

The Official Newsletter of the Society for Personality and Social Psychology

Chris Crandall &
Monica Biernat,
Co-Editors

Savannah in the Rearview Mirror, LA in the Headlights

By Dan Cervone

As a great many of you know from first-hand experience, SPSP held its third annual convention in Savannah, Georgia, on Jan. 31—Feb. 2, 2002. The annual meeting of our group once again proved to be a great success, with memorable moments of science and collegiality extending from Claude Steele's opening Presidential Symposium through the final clash of Mark Baldwin's closing Jam Session.

Of particular note to the Society is our conference's continued growth; 1327 people attended the conference in Savannah, our

largest gathering ever. The meeting has grown substantially in each of the two years since our first conference—which itself drew far more people than anticipated.

A further sign of the vitality of our Society and our field is the conference's demographics. Of the people in attendance in Savannah, more than half came from that group of individuals who provide so much of the energy and the new ideas of any scientific discipline: graduate students and postdocs. SPSP is committed to doing all it can, via registration discounts, travel awards, and diversity fund travel grants, to continue to make the conference attractive to

our young scholars.

This moment of Savannah nostalgia is an apt time to thank Todd Heatherton for his service on the SPSP Convention Committee. Todd now leaves this committee assignment after years of work—since the very beginnings of the planning for our first convention. Through his efforts in establishing the conference, Todd has left an indelible mark on the fields of personality and social psychology.

This is also an apt moment to look ahead. If the SPSP meeting is the highlight of your year (what do you mean "Get a life"?), the good news is that you don't

(Continued on page 2)

New SPSP Fellows Named

The SPSP Fellows Committee meets each year to recommend members for Fellow Status in SPSP and/or Division 8 of APA. This year's committee—Susan Andersen (Chair), Jennifer Crocker and Jeff Greenberg—recommended 14 stellar contributors to the field for this honor, and all were unanimously approved

for Fellow Status in SPSP by the Executive Committee. These new SPSP Fellows are: Roy Baumeister, Bob Cialdini, Rick Gibbons, Tom Gilovich, Peter Gollwitzer, Jeff Greenberg, Bill Ickes, Ziva Kunda, Mario Mikulincer, Jamie Pennebaker, Janet Polivy, Tom Pyszczynski, Fritz

Strack, and Dan Wegner. With the Executive Committee's endorsement, the materials for those individuals who are members of Division 8 of APA have been forwarded to the Membership Committee of APA for its annual consideration of Fellow nominations. Congratulations to these individuals for their (overdue) designation as SPSP Fellows! ■

Inside the Current Issue:

<i>Attempted Murder of SPSP Member</i>	2
<i>GASP Founded, Enjoys Strong Kick-Off</i>	4
<i>Theoretical Prize; Schaller Editorial</i>	6,7
<i>President's Column: Claude Steele</i>	8
<i>ORI Fines Evidence of Fraud, Bans Scientist</i>	11
<i>Are you IAT-OCD? Humor by Peter Glick</i>	12
<i>Graduate Student Committee Report</i>	13
<i>Zimbardo on Why you Should Belong to APA</i>	14
<i>Diversity Committee Report</i>	16
<i>Overcoming Physics Envy</i>	17
<i>Intellectual Imperialism: Editorial by Lee Jussim</i>	18
<i>Publications Committee Report</i>	22
<i>SPSP Listserv Humor by Mark Leary</i>	23
<i>Dean Keith Simonton on Persistent Myths</i>	24
<i>Dumbing it Down: Poor Data Analytic Choices</i>	26
<i>Lou Penner on "IRBs and U"</i>	28

Fire Ruled Arson at Montana Home of SPSP Member Law Prevents Labeling Attempted Murder a Hate Crime

By Teri Garstka

In the early morning hours of Friday, February 8, 2002, a fire engulfed the Missoula home of SPSP Member Carla Grayson. Grayson, a social psychologist at the University of Montana and her partner, Adrienne Neff were asleep in their home with their 22-month old son when an unknown person or persons broke into their home and poured flammable liquid throughout the house, setting it ablaze. Grayson and Neff were awakened by the fire and were able to safely escape their home with their child but lost most of their belongings in the blaze.

Authorities had not identified a suspect at press time, but investigators were treating the fire as arson and as an attempted murder. Under the current Montana state law, authorities are not able to classify this as a hate crime based upon the sexual orientation of the victims involved.

Shortly before the fire, Grayson had

joined other state university system employees in a suit against the system's refusal to extend insurance and other benefits to same-sex partners of employees. The suit, filed on their behalf by the American Civil Liberties Union, was made public on February 4, and alleges that Montana's state

Grayson and Neff received death threats. The fire occurred four days later.

university system violates the state constitution by denying partners of gay and lesbian employees equal access to standard benefits. In the days immediately after the suit was filed, Grayson, Neff, and other plaintiffs received death threats. The fire occurred four days later.

The day after the fire, Grayson spoke to nearly 1,000 supporters who had gathered and urged the community to

channel their anger "into some kind of positive action—to build bridges across the community so that no one experiences hatred and prejudice." Several organizations including local gay and lesbian groups in Missoula have set up funds to support Grayson and her family following the fire; individual letters of support or contributions to offset costs related to the fire as well as security needs for Grayson and her family as well as the other plaintiffs in the case. If you are interested, you may send contributions and/or personal letters to:

The Relief Fund
C/o Pride
P.O. Box 775
Helena MT 59624 ■



*Society for Personality and
Social Psychology
Visit us at www.spsp.org*

From Savannah to Los Angeles: Conferences a Success

(Continued from page 1)

have to wait too long for the next one. Our next meeting is Feb. 6-8, 2003, and it will be held in Los Angeles/Universal City, California. The location not only brings the obvious attractions of Southern California, it also provides a solution to the lodging issues that inherently face a growing conference. The convention will be held in not one, but two *adjoining* hotels: the Sheraton Universal City and the Hilton Los Angeles/Universal City. Updates on the conference, including information for symposium and poster submissions, will be available on our website, www.spsp.org.

This is also a time to welcome new members of the SPSP convention team. Lynne Cooper, of the University of

The location not only brings the obvious attractions of Southern California, it also provides a solution to the lodging issues that face a growing conference.

Missouri, joins Rick Hoyle and me on the SPSP Convention Committee. The

2003 Program Committee, which is charged with reviewing conference submissions and crafting the overall program of scientific presentations, will be chaired by Tim Strauman of Duke University, whose great breadth of scholarship in personality and social psychology makes him the perfect person for the job. (Yes, Tim, I was being serious.)

So let me be the first to say "See you in Los Angeles." ■

Dialogue is always looking for submissions from readers. Please send articles, commentaries, and ideas to crandall@ku.edu or biernat@ku.edu.

News of the Society: Growth, Prosperity, and New Opportunities

The SPSP Executive Committee held its semi-annual meeting on February 3, the day after the close of the successful convention in Savannah. Financially, the Society is doing fine, with about a year's operating expenses in the bank and the ability to maintain publications, awards, and the conference for the near term. The dues increase, enacted this past year, has prevented SPSP from facing some of the budget problems being felt in many of our home colleges and universities. A slight operating deficit this year is due to some unusual expenses, including the transition to a new PSPB editor and the staffing of two editorial offices for this journal. The Society now boasts 3711 members; of these, 1288 are students (1263 graduate students and 25 undergraduates).

The Committee discussed the convention at some length. By all available accounts, the Savannah meeting was a big success (see story on p. 1). Attendance surpassed expectations – some 1327 people attended the meeting – and nine pre-conferences involving over 500 participants were held. Student Travel Awards in the amount of \$300 each were given to 40 individuals (about

25% of those who applied); next year, the plan is to move to a web-based

The Executive Committee is considering methods for expanding the conference and thereby including more presentations

application system and to add a stipulation that graduate students can obtain a convention Travel Award only once during their graduate careers. The program itself was praised, though some raised concern about the high rejection rate for symposia submissions (about 75%). Although a high quality program is desired, the Executive Committee is considering methods for expanding the conference and thereby including more presentations – perhaps by adding a day to the meeting, starting earlier in the day on Thursday, or adding more symposia during each available time slot.

Journals. PSPB continues to garner record numbers of manuscript submissions. In 2001, the journal received 508 manuscripts (up slightly from 488 in 2000), and the acceptance

rate was 17%. Fred Rhodewalt and his new editorial team—Vicki Helgeson, Paula Niedenthal, Steve Rholes, Bill von Hippel, and Steve Wright—began accepting manuscripts in January, and have moved to a centralized office system designed to expedite processing of manuscripts.

Access to PSPB for SPSP members will soon be available online, going back to articles published in January 1999. The hard-copy of the journal

Access to PSPB for SPSP members will soon be available online, going back to articles published in January 1999. The hard-copy of the journal will still be delivered, but subscribers may also gain access via the Ingenta website.

will still be delivered, but subscribers may also gain access via the ingenta

website (www.ingenta.com) after

(Continued on page 31)

Society Considers Adopting Non-Discrimination Statement

The Society is now considering adopting a Non-Discrimination Statement to guide its practices. At the prompting of GASP (the GLBT Alliance in Social and Personality Psychology, see page 4), the Executive Committee did the following at the February meeting:

- 1) Affirm its support of LGBT (gay, lesbian, bisexual, transgender) members of our field, *and*
- 2) Affirm its support for research on

LGBT issues, given the central importance to our field of such LGBT-related topics as close relationships, interpersonal attraction, attitudes, self and identity, stereotyping, prejudice, stigma, gender, disclosure and health.

There was also uniform support on the Executive committee for adopting a formal non-discrimination policy that includes sexual orientation. Because there of the variety of potential legal and business consequences for adopting such a Non-Discrimination

Statement, the Society is in the process of consulting with its attorney to sort out the legal issues. The statement will be on the agenda for the August Executive Meeting of the Society at APA in Chicago. ■

For an APA report on "Examining the Employment Nondiscrimination Act (ENDA): The scientists' perspective," visit: <http://www.apa.org/pi/lgbc/publications/enda.html>

GASP Debuts in 2002; Kickoff Meeting Well-Attended

By Lisa Aspinwall and
Lisa Diamond

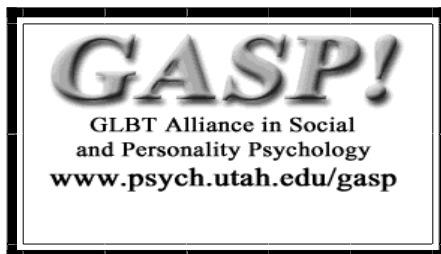
GASP, the Gay, Lesbian, Bisexual, and Transgender Alliance in Social and Personality Psychology, was founded by Lisa Aspinwall and Lisa Diamond of the University of Utah to work with the major professional organizations in personality and social psychology to improve the climate for LGBT students and faculty and to create a shared research and professional community for LGBT researchers and their supportive heterosexual colleagues. GASP is nonprofit and open to all, regardless of sexual orientation or research interest.

Why does GASP exist? GASP has two major aims: fostering a positive and productive climate for LGBT researchers in social and personality psychology, and fostering social psychological research on LGBT issues.

With regard to the issue of climate, it bears noting that many members of our field have long been supportive of LGBT issues and research on an individual level, yet this support is not always reflected on an institutional level. LGBT individuals continue to face considerable prejudice, stigmatization, and isolation, particularly in professional settings. For example, SPSP currently lacks a formal nondiscrimination policy and has only recently (at GASP's request) included sexual orientation in its list of diversity programs. In our view, this state of affairs represents neglect and omission, rather than active discrimination and exclusion, and we are encouraged by SPSP's recent decision to endorse GASP and its goals (see accompanying article).

GASP's brief history provides some indication of the acute need for social and institutional support for LGBT students and faculty. Since being

launched on January 4 with a single listserv posting, the GASP web site has had over 860 hits, and the GASP listserv consists of more than 50 members. Over time, we expect the listserv and the web site to provide a "watering hole" for LGBT colleagues to make contact with each other, exchange professional questions and advice, and identify potential mentors. One of our goals for the web site is to provide a comprehensive list of researchers across the country who are willing to answer questions from



potential graduate students and faculty about the climate regarding LGBT issues in their city and university (If you are willing to be such a contact, please watch for announcements on the SPSP listserv or write to GASP at gaspmail@earthlink.net).

Fostering LGBT research is another important goal of GASP. Just as LGBT individuals have historically faced discrimination and invisibility within mainstream psychology, so too has empirical research on LGBT topics. Much progress has been made in the past decade, and the amount and visibility of high-quality LGBT research is at a record high. Nonetheless, this research is still perceived by some as a highly specialized "special interest" with little relevance for mainstream concepts and research questions in social and personality psychology. In our view, nothing could be further from the truth. LGBT research addresses central questions about social functioning and development, state versus trait models of behavior, gender differences, interpersonal functioning, attraction,

love and sexuality, stereotypes and social judgments, health and coping, and the relative role of genes and environment in shaping human

GASP is pursuing active and visible partnerships with SPSP and other organizations, such as SPSSI and APA's Div. 44

experience over the life course. We want not only to provide professional resources to researchers to explore these issues, but to increase the overall visibility of LGBT research and highlight its critical contributions to the social/personality field as a whole. Notes GASP member Robert Gurney, "Many of the issues and hypotheses studied by social and personality psychologists are either challenged or enlightened by asking how they connect to the LGBT experience. Same-sex versus opposite-sex sexual or romantic attractions, for example, are fundamental to understanding the ways in which similarity is related to attraction. In spite of there being a huge number of studies of similarity-attraction relationships..., less than a handful have addressed the experiences

LGBT individuals continue to face considerable prejudice, stigmatization, and isolation, particularly in professional settings.

of LGBT individuals. We, our research, and our experiences have a lot to offer."

Toward this end, GASP is pursuing active and visible partnerships with SPSP and other organizations, such as SPSSI and APA's Div. 44, to spearhead efforts to facilitate intellectual

(Continued on page 5)

APA Council Report: Prescription Privileges, American Psychologist, and Ageism

By *Monica Biernat*

The APA Council of Representative held its spring meeting in Washington on February 15-17, with SPSP Member Phil Zimbardo, the new APA President, as chair. Though not much on the agenda was relevant to science, a few points of general interest are worth noting:

- The big buzz at the meeting was news that the New Mexico state legislature had approved a bill allowing prescription privileges for psychologists. Since the meeting, the governor of New Mexico, Gary

Johnson (R) signed this legislation into law "authorizing properly trained psychologists to prescribe psychotropic medications to patients." This makes New Mexico the first state in the country to institute such a law (from the APA Practice Directorate Press Release).

- The *American Psychologist* Task Force committee issued its report following review of the "Lilienfeld matter." This committee was chaired by Phil Zimbardo, and highlights of the report appear in the March 2002 issue of *American Psychologist*. The committee made recommendations

regarding the mission statement of the journal, instructions to authors, and editorial policies (including the use of ad hoc reviewers and appropriate communication among between editors and authors).

- The Council overwhelmingly adopted a resolution against ageism. The resolution describes age as "an important element of diversity" and calls upon APA to "reject age based discrimination and to work to stop ageism in society."

SPSP is represented at APA Council by its two elected Division 8 reps, Monica Biernat and June Tangney. Amber Story filled in for Dr. Tangney for the Spring meeting. ■

GASP Debuts at 2002 meeting

(Continued from page 4)

exchange between researchers, teachers, and policy makers about LGBT issues. We plan to expand the web site to contain links to different researchers and laboratories working on LGBT issues, links to bibliographies and syllabi to assist with the integration of up-to-date LGBT research into undergraduate and graduate courses, notices about potential funding sources, and "nuts-and-bolts" information about different methods and measures appropriate for LGBT populations, complete with critical evaluations from researchers working in the field. We also hope to provide a forum where researchers can identify potential collaborators for research projects and/or conference submissions.

The feedback we have received from GASP's inaugural meeting at the SPSP Conference in February suggests widespread interest in all of these aims. The meeting attracted 35 students and faculty, and dozens more wore GASP badges to show their support. Several graduate students commented on the show of support by SPSP attendees who wore GASP badges: "I was so

happy to see those bright pink stickers as I walked around the conference. I was amazed at the number of people wearing them," and "From the inaugural GASP events, plus others I spoke to, it seems like there is a huge interest in the group. The most

Many of the issues studied by social/personality psychologists are either challenged or enlightened by asking how they connect to the LGBT experience.

surprising thing to me was the number of supporters." A dinner afterward attracted nearly 30 people for a Southern buffet, including several heterosexual graduate students and faculty who accompanied their LGBT friends to the meal. In our view, this was a wonderful show of solidarity and a productive beginning.

Our next step involves seeking funding (including tax-deductible contributions) to support web site development,

student research awards, and other activities. We are delighted to see the widespread interest in and support of GASP, and we encourage all of our colleagues to contribute their ideas and feedback about the types of resources GASP might provide. For more information, or to contact GASP, the GASP web site: www.psych.utah.edu/gasp.

You can write to GASP at either gaspmail@earthlink.net or:

GASP
c/o Lisa Aspinwall or Lisa Diamond
Department of Psychology
University of Utah
380 South 1530 East, Room 502
Salt Lake City, UT 84112-0251.
■



*Society for Personality and
Social Psychology
Visit us at www.spsp.org*

Announcing the Theoretical Innovation Prize

The SPSP Theoretical Innovation Prize recognizes an article, book chapter, or unpublished manuscript judged to provide the most innovative theoretical contribution to social/personality psychology within a given year. Any kind of innovative theoretical contribution can be considered for the prize, including presentations of new theories or meta-theoretical perspectives, new theoretically based integrations of disparate areas of inquiry, and significant extensions of existing theories to new areas of inquiry. Theoretical contributions are eligible for the prize regardless of the format of their presentation, whether in stand-alone theoretical papers, within conceptually based literature reviews or meta-analyses, or in some other written format that highlights conceptual innovation.

The prize recognizes theoretical articles that are especially likely to generate the discovery of new hypotheses, new phenomena, or new ways of thinking about the discipline of social/

personality psychology. Theoretical contributions may be judged innovative and generative even before they have accumulated substantial empirical support; therefore an article may be judged worthy for the prize even if it runs the risk of empirical invalidation in the future. The emphasis of the prize is on a contribution's conceptual innovation and potential to motivate new research and further conceptual investigation, rather than on its current level of empirical support.

Eligible articles are those published as papers in peer-reviewed journals or as book chapters during a given calendar year. The prize committee will also consider unpublished manuscripts (of article / chapter length) that are nominated during the given calendar year. Books are not eligible.

The decision on the Theoretical Innovation Prize will be made each year by a committee appointed by the Executive Committee of SPSP. The

Committee also has the option of not awarding a prize. The current committee is Dan Wegner (Chair), Jeff Greenberg, Joanne Wood & Robert Zajonc.

The prize recipient will receive a cash award of \$750. Eligible articles may be nominated by their authors or by other members of SPSP. To nominate an article for consideration, individuals should send 3 copies of the nominated article to the Chair of the Prize Committee (Dan Wegner, Department of Psychology, Harvard University, 33 Kirkland St. WJH 1470, Cambridge, MA 02138), along with a brief (e.g., 1-page) nominating letter describing how the nominated article excels on the prize criteria. The Prize Committee may also consider additional eligible papers even if not nominated by their authors or other individuals. The initial prize will be for papers published or distributed in calendar year 2001, and the deadline for nominations will be May 31, 2002. ■

Letter to the Editor

When reading the informative article by Jerry Suls in the Fall, 2001 issue of *Dialogue* ("Report from the Editor of PSPB: Turning the Corner!"), I was pleasantly surprised to see the statement, "My belief has only become stronger that empirical papers are too long-winded and contain too many citations." which expresses my own long held views. I was also pleasantly surprised to see the statement, "I also concur with Judson Mills," but then had mixed feelings about the rest of the sentence, "that a paper does not need to address every alternative explanation (there should be something left for future researchers to do)." which only partly expresses my own views. That a study doesn't need to resolve all doubts about the question investigated so that no further research is needed is a view that I expressed some years ago in a PSPB article (Mills, 1977). Also in that comment on criteria for publication,

after making the point that there is a flaw in the procedure of a study that would justify its rejection for publication if the authors claim to have found that one variable A influences another variable B and one can give a plausible account of why the results occurred which does not make the assumption that A influences B, I wrote, "If the findings can be explained in terms of a different theoretical viewpoint than that used by the authors, that doesn't constitute a flaw in the procedure. If the authors claim to have found that A influences B and that this happens because theory X (e.g., attribution theory) predicts it, it isn't a flaw in the procedure if a case can be made that theory Y (e.g., dissonance theory) can also predict that A influences B. Disagreement about the general theoretical implications of a study is normal if the findings are novel and the topic important. To use it as basis for not publishing such studies has the effect of stifling theoretical controversy and the development of theoretical ideas." The crucial

distinction between an alternative explanation and a different general theoretical account was also discussed in a recent paper with Hal Sigall in the *Personality and Social Psychology Review* (Sigall and Mills, 1998) in which we said, "A different general theoretical account accepts the existence of a relation between the conceptual independent variable and the conceptual dependent variable and interprets the relation in a different way. An alternative explanation questions the existence of a relation between the conceptual independent variable and the conceptual dependent variable and explains away the result."

-Judson Mills

Mills, J. (1977). Criteria for publication. *Personality and Social Psychology Bulletin*, 3, 523.

Sigall, H. & Mills, J. (1998). Measures of independent variables and mediators are useful in social psychology experiments: But are they necessary? *Review of Personality and Social Psychology*, 2, 218-226. ■

Prizing Theoretical Innovation: An Editorial

by Mark Schaller

Toward the end of his career Richard Feynman (the Nobel Prize-winning physicist) regularly gave speeches in which he implored younger colleagues to be mavericks—to break free from the shackles of intellectual conformity, to work on unfashionable problems, and to try to discover entirely novel theories. Real progress in his science, he knew, depended upon individual scientists' willingness to take theoretical risks, and to encourage riskiness in others. "The chance is high that the truth lies in the fashionable direction," Feynman would say, "But, on the off-chance that it is in another direction . . . who will find it?"

Occasionally our own journals are graced by similarly impassioned appeals for renewed conceptually boldness. But it's easier said than done. Much as we might want to be the mavericks who discover brand-new ways of understanding the human

Any innovative idea will be thought of as wrong by most other scientists simply because it's new . . . it takes greater time, effort, and determination to publish groundbreaking work.

psyche, and much as we want our journals to dazzle us each month with astonishing new ideas, it's just too tempting to play it safe and follow instead the path of conventional wisdom.

And it's no wonder, given that the immediate rewards of working within accepted paradigms typically outweigh

the rewards of doing something entirely new. Truly novel ideas and theories take more careful nurturing. This nurturance may sometimes pay off big in the long term, but it's not easy to overlook the short-term costs of committing resources to risky conceptual ventures.

A part of those costs lies in the sometimes-frustrating process of convincing our peers. Stephen Fretwell (the population biologist) once noted that "any innovative idea will be thought of as wrong by most other scientists" simply because it's new and different. Inevitably, then, it takes greater time, effort, and determination to publish groundbreaking work.

So, no matter how impassioned the pleas for conceptual risk-taking, it takes more than mere words to convince scientists to devote personal resources to conceptual risk-taking. Scientists are people, and people respond to personal incentives. We ask, "What's in it for me?"

Well, now there's a great new answer to that question: SPSP's Theoretical Innovation Prize. Here's a reward for big bold conceptual theorizing; it's tangible, real, and at least reasonably immediate. It ain't gonna make you rich, but it will pad your wallet by a nontrivial amount. And it won't make you famous, but it will bring you—and your ideas—increased notoriety among your immediate peers. You can put it on your CV. You can impress your friends and colleagues.

Because this annual prize provides a real personal incentive to do the sort of big, bold, conceptually novel theorizing that is so necessary to scientific progress, I hope that all of us will be more inclined to take bigger, bolder conceptual risks—and to encourage this attitude in our peers.

I'm not expecting that the allure of this prize will turn all of us into the Richard Feynmans of social/personality psychology, but I do hope each and every one of us will be encouraged to spend a bit more time indulge the Richard Feynman parts of our brains—

No matter how impassioned the pleas, it takes more than mere words to convince scientists to devote personal resources to conceptual risk-taking.

to be more willing to be brash and different, to be more devoted to the nurturance of novel theoretical ideas. And to be more energetic in fighting to get those ideas into the literature where they can do the rest of us some good.

I hope too that this prize serves as a signal throughout the field that we must more actively encourage the publication of risky new ideas and different theoretical perspectives. This prize should serve as an annual reminder to all of us that scientific progress depends upon the publication of papers that don't merely answer questions, but that dare to ask brand-new ones—questions that are sufficiently big and broad that they can't possibly be resolved within the space of a single journal article.

It should also remind us that our empirical literature is most useful when empirical facts are wrapped in conceptual frameworks that are bold enough to stimulate skepticism—conceptual frameworks that push us to consider the empirical facts in ways that we don't necessarily have to, but which might point the way to bigger truths altogether. ■

PRESIDENT'S COLUMN

The Represented World: Its Abandoned Role in Social Psychology

By Claude Steele

At the end of the 19th Century and the beginning of the 20th Century, largely in the hands of social scientists like Comte, Durkheim, Weber and Marx, an implicit negotiation took place over the intellectual territory of the social sciences. What level of analysis would belong to sociology, economics, psychology, and so on? Social psychology at the time was little more than a place-holder discipline at the juncture of sociology and psychology—a recognized, but sparsely populated territory. Now, of course, we are plentifully populated, but sometimes I have wished, in light of the challenges we face as a science, that we could renegotiate our territory. Have we ceded too much ground to sociology and anthropology? By disciplinary birthright, they get to analyze the way in which behavior and psychological functioning are shaped by the context of a person's life—personal and sociocultural. We more psychologically oriented social psychologists, following our birthright perhaps, have come to focus more on the internal processes of social psychological experience.

But between this early negotiation and the present, there was the work of Kurt Lewin who explicitly claimed that social behavior is inherently contextualized, not understandable outside of a "life space" to which it is calibrated. And in the 60's and 70's we enjoyed a heyday of studies showing the nonobvious role of situational factors in causing important phenomena, including people's poor emergency intervention and obedience to authority. But the memory of this work highlights a certain drift in the focus of the field, away from context as an integral part of psychological functioning toward a focus in which internal functioning is described without reference to context, or even to the

person's representation of context.

The more one thinks about it, the more it seems that this representation—the person's representation of her context, that is, the world that she assumes she is contending with as her behavior unfolds—is a concept that is either missing from our analytical tool box, or is lost beneath a pile of other tools. This box is overflowing with conceptualizations of how people represent themselves to themselves (self-concepts, expectancies, etc.) of how people represent themselves in relation to specific aspects of their environments (attitudes, values, and preferences toward people, objects and ideas) of how people operate on information in making judgments, inferences and attributions, of how people represent the goals that motivate them, and so on.

But in understanding psychological functioning and behavior, we haven't much concerned ourselves with how people represent the contexts in which they function. Yet in any setting, we represent and assume all kinds of things: we have theories of other people's minds, motives and capacities; we have ideas about how they will react to things; we have degrees of knowledge about how the setting is organized, socially, functionally, politically; we have knowledge about the roles that people have in the setting and the behavior and judgments potentiated by those roles; we have images of the various groups in the setting; we have knowledge of the meanings that actions and events in the setting will have; we have images of how institutions function in the setting; we have knowledge of various norms; and so on. We have represented in our minds at any given moment, a complete, self-relevant world. As our behavior unfolds, it does so in response to this represented and assumed world. Thus the nature of that world should powerfully shape

behavior and psychological functioning. Of course this world will contain particular representations of the self (e.g., of one's efficacies, preferences, etc.) and of the self in relation to particular aspects of the larger world (e.g., one's attitudes, values and expectancies in relation to particular aspects of one's context, etc.). But the focus here is on one's representation and knowledge of the broader world, the world that is presumed to exist independent of one's existence in it.

But would incorporating this focus, this additional conceptual territory, help illuminate phenomena of interest to us. A few examples come to mind that if not definitive, may be suggestive:

Over the years research in cultural psychology has brought to light an intriguing set of psychological differences between cultures: for example, that compared to Americans, the Japanese are more self-critical, the Chinese and Indians make greater use of context in causal attribution, and Southern Americans hew more strongly to a retributive culture of honor than Northern Americans. Of course these differences, to some degree, reflect socialized differences in habits of thinking and functioning—differences learned at the mother's knee, so to speak, that are deeply engrained and that would be difficult to change even in a new setting. But some of them might reflect less engrained, ongoing adaptations to the differently represented worlds in these cultures. That is, to some degree, the cultural differences documented in this research could reflect more lightly held adaptations to differently represented worlds—adaptations to different representations of how people think and respond, what they care about and expect, how institutions function, etc.—rather than deeply engrained tendencies in the psychological make up of cultural citizens. Should these citizens immigrate to a new culture, many of the behaviors and judgments that characterized them in the old culture, might change rather readily as they gain a new represented world. Some might not. But some might. And this might be especially so when cultural communities

are close enough to have familiarity with each other, as in the case of subcultural groups, or for individuals who have familiarity with multiple cultures. Culture may exert its influence as much through the perceived contingencies of a represented world as through its ability to socialize deeply engrained patterns of thought, behavior and emotion.

Research on stereotype threat is another example. This work shows that having to perform a difficult task under the threat of fulfilling a negative group stereotype can reliably impair one's performance, especially if one cares about doing well. Is this due to the person having internalized the negative group stereotype as a low self-expectancy and then fulfilling it in the face of frustration? Or is it due to the person having to contend with an ongoing contingency of her represented world, that one's frustration on this task could be construed as reflecting the self-truth of a negative group stereotype? In this last view, the performance impairment is not mediated through an internalized state—such as an internalized low expectancy. It is mediated through the person's ongoing contention with a contingency of her represented world, that is, her representation of the context of her behavior and the threat that context holds. Should that representation change so as to eliminate the threatening contingency posed by the stereotype, her performance should improve—as it does in those conditions of stereotype threat experiments that eliminate the represented risk of stereotype confirmation.

David Sherman and I saw another powerful example of how the context, and one's representation of it, can affect one's psychology and behavior in a series of interviews we did with mothers in a homeless shelter. These women had only enough education for minimum-wage jobs and lived in often chaotic and unreliable worlds with no supports, that is, no child care, no health insurance, no housing aid, and no family connection or support. And yet, as we and others have found, they seemed to hold strong middle-class values. For example, they valued independence and self-sufficiency

more than a sample of Stanford women. Their difficulty in sustaining a course of upward mobility seemed less rooted in a deficit of their psyches (say a lack of appropriate values or traits) than in the actual and represented unreliability of the world in which they lived and the lack of resources with which they had to face it. In Lewin's terms, the psyche of these women—their low level of conventional optimism, their short-term pragmatism, their capacity to quickly change mobility strategies—cannot be understood outside of the unreliable life context to which it is an adaptation. The nature of a one's psychology is, in substantial part, afforded and constrained by the nature of one's life context. And to understand one, one must also understand the other.

(To explain social, economic or educational inequality in terms of decontextualized psychological characteristics of the victims, will generally sound harsh because it excludes any comparable description of the circumstances to which that psychological characteristic is an adaptation. It thus implies that the psychological characteristic is the cause of the inequality rather than something that may be sustained by it.)

The work of Bob Cialdini and his colleagues on littering can be a last example. This research often pits an explicit persuasion attempt—"please do not litter because..."—against an implicit norm—say, the absence of any litter in a parking garage that one enters. Almost invariably, the implicit norm—conveying what people actually do in a setting—reduces littering more effectively than trying to change the person's attitude. Dan Kahneman and Dale Miller developed a general theory of how norms are constructed. And subsequent research shows that misperceived norms can lead people to go against their own preferences and values—for example, drinking a lot at college because such behavior is perceived as normative when, in fact, few students want to drink that much. In present terms, norms have their influence by changing the person's representation of the context. Behavior then changes almost automatically, in

reaction to a new perceived social reality. Belief change isn't necessary.

Would measuring this representation be an insurmountable task? Of course measuring all aspects of one's representation of a life space context, or even or a specific context, would be difficult indeed. This may be what ultimately defeated Lewin's efforts in this respect—his grasp of hodological space with its regions, vectors, and boundaries notwithstanding. But measuring parts of it, those parts that are theoretically relevant, should not be difficult. Any number of construct accessibility measures—for example, word completions, lexical decision-making tasks etc.—could be adapted to measure those elements of a person's represented world that are most relevant to a behavior or phenomenon in question.

Rather, the challenge of this idea appears to be more one of theory. It requires that our theory go beyond describing the internal processes that underlie a phenomenon to include clear statements about what exactly, in a person's context or context representation, the process is in contention with. What is the person doing and in relation to what? Of course we do incorporate context into theory. But generally, its role is limited to that of getting an internal processes started. Once a prime or a threat from the environment, for example, has launched a psychological process, the environment and its representation in the subject's mind tend to drop from our conceptual view. Keeping it in view, and understanding that the very nature of the phenomenon we are studying is likely to be shaped by the subjects' full representation of the context with which he is contending, should make our science better, more veridical, more generalizable, and more predictive. We have to remember that this territory was claimed for us. Lewin struggled with how to incorporate context into the understanding of behavior. I, for one, believe that we should sustain that struggle. And in the way of enticement, my suspicion is that doing so could open up new frontiers of theory and phenomena, a period of lower hanging fruit. ■



Additional Ruggiero articles retracted: Office of Research Integrity finds misconduct

By Chris Crandall and
Monica Biernat

After several months of review, on November 26, 2001, the Office of Research Integrity (ORI) has made a "Finding of Scientific Misconduct" in the case of former Harvard and University of Texas social psychologist Karen Ruggiero. The ORI is an office within the Department of Health and Human Services, which oversees the National Institutes of Health (NIH). Dr. Ruggiero was accused of scientific misconduct while conducting research supported by NIH. The ORI report was based substantially on an investigation and report instigated by Harvard University.

ORI found that "Dr. Ruggiero engaged in scientific misconduct by fabricating data in research supported by the National Institutes of Health". The report was printed in the December 12, 2001 Federal Register, and is available online at <http://ori.dhhs.gov/html/misconduct/ruggiero.asp>

Dr. Ruggiero engaged in scientific misconduct by fabricating data in research supported by the National Institutes of Health

Dr. Ruggiero has entered a Voluntary Exclusion Agreement with the US Public Health Service (which oversees the NIH). As a result of the investigation, Dr. Ruggiero has agreed to exclude herself from contracting or subcontracting with any US government agency, or receiving any grants for a period of five years, ending November 26, 2006. During this time,

she may not serve on advisory panels, participate in peer review, or act as a consultant on any Federal grants or contracts.

The ORI found that four articles that reported work performed while Dr. Ruggiero was supported by NIH required retraction. Retractions have already appeared for the first two articles (see *Dialogue*, 16(2), Fall, 2001).

Dr. Ruggiero has agreed to exclude herself from contracting or subcontracting with any US government agency, or receiving any grants for a period of five years, ending November 26, 2006

Two new articles are in the process of being withdrawn. The first new retraction is for the paper "Group status and attributions to discrimination: Are low- or high-status group members more likely to blame their failure on discrimination?" by K.M. Ruggiero and B.N. Major (1998), *Personality and Social Psychology Bulletin*, 24, 821-838. The printed reason was "because serious questions exist concerning the validity of the data which relate solely to my own work and which do not implicate my co-author in any way".

A fourth retraction is also forthcoming. This paper is "Now you see it, now you don't: Explicit versus implicit measures of the personal/group discrimination discrepancy" by K.M. Ruggiero, J.P. Mitchell, N. Krieger, Marx, D.M. & M.L. Lorenzo (2000), *Psychological Science*, 22, 57-67. The printed reason was "because I improperly excluded

some participants who should have been included in the analyses and that this exclusion of data was solely my doing and was not contributed to or known by my co-authors."

These two retractions join those for (1999) "Less pain and more to gain: Why high-status group members blame their failure on discrimination" *Journal of Personality and Social Psychology*, 77, 774-784, and (2000) "Why did I get a 'D'? The effects of social comparison on women's attributions to discrimination" *Personality and Social Psychology Bulletin*, 26, 1271-1283.

Ruggiero is the lead author on all four of the retracted articles. Since 1997, seven research articles appear in PsycINFO with Karen Ruggiero first author; the remaining three have not been retracted at this time.

Because of the structure of the database, most searches—but not all—that select and display the retracted article also display the retraction.

Only two of the retractions appear in PsycINFO as of early March 2002. Because of the structure of the database, most searches—but not all—that select and display the retracted article also display the retraction. APA has been working on fixing the database, but because of technical difficulties and coordination with vendors, it may take a few years before all of the PsycINFO databases actually do this.

Dr. Ruggiero has declined to comment to *Dialogue*. ■

Help Tony's Kids! Signs and Symptoms of IAT-OCD

By Peter Glick

Clinicians have recently become aware of a new anxiety disorder, **IAT-OCD**: an obsessive interest in the Implicit Association Test coupled with compulsive tendencies to create and administer these tests to research participants, colleagues, friends, strangers, and relatives.

This epidemic is growing at an exponential rate, almost exclusively among social psychologists. Because of the alarming spread of this epidemic and the tendency of its sufferers to deny that they have a problem, every social psychologist is advised to complete the following self-diagnostic questionnaire:

Give yourself a point for each affirmative response to the symptoms listed below:

- Have you visited the IAT website?
- More than twice daily?
- Have it bookmarked?
- Have it linked to your homepage?
- Have it AS your homepage?
- Personally account for more than 1% of the over 1,000,000 hits on the site?
- Calculated the future date on which the total number of IAT-website hits will surpass the number of hamburgers sold by MacDonalds?

Which of the following have you experienced?

- Frequent desire to create new IATs?
- Fantasies of being Tony Greenwald or Mahzarin Banaji?
- Fantasies of being Tony Greenwald AND Mahzarin Banaji simultaneously?
- Spontaneous knee-tapping?
- Desire to legally change your name so that your initials would be I.A.T.?
- A need to stop answering these questions to work on an IAT?

Have you tried the IAT...

- As classroom exercise?
- On your relatives?
- On telephone solicitors?
- As a way to spice up parties?

Have you ever...

- Given a talk/submitted a paper involving the IAT?
- Used "IAT" as a verb?
- Attempted to explain the IAT to your mother or father?
- Successfully* explained the IAT to your mother or father?
- Asked a colleague "But have you tried to look at that implicitly?"
- Visited a pornography website because it promised "implicit photographs"?

Add together all affirmative responses. Subtract 10 points if you have been asking yourself "What is an IAT?"

Have you tried the IAT...

As classroom exercise?

On your relatives?

On telephone solicitors?

As a way to spice up

parties?

You may be IAT-OCD!

If you scored above 10, you are in need of the following 12-Step Program for IAT recovery.

Step 1: Recognize that you are an IATaholic

Step 2: Surrender control over your dependent measures

Step 3: Submit to a higher power – we

recommend Gifford Weary

Step 4: Sever all links with the IAT website

Step 5: Abstain from any and all IAT use

Step 6: Never use the term "Gold Standard"

Step 7: Apologize (explicitly!) to everyone to whom you have administered the IAT

Step 2: Surrender control over your dependent measures

Step 8: Remove the 'a' and '5' keys from ALL of your computer keyboards

Step 9: Avoid experiments with priming or reaction time, which may lead to IAT use

Step 10: Recognize that implicit spirituality is an empty concept

Step 11: Stop renewing your Inquisit program

Step 12: Take it one data-point at a time

PREVENTING IAT-RELAPSE:

The IAT is highly addictive and relapse is all too common. Below are some steps you can take toward continued abstinence, your only hope!

AVOID going to conferences where attitude and social cognition research are prominent. (Paradoxically, there is some evidence that repeatedly hearing explanations of how the IAT is conducted in talk after talk after talk may reduce IAT-dependence. However, until this research is replicated with explicit measures, we recommend avoidance!) If you are required by your department to attend job talks where IAT data are presented, look away, hold your ears, and hum loudly at the appropriate points.

(Continued on page 13)

Graduate Student Committee Ends First Year

*By Heidi Eyre
Past-President*

The Graduate Student Committee has had a very productive first year! We organized roommate arrangements for the SPSP conference, conducted a roundtable discussion during the conference, helped with the APA Academic Pre-conference, conducted our own short pre-conference entitled "Women in Academia," set up a graduate student listserv, and elected new officers for 2002-2003.

We also conducted a survey of all of the graduate student members of SPSP in order to see how we could best serve the graduate student populace. We found that students' primary concerns are with getting jobs, receiving funding, and publishing. About half of the 247 respondents had attended an SPSP conference and those that had attended enjoyed the quality of research, the opportunity to network

with faculty members and other students, and the social hours. However, several respondents did not enjoy some of the expenses incurred and the poster session organization. Of those who did not attend, they listed reasons such as expense, lack of funding, and time conflicts as their reasons for not attending.

The new committee is working fervently to address the issues that students brought up in this survey. For example, we are compiling a list of alternative (read *cheaper*) hotels that are near the conference hotel, in addition to helping those who are seeking roommates find one another in order to reduce conference costs. We also want to facilitate more formal discussions on nonacademic jobs, networking, and so forth at the next conference. We are in the process of creating a graduate student webpage to allow students easy access to pertinent information and attempting to increase

use of the graduate student listserv by introducing an anonymous advice column format

Our new 2002-2003 committee members are Camille Johnson, President (johnson.1967@osu.edu), Heidi Eyre, Past-President (hleyre0@uky.edu), and Jo Korchmaros (jojokor@yahoo.com), Megan Kozak (megashoo@yahoo.com), Jennifer Harman (jenjen3@attglobal.net), and Amanda Scott (scott.665@osu.edu), Members-at-Large. Please contact us if you have ideas or would like to help out with any of our projects. We want to get as many graduate students actively involved in the organization as possible!

Finally, a parting joke for you. Question: How many graduate students does it take to screw in a light bulb? Answer: Just one, but it takes him/her 6 years to do it! ■

Help Tony's Kids!: More Signs and Symptoms of IAT-OCD

(Continued from page 12)

DO NOT read the first section of *JSPSP* or *Social Cognition* and check all abstracts carefully before reading the methods section of any article to avoid IAT exposure.

DO NOT be fooled by IAT-abusers who suggest that you take "just one" IAT to see whether you are cured. (We have become aware of some addicts who claim to have an implicit test of IAT addiction. Beware! This only leads to renewed addiction!)

Be sure to attend meetings of IAT-anon support groups. If one is not available in your area, try the on-line support room at:
<http://www.IATabusers.com>.

TESTIMONIALS:

I realized I needed more and more IATs to make me happy, that I couldn't go for a day without making a new IAT.

Laurie R.: It all started with a post-doc. Tony seemed so nice and it was so easy and fun to create my own IATs. It was just a blast at first, but then I realized I needed more and more IATs to make me happy, that I couldn't go for a day without making a new IAT. I

was pushing the IAT on colleagues who were skeptical, telling them to try it "just once," knowing they would get hooked. Now I can't stop myself. I have no explicit control left over my variables. I'm ready to surrender to a higher power than Tony – is there is such a thing?

Stephanie G.: Denial, what denial? I can quit anytime I want. Okay, so after an hour the spontaneous knee-tapping starts. Big deal, I like tapping my knees. It isn't hurting anyone else, is it? Look, 1,000,000 webservers can't be wrong can they? Oh my God, I'm hooked, aren't I? Is there an implicit diagnostic test for this? Please, help me. Okay, I have to get back to the lab. ■

Why should you belong to APA?

*By Phil Zimbardo,
Division 8 Fellow and
APA President*

One of the first things that newly-elected APA Presidents do is get briefed by staff on the structure, function and activities of APA's organization and its members. When I first became President I knew about as much about APA as the typical member—I subscribed to the journals, I read parts of the Monitor, gave talks at conventions, and I knew that there was a large organization "somewhere" doing things in support of Psychology. Unlike most APA presidents, I was a total outsider to APA governance, never having had anything to do with its Council of Representatives, task forces or many committees. I paid my dues, used APA when I needed to, but never worked in its trenches. I won the election based on solely on the credits I had earned as an academic-scientist.

It has been an eye opener for me to discover the range, number and extent of projects, task forces, actions and initiatives meant to further our discipline, advocate for psychological science, and apply psychological knowledge in the service of society. I also had no idea of the large staff infrastructure at APA that serves as our eyes, ears, hands and feet in making sure that psychology gets funded and represented at federal and local levels, in making sure that the very best of science, application and practice come to the attention of policy makers and implementers, and in fostering psychology's collaborations with other scientific disciplines.

I realize I sound like a cult convert, but want to share with my colleagues in social and personality psychology a few of the things that I've learned that APA does for its scientists. I hope it will help dispel the myth that "APA does nothing for scientists or

academics," or "my dues go only to support Practice." The more I have learned, the more I have been motivated to contribute my time, energy and talents to further these efforts (as I will outline at the end of this note). If you want to know the whole gamut of things the Science Directorate does, please check out its web page -- www.apa.org/science.

*APA serves as our eyes,
ears, hands and feet in
making sure that
psychology gets funded
and represented at federal
and local levels.*

Here are a few highlights in just three areas—advocacy, training, and what I will call "burning issues." These activities underscore what APA does "behind the scenes" in service to us all.

Advocacy: You probably all know that APA has a large presence on Capitol Hill through its activism for mental health parity and prescription privileges. But did you know that APA has an equally vocal presence for science matters? APA staffers monitor what is happening on the Hill and in Federal Agencies relevant to researchers (NSF and NIH -- including institutes NIMH, NICHD, NCI, NINDS, NIDA, NIAAA, NIA; and VA, NASA, DOE, and DoD to name a few—a lot of alphabet soup, but rich in funds that we want to tap into). They work separately and in coalitions to advocate for behavioral science funding, and for report language in federal bills in support of behavioral science research. In addition to lobbying efforts, APA staff continually monitor and respond to doings in the federal research arena. Whenever there are requests for comment on proposed regulations or laws or changes to the

research landscape, staff request input from relevant experts and draft a comment or letter from APA. In the last year APA has made comments on a wide variety of proposed legislation/ regulation from education legislation to animal research issues to regulation over medical records privacy and genetics testing, to policies describing standards for the accreditation of research programs. For each of these issues, some Division 8 members have been asked for their input.

APA also advocates in a different way: There is regular APA representation at major meetings of other societies or organizations (e.g., Society for Neuroscience, AAAS, National Academies of Science), where larger science initiatives and issues are discussed. In these venues APA has presented information on ethics, research regulation and IRBs, and has given comments to National Research Council committees on their scope and work plans. I attend a bi-annual conference of the presidents of all 62 scientific societies, where psychology is the only social/ behavioral science represented, and have been able to impress these physicists, biologists and others of the many ways in which psychology is relevant to issues of national defense, terrorism, and more.

Training: APA's most visible student activities occur through its graduate student association, APAGS -- but did you know that the Science Directorate sponsors the "Science Student Council" -- a group of 10 students who engage other science graduate students in convention programming, an extensive web presence, an email network, a grant program and more? The Science Directorate is also involved in direct training activities, including two that social psychologists have found particularly valuable. One is for more

(Continued on page 15)

Why Belong to APA? *(continued from page 14)*

advanced researchers -- the Science Directorate's first "Advanced Training Institute" in 1999 on fMRI attracted a number of social psychologists who wanted to see how they could use new scanning techniques in their own research. The second is directed toward advanced graduate students and young faculty, the Academic Career Workshop. This workshop, which delves into the nitty-gritty of finding, getting and keeping an academic research position, has been offered for two years at the winter SPSP meetings. APA offers many more opportunities for learning—from teaching tips for faculty, to a week course on psychology in general for outstanding science undergraduates, to the Exploring Behavior Week outreach to high school students. I will add that each of these activities is something in which you or your students could participate. I am planning to have APA develop the standard text for high school psychology courses, to collaborate with APS in promoting psychology science at high school science fairs, and to develop new web sites for training high school and College teachers in being more effective in their teaching.

"Burning Issues" Activities: You may know about APA's standard governance groups—the Board of Scientific Affairs (BSA) consists of 9 outstanding scientists (current Chair is Harry Reis, Div 8 Executive Officer), and its three standing Committees, CPTA (Committee on Psychological Tests and Assessments), CARE (Committee on Animal Research & Ethics) and COSA (Committee on Scientific Awards). But you may not know that BSA regularly supports the establishment of working groups or task forces that address timely issues. Two recent ones are a working group on Internet research and a task force on testing on the Internet. Each of these groups, comprised of experts in the topics, has been called together to

survey the issues and make recommendations about what to do next. For example, the research on the Internet group (chaired by Robert Kraut) is looking at technical, ethical, and other implications of using the

I am planning to have APA develop the standard text for high school psychology courses.

internet as a tool for collecting data, as a means of assistance to researchers who are or intend to use this tool. Current groups that are being formed are a working group on the implications of the genetic revolution for psychological research and knowledge, and an ad hoc group to address current issues in research regulation, especially IRB activities. I know the IRB issue is one that is of a lot of concern among social psychologists—you should know that APA is soliciting input from us all—on IRB issues and how they were resolved, and from psychologists who are now or were IRB members or chairs (if you can contribute, please indicate this to science@apa.org).

I could continue this list of things the science directorate and APA do for social psychologists and social psychology—I have not even mentioned the ongoing things for the entire field such as research based awards, student grants, conference awards, and more that demonstrate that APA respects its scientific foundation. But there is a more important point that I would like to address which is the *perception* that APA does nothing. When I mentioned this perception to Science Directorate staff (headed by Dr. Kurt Salzinger), they were pained and wondered if their regular efforts -- substantial communications such as, *Psychological Science Agenda*, the bi-monthly newsletter; listserv notes; and

the Science sections in the monthly *Monitor* -- get read or noticed by social psychologists. Only you can answer that one, but I want to assure you that there are eager ears waiting to hear from you -- mine, Bob Sternberg's (APA's president-elect who will carry on the scientific tradition), and the staff of the Science Directorate. Let us know about your vision for issues psychology and APA need to address. When there is legislation to comment on or when there are emerging trends to be monitored and addressed that you think important tell us so that we can act on your behalf.

Finally, let me mention a few things that I will be focusing on during my presidential tenure, in addition to helping develop a high school text and Psychology Science Fairs. I am working with the heads of APS to find areas in which our organizations can meaningfully collaborate for the benefit of psychological science. I am advancing an initiative to develop a compendium of all research psychologists have done that demonstrates a significant difference in improving some aspect of our lives, individually or collectively. Data are coming in from this survey (to which I would like each of you to contribute, see <http://research.apa.org/survey/compendium/>).

When collated and organized by an task force of our experts this compendium will be invaluable for creating a more positive image of psychology to Congress, the media and to the public. I hope this quick overview has been of some value to you and encourages you to continue your APA membership, join if you are not, and promote APA to your students. One last word, the Chicago Convention (Aug 22-25) will be the best ever, in part because I am working closely with the Board of Convention Affairs to have many new, amazing features, fabulous events, special guests with fun and good times and rock and roll for all. ■

Diversity Committee Gives Awards, Support to Scientists and Graduate Students

By Ann Bettencourt

In 2001, Ed Diener appointed a three-member Diversity Committee to continue and expand SPSP's diversity programs, initiated in 2000 by the Training committee. Through diversity programs, SPSP strives to increase the level of diversity in personality and social psychology, thereby enriching research, teaching, and advising, better preparing the field for a world in which globalization, multiculturalism, and diversity play a central role in human behavior. The Diversity Programs are supported by donations from publishers upon the request of SPSP members and by additional funds allotted by the Executive Committee. Members of SPSP are encouraged to contribute to the program in a number of ways, including by: (1) Making a financial contribution to the Diversity Fund, (2) contacting their publishers to determine willingness to contribute to the fund, (3) becoming SPSP Faculty Mentors via the Social Psychology Network Profile, and (4) providing suggestions to the members of the SPSP Diversity Committee.

Much of the Diversity Committee's activities during the year culminated in the Diversity Awards Program and Reception, held at the 2002 SPSP Meeting in Savannah. The availability of the Diversity Travel Awards was advertised in the fall of 2001 and drew 95 applicants. Six award winners and six honorable mentions were selected from these very deserving applicants. Jamie Loran Franco, Univ. of California, Santa Cruz; Janetta Lun, Univ. of Virginia; Danielle Menzies-Toman, McGill Univ.; Lisa Molix, Univ. of Missouri; Luis M. Rivera, Univ. of Massachusetts; Benjamin Saunders, Univ. of Illinois at Chicago each received a travel award. The honorable mentions were Belinda Campos, Univ. of California, Berkeley; Roxana Gonzalez, Carnegie Mellon; Mary Hulitt, Univ. of Southern

California; Chu Kim-Prieto, Univ. of Illinois-Urbana/Champaign, Maryann Menotti, SUNY- Buffalo; Garcia Viki, Univ. of Kent, Canterbury.

At the Diversity Awards Reception, these recipients were honored by the past and present SPSP Presidents, Ed Diener and Claude Steele, as well as the SPSP Secretary-Treasurer Sharon Brehm, the SPSP Executive Officer, Harry Reis, the members of the Executive Committee, graduate student peers, SPSP Members, and the Diversity Committee members. Claude Steele's inspirational welcoming remarks revealed vigorous dedication to the advancement of diversity in Personality and Social Psychology. The benefits of the Diversity Program were echoed by the responses of the award winners. They reported an increased sense of commitment to research careers in social and personality psychology, generated from receiving recognition and having an opportunity to see the profession in action at the SPSP Meeting. Several of the Diversity awardees expressed hope that the SPSP membership realize the importance of this opportunity as well as gratitude for SPSP's dedication to enhancing diversity in the field of social and personality psychology.

This year, the committee was also charged with expanding efforts to further meet SPSP's diversity goals. As such, the committee has been involved in initiating and supporting programs to improve the climate for lesbian, gay, bisexual, and transgender (LGBT) individuals in social psychology. Toward this end, the Committee worked with Joshua Aronson, chair of the Division 8 Program Committee, to increase the visibility of empirical research on LGBT topics at the APA convention. The result is that SPSP will sponsor an invited symposium at APA, titled "Heterosexism: Characteristics, Causes, and Consequences." Janet Swim is organizing the symposium, and Greg

Herek, a member of the Diversity Committee, will serve as chair and discussant. The Diversity Committee also has been working with the GLBT Alliance in Personality and Social Psychology (GASP) to assist in their organizational efforts. Working with GASP in the future, the committee hopes to continue to make research on LGBT topics more visible in SPSP and to make SPSP a more comfortable place for our members who are lesbian, gay, bisexual, or transgendered.

Deserving of a special note of appreciation are the SPSP members who approached their publishers to encourage donations to support the Diversity Travel Awards Program; including Elliot Aronson, Sharon Brehm, Bob Cialdini, Steve Fein, Sam Gaertner, Saul Kassin, Doug Kenrick, Diane Mackie, David Myers, Steve Neuberg, Felicia Pratto, Peter Salovey, James Sidanius, Eliot Smith, Shelley Taylor, & Phil Zimbardo. Their efforts have led to generous contributions from the following foundations and publishers: the David and Carol Myers Foundation and McGraw-Hill, Worth Publishers, Psychology Press, Prentice Hall, Guilford Publications, and Houghton Mifflin Publishers. We also owe special thanks to Scott Plous and the members of the Graduate Training Committee for their efforts in launching the Diversity Travel Awards Program and for their continuing support toward the success of the larger program.

The Diversity Committee hope to be able to report on additional diversity related program developments. The committee looks forward to contributions and suggestions from the members of SPSP toward our diversity promoting efforts. To contact the members of the SPSP Diversity Committee, email Ann Bettencourt at BettencourtA@missouri.edu; Greg Herek at gpherek@ucdavis.edu; or Lloyd Sloan at lsloan@Howard.edu. ■

Psychology and Astrophysics: Overcoming Physics Envy

By R. Michael Furr

I am not an astrophysicist, but I have seen one on TV. A cable channel recently aired a program called "Supermassive Black Holes," and sometimes even cable TV can get a person thinking.... What in the world (so to speak) do Black Holes have to do with social and personality psychology? A consideration of how astrophysicists conduct their research reveals some interesting parallels with psychology.

1. Unobservable constructs

Some key constructs in astrophysics are not directly observable but are instead *inferred* from the behavior of entities that can be observed. A classic example is gravity. As described in a recent introductory physics book (Seaborn, 1998), Galileo astutely noticed, among other things, that cannonballs tend to fall to earth. He conducted research to describe the relations between muzzle velocity, trajectory, and distance of cannonball flight. Later, Newton drew a parallel between the behavior of cannonballs, the behavior of the moon, and, as legend has it, the behavior of apples. From the behavior of such *observable* entities, Newton posited the existence of an *unobservable* force that he called "gravity" and eventually published a law of universal gravitation. So far, this idea has worked out pretty well.

A bit more recently, and a bit closer to intellectual home, MacCorquodale and Meehl (1948) noted the self-consciousness that psychologists seem to feel when daring to posit the existence of unobservable or hypothetical constructs, and they contrasted this with the apparent comfort felt by physicists. Of course, MacCorquodale and Meehl go on to point out that not all unobservable

constructs are equal. Are gravity, black holes, short term memory, and the superego on equal scientific footing? Perhaps not. Nevertheless, to the degree that research finds, or even *might* find, physiological bases (or correlates) of constructs such as Memory, Intelligence, or Extraversion (e.g., Zuckerman, 1995), we might feel more and more confident in positing and defending the existence of such unobservable constructs.

2. Correlational research

Astrophysicists working with galaxies cannot do too many experimental manipulations, but they seem to get by. One astrophysicist interviewed on the "Supermassive" TV show proudly claimed that "what we do is to search for correlations." The point here is that the underlying correlational methodology and analysis is the same as that used in some of the "softer" areas of psychology. What is odd, though, is that an undergraduate reading the typical textbook in psychological research methods could be forgiven for believing that "correlational" research is a second-class substitute for good experimental research.

Consider a recent investigation of the correlation between the mass of the black hole at the center of a galaxy and the average velocity of stars at the edge of the galaxy (Gebhardt et al., 2000). This correlational study has such crucial implications that some claim "it almost has the status of a new law of nature" (Musser, 2000). OK, so the correlation is .93, which is a bit larger than the effect sizes typically found in Psychology (by the way, it was statistically significant). Still, not bad for what sometimes comes across as a second-class methodology.

3. Error and aggregation in measurement

How does one obtain an accurate image of a star? One strategy that astrophysicists use is to take multiple pictures of the star and aggregate over pictures (e.g., Ghez, Morris, Becklin, Tanner, & Kremenek, 2000). Why? Because each single picture (i.e., item) is affected by error, such as atmospheric disturbances. By aggregating over the images, the random error washes out, leaving a nice clear image of the star itself. There are a variety of other sources of error and a corresponding variety of corrections that Astrophysicists use, but the basic logic of measurement error and aggregation could be straight from the discussion of reliability found in a typical psychometric textbook.

4. Concern over generalizability

This includes at least two issues that psychologists might recognize as random sampling and cohort effects. At least one astrophysicist has admitted the possibility that findings on which much of the science is based may be of limited generalizability. For example, Harwit (1998) states that the knowledge of large-scale dynamics is based on extrapolations made from research on our Solar System, and he suggests that there is "no guarantee that this extrapolation is warranted" (p. 9). Even more intriguing than this issue of "convenience sampling," is a recent study that has been interpreted as showing that the very laws of nature might be changing as the universe ages (Webb et al., 2001). One might ponder the parallels between the study of "our Solar System as it appears in the year 2001" and the study of "American undergraduates in the year 2001." In both cases, might one question the ability to generalize across "subjects" and time?

I hope that this brief survey of similarities will not be interpreted as another case of "physics envy." Clearly, it omits the important and

(Continued on page 21)

Intellectual Imperialism

An Invited Editorial

By Lee Jussim

Agricultural imperialism

A few years ago, while casually skimming through some social science journals, I came across an article on "agricultural imperialism." I almost lost it right there. Talk about taking a reasonable idea (imperialism) to a bizarre, exaggerated extreme. I had visions of vast fields of wheat, armed to the teeth, prepared to wage war on defenseless fields of barley, soy, and rice.

Until I started reading the article. The author's point was that agricultural production was becoming so standardized and excessively focused around a relatively small number of crops (such as corn, rice, soy, and wheat), that many local, unique, and indigenous products were being squeezed out of the marketplace and, functionally, out of production. And the point was not that this was, by itself, intrinsically bad. Instead, over-reliance on a fairly small number of crops would seem to put much of the human race at excessive risk should an act of god (drought, disease, etc.) decimate one or two particular crops. Although the author did not quite put it this way, just as it is important to diversify your stock portfolio, it is important for us, both as individuals and as a species, to diversify our food sources. And the creeping Westernization of agriculture threatened to undermine the diversity of those food sources.

What is intellectual imperialism?

I use the term "intellectual imperialism" to refer to the unjustified and ultimately counterproductive tendency in intellectual/scholarly circles to denigrate, dismiss, and

attempt to quash alternative theories, perspectives, or methodologies. Within American psychology, for example, behaviorism from the 1920s through the 1960s is one of the best examples of intellectual imperialism. Behaviorists often characterized researchers taking other (non-behaviorist) approaches to psychology as "nonscientific" (see, e.g. Skinner, 1990). And, although other forms of psychology did not die out, behaviorism dominated empirical, experimental American psychology for four decades. Although behaviorism undoubtedly provided major contributions to psychology, to the extent that the scientific study of intrapsychic phenomena (attitudes, self, decisions, beliefs, emotions, etc.) was dismissed, ridiculed, or suppressed, behaviorism also *impeded* progress in psychology.

Unjustified rejection of failures to replicate

Intellectual imperialism emerges in all sorts of ways. One common manifestation is reviewers' tendency to reject articles because they do not find (what the reviewer believes) someone else has. Such studies seem to me to have unusual potential to be particularly informative and intriguing. They raise all sorts of possibilities, such as: The original finding or phenomena is not as powerful or widespread as the initial studies seemed to suggest; the new pattern may be as or more common than the original finding; there may be conditions under which one or the other is more likely to hold. But a common knee-jerk sort of reaction is "There must be something wrong with the study if pattern X failed to replicate." Certainly, this is possible. But, it is also possible that there was something wrong (or limited or left unarticulated) in the original study or studies demonstrating pattern X.

Just because researcher Smith published pattern X first, does that necessarily mean that a subsequent study by researcher Jones, who found pattern not X, is fatally flawed? I do not see it—there is no logical or philosophical reason to ascribe higher quality to a study just because it was performed first. Doing so constitutes intellectual imperialism—unjustifiably presuming one study's findings are superior to another's.

The un(or at least rarely)questioned superiority of the experiment

Correlation does not mean causality. A knee jerk reaction we have all been taught since our first statistics class and maybe even our first psychology class. But it is wrong. Correlation does mean causality. If we discover that A is correlated with B, then we now know either that: 1) A causes B; 2) B causes A; 3) C (or some set of C's) cause both A and B; or 4) some combination of 1, 2, and 3 are true. This is not nothing -- indeed, although we do not know the precise direction or set of directions in which causality flows, we know a lot more about causality than we did before we obtained the correlation.

As far as I can tell, it has been overwhelmingly, and perhaps exclusively, *experimentalists* who have touted the absolute superiority of the experiment. Researchers who routinely engage in both experimental and nonexperimental work rarely make this claim.

And the superiority of the experiment has been greatly exaggerated. Whole fields with considerably more scientific status and recognition than social psychology, such as astronomy, paleontology, and evolutionary biology do not rely primarily on experiments for building theory and discovering new knowledge.

(Continued on page 19)

Intellectual Imperialism, Cont.

(Continued from page 18)

Of course, if we compare a *perfect* experiment (i.e., one whose procedures are fully articulated, instituted flawlessly, which leaves open no alternative explanations, and involves no measurement error) to a *realistic* naturalistic study, the experiment is superior. But not if we compare a perfect experiment to a perfect naturalistic study. Our hypothetical perfect naturalistic study is also executed perfectly, is longitudinal (thereby ruling out B, which is measured at Time 2 from causing A, which is measured at Time 1), includes measures of all possible alternative explanations (all possible "C's" in the C causes A and B sense), and all measures are free of error. In such a case, the experiment and naturalistic study are equally perfectly capable of assessing causal relations between A and B.

What about a realistically good experiment and a realistically good naturalistic study (which, of course, is the bottom line issue)? Because this issue is too complex to deal with in this type of short essay, I will make only a few brief points here. Although there may be some net advantage of experiments over naturalistic studies, that advantage is small and quantitative, rather than an absolute quantum leap. Both rule out B causing A (at least if the naturalistic study is longitudinal). This means leaves one major ground for comparison regarding quality of causal inferences: their ability to rule out C's. Experiments do not necessarily rule out all C's. They only rule out all C's that are uncorrelated with the manipulation. An obvious case is demand characteristics (though the possibility of C's correlated with the manipulation is infinite, just as in naturalistic studies). Some studies may produce

differences between conditions, not because the manipulation worked, but because participants figure out what responses the experimenter wanted them to provide.

Naturalistic studies nonetheless do have a harder time ruling out those pesky C's. But, if there is any prior empirical work in the area, any theory, or even any related theories, the researcher may often have a good idea of just who are the most likely contenders for C's. They can then be measured and controlled. Not necessarily as good as an experiment, but not a sloppy second, either, at least not if those C's are reasonably well measured. Indeed, because researchers using naturalistic designs may be more sensitive to C's than many experimentalists, they may often make more of an effort to include, measure, and control those C's in their designs. If so, at least some naturalistic studies may do a *better* job of ruling out C's than some experiments.

Furthermore, even if the causal inferences derivable from a typical naturalistic study are not quite as convincing as those derived from a typical experiment, the naturalistic study will often provide more information about naturally-occurring relationships than will an experiment. To the extent that we are trying to understand basic processes, therefore, I would give the edge to the experiment. But to the extent that we are trying to understand the role of those processes in everyday life, I would give the edge to the naturalistic study. Whether there is any greater net increase in scientific knowledge, even of causal relationships, resulting from experiments than from naturalistic studies is, therefore, primarily a matter of opinion, perspective, and context.

Of course, as a field, we do not really need to choose. Both experiments and naturalistic studies are extremely important, precisely because they

complement each other so well. Put this way, it probably seems obvious. If so, then you are already agreeing with me that any tendency toward methodological imperialism (dismissing, derogating, giving less credence to naturalistic studies over experiments) is not a healthy thing for our field.

The curious case of (in)accuracy.

For years social psychologists, especially those with a social cognition orientation, have waxed enthusiastic over error and bias research, rejecting almost out of hand accuracy research. Consider this:

"Despite the obvious importance to social psychology of knowledge about person perception processes, the development of such knowledge was delayed by a preoccupation with the accuracy of judgments about personality ... The naivete of this early assessment research was ultimately exposed by Cronbach's elegant critique in 1955. Cronbach showed that accuracy criteria are elusive and that the determinants of rating responses are psychometrically complex" (Jones, 1985).

"The accuracy issue has all but faded from view in recent years ... On the other hand, in recent years, there has been a renewed interest in how, why, and in what circumstances people are inaccurate." (Schneider, Hastorf, & Ellsworth, 1979).

This is not just a 20 year old phenomenon. Despite spending pages and pages on inaccuracy, error, and bias, both the recent round of handbook chapters and most undergraduate texts, hardly discuss accuracy at all. The reasons for social psychology's rejection of accuracy research are too long and involved for this essay; two short points, however, highlight the intellectual imperiousness of attempts to denigrate or dismiss accuracy

(Continued on page 20)

Intellectual Imperialism, Cont.

(Continued from page 19)

research. First, how can we possibly reach conclusions about inaccuracy unless we can also reach conclusions about accuracy? This question is mostly rhetorical, because on its face, the question seems ludicrous. It's not completely ludicrous, primarily because research on errors can provide insights into *processes*, but whether those processes typically lead to accurate or inaccurate perceptions and judgments is a separate question that rarely can be addressed by process research. Furthermore, some *biases* (which are not the same thing as errors or inaccuracy) actually enhance accuracy (Jussim, 1991). All this is very rich and interesting, at least to some of us. The entire analysis, however, could not occur at all unless at least some researchers studied accuracy. This suggests that attempts to dismiss accuracy do us all a disservice by attempting to clamp theoretical and empirical blinders on the field.

Second, there is the supposed "criterion problem" in accuracy research (highlighted in the Jones quote). This criticism is so common that it has been known to evoke paroxysms of sweat, angst and even self-flagellation from people engaged in actual accuracy research. Aren't the criteria for evaluating the validity of social beliefs so vague and fuzzy as to render attempts to assess accuracy meaningless?

I have never seen criticisms of the criteria used to establish self-fulfilling prophecies that remotely resemble those leveled at accuracy research. I find this peculiarly ironic because, of course, although the processes by which a perceiver's belief become true are different, the *criteria* for establishing their trueness are (or should be) identical.

Social psychology cannot have it both ways. It cannot be tortuously difficult to identify criteria for establishing accuracy unless it is equally tortuously difficult to identify criteria for establishing self-fulfilling prophecy. Conversely, it cannot possibly be unproblematic to identify criteria for establishing self-fulfilling prophecy unless it is equally unproblematic to identify criteria for establishing accuracy.

Some Scientific Claims Really are Just Plain Wrong

Do not get me wrong. Sometimes mountains of data really do say "X is true and Y isn't." The end (at least until someone comes up with new data saying Y could be true sometimes after all). When there is sufficient research to document the falsity of Y, so be it, and we should all feel free to say that Y just ain't true. But the criteria should be the data -- not our own preferences for one view over another. And, the entire point of this essay is that *premature* denigration or dismissal of an area of research *restricts* our data, thereby *reducing* the quality of the science produced by our field. It is one thing if we have tons of data that Y isn't true. But it is another thing entirely if there is just no evidence that Y is true, because research on Y has been

prematurely stigmatized or trivialized. In such a case, the value and credibility of our field, and our ability to both understand human nature and to improve the social condition, have been sorely limited.

References

Krueger, J. & Funder, D.C. (2001). *Towards a positive social psychology: Causes, consequences, and cures for the problem-seeking approach to social behavior and cognition.* Unpublished manuscript.

Jones, E. E. (1985). Major developments in social psychology during the past five decades. In G. Lindzey & E. Aronson (Eds), *The handbook of social psychology* (3rd Ed), Vol 1., pp. 47-107). New York: Random House.

Jussim, L. (1991). Social perception and social reality: A reflection-construction model. *Psychological Review*, 98, 54-73.

Schneider, D. J., Hastorf, A. H., & Ellsworth, P. C. (1979). *Person perception* (2nd Ed.). Reading, Massachusetts: Addison-Wesley.

Skinner, B. F. (1990). Can psychology be a science of mind? *American Psychologist*, 45, 1206-1210.

■

Did you know? Social/Personality Psychology's Contribution to APA

APA generates nearly 58% of its annual operating income from journals and other publications—producing almost \$50 million in revenues in 2001.

Among the many APA journals, the *Journal of Personality and Social Psychology* produced the largest profit in 2001, more than \$1.4 million. The closest competitor, the *Journal of*

Consulting and Clinical Psychology, generated only \$0.8 million.

A similar profit is projected for 2002. This figure includes only the paper version, and does not include additional licensing income from online access. JPSP has more than 2,400 institutional (library) subscribers. ■

Psychology and Astrophysics: Overcoming Physics Envy, Cont.

(Continued from page 17)

fundamental differences in the nature of research and the overall progress of the two sciences. Nevertheless, it is often useful to step back for a different perspective on what we do and how we do it. For myself, the more I learn about what other sciences do and the challenges that they face, the more I feel that we do quite well for ourselves.

References

- Gebhardt, K., Bender, R., Bower, G., Dressler, A., Faber, S. M., Filippenko, A. V., Green, R., Grillmair, C., Ho, L. C., Kormendy, J., Lauer, T. R., Magorrian, J., Pinkney, J., Richstone, D., & Tremaine, S. (2000). A relationship between nuclear black hole mass and galaxy velocity dispersion. *The Astrophysical Journal*, 539, L13-L16.
- Ghez, A. M., Morris, M., Becklin, E. E., Tanner, A., & Kremenek, T. (2000). The accelerations of stars orbiting the Milky Way's central black hole. *Nature*, 407, 349-351.
- Harwit, M. (1998). *Astrophysical concepts (3rd Edition)*. New York: Springer.
- Maccorquodale, K., & Meehl, P. E. (1948). On a distinction between hypothetical constructs and intervening variables. *Psychological Review*, 55, 95-107.
- Seaborn, J. B. (1998) *Understanding the universe: An introduction to physics and astrophysics*. New York: Springer.
- Musser, G. (2000, October). The hole shebang: Black holes and galaxies may be entwined from birth. *Scientific American*, 283(4), 18-20.
- Zuckerman, M. (1995). Good and bad humors: Biochemical bases of personality and its disorders. *Psychological Science*, 6, 325-332.

Department of Psychology
Appalachian State University
Boone, NC 28607
furrrm@appstate ■

SPSP Publications: Strong and On Course

By David Dunning

The beginning of the year 2002 was a time of routine yet important transitions. Fred Rhodewalt took over the editorship of *Personality and Social Psychology Bulletin*. His slate of Associate Editors—Vicki Helgeson, Paula Niedenthal, William Rholes, William von Hippel, and Stephen Wright—is now in place, as is a full complement of consulting editors.

The tenure of outgoing editor Jerry Suls has left *PSPB* in a healthy and vibrant state. The journal currently receives over 500 manuscript submissions per year, with an acceptance rate of 17%. One traditional sore spot with the journal is showing signs of being alleviated. In recent times, the publication lag of

The tenure of outgoing editor Jerry Suls has left PSPB in a healthy and vibrant state. The journal currently receives over 500 manuscript submissions per year, with an acceptance rate of 17%.

papers had risen as high as 13 months. I am pleased to report that as of June 2002, that lag will have declined by roughly 40% to under 8 months. Some of this has come from the temporary addition of pages to the journal, but it is hoped that continuing measures, such as a 10,000 word limit on submissions, will ensure that the lag remains relatively short—and that *PSPB* retains its place as a central and visible resource to social psychologists and other scholars.

One additional small transition promises to increase the visibility of *PSPB*. Sage Publications, which

publishes the journal for SPSP, is moving to make its journals available for on-line access. *PSPB* is no different, and so Sage approached the Publication Committee with a proposal to provide members with on-line access to the journal, at no additional cost, via the ingenta.com web site. Such access would be available to dues-paying members, would be in addition to the paper copy of the journal arriving in the mailbox every month, and would provide access to current issues and as well as volumes back to the 1999. The SPSP Executive committee approved the proposal at its winter meeting, and the central office is currently working with Sage on the details to implement the service. One note: It is likely that the only way to receive on-line access in a timely fashion every year is to pay one's dues on time at the beginning of the year. After that, it will be possible to inform Sage of members eligible for on-line access only on an occasional basis, and thus late dues would mean in delays in on-line access.

Personality and Social Psychology Review continues its gratifying rate of growth. The number of manuscripts submitted grew to just under 70 in 2001 (a 31% increase from 2000), with an acceptance rate of roughly 20%. Although this is a healthy rate of submissions, the sense is that the journal can absorb a higher number—and so do keep in mind this journal as a destination for your own work. As well, being a new journal, *PSPR* is far from achieving full-penetration in institutional library subscriptions. SPSP members can help by exploring whether their own university library currently subscribes to the journal, and then requesting that the library subscribe. The benefit for doing so extends not only to one's self and one's students, but also helps SPSP (and its members) financially.

In discussions about *PSPR* during its winter meeting, the Publication

Committee began to explore whether editorial terms at the journal should be lengthened. The Committee recommended to the SPSP Executive

Personality and Social Psychology Review continues to grow . . . the number of manuscripts submitted grew by a 31% increase from 2000, with an acceptance rate of roughly 20%.

Committee that the terms of *PSPR* editors be set to six years, rather than the current four, and that present editor Eliot Smith's term at the journal be extended for two additional years. The Executive Committee approved the proposal, and Eliot agreed to serve for the additional time. Thus, I am pleased to report that Eliot Smith has been reappointed to serve as editor for two more years, now receiving manuscripts until around the end of calendar year 2005.

The SPSP newsletter, *Dialogue*, continues to thrive. Working with Monica Biernat and Chris Crandall, the Committee drew up a document that has been a surprising omission over the years—a mission statement describing the goals, content, and administration of *Dialogue*. The statement itself can be found on p. 32 of this issue of *Dialogue*.

The final transition of note occurred in the Publication Committee itself. Brenda Major completed her term on the committee, taking the committee through some busy and energetic times, including the reorganization of *PSPB*. Joanne Wood has agreed to serve as Brenda's replacement. ■

What's On the SPSP Listserv

By Mark Leary

The SPSP listserv provides an important conduit for communication among members of the Society of Personality and Social Psychology. Those who subscribe to the listserv receive news of the Society, job announcements, updates on professional meetings, and other information of interest to social and personality psychologists. In addition, many members use the listserv to seek information and advice regarding unpublished or obscure research, specialized measures and procedures, and other professional difficulties. Other members use it to offer ideas that they think will help others in their teaching or research. The listserv is undoubtedly very beneficial to individual members and the organization at large. However, some of the requests and announcements have struck me as unusual, naive, or downright odd.

- My students are having trouble finding measures of self-esteem, locus of control, depression, self-consciousness, extraversion, need for approval, authoritarianism, neuroticism, emotion, gender, and weight. Can anyone help them?
- I am conducting a meta-analysis of the effects of solar eclipses on the tendency to engage in the fundamental attribution error. I have searched PsycINFO but am having trouble finding relevant articles. Could anyone who has unpublished research on the link between solar eclipses and attributional biases please contact me?
- SPSP members may like to know that this week's issues of *Newsweek* and *Time* contain articles that provide examples of psychological phenomena for class discussions.
- Does anyone know where I can find the June, 1998 issue of the *Journal of Personality and Social Psychology*?

- My department is thinking of having all senior undergraduate majors write a dissertation in order to graduate. I would like to hear about the experiences of other departments that have implemented such a requirement.
- A colleague in the Department of Animal and Sport Science at my university is looking for a good measure of behavior. Can anyone out there help him?
- SPSP members may want to know that I will be appearing in a 25-second segment of 20/20 this week on ABC, discussing why people don't like to be in a bad mood. You may wish to videotape it for use in your classes.
- I have been unable to find any research on the so-called Big Five personality traits. Can anyone help?
- I am doing research on the ipsilateral attitudinal incongruency function (IAIF) and specifically how it relates to hyperbolic dysenteric identity structure. I'm wrestling with whether IAIF is mediated by tensitivity of the mnemonic interpolation or whether bifurcation occurs because the individual fails to distinguish between intuitive and multiplasmic stimuli. Anyone who knows of previous findings relevant to this question should contact me.
- I would like to see a discussion on this listserv of the fact that journal editors are idiots.
- The Department of Psychology at Eastern Mesopotamia University invites applications for a tenure-track position for a social-personality psychologist at the Assistant Professor level. Applicants from all areas will be considered, although we are particularly interested in candidates specializing in the social psychophysiology of postmodern thought. Applicants should have a

minimum of 10 years teaching experience, 40 publications in peer-reviewed journals, a strong record of external funding, and demonstrated excellence in teaching. Submit a CV; personal statement and autobiography of no more than 80 pages; 25 reprints or preprints; teaching evaluations; and 8 letters of reference to the Social-Personality Search Committee, Eastern Mesopotamia University

- My students are studying cross-cultural differences in the fear of Bigfoot and Yeti among residents of the American northwest and the Himalayas. I would appreciate hearing from other researchers regarding the pros and cons of various measures of Bigfoot Anxiety they have used, and whether any measures have been translated into Tibetan. Of course, I will summarize and post the responses I receive on the listserv
- Members of SPSP may want to know that my new book, *The Social Psychology of Digestion*, has just been published and may be ordered on my web site for only \$45.99. It may become the most important book of our generation, and no personality or social psychologist should be without it
- I'm writing for a friend who plagiarized an article from a 1937 *Journal of Abnormal and Social Psychology*, publishing it last year under his own name in a top journal. My friend feels very badly about this and, in fact, couldn't sleep at all last night with worrying. What is the chance that he will get caught? Should he fess up? Is his tenure at my university in jeopardy? What is the likelihood he will face legal prosecution? Send any thoughts or advice you may have about my friend's dilemma directly to me.
- A student of mine is interested in the work of some guy named Baumeister. Has anyone heard of him or know where he can be reached?
- Do any subscribers of this listserv know how to join SPSP? ■

Persistent Myths, Probabilities, and Psychologists as Human Beings

By Dean Keith Simonton

Many personality and social psychologists investigate problems that have immediate applications to the real world beyond the confines of ivory-towered academe. Even so, certain false ideas often continue to circulate in popular culture and the mass media in blind ignorance of well-established findings in psychological research. An example is the “hot hand” phenomenon in basketball and certain other sports (see Alan Reifman’s webpage at <http://www.hs.ttu.edu/hdfs3390/hothand.htm>). Despite sound evidence that this phenomenon represents nothing more than the intrusion of chance in everyday events, the idea persists. Evidently, people have a difficult time realizing that what might appear as something significant is actually nothing more than coincidence. Indeed, research shows that people cannot successfully discriminate random and patterned events.

This all-too-human incapacity to perceive that random events can evince the resemblance of pattern probably provides a psychological impetus behind the widespread belief in “paranormal” phenomena, such as precognition. Any person with an ounce of worry will have any day replete with “bad thoughts”—most of which pass into oblivion because they are never endorsed by a concrete confirmation. But the instant the momentary anxiety is seemingly confirmed by a chance event that, however crudely, corresponds to the recollected image, then, miraculously, a mundane premonition becomes supernatural precognition. So it always seems that the chronic worriers among our friends and acquaintances are those most prone to view themselves as more prescient than those of us who “never saw it coming”—including earthquakes, tornadoes, and terrorist attacks!

This psychological perversion of random events is strengthened all the more by the person’s preconceived notions about the very existence of paranormal phe-

nomena. If you believe in precognition, and if you have a poor appreciation of the incessant contingencies of chance, then the experiences of living are going to provide you with abundant confirmation of your beliefs. This confirmation bias also undermines any attempt to dissuade you of your opinions, e.g., scientific research that contradicts your views. How can a scientist ever disprove what you know from personal experience?

Yet let us not get smug about this commonplace discrepancy between perception and reality. We personality and social psychologists are, for better or worse, human beings, just like the research participants we study. Therefore, we possess some of the same foibles and failings. I remember a neat little paper by one of my teachers, Zick Rubin, in which he traced how the procedures of a published study might be reconstructed in later accounts. The specific study was a well-known 1965 experiment by Elaine Walster. Rubin documented how the order of events were reversed (explicitly or implicitly) in subsequent treatments. He wrote “the sequence-reversal may be attributable in part to the assimilation of the actual procedure to psychologists’ preconceived explanatory frameworks” (1974, p. 81). Rubin thought that such reconstructions were by no means rare, in social psychology or in other subdisciplines. On the contrary, he believed that “they may illustrate theoretically interesting processes” that “are also likely to bias scientific literature” (p. 81).

Many readers of *Dialogue* have stories to tell that would provide additional documentation for Rubin’s speculation. How often have any one of us read some journal or textbook account of our precious research that got a key point fundamentally wrong! I beg the opportunity to provide my own example. I give this illustration because it also exemplifies another fundamental fact: All that training in statistics notwithstanding, psychologists and other behavioral scientists still have a problem grappling with the

phenomenon of chance. Like other human beings, we tend to see deep significance in events that are at bottom random.

This mistaken attribution is apparent in the phenomenon known as multiple discovery and invention. This striking event occurs when two or more scientists independently (and often simultaneously) come up with the same creative idea. Examples include the calculus by Newton and Leibnitz, the theory of evolution by natural selection by Darwin and Wallace, the laws of genetic inheritance discovered by Mendel, De Vries, Correns, and Tschermak, the phenomenon of classical conditioning by Pavlov and Twitmyer, and the theory of emotion by James and Lange. Sociologists and anthropologists have taken the existence of these events as proof of sociocultural determinism. Individual scientists are seen as mere epiphenomena, their contributions coming from the zeitgeist or “spirit of the times.” An idea is simply “in the air” for anyone to pick, and sometimes two or more individuals happen to reach up to pick the fruit.

Starting back in 1978, I began a series of studies that demonstrated that the evidence for this deterministic view was no better than that on behalf of the so-called “hot hand.” No one had bothered to test the “null hypothesis” that multiples could be attributed to happenstance, to the occasional coincidence of random events. These studies scrutinized the diverse aspects of the phenomenon, such as the number of independent claimants to a given discovery, the number of multiples in which each claimant participated, and the time that elapsed between the first and last duplication. No matter what the feature, the same pattern appeared. Multiples had precisely the characteristics that one would predict from a complex stochastic process; they appear no more often than one would expect by chance. Over the past dozen years I have published additional theoretical and empirical studies showing that this stochastic phenomenon can be derived from more comprehensive models of the creative process. For instance, multiples can be predicted using the same underlying

(Continued on page 25)

(Continued from page 24)

model that explains why good ideas are directly proportional to bad ideas both across and within scientific careers.

Despite all of the effort devoted to documenting the case, I continue to see psychologists refer to multiples as proof that a scientist's ideas are the product of the zeitgeist. To be sure, many of these references come from historians of psychology, who are still under the influence of E. G. Boring, a strong advocate of the zeitgeist position. Yet psychologists in other subdisciplines, including personality and social psychologists, have echoed the traditional interpretation as well. What I find particularly amazing is when I see my work cited as the first quantitative demonstration of sociocultural determinism! Surely these episodes would make Zick Rubin smile.

Significantly, this work has appeared in prestigious journals, such as the *Journal of Personality and Social Psychology* and *Psychological Review* (e.g., Simonton, 1979, 1997). I have also featured the theory in books published by major university presses,

namely Harvard, Yale, Cambridge, and Oxford (e.g., Simonton, 1988, 1999). Better yet, these publications have earned major awards, featured reviews, and peer commentary. Thus, one would not think that the stochastic model would be so easily overlooked. But such is so.

Admittedly, it could be argued, somewhat ironically, that my interpretation of the multiples phenomenon simply goes against the contemporary zeitgeist. The theory is out of step with the times, and thus is condemned to obscurity. But that would not explain why I managed to publish the model in the first place, or why it has received a certain degree of professional recognition. I suspect the dissemination failure has more to do with the fact that psychologists are, in the final analysis, human beings, and thus have the same limitations in information processing that the rest of *Homo sapiens* must endure. Everyone wants to recognize shapes in cumulus clouds and spot prophecies in tea leaves.

Hence, I eagerly await the day when

sports enthusiast spurn the very phrase "hot hand," and when prophetic premonitions are viewed as selectively sampled coincidences. In that day, too, the lotteries, casinos, and gambling houses will go broke, and astrologers will be out of business. By that time, as well, psychologists will no longer confuse statistical with substantive significance and will cease speaking of nonsignificant "trends" in their data. Then and only then will multiples to be appreciated for what they really are. At least if I'm lucky.

References

- Rubin, Z. (1974). A new order of things: A case study in the reconstruction of an experimental procedure. *Representative Research in Social Psychology*, 5, 81-87.
- Simonton, D. K. (1979). Multiple discovery and invention: Zeitgeist, genius, or chance? *Journal of Personality and Social Psychology*, 37, 1603-1616.
- Simonton, D. K. (1988). *Scientific genius: A psychology of science*. Cambridge: Cambridge University Press.
- Simonton, D. K. (1997). Creative productivity: A predictive and explanatory model of career trajectories and landmarks. *Psychological Review*, 104, 66-89.
- Simonton, D. K. (1999). *Origins of genius: Darwinian perspectives on creativity*. New York: Oxford University Press.

Dumbing it down – The dangers of appealing to the lowest common denominator

By Kristopher J. Preacher
and Derek D. Rucker

"It's not our job to appeal to the lowest common denominator – it's our job to raise it." - President Josiah Bartlet, *The West Wing*

In 1999, the APA Taskforce on Statistical Inference (Wilkinson et al., 1999) released their long-awaited recommendations regarding data analysis and methodology in the behavioral sciences. One of their recommendations was to choose the "minimally sufficient analysis," meaning that when presented with two alternative data analytic procedures, both of which could address the question of interest, it is in the interest of parsimony to choose the "simpler" one (although they leave the definition of "simpler" open to interpretation). For example, if the hypothesis is that two groups differ in mean level, it probably makes sense to conduct a *t*-test rather than a path analysis, although either procedure could be used to test the same hypothesis. We agree with Wilkinson et al.'s recommendation in spirit. However, we submit that an overlooked, but more frequently occurring, problem than using overly complex analyses is using overly simple ones.

In this article, we concern ourselves with *dumbing down* of statistical analyses. Dumbing down refers to bypassing an advanced technique that is appropriate in favor of a simpler technique that is not. Dumbing down has several negative effects on the scientific enterprise. First, favoring the use of simple analytic procedures over advanced techniques can in many situations yield reduced power, result in less precise estimates, and reduce descriptive clarity. For example, much

research on implicit and explicit attitudes using simple correlations has found weak (or no) relationships between these two constructs. With the use of more appropriate latent variable techniques, it is seen that these two constructs are moderately correlated (Cunningham, Preacher, & Banaji, 2000); this finding suggests that the "simpler" technique underestimates the true association between these constructs. Our point is that state-of-the-art methods are called state-of-the-art for a reason; they frequently result in tests with higher power, greater precision, and increased clarity.

Not only can the benefits of advanced techniques be lost by using simpler methods, but errors can actually be introduced. One classic example is the *median split* technique, wherein an investigator splits a sample into two groups (*high* and *low*) by using the median score on a given continuous scale as the point of division. By force of tradition, ignorance of the costs involved, and the illusion of simplicity, quantitative variables are often dichotomized in this way so that ANOVA may be used in place of linear regression. This is oversimplification with potentially disastrous consequences (MacCallum, Zhang, Preacher, & Rucker, 2002). For years, psychometricians have been fruitlessly warning psychologists that the consequences of dichotomization are loss of power, loss of reliability, the potential for spuriously significant results, and the false impression that latent groups exist. Despite the availability of more appropriate regression techniques (e.g., see Aiken & West, 1991), dichotomization continues to be used. At least 16% of the articles in the *Journal of Personality and Social Psychology* from 1998 to 2000 contain at least one

median split, and frequently more than one (MacCallum et al., 2002).

Other examples of dumbing down include the use of "canned" mechanical procedures often included in statistical software packages, sometimes as default options. These include procedures such as stepwise regression and principal components analysis, both perfectly reasonable procedures but with questionable applicability to most research situations in psychology. The infamous combination of principal components analysis, orthogonal varimax rotation, and retention of as many components as there are eigenvalues greater than 1.0 (a procedure called "Little Jiffy" by its proponents because it is supposedly quick and easy) continues to be used despite considerable literature demonstrating its obsolescence (Fabrigar, Wegener, MacCallum, & Strahan, 1999; Floyd & Widaman, 1995; Ford, MacCallum, & Tait, 1986; Lee & Comrey, 1979; Preacher & MacCallum, 2000; Widaman, 1993).

Given the costs associated with dumbing down, why does it continue to occur? We see at least three primary reasons why researchers might choose to oversimplify statistical procedures. First, researchers may use simple techniques because the results conform better to their hypotheses and arguments. Second, a researcher may oversimplify out of a lack of knowledge; researchers may be unaware of, skeptical of, or untrained in advanced techniques more appropriate for their needs. Finally, researchers may oversimplify analysis because they believe reviewers and/or consumers will not understand the complexity of the most appropriate procedures; in this situation, dumbing down is intended to facilitate ease of presentation. We examine each of these justifications in turn.

Increasing Support for Hypotheses

We submit that inappropriate simplification of data analysis for

(Continued on page 27)

Dumbing it down, Cont.

(Continued from page 26)

increasing support for a conclusion constitutes a type of "soft fraud," as described by Chris Crandall (2001). This is incontrovertibly a misuse of statistics; the outcome of a statistical analysis should never be considered as a justification for using the procedure in the first place. In agreement with Crandall (2001), we believe researchers must be accountable for their selection of statistics and resist the temptation of simplifying analyses for the sake of increasing support for their hypotheses.

Lack of knowledge

The most frequent abuse of statistics is not characterized by deliberate misuse or malicious falsehood. Rather, lack of knowledge leads to the application of obsolete statistical analyses, the inappropriate application of otherwise legitimate analytic procedures, and the use of simple analyses when the benefits of more complex strategies would far outweigh the costs. We suggest that ignorance of how to carry out appropriate procedures is no excuse for implementing simpler but less appropriate (or entirely inappropriate) techniques. It is the investigator's *responsibility as a scientist* to seek out the most appropriate data analytic procedure, learn how to use it, and apply it. Such knowledge can usually be obtained without undue difficulty. Many universities have statistical consultants or knowledgeable faculty members that can assist with such tasks. Keeping abreast of recent articles and advances can also help to resolve this difficulty. Finally, reviewers can alert researchers who are using obsolete or inappropriate methods. One of the primary purposes of peer review is to ensure that the methods and analyses employed in a study adhere to scientific ideals.

Lack of knowledge can also lead researchers and reviewers to be skeptical of the merit of advanced techniques. Researchers may see

advanced techniques as some sort of "magic" that can, in the hands of a skilled user, be tweaked to produce any desired result. This perspective leads some to conclude that these techniques cannot be trusted. Advanced techniques are not witchcraft. In the hands of an investigator who has spent sufficient time learning how to use them, they can reveal much that would otherwise have remained hidden or uninvestigated entirely. We feel that this mistrust is misplaced – all statistical techniques are subject to knowing or unknowing manipulation. The perceived "tweakability" of a particular analytic technique does not constitute a reason to avoid the method, but rather to learn how to use it properly and effectively.

Ease of Presentation

Researchers fully capable of carrying out the appropriate set of analyses may resort to dumbing down for the sake of reviewers or their audience. For example, a researcher may design her experiment in such a way that she can later perform an ANOVA, for the sole reason that she thinks an ANOVA will be easier to conduct (and its results easier to report and understand) than a regression analysis. Similarly, linear regression may be chosen as the proper analysis for a multi-classroom experimental design when the hierarchical nature of the data clearly calls for multilevel modeling. In such situations, the thinking typically is, "I know multilevel modeling is considered difficult, so my readers probably do not know much about it. More to the point, neither will my reviewers." Thus, there are frequently competing motivations to present accurate, illuminating results on one hand, and results simple enough for an article's readership to digest on the other.

How should we resolve this quandary? As a first step, we suggest that reviewers be given the benefit of the

doubt. Reviewers are usually selected because of their expertise and familiarity with the subject in question. More often than not, they are fully aware of what analytic strategies are the most appropriate and which ones are less than optimal, and are willing to learn something new when confronted with the need to do so. Also, whereas it is true that some readers may struggle with statistics, a paper should be written such that the findings can be understood regardless of the consumer's quantitative savvy. For those readers interested in understanding the methodology, a paper has the added bonus of encouraging them to learn and master the methods employed.

Of course, researchers may have complete confidence that reviewers and readers would understand advanced techniques, but defer to the idea that simple techniques provide parsimony. This would constitute a tragic misinterpretation of the APA Taskforce's recommendation regarding the minimally sufficient analysis. The Taskforce also stated that "complex designs and state-of-the-art methods are sometimes necessary to address research questions effectively" (Wilkinson et al., 1999, p. 598), an important point which should not be ignored. Thus, although parsimony is desirable, it should not come at the cost of sound statistical practice.

Conclusion

Our position is that the choice of a statistical analysis should *never* be guided by assumptions about the intelligence or expertise of editors, reviewers, or the reading public. If a particular analytic procedure is obviously the most appropriate one for a research design, then use it – hopefully its appropriateness will be just as evident to reviewers and readers. If authors are worried that reviewers will not understand the analysis, they should be prepared to defend their

(Continued on page 31)

IRB and U: What Institutional Review Boards Are Supposed to Do

By *Louis A. Penner*

In many ways, the life of the personality and social psychology researcher has improved over the last ten to fifteen years. Now, we can communicate instantaneously via e-mail with distant colleagues and nearby students; we can run some of our studies in an electronic medium, and we will never ever again have to carry boxes full of “dollar” cards to a computer center to analyze our data. But on the other hand, and this is a “hand” the size of King Kong’s, we must now deal with the trials and travails of our local Institutional Review Board (or IRB). In this brief article I discuss the origins of IRBs, how they operate, and attempt to dispel a few of the myths about institutional reviews.

OK, I’ll confess it right up front: I think that IRB’s are a good idea. In fact, I’m Chair of my university’s Social and Behavioral IRB. But please note that I said IRBs were a *good idea*; I did not say that they were necessarily a *good thing* in practice. A brief bit of history tells us why IRBs are a good idea. Today’s IRBs can trace their roots back to the war crimes trials that followed

I said IRBs were a good idea; I did not say that they were necessarily a good thing in practice.

WWII, and the almost universal revulsion at the “experiments” done by the Nazi doctors. But the real push for institutional safeguards for human subjects didn’t come until about 20 years later. The murmurings about the

abuse and mistreatment of human subjects in the United States became a roar in the 1970’s when the infamous Tuskegee syphilis experiment was made public. In the context of IRBs the most salient aspect of these experiments is not only the irreparable harm that was done to the unwitting participants (Jones, 1992), but also that the principal investigators, at least initially, seemed almost blind as to the unethical and inhumane way they treated their subjects (all of whom, perhaps not coincidentally, were African-Americans.) The congressional hearings and public outcry over the syphilis experiment, as well as disclosures of other unethical research studies lead to the *Belmont Report*, issued in 1979 by the National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research. (If you want to read this document, the URL is given below.) The *Belmont Report* presented three basic ethical principles that should guide research with humans: Respect for persons, beneficence, and justice. Several years later, a number of government agencies (including NIH and NSF) gave the force of law to the principles in this report when they all adopted the “Common Rule” for “research with human subjects.” (The URL for the Common Rule is given at the end of this article)

It is the “Common Rule” that authorizes IRBs and gives them their power. If your institution gets federal money for research (or maybe anything else), it must comply with the statutes that are part of the Common Rule. So, what’s the problem with the federal government (actually the Office of Human Research Protection—OHRP) requiring that we treat our research participants respectfully, kindly, and fairly? Well, as we all know, “The devil is in the details.”

At one of the national IRB meetings I recently attended, a speaker showed a cartoon with two panels. In the first, Moses is receiving the two tablets that contain the ten commandments. Under this panel is the caption: “The Rules”. In the second panel Moses is pushing a cart that contains maybe 200 additional tablets. Under this panel is written: “The Interpretations of the Rules”. This is the major problem with IRBs. There are almost as many different interpretations of the regulations contained in the Common Rule as there are different IRBs. At some institutions the researcher who submits an IRB application is entering the “Gates of Hell”. The IRB (or its Chair) has little first hand experience with research and is concerned almost exclusively with protecting the institution from possible government censure (or worse) and/or from possible lawsuits by “aggrieved” research participants. The review will probably be long, difficult, and often antithetical to the research process. (These IRBs are also sometimes known by their other acronym, CYA.) At other institutions, the IRB (or its Chair) has the experience and judgement to decide which studies really put their research participants at risk, while at the same time is sensitive to the institution’s research mission. But (and this is important) they are both working from *exactly the same set of federal regulations*.

But how can this be? Leaving aside possible differences in the Big Five profiles of IRB Chairs and Board Members, part of the problem is that the Common Rule and the statutes that accompany it are written in exquisite “bureaucratize”; and as a consequence, they can be interpreted in a multitude of different ways (see Pritchard, 2001). Further, the regulations allow for substantial discretion in how they are interpreted, as long as the basis for a

(Continued on page 29)

IRB and U, Cont.

(Continued from page 28)

particular interpretation is well documented. The latitude allowed (and taken) by IRBs can be a force for good or evil. But irrespective of the valence one assigns to the IRB process, we need to learn to deal with it. So, with that in mind I want to conclude with some common myths about IRBs.

Myth 1. All IRB decisions are based on federal or local statutes or written rules. *Fact:* As suggested above, many IRB decisions are based on the idiosyncratic interpretations and habits of a particular administrator. So, if a rule or decision just doesn't make sense to you, ask to see the federal statute or the part of the written local policies and procedures that provide the basis for this decision. (The relevant document will be an "MPA" or a "FWA"; it must be made available to the public). But before you protest too much, it's probably a good idea to become an informed consumer and take more than the minimum training required by your institution's IRB. (The NIH training modules are useful; also see Puglisi, 2001).

Myth 2. The Common Rule was originally written only for medical research, and social and behavioral research was included later on. *Fact:* While, to be sure, concerns about medical research dominate most IRB staffs, the Common Rule was always intended to cover social and behavioral research as well. However, often medical IRBs have difficulty understanding and estimating the risks that attend social and behavioral research. Therefore, lobby for separate medical and nonmedical IRBs at your institution.

Myth 3. All research proposals must go before a full IRB board. *Fact:* Only proposals that involve more than minimal risk and/or involve certain vulnerable populations must go to the full board. Leaving aside these proposals, an IRB Chair has considerable discretion as to what does

and does not go to the full board. An example: At my university less than 10 percent of the proposals are now sent to the full board; with a different Chair (but exactly the same rules) it was 30 to 40 percent. The message here is clear: Get people who are knowledgeable about and supportive of the research process appointed to the Board and, if possible get them to be the Chair.

Myth 4. Deception research is not permitted by OHRP. *Fact:* The rules require that the informed consent tells participants what they are going to be asked to do or what will happen to them. The rules do not require that you disclose the true purpose of the study at that time, as long as the principles of respect, beneficence and justice are followed in the study.

Myth 5. Some proposals (e.g., anonymous surveys) do not need to be reviewed. *Fact:* This is a tricky one. Anonymous surveys are usually "exempt" from IRB review, but that doesn't mean they do not need to be reviewed. Someone needs to make the determination that an application falls into the exempt category and does not

Soon there will be national accreditation of IRBs.

need further IRB review, but this decision cannot be made by the PI. In some universities a departmental committee does this: at others it's done by the IRB (the feds prefer the latter). So, don't convince yourself that a study is so harmless that no one needs to review it. Irrespective of the risks associated with a study, significant noncompliance **must** be reported to OHRP and the consequences for the individual researcher can be much, much worse than having a project delayed for a few weeks.

Myth 6. If we're just patient, this will all just pass away. *Fact:* It's headed in

the other direction. Soon there will be national accreditation of IRBs. In my view this is a very bad idea, but it's too late to stop this movement. Hopefully, however, it's not too late to influence the way in which accreditation will be done. If psychologists and other social scientists don't get involved in this process, the rules, regulations, and criteria will be written by medical researchers, nonacademic IRB administrators, and the drug companies that fund medical research. It is quite simply not in our interests to allow accreditation of IRB's to proceed without substantial input from social and behavioral scientists. Therefore, I conclude with this article with a plea: Ask, nay insist, that the leadership of SPS, SESP, SRC, APA, and APS, and other relevant organizations formally and informally lobby OHRP and try to influence the forthcoming national accreditation of IRBs. The research you save may be your own.

Useful URLs

The Belmont Report: <http://ohrp.osophs.dhhs.gov/humansubjects/guidance/belmont.htm>

The Common Rule: <http://ohrp.osophs.dhhs.gov/humansubjects/guidance/45cfr46.htm>

The NIH training module: <http://cme.nci.nih.gov/>

References

Jones, H. (1992). *Bad Blood: The Tuskegee Syphilis experiment*. New York: Free Press

Puglisi, T. (2001). IRB Review: It helps to know the framework. *APS Observer*, 14, 1, 34.

Pritchard, I. (2001). Searching for 'research involving human subjects'. *Ethics and Human Research*, 3, 5-12.

■

Paul Ekman Wins 2003 Jack Block Award for Personality Research

The Society for Personality and Social Psychology (SPSP) is pleased to announce that Dr. Paul Ekman is the winner of the 3rd Annual Jack Block Award for Personality Research. The nomination of the award committee (David Funder, Carol Dweck, and Auke Tellegen) was ratified by the SPSP Executive Board at its February meeting. The first winner of this prestigious award was Jack Block (before the award was named), and the second winner was Auke Tellegen.

Dr. Ekman's contributions to psychology have been fundamental to a balanced and broad understanding of

the biological-evolutionary, cultural, and psychological roots of affect and affect expression. His work has been methodologically innovative as well, as he has provided fundamental insights into such topics as the basic nature of emotions, their cross-cultural generality, their expression, and how these insights can be combined in the aid of applied issues such as lie detection.

Dr. Ekman will present an award address at the 2003 SPSP meeting in Los Angeles, where he will receive a plaque and cash prize.

— David Funder

Announcements

2003 APA SCIENTIFIC AWARDS PROGRAM: CALL FOR NOMINATIONS

The American Psychological Association (APA) invites nominations for its 2003 scientific awards program. The **Distinguished Scientific Contribution Award** honors psychologists who have made distinguished theoretical or empirical contributions to basic research in psychology. The **Distinguished Scientific Award for the Applications of Psychology** honors psychologists who have made distinguished theoretical or empirical advances in psychology leading to the understanding or amelioration of important practical problems. To submit a nomination for the Distinguished Scientific Contribution Award and the Distinguished Scientific Award for the Applications of Psychology, you should provide a nomination form, a letter of nomination, the nominee's current vita with list of publications, up to five representative reprints, and the names and addresses of several scientists who are familiar with the nominee's work. The **Distinguished Scientific Award for Early Career Contribution to Psychology** recognizes excellent young psychologists. For the 2003 program, nominations of persons who received doctoral degrees during and since 1993 are being sought in the areas of: **social; behavioral and cognitive neuroscience; perception, motor performance; applied research** (e.g.,

treatment and prevention research, industrial/organizational research, educational research); **individual differences** (e.g., personality, psychometrics, mental ability, behavioral genetics).

To submit a nomination for the Distinguished Scientific Award for Early Career Contribution to Psychology, you should provide a letter of nomination, the nominee's current vita with list of publications, and up to five representative reprints.

To obtain nomination forms and more information, you can go to the Science Directorate web page (www.apa.org/science/sciaward.html) or you can contact Suzanne Wandersman, Science Directorate, American Psychological Association, 750 First Street, NE, Washington, DC 20002-4242; by phone, (202) 336-6000; by fax, (202) 336-5953; or swandersman@apa.org. **The deadline for award nominations is June 1, 2002.**

Association for the Advancement of Applied Sport Psychology. I would like to call your attention to a relatively new organization, the Association for the Advancement of Applied Sport Psychology (AAASP). Originating in 1985, AAASP has a membership of approximately 1,000 individuals with an interest in the psychology of sport and physical activity. One of the three major areas of AAASP (along with performance enhancement and health psychology) is social psy-

chology. A number of social psychologically oriented presentations are made each year at our annual meeting and a number of prominent social psychologists have made keynote addresses (e.g., Baumeister and Leary). I invite you to consider joining AAASP and becoming involved in our community. We have a large number of social psychologists in AAASP and we find the organization to be a useful outlet and link for continuing work in the social psychology of sport. For detailed information on AAASP, you may find us on the web at: www.aaasponline.org. You should also feel free to contact me as well at: danwann@msumusik.mursuky.edu. Daniel L. Wann, Murray State University, AAASP-SPSP Liaison

New Journal. A new journal called the *Journal of Articles in Support of the Null Hypothesis* is now accepting submissions. In the past other journals and reviewers have exhibited a bias against articles that did not reject the null hypothesis. We plan to change that by offering an outlet for sound experiments that do not reach the traditional significance levels ($p < .05$, thus, reducing the file drawer problem and reducing the bias in psychological literature. Without such a resource, researchers could be wasting their time examining empirical questions that have already been examined. The journal is peer reviewed, published online quarterly, and offered to the scientific community free of cost. You can reach our website by going to <http://www.jasnh.com>

New Books by SPSP Members

Facial Attractiveness: Evolutionary, Cognitive, and Social Perspectives, G. Rhodes, & L.A. Zebrowitz, (Eds.) (2001). Westport, CT: Ablex] brings together seminal work from cognitive, evolutionary, social, and developmental perspectives that explores the questions "What makes some faces more attractive than others and why?" Some accounts attempt to explain preferences that are widely shared and that persist over time. Preferences are not, however, identical for different individuals or at different times, and these variations must also be explained. Several contributors speculate that individual learning histories and social goals may account for these variations. Whereas many attempts to explain our preferences focus on sexual attractiveness, people can also be attractive as friends or mentors. These preferences are also discussed. Much of the research in the following chapters has used sophisticated computer-imaging techniques to manipulate facial images, taking researchers beyond a correlational approach and al-

(Continued on page 32)

News of the Society: Growth, Prosperity and Opportunity

(Continued from page 3)

appropriate registration. Full-text versions of manuscripts will be available in PDF format, and a search engine will also be available. Details about the registration process and online access will be provided by Sage in a subsequent mailing.

PSPR received 67 submissions last year (up from 51 in 2000), and had an acceptance rate comparable to that for PSPB. The mean editorial lag for the journal is a remarkable 11 weeks; editor Eliot Smith and associate editors Diane Mackie and Robert McCrae continue their terms through 2003.

Committee reports. The Diversity Committee, Training Committee, and Graduate Student Committee continue

their activities (see reports in this issue). Action taken at the Executive Committee meeting included endorsement and support of GASP, the gay-lesbian-bisexual-transgender alliance in SPSP, an increased budget allotment to the Diversity Committee for operating expenses and activities, and passing of a proposal to include a half-hour meeting for graduate students in the SPSP convention program agenda each year.

The Committee also enthusiastically endorsed a proposal presented by Eliot Smith for a Summer Institute in Social Psychology. This summer school program would be based on the European Association of Experimental Social Psychology (EAESP) model, in which graduate students are brought

together for intensive training in areas presumably not covered by their home departments. Eliot Smith, Chick Judd, and Harry Reis have drafted an NSF proposal that, if awarded, would run a bi-annual, two-week long summer session for some 100 social psychology graduate students. A proposal will be submitted this year, with the goal that workshops begin in summer 2003.

Awards. The Executive Committee considered offering two new Society Awards: Service to the Society, and Service to Personality/Social Psychology. Also approved was a proposal for a new "Theoretical Innovation Prize" (see story on p. 6). The Committee was also pleased to endorse the following awardees: The 2002 Jack Block Award winner is Paul Ekman, and the 2002 Murray Award winner is Seymour Epstein. ■

Dumbing it down, Cont.

(Continued from page 27)

choices, not merely write to the lowest common denominator. In conclusion, oversimplification avoids the benefits granted by more complex methods and sometimes introduces errors and violates assumptions (as with the median split technique). The primary reasons for dumbing down statistical analyses, when considered, are clearly unjustifiable.

References

Aiken, L. S., & West, S. G. (1991). *Multiple regression: Testing and interpreting interactions*. Newbury Park: Sage.

Crandall, C. (2001). Scientific Progress: A need for trust, a need for skepticism. *Dialogue*, 16(2), 20-21.

Cunningham, W. A., Preacher, K. J., & Banaji, M. R. (2001). Implicit attitude measures: Consistency, stability, and convergent validity. *Psychological Science*, 12(2), 163-170.

Fabrigar, L. R., Wegener, D. T., MacCallum, R. C., & Strahan, E. J. (1999). Evaluating the use of exploratory factor analysis in psychological research. *Psychological Methods*, 4(3), 272-299.

Floyd, F. J., & Widaman, K. F. (1995). Factor analysis in the development and refinement of clinical assessment instruments. *Psychological Assessment*, 7(3), 286-299.

Ford, J. K., MacCallum, R. C., & Tait, M. (1986). The application of exploratory factor analysis in applied psychology: A critical review and analysis. *Personnel Psychology*, 39(2), 291-314.

Lee, H. B., & Comrey, A. L. (1979). Distortions in a commonly used factor

analytic procedure. *Multivariate Behavioral Research*, 14, 301-321.

MacCallum, R. C., Zhang, S., Preacher, K. J., & Rucker, D. D. (in press). On the practice of dichotomization of quantitative variables. *Psychological Methods*.

Preacher, K. J., & MacCallum, R. C. (2000). *Repairing Tom Swift's electric factor analysis machine*. Manuscript submitted for publication.

Widaman, K. F. (1993). Common factor analysis versus principal component analysis: Differential bias in representing model parameters? *Multivariate Behavioral Research*, 28(3), 263-311.

Wilkinson, L. & the APA Taskforce on Statistical Inference (1999). *Statistical Methods in Psychology Journals: Guidelines and Explanations*. *American Psychologist*, 54(8), 594-604. ■

Published at:
Department of Psychology
University of Kansas
Lawrence, KS 66045

Phone: 785-864-9807
Fax: 785-864-5696
Email: crandall@ku.edu or
biernat@ku.edu

News of the Society Since 1986

Announcements, Cont.

(Continued from page 30)

lowing experimental tests of what makes a face attractive.

Contributors and chapter titles: Rubenstein, Langlois, & Roggman *What Makes a Face Attractive and Why: The Role of Averageness in Defining Facial Beauty.*; Rhodes, Harwood, Yoshikawa, Nishitani, & McLean *The Attractiveness of Average Faces: Cross-cultural Evidence and Possible Biological Basis*; Little, Penton-Voak, Burt, & Perrett, *Evolution and individual differences in the perception of attractiveness: How cyclic hormonal changes and self-perceived attractiveness influence female preferences for male faces*; Grammer, Fink, Juette, Ronzal, & Thornhill, *Female faces and bodies: N-dimensional feature space and attractiveness*; Enquist, Ghirlanda, Lundqvist, & Wachtmeister, *An Ethological Theory of Attractiveness*; Keating *Charismatic faces: Social status cues put face appeal in context*; Cunningham, Barbee & Phillhower *Dimensions of facial physical attractiveness: The intersection of biology and culture*; Dion *Cultural Perspectives on facial attractiveness*; Zebrowitz & Rhodes *Nature let a hundred flowers bloom: The multiple ways and wherefores of attractiveness.*

Ageism: Stereotyping and Prejudice Against Older Persons. (2002). Edited by T. D. Nelson. MIT Press.

In social perception, people tend to automatically categorize others along three major dimensions: race, sex, and age (Kunda, 1999). While much empirical and theoretical attention has been devoted to the study of racism and sexism, comparatively little research in psychology has been directed at understanding what some refer to as the "third ism," ageism (Barrow & Smith, 1979). But why? There are a myriad of possible reasons, but perhaps the most obvious is that age prejudice is one of the most socially-condoned, institutionalized forms of prejudice in the world - and especially in the United States. As you will see in reading the chapters of this volume, most Americans tend to have little tolerance for older persons, and have very few reservations about harboring

negative attitudes toward older people. Whatever the reasons for the comparative dearth of theoretical and empirical research on ageism among psychologists, it is clear that much more research is needed. This volume addresses ageism from several different perspectives (e.g., gerontology, communication, psychology), and the distinguished chapter authors present the latest theoretical and empirical advances in our understanding of the causes and effects of ageism.

Table of Contents: *Doddering but dear: Process, content and function in stereotyping of older persons*, Cuddy & Fiske; *Ageism. Denying the face of the future*, Greenberg, Schimmel, & Mertens; *Implicit ageism*, Levy & Banaji; *A social-developmental view of ageism*, Montepare & Zebrowitz; *Attitudes toward older adults*, Kite & Wagner; *Ageism in the workplace: A communication perspective*, McCann & Giles; *Ageist behavior*, Pasupathi & Lockenhoff; *The paradox of well-being, identity processes, and stereotype threat: Ageism and its potential relationships to the self in later life*, Whitbourne & Sneed; *Acting your age*, Golub, Filipowicz & Langer; *Will families*

Claude Steele*	President
Jim Blascovich*	President-Elect
Ed Deiner *	Past President
Harry Reis	Executive Officer
Sharon Brehm*	Co-Secretary-Treasurer
Leslie Zebrowitz*	Co-Secretary-Treasurer
Jerry Suls	Editor, PSPB
Fred Rhodewalt	Incoming Editor, PSPB
Eliot Smith	Editor, PSPR
Chris Crandall	Co-Editor, Dialogue
Monica Biernat	Co-Editor, Dialogue
Dan Cervone	SPSP Convention Committee, Chair
Lynne Cooper	SPSP Convention Committee
Rick Hoyle	SPSP Convention Committee
Joshua Aronson	APA Program Committee, Co-Chair
Andrew Elliot	APA Program Committee, Co-Chair
David Dunning	Publication Committee, Chair
John Dovidio	Publication Committee
Joanne Wood	Publication Committee
Allen Omoto	Training Committee, Chair
Kim Bartholomew	Training Committee
Lisa Aspinwall	Training Committee
Anne Bettencourt	Diversity Committee
Gregory Herek	Diversity Committee
Lloyd Sloan	Diversity Committee
Monica Biernat*	APA Council Rep and Member at Large
June Tangney*	APA Council Rep and Member at Large
Susan Andersen*	Member at Large
David Funder*	Member at Large
Judith Harakiewicz*	Member at Large
Gina Reisinger-Verdin	Executive Assistant

*Denotes voting member of the SPSP Executive Committee

Dialogue Mission Statement

Dialogue is the official newsletter of the Society for Personality and Social Psychology. It appears twice every year, in the spring and fall. Its intended readership is members of the Society. The purpose of *Dialogue* is to report news of the Society, stimulate debate on issues, and generally inform and occasionally entertain. *Dialogue* publishes summaries about meetings of the Society's executive committee and sub-committees, as well as announcements, opinion pieces, letters to the editor, humor, and other articles of general interest to personality and social psychologists. The Editors seek to publish all relevant and appropriate contributions, although the Editors reserve the right to determine publishability. Content may be solicited by the Editors or offered, unsolicited, by members. News of the Society and Committee Reports are reviewed for accuracy and content by officers or committee chairs of SPSP. All other content is reviewed at the discretion of the Editors.



Society for Personality and Social Psychology
Visit us at www.spsp.org