern) educational environments and suggesting possibilities for the field. Yes I'm tainted by the post-decisional dissonance processes and self-defensive biases that affect us WEIRD people, but let me give as balanced a presentation as my discrepancy-based esteem will allow.

#### **Research**

From a research perspective, being in a Middle Eastern context is incredibly exciting. Since September 11th, the Muslim world has become an increasingly intriguing target for studies on stereotypes, prejudice, and intergroup conflict. Representing the alternative perspective is critical for developing a more dynamic and interactive understanding of intergroup relations.

There are also tremendous opportunities to explore cultural influences in a geographically central and ethnically diverse location. (Are people individualist? Collectivist? Something in the middle? Both? Neither?) At the same time, looking for processes that are universal (at least to educated, industrialized, rich countries) is a valuable enterprise. For example, in the UAE, I witness a tremendous amount of system justification. However, ideologies that sustain system-justifying beliefs in the West, such as meritocracy beliefs or endorsement of a protestant work ethic, are less approach me to discuss s relevant in the Emirati context. One clever student suggested that instead, perhaps a "wasta" ideology (wasta being an index of the social power individuals hold through tribal affiliation and the other connections the individual has) explains the acceptance of a more nepotistic system. What

my student noticed was that the process of system justification may be somewhat universal, although known supporting ideologies may be insufficient for explaining what sustains the process in a non-Western, non-democratic environment.

Being at AUS also presents simple but frustrating challenges to my research profile. The UAE is a truly global environment. Only 15% of the population are Emirati; 85% of the population comes from somewhere - anywhere - else. Society is clearly stratified, and strata are defined by nationality and socio-economic status. As pointed out above, our university is a microcosm of the larger national makeup. Such diversity is exciting to live with (I have departmental colleagues from China, India, Russia, Germany, France, the U.K., Denmark, Canada, and the U.S.), but a nightmare for error variance. Although we may restrict study participation to certain groups, scale reliability is a challenge (perhaps both due to language variability and a strong desire to play one's cards close to the vest - it's very difficult to get students to report their attitudes toward *anything*). In other words, getting data that can be reliably translated into simple descriptive statistics can be quite difficult.

A solution many colleagues point to is 'simple' - "well you need to use implicit measures then." Certainly we can. This necessitates the creation of databases that include photos of men and women, covered and uncovered, of many different ethnicities, etc., etc. In other words, there is no such thing as a simple solution, and we cannot rely on previously created measures without extensive pretesting. As a colleague reflected after reading a draft of this paper, "getting simple things done can be much more difficult than back home." Progress can be frustratingly slow.

Even when we can appropriately index our constructs, finding contexts that create desired psychological experiences remains a challenge. I use my students as anthropological informants to discover events that have psychological significance to our subject pool. For example, I conducted one study looking at emotional reactions to norm violation after a student expressed how upset it made him to encounter a group of students eating in the center of the building in the middle of Ramadan. I created another study looking at reactions to discrimination after a student brought in a text message advertising a post where payment was (explicitly) determined by ethnicity. Even these studies, however, are affected by the tremendous diversity within the student population where particular conflicts or groups have more or less relevance to different individuals (from different tribes, religions, strata, etc.).

Ultimately the demand is to find new ways to do our science. It requires creativity, active listening, patience, and pretesting galore. Optimistically, I look at the work I've conducted thus far as an investment that - insha'allah - will pay off in the future.

### **Teaching**

AUS doesn't grant a psychology degree. I teach courses (Social psychology, Scientific method in psychology, Stereotypes, prejudice, and discrimination) in a Department of International Studies. Psychology courses are not required for any student on campus, but serve as social science elective courses for students in all schools/colleges. What that means is that students who elect to take psychology courses tend to enroll due to a genuine interest in learning about human behavior or in talking about topics that they are not free to discuss elsewhere. Don't get me wrong, others enroll to be with their friends or because they believe (contrary to evidence) that the course will be easy, but we do get a strong core of students who make teaching worthwhile.

Developing mutual trust with students is critical to any classroom experience. When I first moved, I was concerned that my students would look at me with skepticism as an outsider whose knowledge of how 'humans worked' was limited to the dominant (in their minds, dominating) American perspective. My expectations were quickly shattered. Even though our texts and papers tend to present a Western perspective (filled with Western contextual examples), students identify with processes from impression formation, to deindividuation, to relationship dissolution. I do focus on cross-cultural (East vs. West) influences on nearly every topic we discuss, asking students to note with which outcomes they most identify. For many, it's a somewhat confusing mix.

When I'm explaining psychological processes, my students seem to prefer that I use examples that I am familiar with, often themselves making connections to their own cultural experience. Students have pointed out, for example, that the Holy Month of Ramadan is a time where they are asked to engage in heightened self-awareness (via heightened self-reflection), which should increase motivation to live up to the ideal self via faith, self-control, and charity. In fact, many students examine their religion within the scientific context, noting both points of

intersection (the Ramadan example above) and divergence. Others consider issues like the Israeli-Palestinian conflict or the Arab spring (realistic conflict, system justification, terror management, etc.), or general issues of justice and equality in their own lives. Gender, social roles, and family play a much stronger role in my students' lives than they do in my own, and again I try to solicit examples about how different processes we discuss operate (or may be modified) in students' own lives. I also make slight modifications to teaching materials, for example, by talking about the 'psychological end' to a relationship rather than divorce (which, to varying degrees is under personal control), as something that is predictable by things like satisfaction, comparison level, etc. I find that with these small modifications, students tend to see clear connections between the concepts we understand (given data collected in the West) and their own experience.

Although the critical way many students consider psychological information can be incredibly inspiring, other reactions are equally frustrating. Many students come from an educational background where their primary motive is to memorize and regurgitate information rather than to consider it critically. As a result, their abilities to memorize information are incredibly well developed, but they struggle with an ability to reconstruct meaning. This makes it more difficult for many students to apply or critically think about the information presented. It also increases problems with plagiarism. I've had students respond to essay questions on exams by replicating information from slides or the text directly. Their answer was 'right,' and they didn't cheat, but it was also unoriginal. Communicating that I want to see students' own unique comprehension can be difficult.

Other aspects of the environment also present challenges. AUS is an English language institution. Many of our students have been speaking English from a very young age and have perfect American or British accents. Others struggle much more with reading and writing in English, and their grades suffer as a result. While students are accountable for English language competence and make a choice to study at an English-language institution, ensuring that we can test their knowledge of the material independent of their language skills requires very careful construction of testing items and research questionnaires. My graduate advisor would no doubt have a heart attack if she saw some of the grammatically incorrect questionnaire items I've allowed students to use for class projects. Even worse, those students get better responses than I do when I use 'correct' items. I often feel like an outsider with my own language.

Arab culture is also a bargaining culture - the norm of reciprocity is incredibly strong and permeates all aspects of life. In the teaching environment, that means that students consider grades negotiable. (Of course students try to negotiate grades everywhere, but the expectation of a successful outcome is quite unique here. Their intuitive awareness of Cialdini's influence techniques is stunning; forget used car salesmen, we should be studying people working in open air markets, or souqs.) Across 5 semesters I've become increasingly firm and explicit on syllabi and in person with the nature of grades and how or when students are welcome to approach me to discuss them. This is only partially successful. When students speak to me, I invoke values of fairness and equality that may be somewhat foreign to many of them (and according to Haidt and colleagues, very leftish of me in a very rightish context). In fact, this brings up a larger issue. The fact is, I live in a collective environment but teach in an individualist way - endorsing the values and educational style of the institution. Students, however, are sometimes uncomfortable being individually responsible for their success and failure, and our Western teaching styles may provide an inherent conflict with many of their developed learning skills.

Any faculty person who has the opportunity to work with international students no doubt faces similar challenges. For that reason we might all try to avoid culturally-contingent examples on exams, and to facilitate collaborative learning opportunities for students with different values and skills. Personally, I find my interactions with students to be some of the most rewarding parts of my job. It's also quite clear that they teach me significantly more than I teach them (please don't tell them). What I learn may not always be positive in my WEIRD view, but it's always enlightening.

### **Living**

When I tell people I live in the UAE, I get a number of questions. Let me start with a rapid fire response regarding a few of the most misinformed ideas. (1) I don't cover; it's hot so I usually wear t shirts and long skirts (2) I personally own a car and drive (3) Alcohol and things like pork are available for people who want them - although there are laws that restrict who can partake (4) Yes there is a measure of benevolent sexism, but generally I haven't been treated differently for being a woman (5) and finally: Yes this country has a lot of oil. No I don't have an exorbitant

salary. In fact, I have a very standard salary. However, there are no income taxes in the UAE, and housing is provided. In other words, my salary is what I take home, and aside from my car (and my undergraduate student loans) I have no monthly expenses. It's important to note, however, that all of these things vary by country or region. Sharjah, for example, is the most conservative of the 7 Emirates, and that influences how people should dress, etc.. However, both here and in my travels in the region, I tend to find that if I respect local culture, locals also respect me (again, that norm of reciprocity). Moving on.

Geography is an amazing thing. I can be anywhere from Nepal to Kenya is 4.5 hours, and that's pretty special. However geography also provides realistic challenges to both personal goals (getting to see family, etc.) and professional development. Although Dubai is connected to most global destinations, we can't hop on a plane to attend a workshop or small group meeting in 3-4 hours. In fact, the Dubai - LA flight takes 17 hours, which is the time it will take me to get to SPSP this year - *if* I book the direct flight; considering it costs twice as much as a flight with a layover, I probably won't.

At AUS the president of the University is the ruler of Sharjah. He greatly values education and has built several fantastic educational institutions in the Emirate, the vision and direction of which are largely determined by his long-term goals. Part of Sharjah's unique identity within the UAE is one of being a cultural and educational leader, and therefore the Skeikh has invested into these institutions. It is unclear whether future rulers will maintain this vision for the Emirate, and therefore whether their priorities will be the same.

When considered next to the current "Arab Spring" (a movement that has not actively affected the UAE although we have experienced indirect consequences), it should be clear that "things" can change very quickly here. Although my current position is stable and exciting, the high faculty turnover - as is common at most ex-pat universities - and opportunity for policy and procedure to shift quickly demands that faculty maintain an open mind about their current and future employment opportunities. Although I'm hopeful that everything I'm doing at AUS will pay off, there are realistic risks that the investment may not be given time to flourish due to changes at a regional, national, institutional, or personal level. But this is where I want to be. But daily I also live the processes that we know are likely to reduce prejudice and conflict, engaging in contact and sharing goals and outcomes with groups who are often stigmatized within (and between) societies. I hope that through extended contact, I'm able to influence attitudes of my friends and family, and my students' friends and family as well.

### **Opportunities for the Field**

Educational development in non-Western, non-Eastern, non-democratic countries provides tremendous opportunities for our science to explore cultural diversity and universality. The society I live in, and the environment in which I work is still EIR (in those ways, not very different from East Asia); thankfully people don't fall into distinct categories of WEIRD and not-WEIRD. Instead we need diverse datasets gathered in different parts of the world to gain a more complete understanding of the human experience. While doing short term data collection in other societies is an excellent way to increase our broad knowledge (especially in locations where there are few universities), it's also important to invest time in a place to know how to ask questions, give instructions, or create stimuli that will best create the process we are trying to capture. Encouraging motivated and skilled young scientists to act as ambassadors for our field, while collecting data in service of our collective knowledge, is valuable. Welcoming international students who have the ability to provide a diverse perspective and collect data in their home countries is another way to broaden the diversity of both background and belief within our field.

Realistically, working in non-Western, non-Eastern, non-democratic countries wouldn't suit everyone. At a minimum, doing so requires freedom of movement, an openness to experience (that includes frustrating, different, or difficult experiences), and an openness to non-traditional career (success and failure may be based on different metric than the one used to evaluate a more traditional research or teaching career). But, then again, you get to escape what's WEIRD. If this sounds like you, challenges and opportunities await.

### **Reference**

Henrich, J., Heine, S. J., & Norenzayan, A. (2010). The weirdest people in the world? *Behavioral and Brain Sciences.*, 33, 61-135. doi:10.1017/S0140525X0999152X



**Obituary****Kevin M. Carlsmith, In Memoriam**

Christopher Carlsmith, University of Massachusetts-Lowell  
John Darley, Princeton University  
Rebecca L. Shiner, Colgate University  
Timothy D. Wilson, University of Virginia



Kevin M. Carlsmith died peacefully on November 19, 2011 from cancer in his boyhood home in Portola Valley, CA, surrounded by his family.

An accomplished researcher and a popular professor of Psychology at Colgate University since 2003, Kevin earned a Ph.D. at Princeton University (2001), an M.A. at University of New Hampshire (1996), and a B.A. from Lewis & Clark College (1989).

Kevin grew up next to Stanford University as the son of two academic psychologists, J. Merrill Carlsmith and Lyn K. Carlsmith. At the age of four he was a participant in Walter Mischel's famous study of delayed gratification at Bing Nursery School. He knew many members of the Stanford Psychology faculty informally, and his childhood antics were frequently cited by Lyn in her classes on childhood development. Despite (or perhaps because of) his proximity to the field of psychology, he did not embrace that academic discipline until his freshman year of college, when he discovered it was a topic for which he exhibited both passion and talent. His other great collegiate passion was the outdoors, which he had come to love as a boy on backpacking trips to Yosemite and the Sierra Nevada mountains. Kevin was deeply involved with the outdoor program at Lewis & Clark and led frequent trips into the wilderness of the Pacific Northwest. After college he worked at the North Face and served as a river guide and rock-climbing instructor for Outward Bound. He loved the wilderness for both its beauty and its unpredictability. He taught for four years at the White Mountain School in New Hampshire, tutoring students with learning disabilities, supervising a dormitory, and offering instruction in a variety of outdoor activities all year long.

Kevin's experience at the White Mountain School was transformative in several ways. He realized that while he loved outdoor education, he was equally fascinated by classroom pedagogy and by the opportunity to figure out how his students were thinking. He had rediscovered his interest in psychology, and in 1994 he returned to academia to pursue an M.A. degree at the University of New Hampshire under the direction of Jack Mayer. In 1997 Kevin moved on to Princeton to study with John Darley and earned his doctorate there in Psychology in 2001 with a dissertation on revenge and justice. John Darley remembers that Kevin was consistently prepared and wonderfully well-organized, with well-developed skills in statistics and in expository prose. Kevin himself was proud of his ability to thrive intellectually in such a rigorous academic environment. He had found his calling at last.

Kevin's research examined lay theories of morality and justice, including people's naive theories about important kinds of social behavior (e.g., punishment for deviant acts) and how these theories drive behavior (e.g., the kinds of prison sentences people recommend). One interesting question he examined, for example, is whether people are fully aware of how they form judgments about transgression; there appear to be many cases in which people say one thing but do another when it comes to determining punishment. He uncovered a number of interesting cases in which people's theories about transgression and punishment bear little relation to the rationale behind the legal codes. In addition to examining basic questions about people's views of morality and social behavior, this work has intriguing implications for social policy.

He and John Darley found a joint interest in determining which of the many goals that exist for punishing wrongdoers are the ones that really motivate ordinary people to assign punishment to those who have been convicted of crimes. Certainly, people do this in order to deter crime but Kevin and John discovered that individuals from western cultures tend to have an immediate intuition that the offender "deserves" punishment and the magnitude of the punishment is to a considerable extent shared on most offenses.

A two-year post-doctoral fellowship at the University of Virginia allowed Kevin to work with Tim Wilson and to further refine his research agenda. During this time Kevin conducted research on the affective consequences of revenge, finding that whereas people believe that exerting revenge (punishing a free rider) will make them feel better, it actually makes them feel worse. He also taught the introductory social psychology course at UVa to rave reviews. He once said that he treasured every minute of class and hated letting the students go, feeling that he had more to say about the many fascinating topics in social psychology. Clearly his students felt the same way, giving him some of the best course evaluations in the department. One student sent an unsolicited letter to the Chair of the department that read, “Kevin Carlsmith is a phenomenal professor . . . I view this course as one of my most valuable experiences in the past few years, and will carry the lessons learned here with me forever.”

In 2003, Kevin became an assistant professor at Colgate; he was promoted to associate professor in 2009. He taught a variety of classes at Colgate, including Social Psychology, Statistics, Propaganda and Persuasion (initially developed with Joel Cooper at Princeton), and a freshman seminar of his own design entitled “Just Punishment.” A 2008 letter in support of Kevin’s tenure application described him as “a thought provoking, dynamic, organized, and enthusiastic teacher” who routinely incorporated new academic technology into his classroom. At a gathering in Fall 2011 to honor Kevin, his Colgate students spoke and wrote with poignancy about how his teaching influenced their view of the world in very practical ways. For example, many of his students reflected with laughter and wonder on Kevin’s assignment for them to consciously break a social norm on campus, and to document the reactions of others and of themselves; this is a clear example of Kevin’s ability to help students apply academic material to their own lives and to societal issues. He also served as Chair of the Institutional Review Board at Colgate and as Faculty Advisor to the Psychology Club. His students and colleagues there speak in glowing terms of the contributions that Kevin made to the department and to the school. His advisees praised his compassion and his willingness to let students make the major decisions. Kevin inspired students to pursue challenging theses and ambitious research projects; he championed both efficiency and collegiality in department decision-making; he provided humanity and practical suggestions in administrative capacities; he was a valuable resource for colleagues in thinking through the research design and statistical analyses of their own research.

Kevin published his findings in numerous prestigious journals, and was regularly invited to comment in the mass media, including the New York Times, LA Times, and Canadian Broadcasting Corporation, about contemporary issues of punishment, such as analyzing the motivations and justification for the killing of Osama Bin Laden. He possessed a particular expertise in statistics, an ability that he may have inherited from his father, Stanford professor J. Merrill Carlsmith. The recipient of three grants from the National Science Foundation, Kevin was first author of more than a dozen articles as well as numerous encyclopedia entries, and a regular reviewer of scholarly articles for journals in psychology and law.

In 2009 Kevin received a major grant from the National Science Foundation to advance his research on revenge and punishment. The anonymous reviewers were unanimous in their praise for Kevin’s project. One wrote: “I see Carlsmith's work as transformative in the most profound sense, because his research will help shape the future of research and public discourse on an important scientific, social, and political question: why do people support and carry out torture? This question is not just important for the United States, and not just for the Bush and Obama administrations. This is a global issue.” Another reviewer added: “[T]his proposal is of interest to many disciplines including law, political science, and public policy, not simply to psychology. It is also of great relevance to current events, and has the potential to make an impact not only within academic circles but also on actual public policy decisions. The broader impact of this research is not in doubt.”

Perhaps the most telling comment of all came from a reviewer who expressed frustration at being unable to find any flaws at all in the project’s design:

Reviewers are supposed to read proposals carefully and point out all of the ways in which the proposal could be improved. This grant has me feeling like the Maytag repairman. I think this grant is terrific in all ways, and I have nothing to criticize or even recommend to improve the PIs existing ideas. . . I clearly have no ideas that the PI has not considered already, and the ones I was considering were not as interesting as the ones he proposes. The predictions are interesting and counterintuitive, with pilot data to support them. The experiments are programmatic and ambitious, moving the clear ideas mentioned in the introduction into new and interesting areas. I anticipate that the PI will generate many more interesting follow-ups than he even anticipates at this point. It's among the best proposals I have seen. That it's being

conducted at an undergraduate institution only augments my very positive impression of this proposal. It is terrific, and deserves the highest priority of funding.

In 2001 Kevin married Alison Mathias, a Virginia native whom he had met in a swing-dance class at Princeton University. They have two daughters, Abigail and Julia. A devoted father, Kevin lavished attention upon “his girls” as he affectionately referred to all three of them. He relished the opportunities to introduce his daughters to ice-skating in the winter, Disneyworld in the spring, and swimming at his family’s camp in New Hampshire during the summer.

In 2010-11 Kevin was appointed as a Fellow of the Center for Advanced Study in the Behavioral Sciences at Stanford University. He was eager to introduce his wife Alison and his two young daughters to the splendors of the Bay Area, and he was delighted to have the chance to collaborate with so many other social scientists. He was also pleased to follow so closely in his parent’s footsteps: Merrill had been a Fellow at CASBS in the 1970s, and Lyn was a frequent visitor there as the steadfast companion of Director emeritus Gardner Lindzey. Sadly, Kevin’s cancer prevented him from utilizing the resources there to his full advantage, and his health declined significantly during his year there. During that same year, Kevin provided sensitive and compassionate care to his ailing mother Lyn while managing his own health issues, taking care of his family, and arranging his affairs. A clear-eyed social scientist right to the end, Kevin wrote a blog about his illness that showcased his dry wit, his optimism and zest for life, and his detailed understanding of the disease that afflicted him.

Kevin was always thoughtful and deliberative. Gentle and kind, he retained a fierce desire to live coupled with a serene dignity in the face of death. Even as he battled his own disease, he paid extraordinary attention to his ailing mother to make sure that she was well-cared for, and to his daughters so that they would be prepared for his passing. We will miss his wise counsel; his delight in the achievements of his children; his keen insights into the human mind; and his enthusiasm for family, friends, psychology, and the outdoors.

In addition to his immediate family of Alison, Abby, and Julia, he is survived by his brother Chris Carlsmith and his family of Arlington, MA, and his sister Kim Sampson and her family of Orlando, FL.

## **EXPERT SEMINAR**

### **Time for Writing and Other Endangered Creatures**



Paul J. Silvia, University of North Carolina at Greensboro

In the academic year, as in life, there is a season for all things: a time to teach, a time to collect data; a time to mentor young minds, a time to grade the disastrous output of those minds; a time to serve on the Associate Vice Provost for Parking Service’s Utilization Forecasting Committee, and that’s basically it—then it’s time to go home, cursing about parking during the long trek to your car.

During these many seasons the bitter winter of writing often never arrives. Writing is cruel and tempting and vexing, like a mean mermaid. If anyone needed evidence that being a professor isn’t a real job, the way the job treats scholarly writing is it. In the dark abyss of corporate employment, people actually do the things they were hired to do while at work. In the shiny ivory tower, people do their core work—writing books, articles, and grant proposals—during the evenings, weekends, and summers. If a psychology departments were like a Subaru dealerships, they would post numbers for time spent writing, articles published, and grants submitted on a huge whiteboard, and a few months of bad numbers would get you fired.

I confess that I was inspired to enter academics because one of my undergraduate professors once explained to me why it isn’t a real job. After 10 years as a professor, I’m more convinced than ever that our work only vaguely resembles gainful employment. Real jobs have more structure, more incentives, more performance-based assessment, and fewer committees. Publishing research is a big part of our jobs—for some of us, it is basically why we were hired and why we will or won’t be promoted—but the structure of a day at the office doesn’t afford time to write.

My approach to making time for writing follows from a bigger picture of time management that I’ve worked out over the years. My vision of time management isn’t something I talk or write about much, in part because my thoughts about it are messy and incoherent, in part because the Vice Provost for Parking Services would get mad,

and that's the guy with real power at any university. But here it is, for what it's worth.

A lot has been said about time management, usually by business gurus who are selling books, workshops, and "productivity tools," whatever those are. (If a hammer is a tool for pounding nails, one wonders what gets pounded by a fresh box of file folders.) Many a reader of David Allen's *Getting Things Done* has bought a label maker and wasted a work day in an orgiastic binge of labeling—but at least visitors to my office now know that my white board is a "white board." The time management lore, for the most part, has some good ideas: make to-do lists, set concrete goals framed in terms of behaviors, and save the brainless stuff for tired moments.

At the same time, most time management books suffer from the same few flaws. The typical book lacks humility—not an uncommon thing among writers of books for business people—particularly humility about how well we can control the events in our world. There is structure, and there is chaos, and even your iPad's mighty productivity apps can't keep a chaotic universe in abeyance. Time management is a struggle because of the many random interruptions that happen, like random things do, randomly. We can impose some structure over our workdays, but it's a fantasy to think that we could structure it all if we would only schedule more precisely, set goals more concretely, and label more diligently.

A different way to think about time management—one that I hope is more humble and realistic—comes from my undergraduate background in animal behavior. I've come to think of my work week as an ecosystem and my goals as animals that live there. And as the Discovery Channel has revealed, when a bunch of animals get together, someone is going to get eaten. Some of our goals are predators, and some of our goals are prey. The predatory goals always get accomplished because, by virtue of being important, urgent, time-sensitive, fun, or easy, they devour other goals. Here, as an example, are my week's usual predators:

- All things instructional: Teaching, grading, prepping lectures, & irascibly grumbling about the kids these days
- Writing letters of recommendation that strain the limits of mendacity
- Mentoring wayward graduate students, realizing that I have no idea how to teach them to do what I do, and secretly suspecting that that's for the best
- Anything involving any administrator farther than 30 feet from my office
- Research cataclysms, such as the software crashes, equipment failures, and personnel flakiness that might be dark harbingers of the project's likely failure
- Reviewing manuscripts, grant proposals, promotion-and-tenure dossiers, book proposals, and other reminders of the fact that I'm not doing my own writing
- Getting coffee at the coffee shops next to the office
- Writing memos, reports, documents, budgets, and other detritus of administrative obligations
- The Internet, the meanest mermaid of them all

To be sure, the week's predators, like nature's predators, can be cute and cuddly. Even chinstrap penguins devour some other hapless animal. A predator is simply something that outcompetes something else—many predatory tasks are among the job's most meaningful duties. For example, the apex predator, the great white shark of the professoriate, is teaching. Teaching is central to what we do, and I enjoy it. It's the apex predator not only because teaching times are scheduled for us, but because we take it seriously.

To find your prey, just think about what goals get killed during the work week. Here's my own endangered species list:

- Reading things other psychologists write—or anything not involving mermaids, princesses, or Clifford the Big Red Dog
- Preparing submissions for conferences, those magical places where I hear people talk about their research so I don't have to read it
- Unstructured teaching and mentoring, such as shaping the lumpy clay of my first-year graduate students into erudite lumpy clay with publications
- Thinking, pondering, mulling, reflecting, and contemplating about psychology, i.e., the ostensible duties of a professor that never happen
- Writing, i.e., writing

Writing is the most woeful creature on this list. Time for writing is the giant panda of the professorial ecosystem—it would go extinct without intensive intervention by people who listen to a lot of NPR. Writing is easy to put off and easy to do anywhere, so it’s a fat target for even lowly predators—that’s why a mangy beast like the Arts & Sciences Enrollment Management Committee can munch on your half-done manuscript.

Thinking about the work week in terms of competing goals shows us ways to make time for writing. As professors, we need to do for writing what naturalists and liberal tree-hugging do-gooders do for endangered animals: stick them behind big fences. Easily devoured goals need to be protected. Writing will rarely be urgent enough to outcompete other goals, so it needs its own space—a nature preserve for half-done manuscripts and rejected grant proposals, if you will.

I treat time for writing as a special time: I carve out time for it, and that time gets spent only for writing. Only the biggest predators, like emergencies and illness, can devour it. Scheduling writing works because writing is no longer competing with your classes, administrative duties, and random goals that stumble into your inbox. For many years, I wrote between 8 am to 10 am each weekday. Once I became a papa, I shifted to 5 am to 6:30 am each weekday. The mornings work well for me, but most people I know who use a writing schedule pick more humane times, like 10 am to 12 noon three days a week. Four to six hours a week, scheduled like a class and treated with the same seriousness, will protect your time to write.

For me, the writing schedule is structure and everything else is chaos. I barely bother to plan the free time in my work day. The most important things always seem to get done, simply by virtue of deadlines and urgency. And many smaller things never get done, which I take as a sign that they weren’t important enough to compete for time and action. It’s good that we don’t get everything done. Just as narrow streets and small parking lots discourage needless driving, limited time and chaotic weeks discourage committing to capricious projects and trivial ideas.

There’s a certain perversity about this way of thinking about time management. In short, I think we should protect the most essential and fragile goals—writing, reading, and thinking—and then let everything else fight it out. Urgency, procrastination, and natural selection will ensure that the most significant tasks will happen: courses will be taught, manuscripts will be reviewed, letters of recommendation will be written, and chaos will work its weird ways. It might not be relaxing—such is chaos—but the writing gets written, and that’s all I hoped for anyway.

Paul J. Silvia is an Associate Professor of Psychology at the University of North Carolina at Greensboro. He is the author of five books. His best known book is *How to Write A Lot: A Practical Guide to Productive Academic Writing*, but his strangest book is *Public Speaking for Psychologists: A Lighthearted Guide to Research Presentation, Jobs Talks, and Other Opportunities to Embarrass Yourself* (written with David B. Feldman). An earlier version of this essay appeared in *Relationship Research News*.

## Why Don’t You Give Me Feedback?

Brett Pelham



“Why won’t you give me feedback on my research paper? I thought you said we had a ‘mentor-mentee’ relationship.”

**IN BRIEF****News from the Social Psychology Program at NSF**

Kellina Craig-Henderson, National Science Foundation



Greetings from the Social Psychology program at the National Science Foundation!

Many thanks to Dialogue for making it possible to share news with you. While I am no longer officially serving as the program director for the Social Psychology program at NSF, I continue to be associated with the program and I wanted to take this opportunity to share news about the program in this issue of Dialogue. First, I would like to take this opportunity to recognize the distinguished achievements of our colleagues who have received or were recommended for research grants from the Social Psychology program at the National Science Foundation during the most recent calendar year and fiscal period. As you can see by this list, the portfolio of scientific investments made by the Social Psychology program is broad. Feel free to take a look at the abstracts for these and other proposals funded by NSF and the program in the Awards Database at <http://www.nsf.gov/awardsearch>. And, if you wish to do a broader search in social psychology, click on the "Program Information" tab and enter 1332 as the Element Code.

**MOST RECENT RESEARCH GRANTS**

- Lisa Feldman Barrett (Northeastern University) The Affective Vision Hypothesis
- Cheryl Kaiser & Brenda Major (Univ. of Washington, UC- Santa Barbara) Collaborative Research: Diversity Structures Create Illusions of Fairness
- Camille Johnson (San Jose University) Directed Comparison: Social Comparisons as Social Influence
- Yaacoc Trope (New York University) - Expansive versus contractive relational scope
- David Funder (UC – Riverside) - The Construal of Situations
- Garold Stasser (Miami University) - Missing Links Problems and Participation in Collective Decisions
- Jason Themanson (Illinois Wesleyan University) - RUI: A Neural and Behavioral Examination of Social Exclusion Processes
- Sarina Saturn (Oregon State University) - CAREER: Biological Mechanisms Underlying Individual Differences in Elevation and Altruism
- Sandra Murray (SUNY at Buffalo) - Impulsive and reflective trust and the transition to parenthood
- Steven Stroessner (Barnard College) - Self-regulation and threat: Shifting tactics in the face of threatening stereotypes
- Jeffrey Lucas (University of Maryland, College Park) - Social Structure, Self-Construals, Cognitive Orientation, Trust, and Commitment
- Elizabeth Pinel (University of Vermont & State Agricultural College) - Shared Subjective Experience as a Catalyst for Social Harmony
- Emily Balcetis (New York University) - Self regulation through motivated perception and mobilization
- Jennifer Beer (University of Texas, Austin) - Self-Esteem Threat as a Moderator of the Mechanism Underlying Exaggerated Positivity
- Daphne Bugental (UC- Santa Barbara) - Parental Investment Patterns in a Shifting Economy
- Angela Gutches (Brandeis University) - Memory Specificity Across Cultures
- Janet Swim (Pennsylvania State Univ University Park) - Masculinity and Environmental Engagement
- Scott Eidelman (University of Arkansas) - Stereotype Endorsement in Achievement Settings
- Ed Lemay (University of New Hampshire) - Maintaining Relationships with Chronically Insecure Partners: Interpersonal Security Regulation
- Kenneth DeMarree (Texas Tech University) - Mimicry and Confidence: New Insights into the Positive (and Negative) Consequences of Behavioral Mimicry
- Paul Eastwick (Texas A&M University Main Campus) - A Phylogenetic Evolutionary Psychological Approach to Human Mating
- Kerri Johnson (UCLA) - Social Categorization at the Crossroads: The Mechanisms by Which Intersecting Social Categories Bias Social Perception

- Robert Sellers (University of Michigan) - African American Racial Identity and Coping with Racial Stressors
- Laurie Rudman (Rutgers University) - Motives for Backlash Against Gender and Racial Vanguard
- Will Cunningham (Ohio State University) - Exploring the conflict between self-interest and concern for others
- Simine Vazire (Washington University) -- Blind Spots and Bright Spots in Self-Knowledge
- Tessa West (New York University) -- How Accuracy and Interpersonal Bias Combine to Improve Interracial Interactions and Relation
- Jeffery Simpson (University of Minnesota) -- Interaction of Current and Childhood Environment on Risky Decision Making
- Dana Carney (University of California, Berkeley) -- CAREER: How Power Corrupts: Power Offers Immunity to the Emotional
- Ronnie Janoff-Bulman (University of Massachusetts, Amherst) -- Moral Regulation: A Dual System Perspective
- William Swann (University of Texas at Austin) - Identity fusion and extreme group behavior
- Jon Maner (Florida State University) - Hormonal and Behavioral Responses to Social Threat
- Shelly Gable (UC – Santa Barbara) - Capitalizing on Positive Events
- Leaf Van Boven ( University of Colorado at Boulder) - From Mindless to Mindful Choice: How Introspection Improves Decision Making
- Linda Skitka (University of Illinois at Chicago) - The Revenge Motive: Understanding Public Bellicosity and Closure in a Post-9/11 World
- James Hamilton (University of Alabama Tuscaloosa) - Predicting Trajectories of Post-disaster Adjustment from Pre-Disaster Assessments of Risk and Resilience Factors

#### ***SOCIAL PSYCHOLOGY PROGRAM UPDATES***

From January 2011 to December 2011, the Social Psychology program received and considered proposals for approximately 200 research projects, which included at least 50 that were submitted to other programs but were considered to be relevant to advancing understanding in the field of social psychology.

#### **STAFFING**

At the time of this printing, the Social Psychology program is undergoing transition in staffing. Please welcome Chuck Stangor of the University of Maryland, College Park who is temporarily serving as a steward of the Social Psychology program at NSF. Brett Pelham and I have moved onto other positions. Brett is now at the APA serving as Director of Graduate Education. I am still at NSF but having assumed more management responsibilities, I am only able to assist the Social Psychology program in a limited fashion until permanent rotators are firmly in place. A national search is currently underway for new program directors of the SP program and we anticipate having someone in that role by the start of the July competition.

It often comes as a surprise to people to know that half of the program officers directing disciplinary and interdisciplinary programs at NSF are rotators who come for a year or two to learn about NSF, the proposal and review process, the role of science in the federal context, and to take that knowledge and expertise back to their universities. I'd like to take this opportunity to encourage you to consider applying as a rotator to NSF when a position is available again – it is a remarkable learning experience and you will appreciate having had it. To learn more about serving as a rotator, please see: [http://www.nsf.gov/about/career\\_opps/rotators/index.jsp](http://www.nsf.gov/about/career_opps/rotators/index.jsp). As always, a post for newly announced positions of interest will be available at the SPSP Listserve.

#### **PROGRAM BUDGET**

The Social Psychology program began FY2012 with a percentage of the previous year's budget, which eventually topped out at \$6.3M. This was a result of multiple small incremental increases toward the program's initial base budget. As you probably know, the current fiscal year (FY2012) began with a somewhat rocky start and we were instructed to plan for program activities with only a percentage of the FY2011 budget. We are now in receipt of program dollars and because of our fiscally responsible management of program responsibilities, we can be optimistic about being able to support new and exciting research from the Spring 2012 pool of proposals as well as fulfill the program's existing obligations for ongoing research.

**THANK YOU!!!**

We also wish to recognize the considerable efforts of those members of the Social Psychology community who reviewed proposals in this past year. This includes the members of the advisory panel who meet twice a year to evaluate the scientific merit and broader impacts of proposals, and several hundred ad hoc reviewers who similarly advise on individual proposals. These anonymous reviewers perform a valuable service to the social psychology community and I would like to publicly thank them for their work.

### **ON NSF'S AND THE PROGRAM'S HORIZONS**

There are a number of broad-based, interdisciplinary opportunities for social psychological research across NSF. Below are a few such opportunities that may be of interest to you or your colleagues. If something strikes you as a possibility, please follow up with the program director associated with that program. The very best way of doing so is to email them with a 1-page description of your research idea making sure to include some evidence of the literature you're drawing from as well as the methods to be used. I've provided some links to the program sites.

#### **SCIENCE OF BROADENING PARTICIPATION**

Recently, the Division of Behavioral and Cognitive Sciences (BCS) and the Division of Social and Economic Sciences (SES) within the Social, Behavioral and Economic Sciences Directorate (SBE) announced their intentions to stimulate interest and activity in research related to the Science of Broadening Participation (SBP). A *Science of Broadening Participation* employs the cognitive, behavioral, social and economic sciences to inform approaches to broadening participation and will strengthen our national science, technology, engineering, and mathematics (STEM) capabilities and competitive advantage. Ultimately, the SBP can provide policy makers with the evidence needed for informed decisions. During the current fiscal year, the SBE directorate is providing supplementary support for meritorious proposals that utilize the theories, methods and analytical techniques of the social, behavioral and economic sciences to better understand the barriers as well as factors that enhance our ability to broaden participation in STEM. Supported research may identify from an empirical standpoint those strategies most likely to improve the representation and participation of women, minorities and persons with disabilities who are under-represented in STEM fields. The Dear Colleague Letter announcing this emphasis can be found at: [http://www.nsf.gov/publications/pub\\_summ.jsp?ods\\_key=nsf12037](http://www.nsf.gov/publications/pub_summ.jsp?ods_key=nsf12037)

#### **INTERDISCIPLINARY RESEARCH ACROSS THE SBE SCIENCES**

Rebuilding the Mosaic (<http://www.nsf.gov/pubs/2011/nsf11086/nsf11086.pdf>), which reports the results of the year-long SBE 2020 visioning process, finds that scholars in the social, behavioral, and economic sciences believe that future research will be interdisciplinary, collaborative, and data intensive. The Directorate for Social, Behavioral & Economic Sciences (SBE) therefore encourages investigators to submit proposals that go beyond the boundaries of traditional disciplines, span across the existing core SBE programs, or extend outside the SBE sciences. Visit the Dear Colleague letter (<http://www.nsf.gov/pubs/2012/nsf12030/nsf12030.jsp>) to learn more.

#### **RESEARCH COORDINATION NETWORKS (RCN)**

Like a number of programs within the SBE Directorate, the Social Psychology program is participating in the RCN program. The goal of the RCN program is to advance a field or create new directions in research or education. Groups of investigators will be supported to communicate and coordinate their research, training and educational activities across disciplinary, organizational, geographic and international boundaries. RCN provides opportunities to foster new collaborations, including international partnerships, and address interdisciplinary topics. Innovative ideas for implementing novel networking strategies, collaborative technologies, and development of community standards for data and meta-data are especially encouraged. Proposed networking activities directed to the RCN program should focus on a theme to give coherence to the collaboration, such as a broad research question or particular technologies or approaches. For more information, please see:

[http://www.nsf.gov/funding/pgm\\_summ.jsp?pims\\_id=11691](http://www.nsf.gov/funding/pgm_summ.jsp?pims_id=11691).

#### **SCIENCE, ENGINEERING AND EDUCATION FOR SUSTAINABILITY (SEES)**

This is a critical place for social psychological insight, and I hope that you will consider the opportunities that exist within this foundation-wide, well funded effort. NSF established the Science, Engineering, and Education for Sustainability (SEES) investment area in FY 2010 in order to address challenges in climate and energy research and



education using a systems-based approach to understanding, predicting, and reacting to change in the linked natural, social, and built environment. Initial efforts were focused on coordination of a suite of research and education programs at the intersection of climate and environment, including specific attention to incorporating human dimensions. SEES is expected to be a 5-year effort, extending through FY15. Continuing efforts will focus on supporting research that facilitates global community sustainability, specifically through building connections between current projects, creating new nodes of activity, and developing personnel needed to solve sustainability issues. Future efforts will be expanded to include sustainable energy research in science and engineering, and its socioeconomic and environmental implications. <http://www.nsf.gov/geo/sees/>

**SCIENCE OF SCIENCE AND INNOVATION POLICY (SciSIP)**

The SciSIP program underwrites fundamental research that creates new explanatory models, analytic tools and datasets designed to inform the nation’s public and private sectors about the processes through which investments in science and engineering (S&E) research are transformed into social and economic outcomes. The research, data collection and community development components of SciSIP’s activities will: (1) develop usable knowledge and theories of creative processes and their transformation into social and economic outcomes; (2) develop, improve and expand models and analytical tools that can be applied in the science policy decision-making process; (3) improve and expand science metrics, datasets and analytical tools; and (4) develop a community of experts across academic institutions and disciplines focused on SciSIP. For additional information, please see: [http://www.nsf.gov/funding/pgm\\_summ.jsp?pims\\_id=501084&org=NSF&sel\\_org=NSF&from=fund](http://www.nsf.gov/funding/pgm_summ.jsp?pims_id=501084&org=NSF&sel_org=NSF&from=fund)

**GRADUATE RESEARCH FELLOWSHIP PROGRAM (GRFP)**

The graduate research fellowship provides a wonderful opportunity for your most competitive undergraduate seniors and first year graduate students. Please consider this. More than 2000 awards were made in the last competition! The purpose of the Graduate Research Fellowship Program (GRFP) is to ensure the vitality of the scientific and technological workforce in the United States and to reinforce its diversity. The program recognizes and supports outstanding graduate students in the relevant science, technology, engineering, and mathematics (STEM) disciplines who are pursuing research-based master and doctoral degrees. A competition is conducted for Graduate Research Fellowships, with additional awards offered for women in engineering and in computer and information science. NSF Graduate Fellowships offer recognition and three years of support for advanced study. See the following for additional details: [http://www.nsf.gov/funding/pgm\\_summ.jsp?pims\\_id=6201&org=NSF](http://www.nsf.gov/funding/pgm_summ.jsp?pims_id=6201&org=NSF)

If I can be of assistance in helping you to navigate the programs and resources within NSF, please feel free to contact me.

**Third Summer Institute in Cultural Neuroscience**



Sanjay Srivastava, University of Oregon

We invite you to apply to attend the third annual Summer Institute in Cultural Neuroscience at the University of Michigan in Ann Arbor. SICN is a two-week program that provides graduate students as well as faculty with an overview of core topics and recent research developments related to cultural neuroscience to prepare them to start their own empirical investigations. Attendees will have an opportunity to develop their own research ideas in cultural neuroscience through interactions with peers and faculty members.

**Summer Institute in Cultural Neuroscience**  
<http://culturalneuroscience.isr.umich.edu/home.htm>  
**July 9-20, 2012**  
**Center for Culture, Mind, and the Brain**  
**University of Michigan**  
**Ann Arbor, Michigan**

**Co-Directors:**  
**Shinobu Kitayama ([kitayama@umich.edu](mailto:kitayama@umich.edu))**  
**Carolyn Yoon ([yoonc@umich.edu](mailto:yoonc@umich.edu))**

**Application Deadline: March 15, 2012**

SICN lectures on culture, brain, or both will be delivered by world-renowned scholars. Each scholar will discuss his or her work and place it in a broader scholarly context. Lectures will be followed by small group discussions.

The topics to be covered include:

- Cultural psychology
- Culture, self, & brain
- Culture, aging, & brain
- Neuroeconomics & culture
- Social neuroscience & culture
- Co-evolution of culture and genes
- Mental health & culture
- Evolution & culture

Faculty lecturers include:

- Nalini Ambady, Stanford University
- Shihui Han, Peking University
- William Gehring, University of Michigan
- Joseph Kable, University of Pennsylvania
- Hazel Markus, Stanford University
- Shinobu Kitayama, University of Michigan
- Ethan Kross, University of Michigan
- Randy Nesse, University of Michigan
- Richard Nisbett, University of Michigan
- Denise Park, University of Texas at Dallas
- Chandra Sripada, University of Michigan
- Stephen Suomi, National Institute of Mental Health

Complete applications are due by midnight on March 15. You will be notified of the status of your application by March 31, and will have until April 30 for early registration, and June 15 for regular registration.

Participation fees are \$1,300 for graduate students or post-docs, and \$2,000 for faculty. Discounted rates for early registration (by March 31) are \$1,100 for graduate students or post-docs, and \$1,800 for faculty.

Participants are responsible for their own travel and accommodation costs. We do not offer any scholarships or financial assistance.

For application forms and information, go to <http://culturalneuroscience.isr.umich.edu/home.htm> or contact:

Natalie Dushane  
 Center for Culture, Mind, and the Brain  
 University of Michigan  
 426 Thompson Street, 5241 ISR

Ann Arbor, MI 48106-1248  
 (734) 764-4112  
 Email: [nadushan@isr.umich.edu](mailto:nadushan@isr.umich.edu)

## Reflections on the Summer Institute in Social Psychology



Katherine Corker, Michigan State University and on faculty at Kenyon College, Fall 2012

We, the 2011 attendees of the Summer Institute in Social Psychology (SISP), would like to thank the contributing organizations (SPSP, EASP, SASP), the funders (NSF), and the host institution (Princeton University) for providing us with one of the most enriching, thought-provoking, and valuable experiences of our graduate careers. The lessons we learned and the experiences we shared will surely continue to enrich our development as scholars for many years to come.

SISP is a bi-annual program, begun in 2003, that allows roughly 80 graduate students to spend two intense, experience-packed summer weeks away from their home institutions. Over the course of the institute, students complete courses on substantive and methodological topics taught by leaders in the field. Perhaps more importantly, however, there is an opportunity to establish life-long relationships and collaborations with their peers, many of whom have the potential to become future leaders in the field.

The 2011 SISP attendees began their experience with a cocktail party at Prospect House -- the storied former home of many Princeton presidents, including U. S. President Woodrow Wilson -- followed by a welcome address from Susan Fiske. Dr. Fiske implored us to remember the fun in social psychology, an admonition we surely heeded during our time at Princeton.

The days that followed were spent in classes, exploring new corners of the literature, and in intense discussions with one another about the nature of the field's most pressing concerns. Content courses were offered in five areas: Accuracy of Judgments of Personality and Social Relations (Judy Hall & Tessa West), Health Psychology (Sally Dickerson & Traci Mann), Self Knowledge and Understanding (David Dunning and Simine Vazire), Social Influence in Groups (Fabrizio Butera & John Levine), and Social Psychological Intervention (Hart Blanton & Debbie Prentice).

One-day methods workshops were offered on Implicit Measurement (Keith Payne), Secondary Data Analysis (Kali Trzesniewski), and Missing Data (John Graham). In the evenings and on the weekend we explored more diverse topics, such as Trivia in Metropolitan Princeton, Historical Pubs and Taverns, Norms and Social Mores at the Jersey Shore, Getting Lost in New York City, and The Impact of Karaoke on Group Cohesion.

In all seriousness, the value of this program for the development of social psychology as a field can hardly be understated. We have been exposed to new ideas, fresh ways of thinking, and made invaluable professional connections. Currently, SPSP members are working to renew the grant that funds the SISP program (an NSF training grant). We, the SISP class of 2011, together with the four successful cohorts that came before us, urge the NSF to continue funding this valuable endeavor. Finally, we again extend a heartfelt thanks to the members of SPSP, EASP, and SASP who have made this experience possible for us.

For the 2011 Attendees of the Summer Institute in Social Psychology, Princeton, New Jersey:

Sarah Ainsworth	Brett Ford	Angela Legg	Oliver Siy
Alexis Alabastro	Brittany Gentile	Andrew Leister	Courtney Soderberg
Jan Marie Alegre	Sarah Gomillion	Lucas Mazur	Deborah Son
Jill Allen	Lindsey Graham	Rachel McDonald	Kerry Spalding
Daniel Ames	Katharine Greenaway	Kris Mescher	Victoria Springer
Kathryn Boucher	Sarah Gunnery	Marina Milyavskaya	Mia Steinberg
Tiffany Brannon	Jennifer Gutsell	Rachel Montana	Michael Tamborski
Max Butterfield	Katie Hansen	Ericka Montanaro	Erin Thomas
Jimmy Calanchini	Carlee Beth Hawkins	Samantha Mowrer	Alexa Tullett
Daryl Cameron	Chelsea Helion	Nicole Muscanell	Frederieke van Dongen
Daniel Catterson	Erin Hennes	Daniel Nadolny	Milica Vasiljevic
Jackie Chen	Yanine Hess	Natalie Nardone	Katie Wang
Joey Cheng	Brent Hughes	Rebecca Neel	Adrian Ward
Susanna Cheung	Lauren Human	Evava Pietri	Abbie Wazlawek
Jeff Cho	Christian Issmer	Jessica Remedios	Joseph Wellman
Paul Conway	Priya Iyer	Francesca Righetti	Annemarie Wennickers
Katherine Corker	Senghor Jacoby	Emily Rosenzweig	Geoffrey Wetherell
Benjamin Crosier	Megan Johnson	Mollie Ruben	Ruixue Zhaoyang
Jessica Cundiff	Priyanka Joshi	Jessica Salerno	
Stuart Daman	Andreana Kenrick	Gillian Sandstrom	
David Doyle	Patrick Kerr	Carson Sandy	
Lisa Droogendyk	David Kille	Kathleen Schmidt	
Benjamin Drury	Mark Kurai	Erica Schneid	

### A Note on Style from the Persnickety BASP Editor



Leonard S. Newman, Syracuse University

Many *Dialogue* readers grade lots and lots of undergraduate papers, and they probably spend more time than they'd care to admit scolding students about using "effect" as a verb when what they really meant was "affect." As a journal editor, I don't have to spend a lot of my time doing that. Not because contributors to my journal are better writers than my students (although that's usually the case), but because increasingly, contributors to my journal simply don't use that verb. Many seem to despise it.

I like the word "affect." Smart people use it. Not so smart people know what it means. We all use it liberally in everyday speech, and your spell-checker will not complain about it if you dare include it in your next manuscript. Unfortunately, I seem to have missed the memo a few years ago that informed us in no uncertain terms that "affect" just doesn't sound..."scientific" enough. It must now, in all cases, be replaced with "impact." Ditto for "affected"—"impacted" is what you want to use to give your findings that extra little oomph. Only that could explain why I now regularly receive manuscripts in which "impact" is used as a verb more than twenty times and

“affect” appears not at all. Peers “impact our alcohol consumption;” threat processes “impacted women’s performance.”

Possibly it’s just me, but “impacted” makes me want to schedule an appointment with my dentist—or head to the pharmacy for some laxatives. I am well aware that use of “impact” as a verb is *not* grammatically incorrect. All I’m trying to say is...yuck. While I have no control over these things, it would be great if we could all agree to at least consider defaulting to “affect.” After all, as Dr. Martin Luther King reminded us, “*Whatever impacts one directly, impacts all indirectly.*” Or something like that<sup>1</sup>. And if we don’t call people on this now, I predict that the trend will accelerate, because “*The consequences of an act impact the probability of its occurring again.*” (Did I remember that one correctly<sup>2</sup>?) And as James Carville once said, “*I’d rather not predict, I’d rather impact*” (I think<sup>3</sup>).

**Notes**

<sup>1</sup> “*Whatever affects one directly, affects all indirectly.*”

<sup>2</sup> “*The consequences of an act affect the probability of its occurring again*” (B.F. Skinner)

<sup>3</sup> “*I’d rather not predict, I’d rather affect*”