



The General Psychologist

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Division One
of the American
Psychological Association

APA Convention 2010 Preview

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AMERICAN PSYCHOLOGICAL ASSOCIATION

DIVISION ONE
CONVENTION PREVIEW
2010

Convention Co-Chairs for Division One



Mark Terjesen, Ph.D.
St. John's University



Mark J. Sciutto, Ph.D.
Muhlenberg College

We have thoroughly enjoyed serving as co-chairs of the Division 1 program for the 2010 convention in San Diego. Division 1 takes great pride in providing a forum for research that reflects the diverse subspecialties in psychology. We have tried to compile a program that puts these subspecialties into conversation with one another. We have a variety of invited speakers and symposia as well as a sizable number of submitted programs. This year, we will also host two poster sessions, with one being specifically dedicated to showcasing student research. Below are some highlights of the Division 1 program.

Our Division President, John Hogan will give his address on The Secret Lives of APA Presidents. Other invited speakers will include Dean Keith Simonton from the University of California – Davis (Creativity and Discovery As Blind Variation: Donald Campbell's BCSR Theory After the Half Century Mark), Joseph E. LeDoux from New York University (Why You Should Know About Fear in Rats if You Are a Therapist), Andres De Los Reyes from the University of Maryland College Park

(Informant Discrepancies in Child Assessment: Informing Understanding of Children's Behavior), Arnold J. Sameroff from the University of Michigan (Unifying Individual and Context in a Biopsychosocial Theory of Development), Tom Pyszczynski from the University of Colorado at Colorado Springs (War and Peace: Implications from Research on the Role of Terror Management Processes in the Ongoing Conflict in the Middle East), and Rolando Diaz-Loving from the National Autonomous University of Mexico (What Ever Happened to Sociocultural Variables? Insights from Mexican Ethnopsychology). Descriptions of many of these talks are seen throughout this issue.

Division 1 will also host a session highlighting the 2009 Society for General Psychology award winners. Diane F. Halpern (Claremont McKenna College) received the George A. Miller Award for Outstanding Research Article in General Psychology and will discuss her article, The Science of Sex Differences in Science and Mathematics. Linda Bartoshuk (University of Florida) received the Ernest R. Hilgard Lifetime Achievement Award and will highlight her work in a presentation titled Hilgard and Comparing Pain Intensities Across Individuals. Finally, Susan Pinker (The Globe and Mail, Montréal) will present on her book, The Sexual Paradox: Men, Women, and the Real Gender Gap, which received the William James Book Award.

Douglas K. Candland of Bucknell University will deliver the 12th annual American Psychological Foundation (APF) Arthur W. Staats Lecture for Unifying Psychology. Dr. Candland is currently the Homer P. Rainey Professor Emeritus of Psychology and Animal Behavior.

The Division 1 program will also showcase a variety of symposia that reflect the diverse nature of research in psychology. One symposium will focus on research, education and prevention of health disparities (co-sponsored by the Committee on Ethnic and Minority Affairs). Among the topics of other symposia are: the APA Task Force Report on Sexual Orientation; the therapeutic use of interactive virtual environments; disseminating research findings; knowledge and misconceptions in psychology; diagnostic considerations for psychologists who work with school-aged children; developments in IRBs; use of technology in instruction, assessment and intervention; the fidelity and sustainability of school-based programs; and new approaches toward analyzing assessment and intervention data.

Division 1 will also sponsor two workshops. Kellina Craig-Henderson and Brett Pelham (NSF) will discuss funding opportunities at the National Science Foundation. Bernard Gorman (Nassau Community College) and Louis Primavera (Touro College) will lead a workshop addressing the crisis in statistics education of psychologists.

A schedule that presents Division 1 programming is included in this issue for your benefit. We are very excited about the range of topics represented in this year's program. We look forward to seeing all of you in San Diego!



Also, watch for the first Anne Anastasi Graduate Student Award to be presented in San Diego this August, with a reception to salute all nominees, their mentors, and judges. Photo courtesy of Jonathan Galente.

Each Division 1 presenter was solicited to provide a write-up of their presentation.

What follows are speakers' submissions in their own words.



***Creativity and Discovery as Blind Variation:
Donald Campbell's BVSr Theory after the
Half-Century Mark***

***Dean Keith Simonton, Ph.D.
University of California, Davis***

The title of Simonton's talk is "Creativity and Discovery as Blind Variation: Donald Campbell's BVSr Theory after the Half-Century Mark." Here he will provide an updated evaluation of Campbell's (1960) classic theory that creativity and discovery depend on blind ideational variations (BV). The evaluation begins by providing improved definitions of the central concepts underlying Campbell's argument. These concepts are (a) creativity and discovery, (b) variant blindness versus sightedness, (c) variant fitness and selection, and (d) ideational variants versus creative products. Of special importance is the precise definition of BV "thought trials" in terms of the decoupling of variant probabilities from their respective likelihoods of success – a definition adapted from evolutionary biology.

These definitions then provide the basis for specifying the identification criteria that allow us to determine the conditions under which ideational variations can be considered blind. These criteria are of two types, intended and implied.

Intended BV occurs when the thought trials are blind by design. Examples include (a) systematic searches or scans (e.g., the heuristic procedures used in discovery programs), and (b) stochastic combinatorial procedures (e.g., "mutations" and "recombinations" implemented in genetic algorithms and other evolutionary programs). Any computer program that simulates creativity or discovery by introducing some

random mechanism is inherently producing BV by design.

Implied BV, in contrast, must be inferred from empirical observations of creativity and discovery. These inferences are of two kinds. The first involves ideational variations that feature the telltale properties of blindness, namely variation backtracking (under sequential selection) and variation superfluity (under both simultaneous or sequential selection, but especially when the variants are incommensurate). The second involves the processes that necessarily yield blind ideational variations. These processes include behavioral tinkering, heuristic search, defocused attention (reduced latent inhibition), and associative richness, where the latter encompasses remote and rare associations, divergent thinking, primordial or primary process cognition, Janusian associations and homospatial imagery, allusive or over-inclusive thought, and clang associations. These processes yield blind ideational variations both singly and collectively – the latter because most acts of creativity or discovery require more than one process. Yet the order in which these processes are activated during problem solving is decoupled from their corresponding probabilities of arriving at the solution.

Simonton then addresses common criticisms that BV (a) denies domain-specific expertise, (b) requires ideational randomness, and (c) assumes analogical equivalence with biological evolution. All of these criticisms prove unfounded. To begin with, Campbell's theory permits the pre-selection of ideational variants based on past BVSr episodes, whether Darwinian, Skinnerian, Popperian, or Gregorian (to borrow Daniel Dennett's terms), whether direct or vicarious (i.e., social learning). Moreover, the theory explicitly allows for the operation of non-random but blind processes (e.g., systematic combinatorial search). Finally, the intellectual antecedent of Campbell's theory is not Charles Darwin's 1859 *Origin of Species* but rather Alexander Bain's 1855 *The Senses and the Intellect*, which proposed an earlier version of BVSr in the context of invention. Consequently, it is incorrect to call it "Darwinian" as many (including Simonton) have been wont to do. Bainian would be better.

Simonton's talk then closes with a brief survey of the theory's contributions to our scientific understanding of creativity and discovery. BVSr theory provides a comprehensive explanation of the key phenomena, integrates alternative explanations

that were previously considered antithetical, and offers a fruitful investigative paradigm, especially in the form of stochastic combinatorial models.

The above summary may seem very abstract, but Simonton hopes to illustrate his main points using concrete examples drawn from Maier's classic two-string problem, Kepler's Third Law of Planetary Motion, Watson's discovery of the DNA code, Beethoven's coda to the Allegro con brio movement of his Fifth Symphony, Picasso's sketches for his great Guernica painting, and the introspective confessions of Michael Faraday, William James, Hermann Helmholtz, and Henri Poincaré – as well as radar scans at airports and air-defense systems, multiple-choice tests taken by good, bad, and mediocre students, and what happens “when two roads diverge in a yellow wood.” If he omits any illustration, call him to task at the San Diego APA!

It's now time for an “APA Disclosure of Interests.” Simonton has to confess that he received his Ph.D. at Harvard under David A. Kenny, who had earned his own Ph.D. three years earlier at Northwestern under Donald T. Campbell. Technically, this makes Simonton the doctoral grandchild of Campbell. Whether or not that biases his critique of BVSr will have to be determined by those attending his invited address. Even so, both the Division 1 program committee and Simonton himself can declare that he will not profit financially from delivering his talk! The only interest Simonton has in BVSr is that it's interesting – and true.

Dean Keith Simonton is Distinguished Professor of Psychology at the University of California, Davis. Among his 400 publications are 11 books, including *Genius, Creativity, and Leadership* (Harvard, 1984), *Psychology, Science, and History* (Yale, 1990), *Greatness* (Guilford, 1994), *Origins of Genius* (Oxford, 1999), *Great Psychologists* (APA, 2002), *Creativity in Science* (Cambridge, 2004), *Genius 101* (Springer, 2009), and *Great Flicks* (Oxford, in press). Simonton is former president of the Society for the Psychology of Aesthetics, Creativity and the Arts (APA, Division 10) and the International Association for Empirical Aesthetics (IAEA). He is Fellow of the American Association for the Advancement of Science, the Association for Psychological Science, IAEA, and nine APA Divisions (viz. 1, 2, 5, 7-10, 20, and 24). His honors include the William James Book and George A. Miller Outstanding Article Awards (both Division 1), the SPSP Theoretical Innovation Prize (Division 8), the Rudolf Arnheim Award for

Outstanding Contributions to Psychology and the Arts (Division 10), the Robert S. Daniel Award for Four-Year College/University Teaching (Division 2), the Sir Francis Galton Award for Outstanding Contributions to the Study of Creativity (IAEA), the President's Award from the National Association for Gifted Children, and two Awards for Excellence in Research from the Mensa Education and Research Foundation. More documentation for the curious is available at <http://psychology.ucdavis.edu/Simonton>.



Discovering Meaning in the Discrepancies among Informants' Clinical Reports of Children's Behavior

Andres De Los Reyes, Ph.D.

Comprehensive Assessment and Intervention Program

University of Maryland at College Park

The following abstract draws much from drafts of a manuscript of the Introduction to a Special Section that I am preparing for publication in an upcoming issue of the *Journal of Clinical Child and Adolescent Psychology* (De Los Reyes, 2010; expected publication date: March 2011):

Clinical science has yet to identify a definitive and feasibly cost-effective biological or behavioral marker for any of the mental disorders diagnosed in adults and children (e.g., attention-deficit hyperactivity disorder, conduct disorder, major depression, social anxiety, and substance dependence; American Psychiatric Association, 2000). Thus, comprehensive measurement strategies that incorporate multiple indices of the same behavior or construct have become the mainstay of evidence-based assessment in clinical research and practice (Hunsley & Mash, 2007). In fact, a key component of best practices in evidence-based assessments of psychopathology in children and adolescents (referred to collectively as “children”)

involves use of reports taken from multiple informants (Mash & Hunsley, 2005). To assess any one child on any one psychopathology domain, researchers commonly use an array of informants to provide reports. These informants often include the child him/herself, parents, teachers, peers, clinicians, laboratory observers, and official records (e.g., grades, arrest records, standardized test scores).

Much of the evidentiary support of the efficacy of evidence-based treatments for children rests on multiple informants' reports of treatment outcomes (Weisz, Jensen Doss, & Hawley, 2005). However, discrepancies often arise among multiple informants' reports of child and adolescent psychopathology as well as among reports of related constructs (e.g., parenting, family relationship quality and functioning, parental monitoring) (Achenbach, 2006). These inconsistencies across informants' reports (hereafter referred to collectively as "informant discrepancies") often translate into inconsistencies within the outcomes findings reported within randomized controlled trials, as well as in findings of studies identifying the prevalence of clinical conditions and risk factors for the emergence of these conditions (for reviews please see De Los Reyes & Kazdin, 2005, 2006, 2008, 2009; Koenig, De Los Reyes, Cicchetti, Scahill, & Klin, 2009). As a result, clinical scientists and practitioners have yet to understand how best to interpret these discrepancies. In particular, two questions continue to arise in crucial areas of clinical research: (a) What does it mean when different informants' reports of the same behavior yield inconsistent research findings? and (b) If differences among informants' reports mean something, does their "meaning" make them useful?

In this Invited Address, I will discuss new research that is uncovering meaning in informant discrepancies in reports of children's behavior. Across studies using various designs (laboratory, longitudinal, randomized controlled trial, meta-analysis), I will discuss how this work has revealed that informant discrepancies: (a) relate to where children express behaviors indicative of psychopathology and which informants observe their expression, (b) demonstrate stability over time in both community and clinic settings, (c) predict poor child and adolescent outcomes in ways that the individual informants' reports do not, and (d) can be used to identify meaningful treatment outcomes patterns within randomized controlled trials. In doing so, I will discuss how these recent research efforts have begun to uncover the answer to the first question posed previously: What do informant discrepancies

mean? The preliminary answer is this: Under certain circumstances, informant discrepancies in reports of child psychopathology (and related domains) reveal important characteristics of where children express behaviors indicative of psychopathology. These characteristics may include: (a) the settings in which children express these behaviors, (b) the nature of the interactions that children have with peers and adult caregiver and authority figures, and (c) the characteristics of the informants who observe children's behavior and provide behavioral reports. Lastly, I highlight how future work seeking to build on the findings reviewed in this Invited Address can capitalize on the presence of informant discrepancies to increase understanding of the causes and consequences of, and treatments for child psychopathology; thereby addressing the second question posed previously (i.e., Are informant discrepancies useful?).

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Biography

Dr. Andres De Los Reyes is currently Assistant Professor of Psychology and Director of the Comprehensive Assessment and Intervention Program at the University of Maryland at College Park. He received his Ph.D. in 2008 from Yale University and completed his clinical internship at the University of Illinois at Chicago. His research program integrates clinical, social, developmental, and cognitive psychology areas to understand why different informants' reports of children's behavior yield discrepant research conclusions. The goals of his research are to understand how these informant discrepancies influence the science behind identifying effective treatments, and whether they can ultimately be used to understand how children's behavior varies across contexts, develops over time, and changes over the course of treatment. Dr. De Los Reyes' work has been published in such journal outlets as the *Psychological Review*, *Psychological Bulletin*, *Psychological Assessment*, and *Current Directions in Psychological Science*. He serves on the Editorial Boards of the *Journal of Consulting and Clinical Psychology*, *Journal of Clinical Child and Adolescent Psychology*, *Child and Youth Care Forum*, *Journal of Child and Family Studies*, and *International Journal of Clinical and Health Psychology*. He is currently preparing as Guest Editor for the *Journal of Clinical Child and Adolescent Psychology* a Special Section of articles on the topic of his Invited Address (informant

discrepancies in the clinical assessment of children and adolescents), with a target publication date of March 2011.



Unifying Individual and Context in a Biopsychological Theory of Development

*Arnold Sameroff, Ph.D.
University of Michigan*

The understanding of nature and nurture within developmental science has evolved with alternating ascendance of one or the other as primary explanations for individual differences in life course trajectories of success or failure. In my talk I will offer a dialectical perspective emphasizing the interconnectedness of individual and context to interpret the evolution of developmental science in similar terms to those necessary to explain the development of individual children.

Major advances in our understanding of the intellectual, emotional, and social behavior of children, adolescents, and adults have increasingly involved multilevel processes cutting across disciplinary boundaries in the social and natural sciences. This progress has forced conceptual reorientations as earlier unidirectional views that biological or social circumstance controlled individual behavior are becoming multidirectional perspectives where individual behavior reciprocally changes both biological and social circumstance. The models we use to understand how individuals change over time have increased in complexity from linear to interactive to transactive to multilevel dynamic systems.

I will propose a unified theory of development as a modern synthesis to integrate personal change, context, regulation, and representational models of development. Using examples from research on risk and resilience factors for successful cognitive, emotional, and social development, I will describe longitudinal connections between infant behavior and 30 year adult adaptations. The most general point is the ability of social advantage or

disadvantage to overpower individual characteristics of the child. In our analyses we have found that individual characteristics of mental health, and higher intelligence contribute to developmental competence. However the effects of such individual resiliencies do not overcome the effects of high environmental risk. We consistently found that groups of high resilient children in high risk environments had lower later mental health and cognitive competence than groups of low resilient children in low risk environments. Understanding these results necessitates an expansion of our theoretical models of developmental change.

Contemporary developmental science requires at least four models for understanding human growth: a personal change one, a contextual one, a regulation one, and a representational one. The personal change model is necessary for understanding the progression of competencies from infancy on. It requires unpacking the changing complexity of the individual as he or she moves from the sensorimotor functioning of infancy to increasingly intricate levels of cognition, from early attachments with a few caregivers to relationships with many peers, teachers, and others in the world beyond home and school, and from the early differentiation of self and other to the multifaceted personal and cultural identities of adolescence and adulthood. The contextual model is necessary to delineate the multiple sources of experience that augment or constrain individual development. The growing child is increasingly involved with a variety of social settings and institutions that have direct or indirect impact as exemplified in Bronfenbrenner's view of the social ecology. The regulation model adds a dynamic systems perspective to the relation between person and context. During early development, human regulation moves from the primarily biological to the psychological and social. What begins as the regulation of temperature, hunger, and arousal soon turns to regulation of attention, behavior, and social interactions. The last is the representational model where an individual's here and now experiences in the world is given a timeless existence in thought. These representations are the cognitive structures where experience is encoded at abstracted levels that provide an interpretive structure for new experiences, as well as a sense of self and other. Combining these four models offers a comprehensive view of the multiple parts, wholes, and their connecting processes that comprise human development. The unified theory of development provides an integrated way of looking at things, but also for things. Although we all have a strong desire for straightforward explanations of life, development is complicated and models for explaining

it need to be complicated enough to usefully inform our understanding.

Biosketch

Arnold J. Sameroff, Ph. D., a developmental psychologist, is currently Professor of Psychology at the University of Michigan. His influential theoretical work on ecological transactional models of development has helped to move researchers to more dynamic, system based research efforts for understanding healthy child development; and his research on environmental risk and promotive factors has fostered a more comprehensive understanding of what is necessary to improve the cognitive and social-emotional welfare of children. Among the high-risk groups he is currently studying are infants with physiologic regulatory problems, children with depressed parents, adolescents living in low-resource neighborhoods, and adults reared in families with parental mental illness. He has published numerous research articles and 12 books, most recently *The Transactional Model of Development: How Children and Contexts Shape Each Other* and with Sheryl Olson *Regulatory Processes in the Development of Behavior Problems: Biological, Behavioral, and Social-Ecological Interactions*. Among his honors are the Distinguished Scientific Contributions Award from the Society for Research in Child Development and the G. Stanley Hall Award from the American Psychological Association, Developmental Division. He is a former President of the Developmental Psychology Division of the American Psychological Association, the International Society for Infant Studies and the Society for Research in Child Development.

The Crisis in Statistical Education of Psychologists*



Bernard S. Gorman, Ph.D., Nassau Community College/SUNY and Hofstra University



Louis H. Primavera, Ph.D., Touro College, Graduate School of Psychology and School of Health Sciences

The educational requirements for all undergraduate and graduate psychology students include courses in statistics and psychometrics. Although there is increased interest in qualitative research, our major research journals require statistical analyses. However, we contend that most psychology students, most working psychologists and (unfortunately) many professors who teach statistical methods are not competent to take advantage of modern statistical theory and practices. We face a grave crisis in our training and practice models. As teachers and consumers of quantitative psychological methods we will use the medical definition of “crisis” as a turning point in disease: a point in the course of a disease when the patient suddenly begins to get worse or better”. Although we can talk about “the good, the bad, and the ugly,” we will take a more positive stance. We will start with the diagnosis and etiology of the illness our field faces. We will end with suggested treatments and prescriptions for a much healthier state of affairs by presenting out ideas on a reformed syllabus and some samples of teaching tactics.

Bernie Gorman received his Ph.D. (1971) in Personality and Social Psychology from the City University of New York, and completed postdoctoral studies in psychotherapy at the Institute for Rational Emotive Therapy. He has written numerous articles and presented many convention papers in the areas of personality assessment, multivariate analysis, and relationships between cognition and affect. He co-authored the textbook, *Developmental Psychology* (Van Nostrand, 1980) with Theron Alexander and Paul Roodin, and co-edited the research monograph, *The Personal Experience of Time* (Plenum, 1977) with Alden Wessman. He is the author of several instructional computer packages published by Random House and McGraw-Hill. His most recent volume, *Design and Analysis of Single Case Research*, with Ronald Franklin and David Allison, focuses on the intensive study of individuals over time. He is currently working with William and Beverly Feigelman and John Jordan on the book, *Devastating Losses: How Parents Deal with the Loss of Children through Suicide*, to be published in 2011 by Springer Publications.

Gorman is Professor of Psychology and State University of New York Faculty Exchange Scholar at Nassau Community College/SUNY, where he teaches courses in general psychology, abnormal psychology, and child and adult development. He holds an adjunct professorship in Hofstra University’s Graduate Psychology and Gerontology Programs, where he teaches courses in gerontology, multivariate statistical analysis, computer applications in psychology, and psychometrics. He received the State University of New York Chancellor’s Award for Excellence in College Teaching. For more than 15 years, he combined his interests in measurement research, clinical issues, and teaching as a psychologist in the New York State Office of Mental Health. He served as vice-president of the Metropolitan New York Chapter of the American Statistical Association from 1993-1998. He is a Senior Research Scientist in the Department of Psychiatry at Beth Israel Medical Center, New York, where he is part of a research team investigating the efficacy of psychotherapy. He served as a member of the National Science Foundation Research Coordination Network on DNA microarray technology, where he developed multivariate statistical analysis methods for studying gene expression.

Lou Primavera received his B.A. in Psychology from St. John’s University and his M.A. and Ph.D. in Experimental Psychology from the City

University of New York. He is a New York State Licensed Psychologist and has training in behavior therapy and rational emotive behavior therapy. He had a private practice for more than twenty-five years and specialized in marriage counseling. Dr. Primavera is currently the Dean of the Graduate School of Psychology and the Dean of the School of Health Sciences at Touro College. Previously he was the Dean of the Derner Institute of Advanced Psychological Studies at Adelphi University for six years and is professor emeritus at Adelphi. Before going to Adelphi, Dr. Primavera was at St. John's University for nineteen years. At St. John's he taught in the doctoral and masters programs in psychology. He also held the post of Department Chair for six years and was Associate Dean of the Graduate School of Arts and Sciences for four and one half years. Before going to St. John's, Dr. Primavera held full time faculty positions at Hofstra University, St. Francis College (where he was department chair), and Molloy College.

Dr. Primavera has published extensively and has interests in quantitative methods, drug use, and stigma and discrimination. He was a consultant to the Department of Psychiatry and Behavioral Sciences at Memorial Sloan-Kettering Cancer Center for ten years and has held a number of other consulting positions in medicine, business, and education.

Dr. Primavera is very dedicated to his teaching and won the excellence in teaching award at St. John's and a teaching award at Memorial Sloan-Kettering. He has been a member of a number of professional organizations and has presented at a number of annual conventions and conferences. Dr. Primavera was President of the Academic Division of the New York State Psychological Association, President of the New York City Metro Chapter of the American Statistical Association, and long time Board Member of the New York State Metro Chapter of ASA. Dr. Primavera is a Fellow of the Division of General Psychology of the American Psychological Association, a Fellow of the American Educational Research Association, and a Fellow of the Eastern Psychological Association.

** This session will be offered for CE Credits*

Award Winners

***Diane F. Halpern, PhD, Claremont
McKenna College***

Title: George A. Miller Award for
Outstanding Research Article in General
Psychology: The Science of Sex
Differences in Science and Mathematics

***Linda Bartoshuk, PhD, University of
Florida***

Title: Hilgard and Comparing Pain
Intensities Across Individuals: The
Ernest R. Hilgard Lifetime Achievement
Award

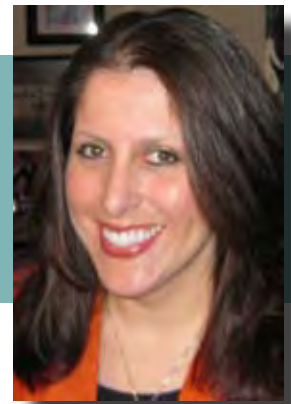
***Susan Pinker, MS, The Globe and
Mail, Montréal, QC, Canada***

Title: William James Book Award---The
Sexual Paradox: Men, Women, and the
Real Gender Gap



Give Evolutionary Psychology a Chance

by Dr. Marissa Harrison Penn State Harrisburg



Dr. Marissa A. Harrison is Assistant Professor of Psychology at Penn State Harrisburg where she teaches courses investigating the biology of behavior. Her research examines interpersonal attraction and human sexuality through an evolutionary lens. Her work has appeared in such scholarly journals as *Evolution and Human Behavior*, *Evolutionary Psychology*, the *Journal of Social, Evolutionary, and Cultural Psychology*.

Evolutionary Psychology, although arguably still in its toddler years with respect to the rest of the field, has been gaining ground in popularity, and the cumulative body of scientific knowledge produced by its thinkers is quite substantial. There are several well-respected, peer-reviewed journals devoted to its presentation and preservation, many academic programs at schools throughout the world are devoted to its study, and international societies of esteemed scholars strive and succeed in furthering its impact. Mark van Vugt of Kent University stated that “in less than 20 years (Evolutionary Psychology) has penetrated virtually every existing branch of psychology, including social, cognitive, developmental and clinical psychology,” and that it exerts influence in “various other social science disciplines such as anthropology, economics and political science.” Still, I have found that even among fellow psychologists, it remains largely misunderstood.

First, Evolutionary Psychology (EP) is not a sub-discipline of psychology per se. It is a very different approach to viewing all behavior and mental processes in an attempt to understand human nature, focusing on the impact of evolution on human behavior and cognitions. As Charles Darwin detailed, millions of years of evolution have led to anatomical adaptations in ours and other species, similarly, EP argues that millions of years of living have led to behavioral adaptations in humans. In a nutshell, all our behavioral adaptations—everything we do—is shaped by and has consequences for our reproductive success. In other words, the reproductive consequences of a response affect the incidence of that response in subsequent generations. Why do we get jealous? It is because human beings who got jealous left more descendants than those who did

not. Why do we speak? It is because human beings who spoke left more descendants. Why do we experience heartache? You guessed it—blame your cave-dwelling, very distant great grandparents. Your heart was broken by that red-haired girl in sixth grade because people who experienced heartache left more descendants. The behavior works, so it is repeated. Now think of this over thousands of generations.

Responses that conferred a reproductive advantage to our ancestors such as those just described—responses that for various reasons worked to increase the likelihood with which one will reproduce—are called adaptations, and we Evolutionary Psychologists strive to describe, predict, and test these reasons. Now here is where it gets (increasingly) tricky: adaptations happen by accident. Adaptations are the product of mutations. They are genetic “accidents” that worked for the better and led to the differential reproduction described above. The genes survived that code for a behavior that worked. Thus, we say said trait has been selected for. So, if all of our behavior has been shaped by accidents, obviously evolution does not work by design to improve the species. There is no deliberate component to evolution. In fact, as my graduate adviser Gordon Gallup of the University at Albany noted, evolution does not function to make us happy—happiness is a mere by-product of evolution. (Note that there are also maladaptive traits that confer reproductive disadvantages, and there are non-adaptive traits that just don’t do anything, but these are fodder for another day.)

Here is where it gets (even) trickier. Most other approaches to psychology seek to identify the proximate causes of a behavior. If one is studying physical attraction, one might focus on the attributes that make someone appealing, such as the shape of a potential partner’s body. You might think of this as the how aspect of human behavior. In contrast, EP examines the ultimate causes of a behavior, or the reproductive consequences—and these are largely unconscious. You might think of this as the why aspect of human behavior. That is, we are genetically preprogrammed to behave in specific ways conducive to our reproductive success. We want our genes to survive, yet we operate in this manner without even being aware that we are doing so. For example, with respect to bodily attractiveness, EP research has shown that attraction to sex-specific body

shapes (ideally an “hourglass figure” for women and a “V” shape for men) is likely mediated by our unconscious knowledge that each of these body shapes attests that the bearer thereof is of high genetic quality and is therefore a good mate (Singh, 1993; Hughes & Gallup, 2003). That is, the respective sexes prefer these body types because this behavior worked and was passed on through generations. Again, this mechanism operates at the unconscious level. Ask your friendly, local college student why she is attracted to the handsome athlete in her Research Methods class, and she might say that the target “has a hot body,” but I most sincerely doubt she would say it is because “his high shoulder-to-hip ratio is indicative of a highly-androgenized phenotype that facilitates status and access to resources, making him a very wise mate choice, and therefore I would like to perpetuate my genetic legacy and produce formidable offspring with him.”

Admittedly, it takes a bit to process and digest the above, and I have found that the issues others often vociferously highlight (to put it politely) with EP often stem from misinterpretation and misattributions of evolutionary theory. All seems copacetic when positivity such as love, bonding, and empathy are considered, but Pandora’s box is opened when torture, mass murder, thermonuclear war, female oppression, and social injustices are addressed. What do we Evolutionary Psychologists make of these horrors—are we saying that it is human nature to be destructive and malevolent? Often, others—even other psychologists—seemingly construe that we believe these specific, terrible behaviors are adaptive, and that we believe they are then somehow correct and acceptable. For starters, we find malice as appalling as anyone else does. To be clear, we do say that each of the behaviors mentioned likely do stem from ultimate responses rooted in human DNA; but be assured we are NOT saying that murder and slavery and the like are biological imperatives, and we are NOT condoning such acts. We are NOT saying that violence against a woman is a “correct” response in any way. Further, we are absolutely NOT eugenicists exclaiming that some people should and should not be allowed to reproduce (see Geher, 2005). Explaining a behavior through an evolutionary lens is by no means an attempt to promote or justify it (Gaulin & McBurney, 2001). As Cummins (2005) pointed out, our advanced human cognition—the modern human brain and mind—allows us to consider the consequences of our actions prior to the commission of any behavior. Although our minds operate in the mode largely shaped by many millennia living on the savannas of Africa, we recent *Homo sapiens* have evolved minds that allow us

to ponder social, moral, and political implications to our behaviors. Let’s use infanticide as an example—arguably the ugliest of crimes. A male’s extreme jealousy may lead to infanticide, and this jealousy may have resulted from a perceived or real threat of infidelity, and infidelity can lead to extra-pair conception which would threaten the male’s own genetic legacy (Daly & Wilson, 1988). However, infanticide is clearly an aberration and not a correct response to any conceivable problem. Anyone could choose NOT to commit such a heinous act and find a socially acceptable way to process negative emotions. As Duntley (2005) highlighted, the urge to act in response to a threat is adaptive, but something as horrible as murder likely stems from a toxic cocktail of personal history, dysfunction, and environmental issues—to be sure, murder is not an allowed, appropriate response in the eyes of an Evolutionary Psychologist.

As Confer and colleagues (2010) eloquently detailed, EP is, in essence, Darwin’s theory of natural selection applied to human behavior. Confer et al. provide a noteworthy account of the intricacies of the theory and give an excellent rebuttal to the major criticisms frequently faced in the field, such as EP’s ability to empirically test its principles, the perception that EP ignores socialization and culture, and how very modern human inventions and vices can be processed (or not) by our Pleistocene-era brains. They also make a concrete point about how facilitating correct interpretation of EP will advance the cumulative body of psychological knowledge—the goal of any scientist. I defer you to their article, co-authored by one of EP’s preeminent scholars, David Buss, for a far more in-depth discussion of the science of Evolutionary Psychology.

In sum, whereas Evolutionary Psychology itself continues to evolve as a rigorous, important science, it still seems the understanding of it is not quite keeping pace, and it is the hope of those of us in the field that our colleagues and students will develop a working knowledge of what it does and does not entail. We Psychologists train our students to critically consider all scientific evidence presented to them, and Evolutionary Psychologists challenge our colleagues to practice what they preach. Our endeavors can shed light upon and aid in the explanation and prediction of human behavior if you would just give EP a chance.

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Submission Deadlines for Fall 2010 TGP Articles

September 1, 2010

Coming in the Fall 2010 issue....

Convention Highlights and Pictures

More Graduate Student Connections

Award Addresses

Submission Deadlines for Spring 2011 TGP Articles

March 1, 2011

Send submissions to the Editor:

Gina Brelsford at gmy103@psu.edu

Note. Student and ECP articles are encouraged.

Please include a picture with your submission.

APA and Division One: Preparing for an “Interesting” Future



John D. Hogan,
President
Division 1
APA

In January, 2010, I attended a leadership conference in Washington hosted by APA. Most of those present were presidents-elect of the various APA divisions. The conference is an annual event and I have been there once before, but both gatherings have been a revelation. APA makes a strong effort to bring its division leadership the most up-to-date information on the organization, as well as on the state of psychology – indeed the larger culture. I want to share some of the things I learned at the conference.

APA itself has been through some difficult economic times. The recent financial downturn had an impact on APA, as it most certainly has had on all of us. In 2009, APA experienced a significant drop in its portfolio and a budget deficit of almost \$4 million. As a result, it cut back on personnel – a unique move for them. It also cut back on other optional activities, paring many to the bone. As a result, it projects a bottom line in the black for the coming year. The revenue from the new edition of the Publication Manual has been a major source of additional revenue although its impact is likely to be short-lived. But, in general, the recent economic issues now appear less threatening.

Membership in APA has remained stable, with a small gain. However, there are signs that there may be problems in the future, some embedded in the larger culture. For one thing, we learned that our newest graduates are different from their seniors in many ways. (Who knew?) It may be no surprise to learn of their deep involvement in the new technologies, but the quality and degree of their information sharing and social networking is extraordinary. In fact, the entire system of information delivery is changing so rapidly, only the young seem capable of keeping up with it. But that raises several questions. Will journals and books remain a major source of professional information for them? Will traditional organizations in psychology retain their importance for this new group?

The demographics of this younger group will eventually change the face of psychology. As you may already know, almost 70% of the new doctoral recipients in psychology are female and the degrees are dominated by the “helping” specialties in psychology. Those figures are almost a perfect reversal from pre-World War II psychology when most of the doctoral recipients were male with a specialty in experimental psychology. But the differences are not just ones of specialty and gender. Early career psychologists (ECPs) enter the profession with substantial financial obligations – their median debt at graduation is now between \$60,000 and \$70,000 – and they feel the financial pressure. The evidence is they are less likely to consider it normative to belong to professional organizations. Indeed, most (70%) belong to no divisions of APA. And they are at risk for dropping out of APA altogether during their first four years of membership. Their concerns tend to be practical ones – they want information on professional development, financial planning, and alternative career opportunities. Many of those who remain are looking for career resources and mentors.

These changing demographics don't represent an immediate threat to Division 1. Our total membership is still reasonably strong, given the standards of other divisions, and we publish an excellent journal and newsletter. Our awards are well-known and well-respected, and our annual convention program is typically top-notch. In addition, I think there will always be a place for general psychology. Paradoxically, some specialties require it. It would be difficult, for instance, to be a historian of psychology and not have the sensibility of a generalist. Likewise, undergraduate teachers of psychology must have a broad view of the discipline. But the division must be a home for many others as well if it is truly to represent “general psychology.” For example, thirty-six divisions of APA are now considered “practice” divisions. Has our division reached out enough to them?

I invite all division members to share your thoughts with me and the Executive Committee on the future of the division. What do you think we do best? What do we need to change? How can we prepare for the future? In particular, how can we address the new demographics? I promise that all of your messages will be gratefully received and read. We are eager to learn your ideas. Please drop me at note at: hoganjohn@aol.com. I look forward to hearing from you.

Meet the Division 1 Liaison to the Committee on International Relations in Psychology (CIRP)



Dr. Hagop
Pambookian

The new Division 1 liaison to the APA Committee on International Relations in Psychology (CIRP) is Hagop S. Pambookian, Ph.D., Professor Emeritus of Psychology at Shawnee State University, Portsmouth, Ohio.

Although, a longtime APA member (Fellow of Divisions 2 and 52, and member of Divs. 1 and 15) he may not be well-known to the Division of Society for General Psychology. However, he has been active professionally and involved in psychological associations nationally and worldwide. Besides his guest university lectureships in other countries, he has presented at conferences (e.g., at APA, SEPA, MPA, ICP, SIP, the European Congress of Psychology, etc.) in the U.S. and around the world. His two keynote addresses must be noted: (a) on "Psychology around the World: The Asian Experience" at the 2nd Asian Psychological Association's (APsyA) Convention, in 2008, in Kuala Lumpur, Malaysia; and (b) about the "International Dimensions of Psychology for a New Century" at the 56th Annual Convention of the International Council of Psychologists (ICP), 1998, in Melbourne, Australia.

Pambookian had, also, organized and presented Division 1 symposia on "Psychology around the World: Insights and Learning for U.S. Psychologists" at the 1992 Centennial Convention of the APA in Washington, DC and on "Binet-Simon Intelligence Scale – A Century of Contributions Worldwide" at the 113th Annual Convention of APA, in 2005, also in Washington, DC

Pambookian has been a Senior Fulbright Fellow, 1978-79, in the USSR and taught psychology at the Yerevan State University in Yerevan, the capital of the Republic of Armenia (then a Soviet Republic). He was the first U.S. scholar to receive a 9-month long Fulbright Fellowship for the Soviet Union, and was the first Fulbrighter in Armenia. Since then, and to acquaint Armenian and other nationality psychologists' of world (i.e., non-Soviet) psychological research and developments, he has donated more than 3,300 English-language psychology books and numerous volumes of psychology, primarily, APA journals to the Fundamental Library of the Armenian Academy of Sciences in Yerevan, Armenia.

He is quite familiar with the CIRP having served on it as elected member for three years (1995-1998). Later, for some time, he represented APA Division 15 as its CIRP liaison. And from 1989 to 1992, he was on the ICP Board of Directors –being also nominated candidate for election to the presidency.

For his many contributions to and promotion of psychology worldwide, Pambookian has been recognized by various scholarly organizations and academies, by being elected as, for example, Honorary Member of the International Academy of Psychological Sciences, in December 1994 (Yaroslavl, Russia); of the Armenian Philosophical Academy, in December 2002 (Yerevan, Armenia); and of the Academy of Pedagogical-Psychological Sciences, in November 2004 (Yerevan, Armenia). Earlier, in May 1997, the Ohio Education Association (OEA) honored him, for his contributions to international understanding and peace, with its "Paul Swaddling Award." Just in October 2009, The Ohio Psychological Association (OPA) honored him with the "Lifetime Achievement by a Psychologist Award" in Columbus, Ohio. And, earlier in May, Pambookian was among a handful of professionals, i.e., first-generation immigrants, honored for contributions to the State of Ohio by Governor Ted Strickland, Ph.D., Psychology (the Governor was Pambookian's colleague prior to Strickland's election to the U.S. Congress in early 1990s).

Following his secondary education at the Melkonian Educational Institute in Nicosia, Cyprus, Pambookian received his B.A., M.A., and Ph.D. degrees in psychology from the American University of Beirut (AUB), in Beirut, Lebanon; Columbia University Teachers College; and The University of Michigan (in Arbor, MI), respectively.



MILGRAM'S LEGACY, 50 YEARS LATER: A FORUM **

Harold Takooshian, Section Editor



On 24 March 2010, 70 students and faculty from as far as Moscow converged on room 204 of Fordham Law School in New York City, for an interdisciplinary forum on the unique legacy of Stanley Milgram (1933-1984), marking the 50th anniversary of the start of Milgram's Yale experiments on obedience to authority.

In 1960, this 27 year-old assistant professor launched what is arguably the most powerful study in the history of social psychology. Stanley went on to a meteoric 24-year career in behavioral research and teaching that crackled with creativity, and ended only with his untimely death in 1984. In this Fordham forum, a panel of experts and alumni gathered to exchange their insights on diverse aspects of Maestro Stanley Milgram.

This month of March, 2010, all of us were reminded of the remarkable truth about obedience that Stanley first documented 50 years ago, as "France is reeling from a documentary about a psychological experiment disguised as a game show" (Beardsley, 2010). Media world-wide were abuzz with a fake reality TV show in France, "Game of Death," where producer Christophe Nick found that 64 of 80 contestants agreed to electrocute an unwilling stranger in the next room, while the studio audience chanted "Shock him!" Sound familiar? Many of us thought the classic obedience experiments, viewed by millions of psychology students and others, may lower viewers' future willingness for destructive obedience. Alas, this 80% obedience in 2010 compares with the 63% Stanley found in the 1960s.

Stanley was best known globally as the larger-than-life researcher who single-handedly created the eye-opening "Milgram obedience experiments," yet those close to him knew him also to be a uniquely gifted teacher, student, colleague, family man, and friend.

John D. Hogan, Welcome. As the president

of the Society for General Psychology (APA Division 1), I'm pleased to extend a special welcome to everyone. It is a great honor to be invited to share in this impressive tribute to Stanley Milgram. His contributions have gone far beyond academia and into the popular culture. I suspect there are very few people who are not familiar with his work and who have not been touched by it in some form. I'm particularly pleased that we are able to offer a section of our Society bulletin, *The General Psychologist*, to document part of his legacy on this special anniversary.

Philip G. Zimbardo, classmate. (by video:)

Did you know that Stanley and I were classmates in James Monroe High School in the Bronx, class of 1949? Yes, long before Stanley and I launched our parallel careers in social psychology, he was the award-winning young scientist and an editor of our Monroe Yearbook, while I was elected vice president of our senior class. I still remember Stanley's words about me in our Yearbook: "Phil Zimbardo, our Vice President, tall and thin, with his blue eyes, all the girls will win." Thanks, Stanley. Of course our work in experimental social psychology intersected many, many times over the years before his untimely passing in 1984—while still in the midst of an immensely productive career despite his heart condition. Most recently I wrote the foreword to the 2009 paperback edition of my dear friend Stanley's classic, *Obedience to authority* (Milgram, 1974).

Jerome S. Bruner, professor. I knew Stanley at Harvard since the 1950s—first as my graduate student, teaching assistant, doctoral candidate, then as my colleague, close friend, and confidante. What diverse gifts he brought to psychology! As a Harvard student, Stanley did brilliant experimental research on the impact of culture on the individual, comparing Norway and France. At Harvard, Stanley stood squarely in the middle, between his mentor Gordon Allport's strong emphasis on the individual, and anthropologist Claude Levi-Strauss' equally strong emphasis on the culture. I invariably enjoyed our frequent and wonderful conversations during his Harvard years and afterwards.

Florence L. Denmark, colleague. I met Stanley in the 1960s before I began teaching at the

Graduate Center of the City University of New York. As program Chair of Social Psychology, Stanley welcomed me and gave me an office on the eleventh floor of the Graduate Center, where I agreed to help him with his administrative duties. Stanley encouraged students and colleagues, including me, when I launched the first U.S. course on the Psychology of Women. He was the person who nominated me for Fellow status in APA through Division 9, Society for the Psychological Study for Social Issues (SPSSI). When I was the Executive Officer for Psychology, overseeing 150 doctoral faculty and 450 students, I was able to reciprocate. Thus, Stanley was promoted to be the first Distinguished Professor of Psychology at our CUNY Graduate Center, in recognition of his extraordinary work.

Thomas Blass, biographer. As Stanley's biographer (Blass, 2004), I ask "What do we know now since the 1960s Milgram obedience experiments?" I can note a few points, documented at my website, www.stanleymilgram.com. First, Stanley powerfully identified one of the few universals of human behavior. Just this month of March 2010, all of us were awed when a French reality TV show found 80% obedience when it replicated the Milgram obedience experiment inside an entertainment setting. Second, Stanley's remarkably wide impact on the entire culture outside psychology was clear since 1965, when one of Stanley's first essays appeared in a literary publication--the Norton Reader. We see examples of how his obedience and other brilliant research impacted popular culture--film, dance, drama, music—and shows no signs of abating. This can soon be seen anew with a release later in 2010 of an expanded new third edition of Stanley's classic 1977 collection of essays, *The individual in a social world* (Milgram & Blass, 2010).

Alexandra Milgram, wife. Stanley was both influenced by and used his family throughout his life. A cousin gave young Stanley a chemistry set which taught him how to carefully do experiments. I met Stanley in 1961 and was able to view the obedience experiments from behind a one-way mirror. Throughout his career, Stanley was a devoted family man, as related in numerous anecdotes as well as can be seen in our family photos. During budget cuts, I worked beside Stanley at CUNY as his administrative assistant. During his Guggenheim Fellowship in Paris in 1972-73, our children Marc and Michele (6 and 8) stuffed and sealed envelopes to help obtain participants for their dad's research on cognitive maps

of Paris.

Alexander Voronov, outside the USA. (with translator Olga Ovanesyana:) Only in 1990, I learnt about Stanley Milgram's obedience experiments. It was the bolt from the blue. I do not know what amazed me more: his experiment's results, or its ingenious method. Since then I have a drive which has not left me for 20 years-- together with my dear colleagues and friends from USA and Russia, I promote the legacy of Stanley Milgram in Russia. For November of 2010, all Milgram scholars and alumni are invited to contact me (a_voronov@inbox.ru) to participate in an international conference on the legacy of Stanley Milgram – either by visiting Russia, or by internet through Skype.

Stuart Levine, inside the USA. In my 46 years as a dean and professor at Bard since 1964, I found that like all of us, our students are fascinated by Stanley's methods and findings. In 1970, Bard College began to offer seminars and workshops focused on the seminal work of Stanley Milgram. More recent variations by Art Poskocil document surprising levels of obedience inside the college classroom. Bard offers its Milgram seminar each year, which continues to capture the imagination of our students.

Michael Almereyda, film-maker. As I intend to make a feature film – a biopic – about Stanley Milgram's life and work, I was pleased when Mrs. Milgram recently named Pieter Bruegel as one of Stanley's favorite painters. (Almereyda projected a slide of Bruegel's "Landscape with the Fall of Icarus.") You can see the affinities: the ability to take in the big picture while focusing on little incidents and details; a fascination with "the Individual in a Social World," a knack for recognizing the extraordinary in the commonplace. My immodest hope is to match this approach and spirit. I am very grateful to Milgram's friends, colleagues and students who have already helped in my research, and I look forward to speaking to others.

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*** Notes: This forum was hosted by Fordham University (its Psi Chi chapter and Psychology program), in cooperation with SPSSI-New York and the APA Society for General Psychology. Photos of this forum can be viewed and downloaded at www.picasaweb.google.com/takoosh. Because of his untimely death at age 51, and his small doctoral classes, Stanley had relatively few students, yet no fewer than 15 alumni were among the 70 participants in this forum, some of whom shared their personal experiences: Drs. Pearl Beck (Vkeles Associates), Leonard Bickman (Vanderbilt), Rita S. Dytell (Mount St Vincent), Elyse Goldstein, Karen Franck, David Greene (Ramapo), Kathryn Hahner, Shelley Juran (Pratt), Suzanne Kessler (SUNY Purchase), Carla S. Lewis, Wendy McKenna (Barnard), Ann M. Rodier (Fordham), Linda Sapadin, Henry Solomon (Marymount Manhattan), Harold Takooshian (Fordham), Christina W. Taylor (Sacred Heart). Thanks to Rebecca Gruia of Fordham Law School for kindly arranging the forum room.*



In the photo above (left to right), forum participants and contact information:

Thomas Blass, University of Maryland-Baltimore County, blass@umbc.edu **Stuart Levine**, Bard College (Emeritus), levine@bard.edu; **Alexandra Milgram**, New York City, a.milgram@verizon.net; **Jerome S. Bruner**, New York University Law School, jerome.bruner@nyu.edu; **John D. Hogan**, St. John's University, hoganj@stjohns.edu; **Robert W. Rieber**, Fordham University, rieber@fordham.edu; **Florence L. Denmark**, Pace University, fdenmark@pace.edu; **Michael Almereyda**, filmmaker, Almereyda@aol.com; **Alexander Voronov**, State Academic University of Humanities-Russia, a_voronov@inbox.ru; **Eva Fogelman**, New York City; EvaFogelman@aol.com; and **Harold Takooshian**, Fordham University, takoosh@aol.com; (photographer); **Christina W. Taylor**, Sacred Heart University, taylorc@sacredheart.edu

Stanley Milgram, Family Man by Alexandra Milgram

Good evening. I decided to speak about both the influence on and use of the family to Stanley throughout his life.

As a youngster Stanley received a chemistry set from a cousin. Stanley enjoyed carrying out experiments both at home, and in the nearby Bronx Park if he knew they would be explosive. Although none of Stanley's experimental social psychology experiments that he did in later life ended in physical explosions, they were all carefully designed and carried out -- much as the chemistry experiments were.

Stanley's parents were very interested in the news during World War II. His father had family still living in Hungary. Stanley's obedience experiments were influenced by the Holocaust.

I met Stanley in January 1961. He had already begun the pilot study for the obedience experiments at Yale



University. Eventually I was able to view some participants from behind a one-way mirror. Many of the participants wanted to break off, or at least have the experimenter look in on the victim, even though they could not bring themselves to break off. I did observe a few subjects who refused to continue to the end.

When Stanley prepared his application for a grant to the National Science Foundation to receive funding for the obedience experiment, he finished it while visiting me in New York. The two of us ran up the steps of the main post office in New York just in time for Stanley's application to meet the deadline. Once while Stanley and I visited his mother, Stanley indicated how well the obedience experiment was progressing. When he proudly noted how many participants he was running a week, she asked him, "Is that all?" Instead of explaining what was involved in running the participants, Stanley returned to Yale and arranged for a second team to run the participants. Soon Stanley realized that this would not work, and he returned to just having one team running the experiment.

In 1968 Stanley engaged my nephew, who is a mathematician, to assist him in some mathematical problems on the crowd study.

Once when my mother visited Stanley and me, she asked Stanley, "Why don't young people today get up in a bus or subway to give their seat to a white haired lady?" When Stanley asked her if she ever made the request, she replied, "No." The next week Stanley returned to his experimental social psychology class and designed the Subway Study. They found that when there was no reason given for requesting the seat 56% of the people asked, got up and gave their seat. Fewer people relinquished their seat if there was a reason given. Stanley found it difficult to make the request.

Stanley had my sister prepare the index for the first edition of *The Individual in a Social World*.

When we lived in Paris during the academic year 1972-73, Stanley was on a Guggenheim Fellowship to make a psychological map of Paris. His French assistants were often going on vacation, so Stanley engaged both of his children to stuff and seal envelopes with requests for people to contact him if they were interested in participating in the study. At the time our children were six and eight. They still fondly remember the experience.

When it became difficult to obtain grants for research, I worked for Stanley a few mornings a week. Although Stanley was very intelligent and

independent in thinking up and doing experiments in social psychology, as well as employing students and assistants, he also at times resorted to different members of his family for assistance and inspiration.

Stanley Milgram, Student at Harvard *by Jerome S. Bruner*

Stanley Milgram and I had a long and forever evolving relation with each other. Over the years, he was my student, my teaching assistant, one of my doctoral candidates, and increasingly my close friend, colleague, and confidant. But never mind reminiscences. I want to talk about Stanley's gifts, what he brought to psychology - and to his students, his colleagues, and his friends.



Let me begin by celebrating one feature of his giftedness, one too easily overlooked: his uncanny sensitivity to the conflicted "ordinariness" of life - how we manage to maintain our private lives while responding sensitively to the interpersonal culture in which we are compelled to live. How, indeed, do we bring this off so seemingly effortlessly? The most renowned example of this sensitivity was, of course, his famous obedience research. Yes, you do what is asked of you by those in authority but manage, somehow, to remain oneself, to continue to be, as it were, "of one mind." But then, being of one mind is not that excluding. For it's also as important to be a Frenchman or a Norwegian as it is to be oneself. Let's not dwell too exclusively on Stanley's classic obedience experiments. Leaf through the pages of his collected papers (*The Individual in a Social World*, 1992) and a broader picture emerges. Take, for example, his comparative study of conformity in two cultures, France and Norway. He remarks that "a national culture can be said to exist only if men adhere, or conform, to common standards of behavior; this is the psychological mechanism underlying all cultural behavior." (Milgram, p. 160). But he then poses the troubled question as to how you can study such conformity behavior by "experimental techniques" (to use his phrase), which is how he, Stanley, wished to proceed? Interestingly, he tells us that he found his inspiration in Solomon Asch's classic

experiments on length judgment in which “a large fraction of subjects went along with the group rather than accept the unmistakable evidence of their own eyes” (p. 162). Asch’s subjects, you’ll recall, mostly judged easily discernible line length in conformity with the judgment of other subjects than with the testimony of their own senses. That set Stanley thinking. Maybe that’s how to get at conformity in a controllable setting.

So Stanley, you will recall, had his subjects judge the duration of tones they heard through headphones, but before a subject gave his own judgment, he had to listen through his headphones to the unanimous wrong judgments given by five other “simulated subjects.” Well, 62 percent of Norwegian and 50 percent of French subjects went along with the wrong judgments of those simulated subjects. And it was virtually impossible to wipe out these conformity effects by cautioning subjects against them. Yes, that would reduce errors somewhat, but not much - and not for long.

Interestingly, you’ll recall, the Norwegians were steadily more conformist than the French - which poked Stanley’s speculation still further. Here’s what he said: “I found Norwegian society highly cohesive. Norwegians have a deep feeling of group identification, and they are strongly attuned to the needs and interests of those around them. ... Compared with the Norwegians, the French show far less consensus in both social and political life. The Norwegians have made do with a single constitution, drafted in 1814, while the French have not been able to achieve political stability within the framework of four republics. ... There is a tradition of dissent and critical argument that seeps down to the local bistro.”

So how do a culture’s demands from “outside” find their way “inside” us, into our private experience? Here I must shift gears somewhat and say a word about the intellectual climate of those times. And let me do so by referring to the work of another friend of mine, a Frenchman and as dedicated an anthropologist as Stanley was a social psychologist - the brilliant anthropologist, Claude Levi-Strauss. He provides an instructive contrast. He, Levi-Strauss, passionately believed that culture had to be studied and delineated independently of the individual subjective lives of those who lived under its influence, that it was trans individual and was to be understood in those terms. It transcended individuality by achieving an institutionalized, historicized form: it was generic, not individual. I do not propose to resolve the dilemma posed by

these two viewpoints, Stanley’s and Claude’s. How indeed do we, in the human sciences, come to terms with the relation between individual subjective uniqueness and institutionalized culture? I bring it up now because I think it dominated Stanley’s thinking and, indeed, the thinking of his times. Not surprising, for it also was a principal preoccupation of Stanley’s principal adviser in his early years at Harvard: Gordon Allport.

Take, for example, the opening sentence in the “Preface and Acknowledgements” of Stanley’s collected papers (Milgram, p. vii). It reads, “The late Gordon W. Allport taught that social psychology examined how the thought, action, and feelings of the individual were affected by the implied, actual, or imagined presence of others. At the center of his definition was the individual; the individual remains at the center of my own conception of the field.” (Milgram, p. vii). Or take Stanley’s response to an interview question put to him by Carol Tavris (Milgram, p. 10): What makes a creative social psychologist? “You know,” he replied, “social life is a nexus of emotional attachments that constrain, guide, and support the individual. To understand why people behave as they do you have to be aware of the feelings aroused in everyday social situations.” Or later in the interview (p. 13), he remarks, “When authority goes awry, individuals do not seem have enough resources to put on the brakes.” In a word, the study of social psychology is a study of the balance between exterior social constraints and inner individual impulses. Without that balance there could be no bearable life. But note that Claude Levi-Strauss believed passionately that an understanding of life in society came not from examining the individual but from a close study of supra-individual patterns in the culture at large - the division of labor, the institutionalization of religion, the formation of communities. Stanley, on the other hand, was convinced that culture is best understood in terms of its subjective representation in the individual’s psyche, whatever it may be. Yet, yet, yet! Why then was Stanley so endlessly preoccupied, say, with what living in a city demanded - or why those remarks about the Norwegians and the French? I think he saw conformity as a mechanism for relating that underspecified thing called culture to that highly specifiable and easily measured thing that we call “individual human behavior.” But he knew it was not enough.

There is no question in my mind that Stanley appreciated the dilemma to which I refer. Let me close, then, with a quote from my

foreword to the 2004 reprint of his 1974 famous *Obedience to Authority*. “What always intrigued me ... was [Stanley’s] delight in rescuing the seemingly obvious from its seeming banality, his gift for making the familiar strange again. It is a poet’s gift, and when it shapes a scientist’s approach to his work, it produces wonders - and often shocks as well. This book is a tribute to that gift for making the familiar strange. Nobody in our time will ever again take obedience to authority for granted.” And we will all be the richer for Stanley having been among us. And for the freshness he brought to our understanding of the human condition.

Stanley Milgram, Renaissance man *by Thomas Blass*

As Milgram’s biographer (Blass, 2004), I am pleased to share this good news with those who admire the unique work of Stanley Milgram. After many years out of print, his classic volume will soon appear in its third edition-- *The Individual in a Social World: Essays and Experiments* (Milgram & Blass, 2010).



Milgram had a wide range of interests. He considered himself a neo-Renaissance man, and possessed a relentless curiosity about the hidden workings of our social world, which he tried to make visible through experiments and “think” pieces on a wide array of topics.

Yet, well into the 1970s, these other works were not well known, being overshadowed by the obedience experiments. He once told his brother, Joel, that he felt like the actor, James Arness, who was known only for his starring role in a TV series, *Gunsmoke*, but not for any of his other acting roles. In 1977, in order to correct the situation and inform readers about the diversity of his accomplishments, Milgram published the first edition of this anthology. In 1992, a posthumous second edition came out, edited by two of his former students, John Sabini and Maury Silver. It contained articles Milgram had written after the publication of the first edition.

This edition combines the articles that appeared in both of the earlier editions and adds ones that had

not appeared in either of them. As in the previous editions, readers will find an improbable potpourri of experiments and essays capturing the variety and breadth of Milgram’s interests. However, beneath their diversity, the readings have an important, unifying characteristic. There are articles that originally appeared in both professional journals and general circulation magazines. But, regardless of the intended audience, they are invariably lucid, readable and remarkably jargon-free. One of the most admired social psychologists, Roger Brown, considered Milgram to be “one of the best expository writers in psychology.” Given the quality of Milgram’s writing, I was surprised to hear from his widow, Alexandra, when I interviewed her for my biography, *The Man Who Shocked the World: The Life and Legacy of Stanley Milgram*, that writing did not come easily to him. The sweat is well hidden beneath the fluidity and effortless quality of most of his writings.

Over the years, I have assigned previous editions of this book to my students. Typically, they have told me that they not only gained new insights about social behavior, but also that the experience was a thoroughly enjoyable one. I hope – and believe – that readers will come away from this new edition with similar feelings.

Stanley Milgram, Mentor *by Christina J. Taylor*

I was the last student to complete a doctorate with Stanley Milgram. Very sadly for all whose lives he touched, Stanley passed away on the night of my doctoral defense, December 20, 1984. Stanley gave me a priceless gift by presiding over my defense at the CUNY Graduate Center on the last day of his life. At the time, I was an aging graduate student of 35 and Stanley was a young, 51 year old, Distinguished Professor. That was 25 years ago – today I am ten years older than Stanley was when he died.



Over the two and one half years that I worked on my dissertation research, I seldom saw Stanley in person

– corresponding through the mail and by phone with him and Professor Irwin Katz, my other advisor. We all knew Stanley was seriously ill, but of course I hoped for the best as I toiled on my research, and in the autumn of 1984 I mailed a complete draft of my thesis to Dr. Milgram at the Graduate Center. Within a short time, close to my birthday on October 12, Dr. Milgram delivered the best birthday present ever when he called to tell me he was mailing the thesis back with instructions for what he described as minor revisions, and that he was moving forward to schedule the defense. He himself chose the outside readers, so along with Professors Katz and Florence Denmark, he asked Professors Alden Wessman and Suzanne Ouellette to read the dissertation. I knew at the time that there was a profound sense of urgency attached to Stanley's scheduling of the December 20th date. Needless to say I was relieved that the end of seven years of doctoral study was in sight, but worried about the upcoming oral defense. Once the revisions were done, I mailed the final copies to Dr. Milgram and the readers, and with the anxiety of work on the dissertation done, I quit smoking – a personal goal I promised myself when I completed my doctorate. Over the next weeks of November and December, I dealt with my nicotine withdrawal along side preparations for the defense.

I boarded the train in Fairfield, Connecticut Friday morning December 20th to meet with Stanley prior to the committee meeting. I brought a camera because I realized that in all the years of working with Stanley I did not have a picture of him – an ironic fact given his appreciation for photography. I met him in his office with the camera and he immediately set about taking pictures prior to going to the dining commons to get tea and to catch up. Our conversation ranged over many topics, but I especially remember talking with him about death. I was most struck by Stanley's attitude of complete acceptance and equanimity regarding his illness and mortality. As long as I had known Stanley he had an exceptional ability to be fully present to people and to the immediate situation. More than most people, I think that Stanley had the ability to live in the moment, certainly an important skill for a social psychologist. And on what was to be the last day of his life, he was as resolute and keenly present with me as I had ever seen him be.

During our chat in the Graduate Center dining commons, Stanley said that he didn't feel well and that he thought he was experiencing heart symptoms. We left to return to his office, and despite not feeling well, he stopped to take photos on the way. When we got back to his office, I left to wait for him at the

defense classroom. I really believed that he would call for help and that the doctoral defense would be postponed. I explained that to Florence Denmark when she arrived for the meeting. In short order, however, Stanley appeared and we went forward with the defense. At the conclusion, and after the congratulations and more photographs, Stanley and I returned to his office. He told me he had taken medication before going in to chair the meeting. This act of dedication is a testament to the truly extraordinary teacher that he was. Before leaving him that afternoon, he very carefully showed me where to find the paper work for my degree on his office shelves – I knew, and he knew, that he might not be there to do the administrative follow-up.

The next morning one of my classmates called to tell me that Stanley had died. On Sunday I attended his funeral, where Sasha Milgram graciously told me that Stanley said that I had done very well in the dissertation defense. I think Stanley was very kind to say this because I felt that I had withered a bit under some of the criticism.

These are my recollections of the final day I spent with my mentor. There is research that shows people appear to extend their lives to reach special milestones or “ceremonial finish lines” (Shimizu & Pelham, 2008, p. 43). I believe that Stanley did this for me – helping me to the finish line. This was an extraordinary act of kindness – a social psychological intervention of the first order! Studying with him was an honor, and his personal qualities – warmth, sometimes tipping toward fire, humor, playfulness, and generosity – made it a rewarding adventure. I am deeply thankful to him for all that he did. He has been, and will always be, a source of inspiration to me both in my personal life and my career as a psychologist.

Stanley Milgram, Inside the USA *by Stuart Levine*

Fifty years after the launch of Stanley Milgram's obedience experiments in 1960, any U.S. social or even introductory psychology course is simply incomplete if it does not describe Milgram's obedience findings. This is more than “classic,” it



is “essential” reading in U.S. psychology. Here, I present how central Milgram’s work has become in the Bard College psychology curriculum, and why.

I came to Bard College in the fall of 1964 as an instructor of Psychology. My charge was to teach social psychology. I have remained at Bard for almost half a century and served as dean for 22 of those years. Bard has provided me the opportunity to teach small seminars on topics and people central to the discipline. I recall beginning with the area of social conformity and the work of Solomon Asch – who by the way was one of my earliest teachers at the New School for Social Research here in Manhattan. I taught my first Milgram Seminar in 1970 as the obedience studies had grown to a considerable number and as I began to find my favorites, those that either critiqued the Milgram paradigm, or that sought out other contexts where abundant amounts of obedience were observed.

My wish today in the time allotted to me is not to dwell on nostalgia for the work of the great man but to briefly describe a research effort advanced with some of my students at Bard over the years. This was but one indication of the legacy of which we come to speak. But on this occasion some bit of nostalgia cannot be entirely omitted. In the fall of 1977, while teaching an obedience seminar, I turned up several pieces of work, which captured the imagination of my students and me. One was a study of obedience in the college classroom by Arthur Poskocil, a sociology Ph.D from the University of Michigan. I wrote to Milgram at the City University and sent him reprints of both articles. In early November he wrote back stating how difficult it was keeping up with articles dealing with his original research and thanking me for my correspondence.

Hearing from him was a joy for me. This evening I thought I would provide a clip of this past work in the college classroom because it still intrigues my students and more importantly me. For me that is a good part of the legacy of Stanley Milgram. His work enlivens the content of teaching students social psychology today as it has over the past years.

One additional piece of personal nostalgia for me is to cite the remarkable labors of Thomas Blass to the legacy of which we speak this evening. Blass is very important for me and I now take the title of his book for my Milgram seminar – “The Man Who Shocked the World.”

The study I report I believe may be essential in an effort to understand one source of obedience in society. It was interestingly done and suggests the possibility for future research. Some of that work has been done by past students at Bard College.

Art Poskocil was a teacher in colleges located in the mid-western region of the United States. He taught in a number of schools, some big state universities with large classes and some very much the opposite of that with seminar-type classes.

In his article in *Teaching Sociology*, he reports that he was a student in several classes where the details and results of the Milgram study were discussed. During these discussions he felt that the Milgram findings were too often and too easily passed over as applying only to a laboratory exercise in New Haven. Poskocil believed it was important for his own students to confront the reality of the situation and be forced to deal with the fact that in our “non-totalitarian” society high levels of “destructive” obedience were possible, and indeed could be demonstrated. This even in a context--the college classroom--surely intended to motivate the opposite: independent thinking. Over the course of several semesters he decided to surprise his classes with a ridiculous quiz. In actuality the quiz was absurd. Items were of the following type: 5. Sociology is best described as: (a) the study of the art of sewing; (b) just another “ology;” (c) the study of man’s social behavior; (d) a communicable disease.

Poskocil stated that he was not sure what he expected but it was far from the complete cooperation that followed his handing out the quiz papers. Over the course of a number of years he gave this absurd quiz to as many as 800 students at 5 different colleges, in advanced and introductory level classes in psychology and sociology. He gave the quiz in class sizes ranging from 10 to 90 students. The results compiled from these instances were that over 95% of his students complied fully, and importantly, uncomplainingly when asked to take the quiz. This was even the case when at the outset of the semester he had told his classes that there would be no multiple choice tests and also intentionally had asked his classes to challenge him if he asked them to engage in something perceived by them to be educationally meaningless.

In the discussion that followed the exercises Poskocil ventured to ask the class not only to explain the observed extreme compliance but he asked them

what they might have expected were there to be a vocal protester in the group. It was very much agreed that such a protester would produce less obedience. Poskocil tested this out by having stooges in a subsequent class protest strenuously. Not much changed, and in at least one case others asked the lone objector to either take the quiz or leave.

This was the basic result of Poskocil's study. Being a clever sort and well versed in the Milgram paradigm he turned to "predictors" to compare with what he observed in his classes.

He asked groups of fellow faculty, other students and an "adult group" to state their expectancies concerning student behavior. The results of this survey were illuminating. 74% of the faculty asked said that the students would either question or not comply. Only 50% of students asked said the same thing. And about the same result was obtained with the 'adult' group. What are we to make of this investigation, and what did Poskocil himself have to say about it? By way of explanation he related a story of an observation of a grade school class and cited "the immense amount of control a school teacher can exercise over the minutiae of her pupils' behavior." In one of the more trivial incidents one of his graduate students reported that as she entered the fifth-grade classroom where she was to observe, the teacher asked, "Which of you nice, polite girls and boys would like to take the visitor's coat and hang it up?" Immediately a sea of waving hands appeared, as though everyone absolutely desired this honor. This teacher's choice of words and message is important. She could of course have simply asked someone to hang up the coat, but instead she used the situation as an opportunity to exhibit to her children and herself, and prove to the visitor, that pupils are docile creatures, eager to hurl their 'company' selves into this comedy of welcome.

This is an absurdity, trivial but absurd, but the children have little choice except to be bound into it; for a failure to respond to the teacher's request, as put, could only be construed as close to treason.

Perhaps in all of this we observe the forerunner to the more extreme piece of "absurdity" contained in the Milgram exercise. The learner makes an error in identifying the word pair blue-sky and the command is given to deliver a high voltage electric shock to a complaining victim or one that has long ago stopped answering. In the grade school class we see an instance of the beginning of compliance to the absurdities of what we are asked to do.

I had some very smart students over the years who

after reading Poskocil's study say why not comply? You get a quick 100% on a quiz. The event is absurd but so are many other things we engage in. There is nothing to lose. One might argue with such a conclusion and state that we have much to lose. We designed an alternative experiment where there is something to lose. A teacher distributes an absurd exam but his time it is difficult in the extreme. They are reading chapter 2 of their introductory textbook and the exam is based on some advanced work. What will happen then?

Complaints will be heard all around. 'This test is far from that which we are studying.' The teacher responds - this is the exam, "please take the test.'

What happens? My students and I engaged in such a study and we found obedience. Students simply guessed at the correct answers. By chance they would get 25% correct. But there was a flaw in the study. Not everyone may have read the chapter. Alternative - the teacher announces "on Wednesday we will have a quiz on the first 4 pages of chapter 6." Surely everyone will know the material. The test is given not on chapter 6, but on something else. What then will happen? I don't know but I will bet compliance. Thank you for the invitation to celebrate the 50-year mark, to salute the legacy of Professor Milgram.

Stanley Milgram, Outside the USA *by Alexander Voronov*

In the past half-century since 1960, Stanley Milgram's obedience research has become essential reading not only in the USA, but outside as well. Here I summarize some developments in my own and other universities in Russia, where social influence is naturally a topic of historical importance.



1. Teaching. Milgram's work has become infused in a wide swath of courses, even outside psychology-- general psychology, social psychology, management, sociology, methods. The film "Obedience" was translated into Russian in 1993. (b) Several students also conduct dissertations on obedience. For example, in 1997, I supervised three dissertations on obedience at my Russian State University for the Humanities, one by Armen Akopyan attempting a psychoanalytical approach to the problem. (c) Two upper-level seminars on

“Psychological mechanisms of destructive obedience to legitimate authorities: Stanley Milgram’s experimental paradigm” were implemented by me in two major universities: Moscow State University in 2006 and State Academic University of Humanities in 2007. (d) following the idea of Professor Stuart Levine I asked students in my obedience course in the fall of 2007 to prepare questions for U.S. experts on obedience who participated in our Moscow conference—Thomas Blass, Art Poskocil, Stuart Levine. For example, Olga Sidorova prepared this question:

“Dear sir Stuart Levine, I was present at your lecture in the Institute of Psychology and participated in its translation. After reading some of your works, I have formulated a question: You mentioned the change of social behavior during the sufficient period of time. Have you ever suggested that the destructive forms of obedience are the unknown and not-researched type of consequence of legal or authoritarian obedience? Are they related or not? By this, I mean that the intensive teaching of individual during her childhood how to behave in legal society with the distinctive legal authorities may lead to the transfer of the psychological pattern to the most absurd situation.”



2. Research in Russia focuses primarily on the role of personality traits in destructive obedience and resistance to authority in Russia. (a) In a field study of the military draft (an idea suggested to me by Thomas Blass), we compared two groups of people (ages 18-19) drafted in 1996—one more inclined to obedience to authority, and one less. Of 250 surveys, the “disobedient” group was 99 youth in the Moscow office of the Radical Antimilitary Association who refused to serve in the army. The obedient group was 151 draftees at the Moscow arsenal gathered for military departure. The main results: Both authoritarianism and cynicism (in Snow’s scale) were higher in obedient than in disobedient; the separate two-factorial analyses of cynicism for obedient and cynicism for disobedients were not the same. The result of this two-factorial analysis resembles a similar model that J. Snow came up with in 1995 for American students: the mindset of Russian youths trying to avoid army service using legal methods (as opposed to that of the draftees) is closer to the mindset of American students.

The following are the studies conducted in the 2008-2009: (a) Eugenia Enina’s “Investigation of the

phenomenon of obedience to authority in higher learning institutions” tested for a correlation between individual personalities and obedience to authorities in a higher education setting, using the classroom methodology of Poskocil (1977). (b) Tatyana Al’-Batal, “Repeating the field studies that were conducted on passengers riding the subway (Milgram & Sabini, 1978) in both Moscow and St. Petersburg, testing whether passengers will give their seat following an unusual request. Her final results will be announced at our Moscow conference. (c) Svetlana Panina, “Study of conformity of children in elementary school” (d) Alexander Voronov, in 2009 while replicating in class a simplified version of the Asch experiment (I always do this in the first class of my obedience course), I added to the influence of the group norm first the group pressure, then the pressure of the teacher’s authority. This allowed me to show to students the continuum of social influence already in the first lecture (gradual increase in the number of conforming students).

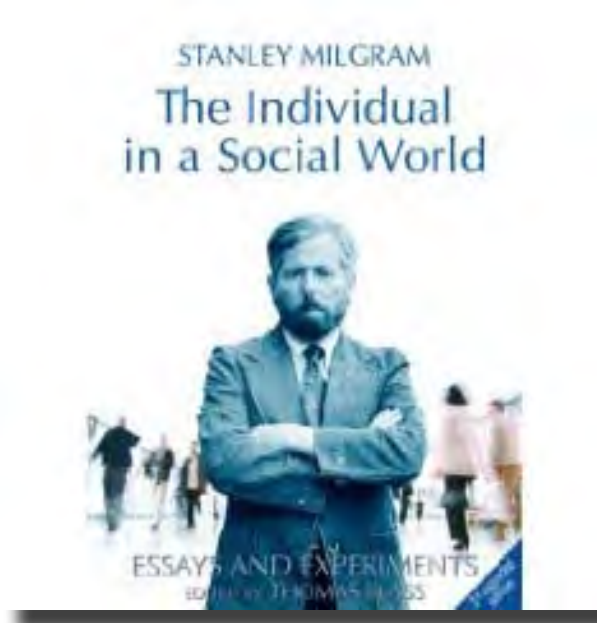
In addition, I should note: (a) The outstanding work of Dr. Regina Ershova to share Milgram’s ideas among the students of Kolomna State Pedagogical since 2006. Doctor Ershova was the advisor of the works above of Eugenia Enina and Svetlana Panina. (b) Two of my colleagues in Moscow have helped me in doing the statistical evaluations of the material that we gather: associate professor V.Y. Sinitzyn of the Russian State University for the Humanities and professor D.I. Piontkovsky of the Higher School of Economics. (c) We appreciate active cooperation from American professors like Miguel Centeno (Princeton), Thomas Blass (UMBC), Stuart Levine (Bard College), Harold Takooshian (Fordham).

3. Conferences. On December 20-21, 1994, my students in the Russian State University for the Humanities (Management Department) arranged an academic conference, “Stanley Milgram and his contribution to social psychology and the psychology of management,” to mark the tenth year of Milgram’s untimely death on December 20, 1984. (b) On 21-23 December, 2004, an International Conference was held at the Moscow State Pedagogical University (Psychology Department), with the active support of the Dean of this Psychology Department, Professor E.S. Romanova. It marked 30 years of the publication of Milgram’s 1974 book, *Obedience to Authority: An Experimental View*. The world’s leading expert on the study of Stanley Milgram, Thomas Blass delivered the keynote lecture, “The continuing legacy

Milgram Forum...

of Stanley Milgram's experiments on obedience to authority" and I delivered the plenary lecture, "The study and the development of the Stanley Milgram paradigm in the USSR and in Russia." (c) On 28 November, 2007, Stuart Levine of Bard College gave two lectures to the students of my higher level course "Psychological mechanisms of destructive obedience to legitimate authorities: Stanley Milgram's experimental paradigm" at State Academic University of Humanities (Russian Academy of Sciences, Moscow): "The Critiques of Stanley Milgram's Study of Obedience to Authority - Ethical Issues and Beyond," and "The Search for Evidence of Obedience in Contexts Outside of the Psychology Laboratory and Far Afield from Yale University." (d) On April 23, 2009 I was invited to read the report on Stanley Milgram's legacy in Russia at a psychological colloquium of the annual Andrew Jay Bernstein '68 Memorial Lectures (Bard College, New York).

4. Future. In November 2010, we invite all Milgram scholars to participate in an all-Russia conference on Experimental Psychology (either live or by Skype)-with the participation of the Institute of Psychology (Russian Academy of Sciences, Moscow) and psychology departments of three universities: Moscow State University, State Academic University of Humanities (Russian Academy of Sciences, Moscow), Moscow State Regional Social-Humanitarian Institute (city Kolomna), to discuss the scientific legacy of Stanley Milgram. Please contact me in advance at a_voronov@inbox.ru



Pursuing Patterns, Puzzles, and Paradoxes

By Linda S. Gottfredson
University of Delaware



A true scientist is bored by knowledge; it is the assault—on ignorance that motivates him—the mysteries that previous discoveries have revealed.

Ridley (2000, p. 271)

Although not bored by knowledge, I certainly do love mysteries. It has been my great fortune to earn a living solving them and, greater yet, to pick which puzzles to pursue. I still marvel at the opportunity.

Early in my career I discovered how scientifically important a knotty puzzle or seeming paradox can be. By paradox I mean two well-established findings that seem to contradict each other. Both are accurate because both have been replicated many times. But how can A and not-A simultaneously be true? They cannot, of course, which means that our thinking about them must be flawed in some fundamental respect. Therein lies the opportunity to discover something important.

One advantage of paradoxes is that there is clearly an interesting problem to solve. They also provide special scientific leverage precisely because they pose special challenges. First, they constrain our theorizing about potential explanations. Having a specific contradiction to explain not only provides a well-defined target, but hitting it also requires explanatory discipline. By definition, paradoxes close off familiar paths we would otherwise be predisposed to follow. Second, having to resolve seeming contradictions between two competing bodies of evidence forces one to break set, suspend judgment, and re-examine the deepest conceptual and analytic foundations on which the conflicting conclusions rest. One must look at old evidence with new eyes. Breaking set is perhaps the hardest thing to do. Finally, resolving seeming paradoxes requires throwing a wide net for new ideas and evidence, that is, for embracing the totality of evidence to the extent possible. Seeking relevant knowledge in unfamiliar disciplines increases the odds of serendipitous insights and allows novel empirical patterns to emerge.

Stalking puzzles is only one among other sorts of research I have done, including survey research, policy analysis, secondary analyses of large public datasets, and case studies of professional practice. While solving seeming

paradoxes has been more demanding and unpredictable than other research, it has also been the most exciting and rewarding. I describe several puzzles below that instigated long expeditions across various disciplines, which eventuated in explanations for phenomena of general interest. These examples illustrate useful research tactics I discovered in the process, as well as some strikingly counterintuitive lessons in conceptualizing and modeling human behavior.

Four Puzzles and Their Resolution

The following four puzzles are not all important in themselves, but pursuit of them led eventually to creating theories to account for some previously unexplained human phenomena. By creating a theory I mean mining existing research literatures to build a nomological network of empirical evidence around some phenomenon until a coherent causal pattern appears that can “explain” it.

1. Instigating puzzle: Why do children's vocational aspirations recreate the social class and gender inequalities of their parents' generation long before they ever confront any labor market realities or need to make choices?

Phenomenon explained (in vocational psychology):

1. How various internal and external factors in social and cognitive development successively lead children to circumscribe their perceptions of self and work, and hence their career aspirations, from birth through young adulthood (Gottfredson, 1981, 2002).
2. Instigating puzzle: Why does highest level of education attained best predict who gets good jobs, but intelligence level best predict who performs them well?

Phenomenon explained (in educational sociology): Why the occupational prestige hierarchy represents a hierarchy of increasingly complex and cognitively demanding work tasks, and how the division of labor evolved to distinguish jobs primarily according to overall cognitive complexity of work and only secondarily according to functional field of work (Gottfredson, 1985, 1986).

3. Instigating puzzle: Humans possess a highly general intelligence (*g*) that has practical value in virtually all life arenas, so how could specific aspects of our evolutionary

environment have ever selected for such a domain-general problem solver?

Phenomenon explained (in evolutionary psychology): How human innovation could have accelerated selection for higher intelligence in our species by disproportionately increasing the risk of accidental death among the least intelligent members of a group while improving the average survival rate overall (Gottfredson, 2007).

4. Instigating puzzle: Why are social class disparities in health and health behavior so pervasive, regular, and linear across such disparate different times, places, diseases, types of health system, and levels of wealth?

Phenomenon explained (in social epidemiology & health psychology): How inconspicuous cognitive errors in preventing and managing injury and illness cumulate over time, place, and maladies to increase rates of preventable morbidity and mortality disproportionately and linearly across social groups of successively lower average phenotypic (developed) intelligence, such as lower social classes, lower-level occupations, and lesser educated individuals (Gottfredson, 2004; this is the article that won the 2008 George Miller Award from Division 1).

Spotting Contradictions

Puzzles and contradictions abound. They are not necessarily self-evident, so must often be discovered or discerned. The first three puzzles listed above had not been recognized in their respective disciplines because one half of the contradiction lay isolated in a different discipline: respectively, in (1) sociology, (2) personnel selection psychology, and (3) individual differences psychology. The mystery of the social class-health gradient (4) has occupied health scientists for decades, but that field has for various reasons shut itself off from individual differences psychology and thus a potential resolution.

In the case of puzzle (1), both vocational psychologists and status attainment researchers in sociology were ignoring or dismissing the others' research because there was little if any overlap in their aims, assumptions, methods, and journals. Sociologists tended to be hostile to the notion that individuals differ in skills and interests or that they might voluntarily choose different life paths. Vocational psychologists had recognized the existence of social barriers but not devoted serious attention to them, perhaps because their subject pools usually consisted of college students. The class- and gender-based constriction of vocational preferences from the youngest ages therefore did not draw the attention of either discipline.

In the next two cases (2 and 3), the fact that one set of findings was clearly true was explicitly used by its

home discipline to deny that the other set (from another discipline) could possibly be true. I take them in turn. When I was a graduate student in sociology, major thinkers in that field were arguing that intelligence differences had little or no functional importance in real life. One supposed proof rested on the fallacious reasoning that because (a) educational level predicted occupational status level better than did IQ and (b) educational level did not predict on-the-job performance, then (c) intelligence could not possibly predict job performance. In short, one set of findings (from sociology) was used to neutralize the contrary set (from personnel selection psychology), leaving no puzzle for sociologists to explain. Their error had been to conflate what it takes to enter more vs. less prestigious occupations (the sociologists' concern) with what it takes to perform any one of them well once hired (personnel selection psychology's concern). The puzzle is how both relations could hold if employers are behaving rationally when hiring and evaluating workers.

Turning to the other example, puzzle (3), evolutionary psychologists generally argue that all evolutionary adaptations evolved to solve highly specific problems in a species' survival and reproduction. The human brain, they suggest, is like a Swiss Army knife, which has specific blades for specific uses but none of which have value for many uses. Because adaptive problems are specific, the presumption is that the mind must have evolved separate modules to solve them. It cannot be a general problem solver or learning machine, if only because a combinatorial explosion of possible responses would result from the unconstrained processing of information. However, a century of research in individual differences psychology has documented that there is, in fact, a highly general intelligence that confers practical advantages in many areas of life. General intelligence exists, so the puzzle does too. The evolutionary psychologists had erred by rebutting a straw-man general processor, namely, the behaviorist conception of associative learning. The real challenge thus remains—how could a highly general intelligence have evolved?

Contradiction (4), which is the inexplicable generality and linearity of social class-health gradients, shared many features with (1) above. Like sociologists, social epidemiologists tend to favor social-barriers explanations of social inequality. Both have been reluctant to consider the possibility that group differences in occupational and health outcomes, respectively, might arise to any degree from, respectively, average gender and class differences in interests, abilities, and life goals that affect occupational choice and class differences in average intelligence as they affect health self-care. Average group differences in these psychological traits had been replicated many

times, but social epidemiologists have generally presumed them invalid or inconsequential.

I say this not to criticize the disciplines, past or present, but simply to point out a fact of life. All scientific disciplines work from guiding assumptions, as they must. Their assumptions often differ or conflict, yet remain implicit or taken-for-granted. For example, both vocational psychology (my first adopted field) and status attainment research in sociology (my field of graduate study) seek to understand why different kinds of people end up different kinds of work. But that is where the similarity ends. One arrays occupations horizontally (interest or personality type), and the other vertically (status level). Where one focuses on how individuals differ in interests and abilities, the other treats individuals as psychologically fungible and thus focuses on the external forces blocking their ascent up the social ladder. The first speaks of personal choice, the second of social barriers.

Exploring Across Disciplines

My experience is that solid, replicable contradictions are more likely to come into view when juxtaposing contrasting bodies of thought and evidence. The contradictions will not be obvious to the disciplinary tourist, but will materialize only after immersion in some part of the foreign discipline or sub-discipline.

Disciplines literally speak different languages, where the same word can mean different things—usually that discipline's favorite part of the metaphorical elephant. So, the word occupation may stand for location in a social hierarchy (sociology), personality type of incumbents (vocational psychology), earnings trajectory (economics), configuration of duties performed (industrial psychology), and so on. All this makes for confusing and fraught cross-disciplinary communication. Doing interdisciplinary research is like moving to a foreign land with a different history, language, and culture. It takes time, exposure, and effort—immersion—to finally “catch on.” Only by translating vocational psychology and status attainment into the other could I even begin parsing the first puzzle above. And so it has been with all the others.

One needs to read and think enough in the foreign literature to develop an intuition for its patterns of results, which requires understanding the strengths and limitations of its data. These patterns of results may not be the ones that its own practitioners emphasize, because they come to the data with different aims and assumptions. To understand the empirical evidence, I focus on data and methods. I mostly ignore the authors' own conclusions except as they challenge my own or help me understand how the field reasons.

My initial forays into other disciplines are generally wide-

ranging and exploratory, the aim being to acquaint myself with a field's terminology, assumptions, methods, sources of error and doubt, and patterns of results. If already working on a specific puzzle, I look for how the field might contribute to or contradict the nomological network I am accruing around my phenomenon of interest. As a practical matter, this means locating a few top-notch articles or pertinent summaries and then looking at the articles they cite and then, using the Web of Science database, the articles that subsequently cite them. I follow good leads until they either converge or cross the boundaries of relevance often enough to know I have reached the point of diminishing returns—and usually cognitive overload as well.

The fact that some piece of information somehow seems relevant does not necessarily make it useful. One still has to see why it is relevant and how it contributes to resolving the apparent contradiction. A familiar old fact can stare you in the face for months or years before you realize how crucial it is. Here is an example relating the case (2) above.

I spent several years scouring various literatures to find datasets to test the sociological claim that employers hire more intelligent applicants for higher-level jobs, not because they make better workers, but because intelligence is a socially-constructed pretext for justifying the privileges of the privileged classes. A companion claim was that the occupational hierarchy serves no functional purpose but is merely a power hierarchy by which some classes can dominate others. One major theorist was claiming that virtually anyone could do virtually any job if they received the proper training. Much of this was empirical nonsense, but what struck me was that the field took the hierarchy itself for granted. And, except for their hierarchical ordering, the occupations arrayed along it were, for that field, just black boxes that could be filled with anything or nothing. All that really mattered was attached to their surface—prestige, income, and educational requirements.

Now, vocational psychologists had spent many decades grouping occupations according to work activities and developing elaborate procedures for helping clients identify which occupations best matched their interests and capabilities. Military and civilian employers had spent decades analyzing the component duties and aptitude demands of jobs for purposes of worker selection, classification, training, performance evaluation, and setting pay scales. It was these fields' datasets I was merging and analyzing to determine which abilities best predict performance in hundreds of occupations up and down the occupational hierarchy.

My instigating puzzle had forced me to think about why

employers would hire workers by a qualification that did not actually predict on-the-job performance. After reading some of the microeconomics literature on signaling, I realized that education is a cheap and fallible but reasonably valid way for employers to sort applicants into different occupational bins by average intelligence level. Higher intelligence enhances on-the-job performance to some extent in all jobs (as personnel psychologists were showing), but especially in higher level work because, as I was showing, that work actually is more complex and cognitively demanding.

In proving that the occupational hierarchy is not just a socially constructed excuse for social inequality I myself had taken for granted a crucial phenomenon. Occupations are not discrete boxes of fixed sets of tasks. Rather, they are fuzzy, evolving constellations of tasks that often change depending on the skills and abilities of the individuals performing them. In fact, I had earlier viewed this as a problem in my research; just a nuisance and source of error. How could I classify occupations by aptitude demands if the occupations themselves kept shifting in content? But here, all along, was the mechanism by which the occupational hierarchy could have evolved over human history.

By combining the now-obvious protean nature of occupations with well known facts about human intelligence, I could also explain the surprising finding in sociology that the occupational prestige hierarchy is fundamentally the same worldwide. First, cognitive diversity is a biological fact in all human populations. Moreover, our many distinct abilities tend to line up along a single general dominating dimension, general intelligence or *g*, which forms the common core of all mental abilities. Second, when work tasks gradually proliferated in kind and number as human groups grew in size and complexity, work tasks would have become segregated on a recurring basis into separate occupations.

Third, the division of labor most likely to survive and reproduce itself over generations would be one in which its various sectors have reliable inflows of workers who have the requisite combinations of aptitudes for learning and carrying out that work. A major recurring biological constraint on such flows would be the genetically-influenced structure of human differences in ability. As noted earlier, those differences are organized primarily according to general intelligence level and only secondarily according to profile differences in verbal vs. spatial, and so on, meaning the division of labor would have to be too. And so it is (Gottfredson, 1986).

Writing to Impose Explanatory Rigor

Broad exploration is fun, interesting, and relatively easy.

It can generate lots of ideas. The hard work comes in winnowing, organizing, and testing them to develop a plausible explanation. Good explanations are tight data-based arguments, not compendia of facts or fanciful speculation. In my experience, only writing provides the necessary intellectual discipline to develop a rigorous, compelling argument. I therefore start writing when I glimpse an answer to the paradox. I do not stop exploring, but focus it more tightly.

To be clear, all four publications began as manuscripts that had been either accepted or invited for publication. At some point I had to start writing and eventually produce a publishable manuscript. In no case, however, did I know exactly what I would say. I knew only that I had a puzzle I wanted to work through.

In the first case, (1), I had temporarily withdrawn a little 8-page paper from publication in order to add a missing paragraph. My struggle to get it right ended up 6 months later as a monograph. My short conceptual piece argued that vocational choice proceeds by children rejecting the least acceptable options for someone like themselves, not by identifying the best. Even before adolescents start consciously thinking about vocational options, they have already ruled out whole swaths of the occupational world that might actually fit their abilities, interests, and life goals, but which they are not likely to revisit unless prompted to do so. My subsequent effort to explain how this process unfolds and provide evidence for it forced me to look back into my notes, rummage again through my accumulating piles of books and articles, and venture out into new corners of the literature in order to answer more specific questions and test new hypotheses. "What does the literature say about preschoolers' conceptions of gender differences?" "Do the bases of rejection shift as children become cognitively able to recognize more abstract dimensions of our shared social order (gender, class, ability, personality, and so on)?" "At what age do children's perceptions of the social order mirror those of adults?" I spent months alternating between writing and returning to the vocational, developmental, and sociological literatures for specific sorts of information.

In case (3), I started writing when I had the insight that the relatively sudden evolution of *Homo sapiens sapiens'* general intelligence could have been driven by the proliferation of evolutionarily novel, low-probability causes of death created as side-effects of human innovation. Human groups organize to protect themselves against the most obvious threats to survival—war, starvation, weather, and predation—not against ones they think random or "accidental." No specific modules could have evolved to protect against each of the many low-probability causes of death that proliferated with technological advance—such

as clothes catching fire, drowning while fishing off piers or boats, accidentally nicking oneself with a poisoned dart, or being gored by domesticated animals. These are the same evolutionarily novel hazards that still cause so much unintentional injury and death today: piercing, cuts, falls, crushing, burns, scalds, poisons, dog bites, and so on.

Unintentional injury is the major cause of death from toddlerhood through early middle age in both developed and developing countries. This is what had led me to look at the industrial accident literature when I was working on the puzzle of the social class-health gradient, (4). The accident literature described what now seemed obvious in hindsight—hazards are ubiquitous, the key question is not what causes accidents but what prevents them, and prevention is a quintessentially cognitive process. The accident prevention process could explain how the hazards created by human innovation might put less intelligent members of a group at slightly higher risk of death during their reproductive years. Evolution works with such tiny differences in risk.

I consulted anthropological accounts of hunter-gatherer groups to test my deadly-innovations hypothesis. I found that accidental death was indeed a major cause of death in pre-literate societies, but the evidence also showed that the deadly innovations I had in mind were far too recent to account for the evolution of high human intelligence. For instance, pre-contact Ache in the Twentieth Century used fire, had digging and cutting implements, and hunted with bows and arrows, but few of them were fatally injured by those implements (Hill & Hurtado, 1996). Far more had died from stepping on poisonous snakes. In any case, bows and arrows are a relatively recent human innovation.

But something had to have selected for we human's highly general ability to learn and reason, so I began searching for evidence and speculation about the earliest Homo sapiens and their environments. What about more basic hunting, gathering, and cooking technologies? A passage in *The Promethean Fire* (Lumsden & Wilson, 1983) made me realize, with a jolt, that I had falsely equated human innovation with physical technology. Innovation is more basic than that. It is simply having a mind's eye: that is, being able to imagine something beyond what we are seeing, feeling, tasting or otherwise experiencing in the present moment; to imagine times, events, objects, beings, and circumstances that do not exist now and may never. **To become tool makers and technologists we first had to become imaginers, to see beyond the concrete here and now. Recognizing hazards as potential threats to well-being is an act of imagination.**

With that insight, I re-examined the hunter-gatherer reports. Death by stepping on snakes occurred while hunters were

looking into the forest canopy for monkeys, their attention diverted upward by being able to kill them at a distance owing to their bow and arrow technology. Even primitive projectiles would pose the same hazard. Innovations that divert attention, create physical forces that exceed normal human tolerances, or expose individuals to more hazards or less familiar ones increase the need for prevention, and hence effective reasoning and "foresight."

The foregoing experiences illustrate how the writing process provides discipline and focus, and is thus integral to conducting this kind of research. Writing is part of the expedition, not a report on it, because it helps expose **flaws in logic, gaps in the evidence, and missing links in the chain or argument**: "What do I really mean here? This doesn't follow or feel quite right. How can I account for X? Might there be data on that?"

Writing can also impose blinders, however. Tight focus in the last thing one needs if heading down the wrong path. The sense of encountering a writing barrier usually signaled my having bumped up against some conceptual problem. Thinking is really hard work, as I tell my students, but just doggedly pushing ahead can get you deeper into a dead-end corner, especially if you are fatigued. Strategic time-outs may help one return with fresh eyes. Everyone finds their own style, but I have discovered that it helps to let my mind wander on a particularly vexing problem when I am relaxed or carrying out tasks not requiring much cognitive effort. Ideas can play and rearrange themselves, letting crucial similarities, distinctions, or bits of information pop into the foreground.

Reconceptualizing Models and Methods

These sorts of cross-disciplinary research expeditions could be called theoretical syntheses, inductive pattern-analysis, or perhaps meta-research. My expeditions are usually unplanned and unanticipated, their conduct messy, the evidentiary guide-posts necessarily error-ridden, the end-point a surprise, and the publication lengthy. But if successful, the resulting explanation will seem transparent and the evidence for it obvious in hindsight. It is a research method without set procedures. It does not rely on statistical tests or mathematical modeling, yet has provided unanticipated lessons about both.

I like statistics and mathematical modeling, and read about them for pleasure when I can. But when mechanically applied as accepted methodology, they are apt to obscure rather than illuminate the structure of evidence. Their mechanical application short-circuits hard thinking about what the measured variables mean conceptually, why those constructs would or would not be causally related, how they are distributed in the population, and whether **statistical significance tells us anything useful about them.**

This became clear to me in graduate school. The recent methodological advances that established status attainment research as *avant garde* in stratification research—regression analysis and path analysis—seemed likely to ensconce rather than expose consensual misconceptions. The new statistical modeling was useful for probing large datasets, but the precision of its parameter estimates conferred only the illusion of validity. Its users were rechristening conceptual questions as statistical problems, such as multicollinearity and the omitted variable problem. I remain skeptical of all statistical and mathematical modeling procedures that pre-process evidence while leaving little trace of its original structure. Hence my preference for starting out close to the data by scrutinizing frequency distributions, means, standard deviations, how variables are actually measured, degree of measurement error, zero-order correlations, and sample composition.

For me, the two most deceptive words in social science are controlled for, as in “We controlled for X to see what influence Y has on Z,” because there is usually little empirical basis for presuming that particular causal model—and often good reason to doubt it. For example, social scientists routinely rule out empirically plausible explanations for disparities in success and well-being by controlling away valid variance as mere “confounding.” This reflex to “statistically control for” has long stalled progress in pinning down causal relations in human behavior. The question, then, is how to make progress despite the inferential ambiguities of non-experimental data obtained from samples that are not genetically informative either.

As already described, it is useful to triangulate many sorts of evidence. Another is to make non-obvious predictions that other proposed explanations would not. I have been able to do that to some extent by calling upon two heuristics that allow me to apply one discipline’s body of evidence to another discipline’s research problems.

In the case of puzzle (4), I analyze chronic diseases such as diabetes, and health self-care in general, as having a job. Chronic diseases mirror paid jobs in requiring training, having regular duties to perform, being cognitively complex, and requiring self-direction and independent decision making. Quick consultation with the employee selection literature reveals that these task demands put a premium on proficient learning and reasoning, and hence on higher intelligence. The job analysis literature then illuminates which particular aspects of these jobs are more complex and thereby create special risks—or cognitive barriers—in health self-care. Comparing chronic diseases to paid work also shows that the former are more demanding in crucial ways, for instance, in

allowing no evenings or weekends off, and no retirement. Moreover, the heuristic makes immediate sense to health care practitioners and patients, and implications for patient education, supervision, and job simplification follow directly from it.

The second heuristic was supplied by a colleague, fellow sociologist Robert Gordon, in an article on “life as a test” (1997). It has had a most profound effect on my thinking, both conceptual and statistical. He describes the ways in which routine daily activities do and do not resemble items on psychometric tests of intelligence, including how standardized they are and the degree to which they call upon *g*, or general intelligence. The most important point for me was that single IQ test items never measure intelligence very well. Indeed, they are individually quite poor measures of it. Their power comes with aggregation, whereby their common variance cumulates and their errors cancel each other out. Applying the Spearman-Brown Prophecy Formula for test reliability, we can calculate how many items are required to produce a test that measures virtually nothing but *g*, depending on how *g*-loaded its items are. If items are only weakly *g*-loaded, we just need more of them.

This life-as-a-test heuristic was central in resolving puzzles (3) and (4). In the case of the evolution of human intelligence, (3), each hazard can be conceived as a weak test item. The risk of accidental death would be miniscule for each. Moreover, each would be only lightly *g*-loaded, the greater part of the variance in injury and death being random (“accidental”). Yet, when cumulating tiny differences in *g*-related risk over myriad hazards, many individuals, and many generations, the odds will aggregate against less intelligent members of the species. No selection factor operating over evolutionary time would be noticeable within a generation, which means, counter-intuitively, that any process obvious to the naked eye would be implausible as a selection factor.

Turning to the puzzle of the social class-health gradient, (4), we see the same principle at work in the daily prevention and management of injury and illness. Maintaining good health requires consistent effort on a daily basis to prevent illness and injury and to limit the damage they cause. Preventive efforts are likely to be less frequent, less consistent, and less effective among individuals who cannot conceive the risks of failing to expend effort that has no obvious payoff. Psychometric principles also tell us which kinds of outcome criteria will best capture the consequences of poor or inconsistent performance. Specifically, they will include outcomes that cumulate over more items (time, events, behaviors), are more reliably measured, depend more heavily on patients exercising their own independent judgment, and instances where

patients differ more widely in intelligence. That is, the life-as-a-test heuristic suggests novel predictions about when and where disparities in health and health behavior will be greatest.

Both heuristics encourage scrutiny of the distribution of the external task demands that people face in daily affairs. Psychology has many instruments for ascertaining distributions of abilities, but few for the distribution demands that social and physical environments make of us. Both heuristics also caused me to question one of our most basic methodological assumptions, namely, that bigger effects (larger correlations, standardized mean differences, etc.) are necessarily better, more informative, and more important than small ones, and, conversely, that inconspicuous effects are obviously unimportant. Evolutionary logic and the Spearman Brown Formula suggest, however, that consistency of effect may outweigh size of immediate effect when reliably small effects have a chance to cumulate and compound over long periods of time and large populations. This means that effect sizes cannot be directly compared when measures represent different degrees of aggregation, as would be the case for grade-point average vs. single achievement tests, or an A1c laboratory result vs. a one-shot measure of blood sugar.

Both heuristics now have me pondering how to represent and analyze the topography of effect sizes across different health tasks and groups of individuals. Their pattern could help explain the mystery of the class-health gradient. Such analyses would require gathering the same parameters, such as regression slopes, for different combinations of predictors and outcomes in the same study and from different studies as well.

If my intelligence-based explanation of the class-health gradient is correct, we should find systematic, predictable differences in gradients. For strictly statistical reasons, gradients should be steeper (slopes larger) when predictors and criteria are more reliably measured, there is less sampling error, and there is less restriction in range in the sample. For substantive reasons, I would expect gradients to be steeper when health tasks are more complex, predictors correlate more highly with patient intelligence, there is greater need or opportunity for patients to exercise independent judgment, and the health behavior or outcome measure is more cumulative in nature. Here is a case where meta-research may intersect meta-analysis.

In summary, there is no recipe for doing good science. I have tried to describe some strategies, however, that I have found useful. Foremost among them are to read widely on phenomena that intrigue me, pin down why perspectives on them differ and what each has to offer,

try to locate the essential core of available evidence, not put much stock in any single study or piece of evidence, watch for recurring patterns in the most reliable data, think through likely counterarguments, and look hard for holes in my evidence and logic. Most important, be open to having your basic presumptions shattered. As with good mystery books, the endings are often not what you expect.

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Retro Book Review:

Human Relations in Interracial Housing

by Wade Pickren, Ph.D.



Human Relations in Interracial Housing

Daniel M. Wilner, Rosabelle Price Walkley, &

Stuart W. Cook

Minneapolis: University of Minnesota Press

The 15 years after the end of World War II (WWII) in New York City were active years for social psychology research engaged with real-life applications. There were numerous psychologists in the city who had been educated in an activist ethos at places like City College of New York, including Leon Festinger, Morton Deutsch, Gardner Murphy and others. As historian of psychology Fran Cherry has documented, there was still a place for a non-reductive approach, with the research done in the community rather than a laboratory. The commitment was to science, but to science in the service of social problems rather than strict methodological purity. This was then and is now a delicate balance to achieve and to hold (Cherry & Borshuk, 1998). For example, two such activist research programs were community self-survey of race relations (developed at Fisk University) and the incident control project, which was designed to teach people “how to stop the bigot” that is, when there was a public display of racist remarks and behavior.

Stuart Cook was the director of the agency who led these efforts. In 1949, Cook moved to New York University to establish the Research Center for Human Relations and in 1950 became the department head for psychology. One important issue that Cook and his colleagues focused on was interracial housing. Two large research projects on housing were conducted by the center. The two books that reported the results of these studies were *Interracial Housing* by Morton Deutsch and Mary Evans Collins (1951) and the present volume. The larger context for these studies was the acute housing shortage in the immediate postwar era and the concomitant Housing Act of 1949, passed by the U. S. Congress with the ambitious goal of constructing large, planned communities for low-income families, many of whom were African American. Cook and his colleagues saw this as an opportunity to do research that would inform policy while also shedding light on how to reduce racial discrimination and improve intergroup relations.

Cook attracted a bright and promising group of psychologists to the faculty and the Center, in-

cluding Milton Schwebel, Marie Jahoda, and Morton Deutsch.

Isidor Chein joined them in 1953.

Jahoda, Deutsch, and Cook were each involved, separately, in studies on interracial housing. Their work proved to be directly relevant to a new social psychology theory, the contact hypothesis, first named by Gordon Allport in 1954, based, in part, on interracial housing research. Briefly, the contact hypothesis is that intergroup [where groups are different on some important dimension such as skin color or ethnicity or social class] contact under certain conditions can reduce prejudice and produce more positive intergroup attitudes.

The context for the housing research was the large in-migration of African Americans to the northern United States, as well as the continued immigration to the United States. With these migrations, the amount of intergroup contact had risen dramatically. The housing shortage and the population growth of New York City made such contact nearly unavoidable. One area of great interest was housing. Because of the Depression, housing stock had not kept up with the rise in population, and in NYC and nearby cities, there was a rise in the number of housing developments where there was mixed housing, Black and White residents. The mixed housing was of several kinds. Some developments or apartment buildings were fully integrated. Some were area segregated, that is, whites lived in certain buildings, blacks in others. This created a natural laboratory for the kind of action research then stimulating so many young social psychologists. It raised questions such as, What was the impact of these new housing patterns? What were the implications for the contact hypothesis? Was prejudice reduced?

The émigré psychologist, Marie Jahoda, conducted one of the first studies of interracial housing in collaboration with colleagues from Columbia (Jahoda & Salter-West, 1951). The housing project was low-income and the Black-White ratio was 50-50. In terms of the contact hypothesis, tenants of both races reported a higher number of friendships with members of the other race and whites had more favorable attitudes toward integrated housing.

Morton Deutsch and Mary Evans Collins, a research associate at the Center, were funded by the Marshall Field Foundation to start research on interracial housing comparing bi-racially segregated housing in Newark and integrated housing in New York

City, a total of four low income housing projects. The African American population was at least 40 per cent in each of the projects. Deutsch and Collins suggested that the data indicated that it was the experience of living in close contact with people of other races that had led to the attitude change. Most significantly, the results of the study led the Newark housing authority to change their policies to encourage integrated housing.

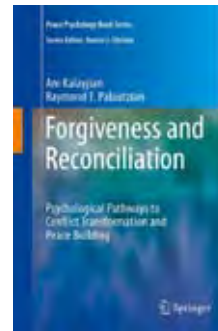
Wilner, Walkley, and Cook: “Human Relations in Interracial Housing”

As a follow up to the Deutsch and Collins study, the Research Center of Human Relations undertook a larger project on interracial housing outside NYC. In the four projects studied all were building segregated, but the buildings were interspersed, so that while a building may be all white, it would be next to an all black residence. What Daniel Wilner, Rosemary Walkley and Stuart Cook found was support for intergroup contact as a means of reducing racial tension and prejudice.

The attitudes of the members of one racial group toward the members of another will tend to become more favorable if there is sufficient contact between the two groups, provided that a) the contacts occur between individuals who do not differ markedly in their social status in the contact situation, b) and the contacts do not occur under circumstances in which there is competition for limited goods or facilities.

As in the Deutsch and Collins study, it was those whites who lived closest to blacks who reported the most favorable attitudes. Wilner et al provided a fair amount of circumstantial evidence that attitudes had changed as a result of close proximity, it was not just that those who already had favorable attitudes were the ones who chose to live near African Americans.

These were remarkable studies for their times. In some ways they represent some of the last community-based social psychology research of that era. It was research that was intended to make a direct impact on policy makers and the general public. By the end of the 1950s, there had been a retrenchment from this kind of engaged research, in favor of laboratory-based, experimental research where variables could be controlled. While such studies may have made social psychology more “scientific” by the canons of the day, one wonders if something valuable was not lost by the abandonment of research intended to make a difference.



Kalayjian, A. & Paloutzian, R.F. (Eds.): *Forgiveness and Reconciliation: Psychological Pathways to Conflict Transformation and Peace Building*. New York: Springer, 2009.

Anger, hatred, resentment, grudges – when the products of conflict smolder for years, decades, or centuries, the idea of peace may seem elusive and unrealistic. At the same time, people and societies need to move beyond these negative traumatic effects so they can heal. *Forgiveness and Reconciliation* explores in depth two different yet essential components of this peace-building process.

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o Reconciliation efforts in Rwanda, Darfur, India, and Pakistan
o Restorative conferencing and its role in fostering forgiveness

o Lessons in empathy and repentance from lifers in prison and promoting reconciliation through arts and the media

o The potential for forgiveness despite revisionism, denial, and continued injustice and reconciliation in the divided society

o Dialogue processes as a key to forgiveness and reconciliation

Forgiveness and Reconciliation breaks new ground as a volume that will enhance the work of social and peace psychologists, students and researchers in intergroup and international relations, and peace and conflict studies. **New electronic download version available; paperback available in May of 2010.**

An Innovative Integrative Approach to Self-help and Counseling

By Peggy Brady-Amoon, Ph.D. Seton Hall University



Stop beliefs that stop your life:

Fixed beliefs and life pattern theory.

by Rivka Bertisch Meir, Ph.D., M.P.H.,
L.M.H.C., AuthorHouse, 2009. 151 pages (paper)
ISBN: 978-1-4490-3889-2 (sc).

Written both for the general public and mental health professionals, with separate sections for each, *Stop beliefs that stop your life: Fixed beliefs and life pattern theory* offers an easy-to-read compendium of practical recommendations and guidelines for understanding and changing counterproductive beliefs.

Based on her innovative integration of applied theory and extensive practice, the author makes a compelling case that making modest changes in the way one thinks leads to enhanced physical, emotional, and psychological health and improved relationships. She further contends that the approach outlined in this book supports group interaction including international diplomacy and offers an elegantly simple way for reaching these worthy goals.

This book stands apart from the many trade books written for the general public in its focus on self-help and in its foundation in established psychological theory and research. Part I, written for the general public as well as professionals, is composed of seven chapters that offer an overview of the author's conceptualization of fixed beliefs and her approach to processing, that is, the self-help and client-centered method for modifying counterproductive beliefs. Her standardized bibliotherapeutic approach supports individual self-help, peer-supported self-help, and counselor-supported self-help. In fact, Dr. Bertisch Meir contends that self-help is at the core of human transformation.

The four chapters in Part II, geared for professionals and others interested in a more in-depth explication of fixed beliefs and life pattern theory and the process, provide advanced theoretical and practice formulations.

Part III, which concludes this fine little book, contains resources for further understanding and practice including lists of common fixed beliefs by personality types as well as additional exercises for supplemental and more advanced process work.

Directly and indirectly, fixed beliefs and life pattern theory builds on a wide range of established psychological theory as well religious/spiritual thought. Dr. Bertisch Meir specifically credits a number of approaches including the cognitive approaches, particularly Beck's and Ellis' concepts of maladaptive and automatic thoughts; the psychodynamic formulation that fixed self-beliefs originate in early childhood; and Adlerian concepts of social relatedness. Dr. Bertisch Meir's theory also appears to incorporate aspects of Jungian personality dimensions and developmental, family systems, and humanistic principles. The latter is evident in Dr. Bertisch Meir's belief in clients' ability to transform themselves, her approach to counseling, and the counselor's role in the process. Lastly, Dr. Bertisch Meir's approach is consistent with themes frequently found in a range of religious/spiritual traditions, namely the importance of personal humility and respect for others.

Contrary to the implicit assumption inherent in self-belief theory and practice that higher is generally better, Dr. Bertisch Meir is unequivocal that beliefs that we are "better than, more than, or superior" (p. 1) to others – in any one or more otherwise desired areas, such as intelligence, responsibility, and sensitivity, lead to personal and relational problems as well as disease. She makes a strong case that fixed beliefs that we are better than others leads to a false sense of superiority that separates us from those we judge as having less or being lower than us in that particular area. We disappoint ourselves, she claims, when we fail to live up to our own inflated sense of self and we are disappointed by others when they fail to acknowledge and celebrate what we consider, perhaps tenuously, to be our specialness. Dr. Bertisch Meir is not opposed to realistic assessment of our attributes, abilities, and skills. She does, however, caution us that fixed beliefs that we are better or have more of a particular

quality than others is what limits us – and others.

Rather than criticizing other people for having what you consider to be less of a particular attribute, Dr. Bertisch Meir takes the approach, found in many religious/spiritual traditions and psychological theories, that the only person we can change is ourselves; that by respecting differences and avoiding judgment and criticism, we open the door for our own growth, enhanced relationships, and a cascade of other benefits.

In addition to her theory, and perhaps the most important part of this book are Dr. Bertisch Meir's practical, how to, step-wise guidelines for people seeking to change fixed counterproductive beliefs and therefore their self-concepts, functioning, and relationships with others. She calls this "integrative method" the "process" (p. 4), claiming that the process is a way to understand oneself better, access inner resources for personal transformation, and establish healthy interdependent relationships with others.

That method is deceptively simple, and is designed to be used in self-counseling, with a peer, in group and/or with or by a professional – with the caveat that simply reading about change is not the same as working a program. She writes that "The Process is technique that uses a series of structured and specific questions, presented in a precise sequence, in order to produce a very clear picture of the negative consequences of a behavior or a belief" (p. 19). In addition to the basic process Dr. Bertisch Meir offers advanced techniques for people who are "stuck" and for groups, both based on the same basic principles.

Within this context, the role of the counselor, self, peer, or professional, is that of a supportive, non-judgmental, peer. The counselor's role is to "direct" the process (p. 23) by asking a series of specific questions in the order presented in the text, listen carefully, acknowledge and write down the client's response, thereby supporting the client's insight and growth. Supplemental exercises for understanding oneself and getting "unstuck" are also included.

Stop beliefs that stop your life: Fixed beliefs and life pattern theory makes an important contribution to the self-help and counseling literature. It is theoretically based and simple to implement. I recommend that the next edition – or perhaps Dr. Bertisch Meir's next book – include additional information about research support for her theory and method. Nonetheless, I highly recommend this book as a bibliotherapy resource and as a supplement to counseling for clients and counselors alike.

Peggy Brady-Amoon, Ph.D., is an Assistant Professor in the Department of Professional Psychology and Family Therapy at Seton Hall University. As a counselor and counseling psychologist, her current work focuses on the role of self-beliefs in adjustment and academic achievement and on professional identity and career development.



Tattered Umbrella's: Douglas Fryer and the Handbook of Applied Psychology

by Kelli Vaughn-Blount York University



Kelli Vaughn-Blount is a doctoral student in the History and Theory of Psychology program at York University in Toronto. Her primary research areas include the history of women in psychological science, feminist psychology, and the teaching of psychology. Kelli is the current News, Notes, and Sources Editor for the History of Psychology journal and publisher of the web based Journal of Scientific Psychology (psyencelab.com). She is also the current Past-President of the APS Student Caucus (APSSC).

In addition, Kelli is the current program coordinator for Alexandra Rutherford's online archive project on the history of feminism and women in psychology, Feministvoices.com, and technical producer of the 2009 film The Changing Face of Feminist Psychology.

In 1936, Dorothea Lange took several pictures of a migrant worker and her children on the side of the road. A single photo of the woman became known as the "Migrant Mother" and came to symbolize the Great Depression across the United States (Gavin, 2000). Lange was at the right place, and time, to capture an image that reflected the story of a poverty-stricken country - but that's not why she took the picture. She could never have predicted that moment would be the perfect reflection of the devastation of the depression era. Like Lange, Douglas H. Fryer could not possibly have known that the editorial assignment he received in 1937, a year after Lange took her photo, would offer a picture of applied psychologists' tattered attempt at unification, as well as that of the divisions of psychology itself.

During the late 1930's, Douglas Fryer was a prominent industrial psychologist at New York University (NYU) (Koppes, 2006). Fryer, who had received his training under G. Stanley Hall and James Baird at Clark University in 1924, had accomplished a great deal in his relatively short career (Katzell & Austin, 1992). He was hired by NYU within a year of graduating and

almost immediately founded the undergraduate psychology department. Two years later, Fryer would also be instrumental in the development of its graduate program (NYU Today, 2008). By 1936, Fryer would be chosen to develop and head a committee to explore the development of the new organization for applied psychologists (Benjamin, 1997).

The applied psychologists of the period, including consulting, educational, industrial, and clinical psychologists, were growing weary of the scientific focus of the American Psychological Association (APA) (see Evans, Sexton, & Cadwallader, 1992). They were also weary of the general disdain of their academic colleagues toward applied work. Douglas Fryer recognized these tensions and fully supported the need for a separate venue for applied psychologists. Thus, one year after forming the exploratory committee, he accepted the presidency of the resulting national organization aptly titled the American Association for Applied Psychologists (AAAP) (Benjamin, 1997; Koppes, Thayer, & Vinchur, 2007). As with any new organization, the AAAP's first order of business was to establish an internal structure and organizational goals. One of these goals was the creation of a handbook that would show the psychological community what areas were now embraced under the umbrella of applied/professional psychology (Fryer & Henry, 1950). Douglas H. Fryer would again accept his profession's call to duty but this time as the primary editor of the soon to be titled *Handbook of Applied Psychology* (Koppes, Thayer, & Vinchur, 2007).

Murphy's Law and the Making of a Handbook

The infamous Murphy's Law says "if anything can go wrong, it will" (Bloch, 2003, p. 2). During the 12 years between the handbook's conception and its publication, in 1950, Fryer may have felt as if Murphy's words had become more of a prison sentence than a law. That said, the preparation of the handbook initially appeared to be an easy task. The participation of the membership, as authors,

was guaranteed up front and the co-editor would be his NYU junior colleague, Edwin R. Henry. All that was left was to choose the individual authors and determine the layout of the work (Fryer & Henry, 1950; Katzell & Austin, 1992).

That first year, however another battle was brewing. Although the official beginning of WWII was declared in 1939, the early rumblings of war actually began in 1937. According to Fryer (1950) "Much of the writing and editing of the handbook was done during the early years of the late war" (p. v). Fryer (1950) goes on to explain the delay in publication was for "various and obvious reasons" (p. v). Both terms are true in that for a second time during the collection of the materials for the handbook a victory would clash with a brewing storm.

Just as the global battles were winding down, a new one was heating up back home but this time within the American Psychological Association. The APA had not taken the loss of the AAAP membership lightly, and was concerned with a trend of fractionalization within the discipline. The members of APA began to develop a restructuring plan that would allow a place for both the traditional psychological scientist and emerging professional psychologist to have a voice (Evans, Sexton, & Cadwallader, 1992).

The APA restructuring plan bore at least superficial resemblance to suburban development plans of the period. The design incorporated a central commercial structure (city, organization) that would act as the connecting point for multiple (suburban, professional) areas, which could be tailored to fit the inhabitants. All they needed to do was to sell the city dwellers (experimental psychologists) and the outlying communities (applied psychologists) on the new structure. In the suburban development plan, this was done with the promise that building connecting highways between the cities and the 'burbs would in no way change their landscape or require giving up local control; in fact, the opposite would be true in that they would have more say within the larger commercial city. As if foreshadowing the future story of suburbia, the APA's sale of a similar promise worked with the members of the seven-year-old AAAP. In 1944, the AAAP collective agreed to become number thirteen, of the newly formed nineteen, subdivisions of the reorganized APA. The merger became final in 1945 (Benjamin, 1997; Evans, Sexton, Cadwallader, 1992; Katzell

& Austin, 1992; Koppes, Thayer, & Vinchur, 2007)

The purpose of Lawton's *Handbook of Applied Psychology* was now to reflect a new unified umbrella of applied psychology that no longer existed. It had taken only seven years, a strong storm of war, and a reorganization to tear the umbrella apart. Fryer (1950) admitted as much in the handbook's introduction when he mentions that entries had to be included, modified, and excluded as "several contributors had developed new interests" (p. v). By the time the handbook was published, several contributing authors had retired, or moved on to other areas, including a few who had not actually graduated when their initial writing was completed. In fact, several contributors had actually died since their works had been written (including Lillian Martin, Joseph Jastrow, and Carl Seashore, among others). The handbook, and its editor, had endured the war, but not without casualties.

Finally by 1950, the handbook and Douglas Fryer's sentence were complete. In the end, the 115 chapters of the handbook contained entries by 116 applied psychologists, a list of landmarks in professional psychology, and a 2200-entry bibliography. The author's list reads like a historical who's who of professional psychology including names like Paul Aschilles, David Shakow, Lillian Gilbreth, George Lawton, Jack Dunlap, Lillian Martin, Joseph Eaton, George Bennett, and many others. The editors also attempted to include an outline for early career professional psychologists with sections on administration of services and training in the area. The twelve years between conception and publication appeared to be worth the wait.

If one more thing goes wrong: The Aftermath

If the book's reviews are any indication, a biography of the *Handbook of Applied Psychology* would be titled 'How not to write a handbook in 10 easy lessons.' Considering the enormity, and historical significance, of the undertaking Fryer might have logically assumed that the amount and depth of the reviews would be comparable. In actuality, the reviews - both in quantity and quality- were just as underwhelming as an actual reading of the handbook itself. Only six reviews could be located (McKinney, 1950; Broek, 1951; Gage, 1951; Thorndike, 1951; Long, 1952). Five of the six reviewers agreed on four points: (1) that

their personal subfield was under represented; (2) there was too much of everything and not enough of anything; (3) no one was exactly sure what constituted a “handbook” but were certain this was not; and finally (4) that while the author list was impressive, the articles in the two-volume handbook were of no use to either professionals or students.

The Journal of Applied Psychology (1951) would offer the only supportive review of the handbook. Unfortunately the reviewer, Albert Thompson, was so in favor of the handbook that it is impossible not to wonder about possible biases. Thompson provided an enthusiastic description of the text as a “monumental undertaking... massive & imposing.. Serving as a monument to psychology as a profession as well as a science” (p. 367) (although even Thompson managed to include some criticisms, noting that reading the volumes in their entirety was like “looking down on an ant-hill, watching the ants going busily about their varied tasks, some going one way, some seeming to accomplish a lot, and some seemingly running around in circles” p. 367).

The fate of Fryer’s *Handbook of Applied Psychology* was disappointing. A quick scan of APA’s current databases shows no citations for the handbook, excluding those of these reviews. GOOGLE Scholar offers a few more, but still minimal, 56 citations of the text almost exclusively in the area of industrial psychology. This is not surprising given Fryer’s history in the area, his involvement with AAAP and the establishment of Division 14 (representing industrial psychology), and later as Chairman of Section 1 of the American Association for the Advancement of Science (Katzell, & Austin, 1992). Douglas Fryer’s reputation would ultimately go unmarred by failure of the handbook. Fryer would later contribute to writing the history of the early years of industrial psychology. He proposed and organized an event on the pioneers of the field at the 1958 APA convention (Koppes, Thayer & Vinchur, 2007), and proposed the same year that a history of Division 14 be prepared (Koppes, 2006).

Parting Thoughts

Douglas Fryer did not intend his *Handbook of Applied Psychology* to provide a historically valuable snapshot of the formation of the formal fracturing of the unification of applied psychology, or

informal fracturing within the discipline itself. I, however, would argue that it has done just that. The problems with the conception, design, and reception of the handbook seem to reflect the strains and complexities incurred in the unprecedented development of sub-disciplines within psychology during WWII. Perhaps reviewer Albert Thompson (1951) describes it best, stating that:

“The Handbook even illustrates the difficulties psychology faces in getting its research findings put to practical application; On the other hand, one does find reflected in the articles many of the basic problems affecting psychology as a profession. There is disagreement, or at least confusion, as to the function of the psychologist as a therapist [original author’s italic]. The increasing need for specialization as knowledge expands is counterbalanced by attempts to maintain a broad, common foundation for all psychologists.” (p. 370)

Fryer himself was freed from the difficult editorial task of constructing the handbook in the early 1950’s; unfortunately, the residuals of the events that caused his difficulties still plague the discipline today. Today we still wonder if the multi-layered subfields of psychology can truly be one discipline and doubt that any one organization can ever really represent so many varied interests. There still seems to be no umbrella under which we can find shelter from the storm. Edgar Doll (1946) stated, “the whole of psychology is more important than any of its divisions but has a barren future independent of them” (p. 188, as cited in Evans, Sexton, & Cadwallader, 1992). Perhaps the same could be said for the history of applied psychology and the picture provided by Douglas Fryer’s *Handbook of Applied Psychology*.

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What They're Reading . . .

Edited by Vivian McCann

Portland Community College



Culture, critical thinking, and excellence in teaching: all topics of great interest to most of us. It seems to me that more and more books aimed at these topics are showing up on the shelves of bookstores these days – whether those shelves be real or virtual. Which ones are worth a read? Two esteemed thinkers in the field of psychological science offer their thoughts to help you make your spring and summer reading selections.



Stephen L. Chew has been a professor and chair of psychology at Samford University in Birmingham, Alabama since 1993. He received his Ph.D. in experimental psychology from the University of Minnesota. His research interests include the cognitive basis of effective teaching. He was awarded the Buchanan Award for Classroom Teaching Excellence from Samford in 1999 and in 2005 he received the Robert S. Daniel Teaching Excellence Award from the Society for the Teaching of Psychology. He has been a keynote speaker and workshop leader at numerous conferences on teaching in general and on the teaching of psychology in particular. In his spare time he is a mediocre handbell player in his church handbell choir. "The books I've been reading recently align with two of my major interests: how we can utilize psychological knowledge to improve teaching, and why well meaning people make critical errors," says Stephen.

The Atomic Chef: And Other True Tales of Design, Technology, and Human Error

by Steven Casey

The Atomic Chef is a collection of 20 stories of how failure occurred when humans interacted with poorly designed technology or procedures, often with tragic consequences. The stories come from a wide range of areas and time periods: an overlooked piece of tape brings down an aircraft; procedural errors allow a terrorist to board an airplane with explosives in his shoe; a safety policy implemented in the wake of the Titanic tragedy leads to the capsizing of another ship with even greater loss of life. Casey presents each story in a clear, accessible form, documenting how a series of design flaws and human errors cascade to potential disaster. He presents no psychological analysis or commentary, but each story contains essential lessons about the importance of good design based on psychological principles, the danger of unintended consequences, and the volatility of complex systems. This is Casey's second collection of stories. These stories are excellent for teaching human factors or for demonstrating how psychology is relevant to a wide range of problems.

The Logic of Failure: Recognizing and Avoiding Error in Complex Situations

by Dietrich Dörner

This is a fascinating book about trying to control and manage complex systems such as an economy or a government. Dörner created computer simulated countries, and let ordinary adults without any special training or experience in management have dictatorial powers to set policies and manage each country. Many well-meaning participants failed miserably because they failed to appreciate the complexity of the system, failed to adjust to changing circumstances, and failed to identify critical problems. Simplistic solutions led to unexpected problems that usually ended in catastrophe. In one study, a participant tried to improve the welfare of a nomadic tribe by introducing improved medical care to reduce mortality and the use of artificial fertilizer to improve farming. After an initial improvement, the tribe suffered overpopulation that overwhelmed the food supply and, despite efforts to correct the situation, resulted in famine and increased mortality. It is easy to see many applications of Dörner's work to complex situations such as the healthcare crisis, poverty, organizational behavior, and even teaching. This book really helped me see the world in a more sophisticated way.

Why Don't Students Like School: A Cognitive Scientist Answers Questions About How the Mind Works and What It Means for the Classroom

by Daniel T. Willingham

This brief, accessible book is more about applying cognitive research to improve student learning than it is about why students might not like school. Willingham organizes the book around questions that parents and teachers ask about how students learn, and he answers them using current cognitive research. There is a real need for a concise, accessible book that explains how cognitive research can improve student learning for lay people, and this book is aimed at that gap.

The book has a lot of valuable information for teachers that is not typically included in books on teaching, but I found the writing uneven and some of Willingham's answers a bit unsatisfying. On the plus side, Willingham does an effective job of describing the limitations of the cognitive system and their implications for learning. He also assails the common misconception among teachers that engagement is the key factor for better learning. He reviews cognitive research that shows what students think about is the more critical factor, and he describes the implications for teaching. He also explains the role of knowledge in comprehension, and the importance of deliberate practice. On the down side, many of Willingham's examples seem to be more about showing his breadth of knowledge rather than elucidating the text. And even though he does discuss a great deal of important research, I still think there are some significant gaps, such as research on the importance of cognitive load, the tenacity of misconceptions, or how to design effective examples. Still, the book is certainly worthwhile and it appears to be the best current option for a lay explanation of cognition and instruction.

Visible Learning: A Synthesis of over 800 Meta-Analyses Relating to Achievement

by John Hattie

Regan Gurung recommended this extraordinary book to me and I think it is a "must read" for anyone who cares about teaching. Hattie spent 15 years synthesizing over 800 meta-analyses that included over 52,000 studies on the factors that influence student achievement. The book covers virtually all factors related to teaching, and synthesizes the best available evidence about the impact of each factor on student learning.

One of Hattie's critical conclusions is something that I've suspected for years: virtually all educational innovations improve student learning. Hattie reports that 95% of all reported teaching innovations led to positive changes. Based on longitudinal studies, Hattie found the average improvement for any teaching innovation is an effect size of $d = 0.4$. He concludes that this improvement reflects a combination of what a teacher can accomplish in a typical year and student development not related to teaching. As Hattie says, "One only needs a pulse, and we can improve student achievement." Thus, to find that a new teaching method improves learning does not necessarily mean the method is worthwhile; instead, for a teaching factor to be truly effective, it should exceed that baseline effect size.

It's fun just to flip through the book to see what makes the cut and what doesn't. Intuitively, one might believe that when instruction is individualized for each student's strengths and interests, it should be very effective; the meta-analysis, however, yields an improvement of only $d = 0.23$. Do teacher strategies matter? A meta-analysis of all teaching strategies yields a $d = 0.6$, and Hattie lists the specific effect sizes of 24 different strategies. Of course, meta-analyses are only as good as the studies that go into them, but his book represents an remarkable synthesis of the literature. Hattie's goal was to provide an empirical basis for a comprehensive theory of teaching, and to replace the current "whatever works" approach with one that examines "what works best." He has succeeded magnificently.



Carol Tavis is a social psychologist who has devoted her career to writing and lecturing about the importance of psychological science and critical thinking. (She often feels like Sisyphus, but persists nonetheless.) She is author, with Elliot Aronson, of *Mistakes Were Made (But Not by Me): Why we justify foolish beliefs, bad decisions, and hurtful acts*. Her other best-known books include *Anger: The misunderstood emotion*, *The Mismeasure of Woman*, and, with Carole Wade, two textbooks in introductory psychology. Carol has always taken to heart the anthropologist Marvin Harris's advice to her when she was a grad student: "I don't see how you can write anything of value if you don't offend someone." On that criterion, Carol figures some of her writings must be pretty valuable.

Crazy Like Us: The Globalization of the American Psyche

by Ethan Watters

In spite of the rise of concern to bring “diversity” to psychology, and the field’s determination to make psychology the study of all human beings, culture still don’t get much respect. It is difficult to define and measure. It doesn’t lend itself to fMRIs and PET scans. It’s bulky—is a Chinese American who grew up in Los Angeles influenced more by the cultural norms of being Chinese, American, or Californian? And it is difficult for many instructors to weave “culture” into their lectures, for fear that a statement of some average cultural difference will sound like stereotyping—that is, an implied negative contrast. Years ago, a student of mine said, “How come when we students speak of ‘the’ Japanese or ‘the’ blacks or ‘the’ whites or ‘the’ Latinos, it’s called stereotyping, and when you do it, it’s called ‘cross-cultural psychology’?” I praised that student for good critical thinking and plunked the remark into our textbook, but it remains a tough question to answer!

This is why instructors who believe that culture is as powerful an influence on human behavior as genes or neurotransmitters should read Ethan Watters’s new book, *Crazy Like Us: The globalization of the American psyche*. They will find here fascinating examples of how culture affects a central problem in psychology: our notions of mental illness and its defining symptoms. Watters, an investigative reporter who is well versed in psychological science and a fine writer, goes beyond the DSM’s list of “culture bound syndromes”—sets of symptoms unique to a particular culture or geographical region, such as amok, zoro, taijin kyofusho, and zar—to argue that American culture is exporting not only McDonald’s restaurants and shopping malls but also our forms of mental disorders.

Watters makes his case in four lengthy chapters: “The Rise of Anorexia in Hong Kong”; How Posttraumatic Stress Disorder became “The Lingua Franca of Human Suffering” in Sri Lanka; “The Shifting Mask of Schizophrenia in Zanzibar”; and the “Mega-marketing of Depression in Japan,” which shows how the pharmaceutical industry’s advertising literally changed the Japanese experience of depression as well as the treatment offered to cure it.

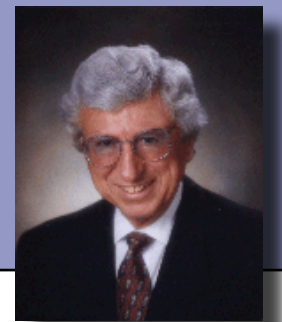
Watters argues that thanks to the tireless efforts of mental health care providers, drug companies, and advocacy groups, the biomedical and genetic model of mental illness has taken over around the world. American psychologists who think this change is an unequivocal improvement (certainly it is an improvement over thinking that mental illnesses are caused by bad mothers or bad spirits), will find much to ponder in Watters’s stories. For example, he shows that the biomedicalizing of mental illness has generally increased the perception of dangerousness of people with various disorders and increased the stigma they live with. And it is obliterating the traditional, community forms of treatment that often seem unscientific and primitive to outsiders, but which may offer the best relief to sufferers—precisely because native interventions fit the sufferer’s cultural experiences, perceptions, and values.

The opening chapter on anorexia in Hong Kong is illustrative and surprising. Western psychologists would not be surprised to learn that America’s obsession with slimness has been exported to any country with televisions and the Internet. But when Watters visited Sing Lee, an eminent researcher on eating disorders and the first to document the once extremely rare cases of anorexia in China, he learned that Chinese anorexic women originally had different symptoms from those of American anorexics: Starve themselves they did, but they did not have the classic defining symptom of fear of fatness, nor did they misperceive their bodies as being overweight. Over time, the American version of the disease became more widespread. The variations in symptoms between English and American women and the Chinese women were sometimes so pronounced, Watters writes, that Lee wondered “if he was seeing the same disease” (p. 15).

And that observation, of course, goes to the heart of the question. Human suffering in the aftermath of tragedy is universal; schizophrenia and depression are found almost everywhere. For my part, Watters minimizes the contributions of genetics and biology to understanding why some disorders are indeed found universally, even if they appear in different guises. But this book is an important corrective to the impulse to maximize those factors. What mental illness means to those who suffer from it; how it expresses itself; how and whether it responds to psychological or medical interventions—these are matters that require cultural understanding and a willingness to set aside Western arrogance. Watters’s book is an excellent place to begin.

Sexual Orientation, Faith Tradition, and the Disappearance of the Leona Tyler Principle

by Nicholas A. Cummings, Ph.D., Sc.D.
Former President, American Psychological Association



Introductory Comments from Dr. A. Dean Byrd, Symposium Chair

The Leona Tyler Principle is probably not as familiar to members of the American Psychological Association (APA) as it should be. Intended to be a firewall as Dr. Cummings would say to protect the veracity of psychological science and the integrity of psychological practice, the Leona Tyler Principle was accepted by APA and never rescinded (Wright & Cummings, 2005). The principle basically states that in speaking as psychologists, that any advocacy should be based on scientific data and demonstrable professional experience. Absent such validation, psychologists are free to speak as any concerned citizen, either as individuals or collectively.

Political correctness seems to determine what is published or funded. Indeed, it is no longer questionable whether or not ideology influences science. However, it is clear that science only progresses by asking interesting questions, not by avoiding those questions whose answers might not fit a particular agenda.

This symposium addresses sexual orientation and faith tradition in the context of the Leona Tyler Principle. In such discussions, not only is it important to determine what science can and cannot say, but it is important that ethicality and diversity be abided. In fact in a recent APA statement, there is a renewed call to respect a person's right to self-determination. And in regards to diversity, the University of Akron psychologist perhaps stated it best: "Respect for religious diversity demands that psychologists give as much weight to belief as to sexual identity" (Benoit, 2005, p. 322). Our symposium today focuses on the question of whether or not APA's advocacy on behalf of issues relating to sexual orientation and to religious stances toward sexuality have to date been properly based on scientific data and demonstrable professional experience.

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Until psychology gets over its fascination with disciplinary navel lint, the profession will continue to fail in its primary mission.

-Ron Fox, Ph.D., Former APA President (2003)

As the fourth iteration of our annual symposium convenes, my proposal for APA reform endorsed by four former APA presidents and a substantial number of our leaders continues to be ignored. In order to rest the stranglehold on APA governance by an oligarchy of about 200 which perpetuates by rotating itself through various offices in a kind of organizational

musical chairs, it has been proposed that our national association be democratized by returning to a simple and direct one-member-one-vote system for all APA offices. The entrenched oligarchy has ignored the proposal, resulting in a precipitous decline in both division and APA membership. A number of hard-hit divisions which have refused to relinquish power have resorted to falsely boosting their roles by keeping names on their rosters until dues has been delinquent for at least three years. They would rather endanger the APA itself than give up their authoritarian control.

In expressing his disappointment that psychology has failed to address the pressing problems of society, former APA President Fox neglected to ask how could

a discipline that refuses to put its own house in order solve the monumental problems of society? It would be like expecting profundity of Alfred E. Newman, the late hero of Mad Magazine, who made famous the quote, "What, me worry?" And should the APA worry about what has happened to psychology in the last ten years? Let's take a look.

- The APA became the first and only national professional society to be censured by the U.S. House of Representatives. The APA was accused of advocating pedophilia in its journal, and the testimony of the APA's CEO was so inept that the censure motion was approved unanimously! I talked with several Members of the House of Representatives who stated the APA's over-concern with political correctness and academic freedom made psychology look as if it condoned pedophilia. Most psychologists are not even aware this fiasco occurred. After all, why should the APA tell us and make us worry?

- Most practicing psychologists are aware that their incomes have not kept pace, and have even declined. But have they been told that in the past decade the percentage of the national healthcare budget that goes to mental health and substance abuse has dropped from 8% to 4.5%? Even worse, Towers-Perrin has found that most of the 2008 dollars went to pay for psychotropic medication, and only 1.5% went to psychotherapy or behavioral interventions. Furthermore, this figure is expected to decline to 1.2% in 2009 (Pearson, 2009).

- In its assessment of developments in mental health, a 2007 special committee of the American Medical Association concluded that "psychotherapy is going the way of luxury ice cream," nice but unnecessary. But why worry?

- According to the U.S. Bureau of Labor Statistics, during the past three years psychologists became the lowest paid doctoral profession in healthcare. With incomes clustering in the \$60,000 to \$70,000 range, we were shocked to learn that the recently retired head of the APA's Practice Organization earned well over \$600,000 per year. His retirement account of almost \$90,000 per year was more than most psychologists' incomes. As a practitioner, do you believe you received commensurate value for your dues that paid for that salary?

Never Say Never

What would lead the intelligent, sophisticated leadership of the APA of our national organization to take the extreme, indefensible position that change in

sexual identity is not possible, and to seek to brand interventions designed to do so as unethical? The old saying, "never say never," underscores the folly, as it takes only one exception to disprove the "never." Jones and Yarhouse (2007) in this symposium have done well over that. There are three simple, but tragic steps in this process of scientific distortion:

- First, ideology infects and perverts science. Mental health historian Gerald Grob (1993) has traced the manner in which mental health has been particularly susceptible to the intrusion of ideology in the absence of solid research findings, leading to misstep after misstep. It first begins with the best of intentions: purifying our air and water, protecting the habitat, combating racial, social and sexual injustices. Then there is always a giant leap in the absence of fact, and not the least of these has been the emptying of our state hospitals promoted by misguided compassion. Called deinstitutionalization, it made the street and our prisons the de facto mental hospitals of America. With similar good intentions the APA took up the plight of gays and lesbians, all very well, until the next step took over.

- Second, political correctness is mobilized by proponents in the absence of definitive research findings. Political correctness is substituted for proof, and those disagreeing with the premise are branded as stupid, racist, lacking in compassion, homophobic, or just a right-wing nut. Rev. David Code is typical of hundreds, if not thousands of daily attacks. Code is an Episcopal minister who in his mid-career entered a counseling psychology program at Penn State University. He already held degrees from Yale, Princeton and La Sorbonne Universities. As part of his ministry of many years he had his own radio show on marriage and parenting. After a fellow student complained that he was a "family values minister" and ipso facto must be anti-gay and anti-feminist. The irony is that Code is a liberal who is highly supportive of gay and feminist issues. He was ordered to desist from all of his ministerial duties. He refused and was dismissed even though he had been a straight A student (Cummings, O'Donohue & Cummings, 2009, pp. 147-8).

- Finally, political correctness becomes an effective enforcer, masking the absence of scientific evidence, and intimidating opposition into silence. Those who do not remain silent are punished as was the Reverend Code. In this phase those seen as politically incorrect risk denial of admission as students, and colleagues so deemed risk not being hired or of losing a job, tenure, promotion, funding or just being shunned. The past thirty years are replete with politically correct positions that were proved false: global freezing

in the 1970s (Newsweek and Time Magazines), running out of food (Paul Ehrlich in 1968), the extinction of all birds (Rachel Carson in 1962), masculinity and femininity as culturally-induced artifacts (Gloria Steinem in 1978), refrigerated moms cause childhood autism (Bruno Bettelheim in 1948) and these are only a few of the political missteps. (Note: These are discussed extensively in Cummings & O'Donohue, 2008). As these collapsed, the politically correct crowd merely walked away from the shambles as if nothing had happened, and began spinning and enforcing the next ideological untruth as they took advantage of the public's short memory.

The tragedy of political correctness is that the intimidation frightens away research that might shed light on the social problems of today. Research into intelligence and learning that might solve the tragedy of why our children cannot learn in our schools has been banned as the Bell Curve exists in all of nature except human intelligence. Gender and racial differences in susceptibility of disease are an integral part of medical research, but are taboo in psychology. And now the unfettered research into causes and effects of same-sex attraction and behavior result in accusations of homophobia.

Facts? What Facts?

The Jones and Yarhouse findings that (a) change in sexual orientation is attainable in a modest number of cases, and (b) harm from the interventions is rare, if it exists at all. These findings coincide with the extensive therapeutic services over 25 years at Kaiser Permanente healthcare system in San Francisco during the era (1955-1980) when I was chief psychologist and that city was rapidly becoming a gay and lesbian Mecca. We hired perhaps what were among the earliest lesbian and gay therapists as hundreds of homosexual patients came to us for treatment, some desiring change in orientation, but most because they were not as happy with the San Francisco life style as they thought they would be before moving there. The majority of patients who came to us expressing distress with their homosexual behavior were able to achieve well-adjusted life styles, often resulting in long-term, successful same sex relationships. A minority of the number who expressed a desire for change achieved it. A third group whose unhappiness and reckless promiscuity was complicated by drug and anonymous sex addictions required long-term interventions, and they frequently contacted HIV-AIDS before their therapy positively impacted.

None of the patients who expressed a desire for change, whether successfully achieved or not, were harmed by the therapy. To be sure there was often tension and turbulence, but this was part of the therapeutic process and it did not differ from the course oft found with intensive interventions in all kinds of psychological issues. We also learned:

- Same-sex attraction and behavior are not unitary phenomena. Gay men ranged from those who were very masculine to what in the gay parlance of the era was called "Nellie" or "cuntie." Similarly, many lesbians were quite feminine, while others were known among their peers as "butches" or "daggers."

- Causation did not seem to be unitary, with genetics, in utero, or environmental factors seemingly playing differential roles. There were men who demonstrated "girlish" behavior from infancy, and similarly there were women who as early expressed solely boyish interests. Some had been seduced into homosexual behavior early, sometimes even before puberty, and even by an older sibling, resulting in a kind of sexual "imprinting." Some few turned to same-sex behavior by reason of heterosexual panic, and these readily changed once their intense fear of the opposite sex was resolved. These are only a few differentiating examples.

- Men's homosexual behavior was more fixed than that of women who were often quite malleable. Consequently change in same sex attraction and behavior occurred with women than with men. Grossman (2009) has compiled researches that demonstrate the fluidity of female sexuality. Many women change their sexual attraction from men to women, or vice versa, depending on circumstances, with some doing it several times in their lifetimes.

Our conclusions were obvious: change is possible for a minority of those who seek it, and the interventions are not harmful. We also operated under the premise that it is a primary facet of psychotherapy that the patient determines his/her goals in treatment. The APA's continued interest in prohibiting such interventions is at once egregious and short-sighted. That change occurs in a relative minority of cases is not a legitimate argument that it should not be attempted. If this were the standard, we would abandon the treatment of alcoholism and drug addiction because of the disappointingly low success rate.

Ban the Role of Religion?

A number of patients who come to us for change do so because homosexual behavior is ego-dystonic;

i.e., in such conflict with their morality or religious upbringing that it is a source of severe distress. To inform such patients, in accordance with political correctness, that change is not possible and they must accept same-sex orientation, is both an untruth and an indefensible violation of patient-determination.

Faith is a powerful motivating force in the lives of many, but one that has been understudied and unappreciated by most of psychology. Others, such as avowed atheist Albert Ellis (1980) has pointed out that "reading the New Testament has changed the course of more lives than all the psychotherapists in the world put together." I shall not belabor the role of faith inasmuch as my colleagues and I have recently published an entire book on *Psychology's War on Religion* (Cummings, O'Donohue & Cummings, 2009), and will just point out: (a) Alcoholics Anonymous (AA) for decades has been the most effective and enduring intervention for alcoholism, and at the core is the surrender to a Higher Power. It has been extended to Narcotics Anonymous (NA), Gamblers Anonymous (GA) and to other addictions. It has changed and saved the lives of millions since its inception in the early 1930s. (b) Psychotherapy, in spite of psychologists' penchant for doing it and often interminably, has a relatively low rate of appreciably altering personality disorders. However, there is frequently a profound change in sociopaths and borderline personality disorders, mostly in our prisons, as the result of religious conversion, either to Christianity or Islam.

The Leona Tyler Principle: R.I.P.

We once had a firewall protecting the veracity of our science and the integrity of our practice. It was called the Leona Tyler Principle (Tyler, 1969), and it was enacted by the APA Council and the Board of Directors to do precisely that. In chairing the Council and the Board during my presidency in 1979, I judiciously respected it, as did my fellow former presidents of that era. We ruled out of order any attempt to circumvent it, and one such attempt was to disenfranchise faith-based doctoral programs in clinical and counseling psychology because they might require a credal oath. I consistently ruled it out of order unless the proponents could demonstrate these programs were substandard in education and training, something they were not able to do. Rule 4 protecting such institutions was enacted, and we find attempts in 2009 to repeal that provision. After my tenure, and with the growth of political correctness, somehow the Leona Tyler principle which was never repealed, was increasingly ignored and allowed to fade in everyone's

memory. Our younger psychologists, many of whom have been seduced by political correctness, have never heard of the Leona Tyler Principle. As a result, we no longer speak as a science and profession.

I lived through the McCarthy era and the Hollywood witch hunts and, as abominable as these were, there was not the insidious sense of intellectual intimidation that currently exists under political correctness. In the previous era you knew who your oppressors were: the John Birch Society, anti-Semites, segregationists and, more benign, the evangelist in the gospel tent down the street who wanted to save my soul. Now misguided political correctness tethers our intellects, corrupts our science, and cripples our practices.

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Division 1 Convention Program

Time	Thursday 8/12	Friday 8/13	Saturday 8/14	Sunday 8/15
8 - 8:50 AM	Health Disparities (S-2) Room 31C (CC)	Disseminating Research (S) Room 25A(CC)	Issues in Academia (PA) Room 25B(CC)	Personality, Therapy & Culture (PA) Room 27A(CC)
9 - 9:50 AM		Child Assessments (I) Room 30D(CC)	BUSINESS MEETING Room 33B(CC)	Sociocultural Variables (I) Room 30E(CC)
10 - 10:50 AM	APA Task Force on Sexual Orientation (S-2) Room 26A (CC)		Diagnosis with Children (S-2) Room 15B(CC)	Assessment & Intervention Data (S-2) Room 33C(CC)
11 - 11:50 AM	Creativity and Discovery (I) Room 25A(CC)		IRB Developments (S-2) Room 5A(CC)	Use of Technology (S-2) Room 26B(CC)
12 - 12:50 PM	Cognition, Perception, & Development (PA) Room 24C (CC) Fear in Rats & Therapy (I) Room 30C(CC)	Student Research (PO-2) Room 30D(CC)	Conflicts in the Middle East (I) Room 1B(CC)	
1 - 1:50 PM	NSF Funding (W) Room 24C(CC)		PRESIDENTIAL ADDRESS Room 28D(CC)	School Based Programs (S) Room 23B(CC)
2 - 2:50 PM	Virtual Environ. (S) Room 25A(CC) Statistical Education (W-2) Room 31B(CC)	AWARD SESSION (A-2) Room 15B(CC)	AWARD SESSION Division 52 suite San Diego Marriott Hotel	
3 - 3:50 PM		Psychology Misconceptions (S-2) Room 32B(CC)		
4 - 4:50 PM		Biopsychosocial Development (I) Room 30B(CC)	Transcending Boundaries (PO) Exhibit Hall ABC(CC)	

Social Hour: 5-6:50PM Saturday 8/14; Atlanta and Chicago Rooms, San Diego Marriott

Executive Committee Meeting: 7-9:50pm; Thursday 8/12; Columbia Room 1, San Diego Marriott

Key: A= Address/award, I=Invited Address, PA =Paper Session, PO= Poster Session, S= Symposium, W=Workshop CC= Convention Center * One hour and 50 minute sessions are indicated by a "2" above. All other sessions are 50 minutes. a = combined event with Divisions 24 (Theoretical and Philosophical Psychology) and 26 (History of Psychology)

D1 is pleased to collaborate with the APA International Division, to host receptions in the international suite in the San Diego Marriott Hotel.

Report

APA Council of Representatives Meeting

by Bonnie R. Strickland, University of Massachusetts



The 2010 Council meeting was held February 18-21. I also attended several of the Caucuses associated with Council; these included the Coalition for Academic, Scientific, and Applied Psychology, the Ethnic Minority Caucus, the Gay, Lesbian, Bisexual and Transgender Caucus, the Scientist/Practitioner Caucus and the Women's Caucus. Each of these Caucuses discussed agenda items of interest to their constituents.

After a Plenary session on February 18, Council Business began on the 19th with recognition of those APA members, who had died since the last Council meeting in August, 2009.

President Carol Goodheart opened the meeting and announced her Presidential Initiatives. These include Collaborative Leadership, Advancing Practice, Supporting Caregivers, Advocacy, Growing a Sense of Community, and Collaborations with Others.

CEO Norman Anderson discussed the ways APA is trying to influence the health care debate through interactions with Legislative Committees in both the House and the Senate emphasizing the recognition of psychologists. He gave us an update on our Diversity Plan and he explained the APA web relaunch. He talked about the Strategic Plan and gave an update on the budget. The success of the Publication Manual has helped our budget situation significantly.

We approved the consent agenda and received the reports of Past-President Bray's Task Force on the Future of Psychological Practice and Psychology's Contribution to Ending Homelessness. As part of the Strategic Plan, Council adopted the following core values:

- The Continued Pursuit of Excellence
- Knowledge and its Application Based upon Methods of Science
- Outstanding Service to the Members and to Society
- Social Justice, Diversity, and Inclusion
- Ethical Action is All That We Do

As always, a primary focus of the Council Meeting was the budget. The equity in our buildings stands at \$77.4M. Our long term investments are \$56M. Our 2009 operating budget had total revenues of \$1,147,210 and expenses of \$1,081,250 so we did not operate at a loss. We also approved a budget for 2010 with revenues of \$1,144,750 and expenses of \$1,133,384. The budget

includes restoration of \$60,000 for the Archives of the History of Psychology. Council proposed a delay on the recommendation to change the ramp up dues for early career psychologists from 8 years to 6.

Our membership remains at about 150,000 with a slight gain in affiliates. Membership dues will remain the same and Council defeated a motion to eliminate dual discounts. Council approved a Public Education Campaign which includes an evaluation component.

Considerable discussion took place concerning the Revised Model Act for State Licensure of Psychologists. The major concern was whether to exempt school psychologists from calling themselves school psychologists in that psychologists should only be licensed with the doctorate. The Model Act passed with the exemption.

Council adopted language which resolved inconsistencies in the Ethical Code about resolving conflict.

Another long discussion revolved around dual discounts. After considerable deliberations, Council voted to give a dues reduction of \$25 for APA members who are also members of State/Prudential and Territorial associations as well as members of the Association for Psychological Science and groups who belong to the Federation for the Advancement of Behavioral and Brain

Council voted to approve the development of Treatment Guidelines for psychological health care.

Finally, in view of the continuing controversy concerning the use of the Manchester Hyatt as a convention hotel in San Diego, Council voted to move its APA meeting site to another venue. Many Divisions, including Division 1, have also asked that their activities not be held in the Hyatt.

Respectfully submitted,

Bonnie R. Strickland
Division One Council Representative

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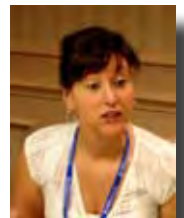
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Ad Hoc Committees of the Society for General Psychology

Division One has several committees to examine issues that (a) impact psychology across specialties, (b) are relatively overlooked, or (c) may engage our younger colleagues or students. These committees are listed below.

The charge of each committee chair is to define the committee's mission, appoint a few Division One members to serve on it, including one early career psychologist (ECP), and to provide a report on its activities to The General Psychologist. In addition, it is hoped that some of the activities of these committees will turn into sessions at the APA convention. Members who would like to join a committee should contact the chair of the committee. Members who would like to chair or simply suggest a new committee topic should contact the current President of the division John D. Hogan (hoganjohn@aol.com) or President-Elect, Nancy Felipe Russo (nancy.russo@asu.edu).

1. Early Career Psychologists - Chair: Gina M. Brelsford

Mission: Work with APAGS and others to recruit and engage students and ECPs in general psychology.

2. Coping with Technology - Chair: Richard S. Velayo

Mission: Examine the negative impacts of email and changing technology on the field of psychology (teaching, science, practice) and practical means of coping with technostress.

3. National Speakers Network - Chair: Harold Takooshian

Mission: Use CODAPAR funds to identify convenient speakers for local student and community groups, by developing (in cooperation with Divisions 2, 21, & 52, Psi Chi, Psi Beta, TOPSS) a web-based zip code list of willing Division One fellows, with their contact information and preferred topics. The division has applied for a second grant to continue this work.

4. IRB/Scientific Integrity - Chair: Richard O'Brien

Mission: Probe the impact of IRBs on science, scientists, and society, as well as academic freedom, junk science, and other trends threatening the integrity of the scientific enterprise.

5. Advisory Committee - Co-chairs: Bonnie Strickland, Harold Takooshian

Mission: Insure continuity within Division One, using a panel of past officers/presidents to help guide Division One procedures.

6. Publications - Chair: Bob Johnson

Mission: Oversee effective communication within the Society, coordinating TGP, RGP, book series, Website, listserv, and possible member surveys.

7. Evolutionary Psychology - Chair: (open)

Mission: Develop and give a home to this interdisciplinary specialty.

8. Photography and Psychology - Chair: Joel Morgovsky

Mission: Seek out the many members of APA who are deeply involved with photography; become a networking hub and community of psychologist/photographers.

9. Science and Practice - Chair: Mark Koltko-Rivera

Mark is developing an exciting research program that requires cooperation across much of psychology.

10. Outreach - Chair: Rivka Bertisch Meir

11. Fellows - Chair: Florence Denmark

12. Membership - Chair: Brian H. Stagner



TGP Editorial and Design Group

The General Psychologist is a publication of The Society for General Psychology, Division One of the American Psychological Association.

Editor:

Gina M. Brelsford

*Penn State Harrisburg
gmy103@psu.edu*



Associate Editors:

Senel Poyrazli

*Penn State Harrisburg
poyrazli@psu.edu*



Harold Takooshian

*Fordham University
Takoosh@aol.com*





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Membership Application

To become a member or affiliate of Division One, The Society for General Psychology, please fill out the form below and send it, along with the appropriate fees, to Division Services, American Psychological Association, 750 First Street, NE, Washington DC 20002.

Membership Category (check one):

- APA Member (includes Fellows, Associates, and Affiliates): Membership is \$25, including \$16.50 for the journal, Review of General Psychology
- APA Dues-Exempt Member (also known as Life-Status Member): Division One membership is free. To receive the Review of General Psychology, please submit \$16.50.
- Student Affiliate of APA: Membership is \$7.50. To receive Review of General Psychology, submit an additional \$16.50, for a total of \$24.
- Other Student: Membership is \$7.50. To receive Review of General Psychology, submit an additional \$16.50, for a total of \$24.
- Professional Affiliate and International Affiliate: Membership is \$7.50. To receive Review of General Psychology, submit an additional \$16.50, for a total of \$24.

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