



General Business

Minutes of the Meeting of the Executive Committee

Thursday, August 22, 2002

Hilton Chicago Hotel, Chicago, Illinois

Present: Linda M. Bartoshuk, President; J. Bruce Overmier, President-Elect; Gregory A. Kimble, Council Representative; Lewis P. Lipsitt, Past Past President; Peter Salovey, President-Elect-Elect; Lee H. Matthews, Treasurer; Michael Wertheimer, Secretary; Susan Mineka, Member-At-Large; Bonnie R. Strickland, Member-At-Large; Donald A. Dewsbury, Historian; Harold Takooshian, Fellows Chair; Douglas K. Candland, Editor-Elect of Review of General Psychology; Marylou Cheal, Member of the Program Committee. Absent: Lyle E. Bourne, Jr., Past President; C. Alan Boneau, Newsletter Editor; Lynn A. Hasher, Member-At-Large; R. Duncan Luce, Member-At-Large; Frank Farley, Member-At-Large; Wendy M. Williams, Member-At-Large; Mark E. Mattson, Membership Chair.

President Linda Bartoshuk called the meeting to order at 8:10 a.m. The minutes of the 2001 executive committee and business meetings, as published in the newsletter, were approved. The executive committee expressed its appreciation to Harold Takooshian, Linda Bartoshuk, and Marylou Cheal for their efforts in generating a fine convention program for the division at the 2002 APA convention. The following election results were reported: President-Elect: Peter Salovey, Council Representative: Michael Wertheimer, Member-At-Large: Wayne Camara. It was announced that David Lubinsky of Vanderbilt University and Nancy Segal of California State University, Northridge, will be the division's program chairs for the 2003 APA convention, to be held August 7 to 10 in Toronto. The current "cluster programming" strategy, which was used for the 2002 APA convention and will be used again in 2003, is to be systematically evaluated by the APA Board of Convention Affairs; one problem that emerged during the 2002 convention is that plenary sessions conflict with cluster programs. The theme for the cluster that includes Division 1 (as well as Divisions 2, 3, 6, 7 and 15) in 2003 has been set as "rational mind and emotional mind."

Since the division's financial resources have been gradually dwindling during recent years, it was decided not to hold a "midwinter" meeting of the executive committee in 2002-2003; instead, division business will be dealt with by e-mail and, if necessary, occasional confer-

ence calls about such matters as the new contract with APA for the division's journal (*The Review of General Psychology*) and requests for nominations for APA boards and committees. Lewis Lipsitt will send the list of e-mail addresses of members of Division 1 to Bruce Overmier, so that the latter can use it for such purposes as soliciting vote allocations to the division during the annual apportionment ballot, asking all members to vote, and requesting members to ask the libraries at their institutions to subscribe to *The Review of General Psychology*.

Peter Salovey, editor of *The Review of General Psychology*, reported that the journal has been thriving substantially, but that APA Central Office may not have done as much as may be desired to try to market the journal to institutions; the journal continues to operate at a substantial annual financial loss (to APA, not the division). A new contract with the APA for publication of the journal is currently under negotiation. Douglas Candland, editor-elect, announced that the quality of the manuscripts that have been submitted to the journal is generally very good, that he is planning to generate some special issues, and that he is engaged in efforts to add women and European psychologists to his editorial board. The editor of *The General Psychologist*, the division's newsletter, C. Alan Boneau, submitted a thoughtful, lengthy written report (in absentia), detailing among other issues the success of his efforts to send issues of the newsletter by electronic mail to those members who have access to it.

Lee H. Matthews presented his treasurer's report, indicating that while the division still has adequate resources, these have been shrinking over the years as division membership has dwindled. The division's fund balance (assets) at the end of the calendar year was, respectively, in 1999, 2000, 2001, and 2002 (to July) \$52,377, \$49,479, \$44,659, and \$42,899. Annual income has decreased slightly over those years, while annual expenses have tended to grow. It was decided to continue to make regular annual contributions of \$50 to the Coalition for Academic, Scientific, and Applied Psychology; \$50 to the Scientist-Practitioner Caucus; \$100 to the Federation of Behavioral, Psychological, and Cognitive Sciences; and \$100 to the Archives of the History of American Psychology at the University of Akron. Efforts to assure that the honoraria for the Staats lecture series are retrieved from the American Psychological Foundation are still underway.

Mary Bullock from the APA Science Directorate and Patricia Kobor from the APA Public Policy Office reported

on current APA Central Office initiatives that may be of interest to members of Division One, such as work on use of the internet in research and testing; genetics research; guidelines for institutional research review boards; and APA concern with the reorganization of federal funding programs for behavioral science (including the National Institutes of Health). It was noted that the recent reductions in APA Central Office personnel because of a budget shortfall have had a particularly deleterious effect upon the APA Science Directorate; Bruce Overmier and Linda Bartoshuk, as president-elect and president, were asked to send a letter to Raymond Fowler and Michael Honnaker in APA's Executive Office expressing the Division's concern about the underfunding of science endeavors in APA Central Office.

Mark Matton reported (in absentia via Harold Takooshian) that membership in Division 1 peaked in 1988 with 6,234 members (then the largest division within APA), but declined each year since then (except in 1995); the trend of membership decline during the last decade or so has been true of almost all divisions. As of the end of 2001, Division 1 was 13th in size among APA divisions, with a total number of members of 2,209. A major effort by Harold Takooshian to bring fellows of the APA, who are not members of Division 1, into the division was highly successful, yielding 61 new members as fellows-elect of Division 1 effective January 1, 2003. Fifteen additional members of Division 1 who were not yet fellows of the division but were fellows in other divisions were also approved as fellows in the division. Takooshian was congratulated for his substantial efforts, and their success.

There were some problems this year in the implementation of the division's awards programs. While decisions have been made about some of the awards, decisions about others are still pending. A concern was raised that, given the trend of continuing erosion in the division's assets, the monetary awards may be too high. Accordingly, it was decided that henceforth the awards will be in the amount of \$500, plus reimbursement of the convention registration fee for those awardees who choose to attend the convention.

Donald A. Dewsbury repeated his annual request, both orally and in writing, that present and past officers submit to him as the division's historian any and all materials (including printouts of e-mail correspondence) for inclusion in the division's archives.

Gregory A. Kimble reported that the fifth volume of the Division 1-sponsored series, *Portraits of Pioneers in Psychology*, is currently in press at Lawrence Erlbaum Associates, to be published jointly by Erlbaum and APA late this calendar year, with a 2003 copyright. The series has, to date, brought in about twice as much in royalties to the division from APA as the division has contributed to the expenses of generating the volumes (a subvention of up to \$1,000 for each volume to cover costs of mailing, supplies, index production, etc.). Bruce Overmier was charged with exploring whether the series should be

continued with at least one more volume after the fifth, and, if so, under whose editorship.

The Division 1 web page, even though some additional material has been posted on it, is still somewhat out of date. It was suggested that Marc Carter at Hofstra University might update it, but that has not yet happened. Linda Bartoshuk volunteered to ask APAGS to suggest someone who might be willing to take over as webmaster in exchange for a subscription to the division's journal and newsletter, and membership in the division.

It was pointed out that Bob Catenor in APA Central Office is responsible for all APA listservs. Lewis Lipsitt, who earlier had tried to develop a listserv for Division 1 but encountered a number of frustrating snags, reported that Matthew Goodwin will send a note out to all Division 1 members who have access to e-mail, asking them whether they wish to be included in the Division 1 list.

The terms of Lynn A. Hasher and R. Duncan Luce as members-at-large of the executive committee, and of Gregory A. Kimble as the division's council representative, end as of December 31, 2002. Lee Matthews reminded the members of the executive committee that the division's bylaws, as officially revised in 2001, call for only a total of three, not six, members-at-large. Hence next year the division will not need to elect a new member-at-large, since the terms of Frank Farley and Wendy M. Williams continue through calendar 2003, and Wayne Camara's term in that role is from January 1, 2003 through December 31, 2005. The terms of Susan Mineka and Bonnie Strickland continue through calendar 2004.

A few items of old business remained unaddressed, including the Division 1 "initiatives initiative" suggested several years ago by Robert Perloff, a report on the status of the Committee Against Medicalizing Psychology that was brought before the division two years ago, and a proposal by the Coalition of Academic, Scientific, and Applied Psychology for development of a pool of science-oriented psychologists for nomination to positions in the APA governance. Time also prevented action on a new business item, a request for nominations by the division for editors of *Contemporary Psychology*, *Developmental Psychology*, and the *Psychological Review*; members of the executive committee were encouraged to make their own personal nominations.

The meeting was adjourned at 10:55 a.m.

Respectfully submitted,

Michael Wertheimer

Report on the Business Meeting of The Society for General Psychology,

Sunday, August 25, 2002

The meeting of the Society was called to order by President Linda M. Bartoshuk at 8:01 a.m. Most of the meeting was devoted to reports of the actions of the executive committee on Thursday, August 22, and ratification of those actions by the Society as a whole. Further discus-

sion was directed to several items.

As for the division's journal, the *Review of General Psychology*, its contract with APA ends as of December 31, 2003, and negotiations are currently underway for a new contract. The deficit the journal has incurred since its inception is now on the order of \$300,000. APA has offered to "buy out" the journal from the division (but with the division retaining all editorial decisions), in order to wipe out the debt and use it to enhance APA's own balance sheet, but there are still several other alternatives that are being explored. No decision has been reached as yet; Bruce Overmier, Linda Bartoshuk, Peter Salovey, and Lee Matthews were authorized to continue negotiations with APA on behalf of the division.

It was suggested that the division's newsletter, *The General Psychologist*, should return to covering news, and to printing such articles as awardees' addresses and invited addresses, as well as chapters from the Portraits of Pioneers in Psychology series of volumes. Continuing electronic publication of the newsletter was encouraged. Bruce Overmier was charged with taking responsibility for looking into the future of the newsletter, and C. Alan Boneau was congratulated for his long-term devotion to the newsletter and his enormous contribution to it.

Gregory A. Kimble gave a report on the recent meeting (on August 21, 2002) of the Council of Representatives. One item was that there was a major shortfall in the overall APA budget, with income substantially below projected estimates and expenses significantly higher than initially expected. As a result, one of the two annual consolidated meetings of APA boards and committees has been canceled. Furthermore, there were major reductions in personnel within APA Central Office, reductions that occurred in a remarkably humane way: no pink slips, but inducements for early retirement and for changing to part-time employment as well as encouragement for taking leaves without pay. These policies were so effective that this year's budgetary shortfall has been successfully made up. One important action by the APA Council of Representatives on August 21 was the passage of a new version of the APA Ethics Code; this difficult chore was accomplished after much discussion, consultation, and compromise on the part of a number of deeply concerned constituencies within the council. This action was clearly a major legislative achievement.

The first five volumes of the Division 1-sponsored series *Portraits of Pioneers in Psychology*, co-published by APA and Erlbaum, have been edited by Gregory A. Kimble and Michael Wertheimer (with Charlotte White as co-editor as well of Volume 1 and C. Alan Boneau as co-editor of Volume 2). Kimble has indicated that he wishes to step down as editor of any further volumes. The division decided to authorize a sixth volume in the series, under the co-editorship of Ludy T. Benjamin, Jr., Donald A. Dewsbury, and Michael Wertheimer, and to contribute a subvention of up to \$1,000 for production of the volume.

It was suggested that explorations should be undertaken with Division 26, on the history of psychology, about possible co-sponsorship of the series.

The meeting was adjourned at 8:59 a.m.

Respectfully submitted,

Michael Wertheimer

Appendices available from the Editor:

Report of the editor of *Review of General Psychology* (Salovey)

Report of the editor-elect of *Review of General Psychology* (Candland)

Report of the editor of *The General Psychologist* (Boneau)

Treasurer's report (Matthews)

Membership report (Mattson)

Fellows report (Takooshian)

Historian's report (Dewsbury)

Report on Volume 5 of *Portraits of Pioneers in Psychology* (Kimble)

Report on APA's Science Policy Insider News (Kobor)

Call for nominees for Fellow in 2003

Members of APA Division One, the Society for General Psychology, are now invited to nominate others (or themselves) for election as a fellow, based on their "unusual and outstanding contributions" to general psychology. Phone or write soon for a packet of forms, including our Division's 12 criteria. This year all completed materials must be submitted by 5 pm Friday, 13 December 2002: 4 copies of the nominee's vita, personal statement, and endorsements from 3 current APA fellows. At least 2 of the 3 endorsers must be fellows of Division 1. (Those who are already a fellow of another APA division can ask about a streamlined nomination procedure.) Contact Harold Takooshian, SGP Fellows, 314 Dartmouth, Paramus NJ 07652, USA. Phone 212-636-6393.

Call for nominees for President-Elect

This issue contains a postcard ballot for nominations for the office of President-Elect of the Society. Please submit the names of candidates, who must be Fellows of the Society. Recent bylaw changes have downsized the Executive Committee from 6 to 3 members and the decrease will come about through attrition—no nominees for this position are needed this year. The deadline for receipt of nominations is January 1, 2003.



Interview

Ray Corsini: A Life that Spans an Era

Conducted by

Robert Perloff
University of Pittsburgh

Frank Dumont
McGill University

Raymond J. Corsini's life spans much of the 20th century. (He was born June 1, 1914.) He is manifestly one of the most important psychologists of his time. The Biographical Dictionary of Psychology lists him as one of the most important psychologists of the past 150 years. He is perhaps best known to scholars and students in counseling and psychotherapy as first editor of Current Psychotherapies (Corsini & Wedding, 2000), the text that has sold more copies than any other in this field. His production of scholarly work in the fields of prison psychology, industrial/organizational psychology, psychotherapy, and educational psychology is demonstrably gargantuan. His work as an encyclopedist and editor of dictionaries is also notable. Indeed, his 4-volume award-winning Encyclopedia of Psychology is widely acclaimed as one of the best in its genre. His The Dictionary of Psychology, now in its second printing, is the most comprehensive ever published in the English language. His oeuvre is impressive. It numbers well over 40 published volumes.

Like some of his close friends, Albert Ellis and Heinz Ansbacher, for example, his career has spanned most of the 20th century. He has known and closely collaborated with some of the most important of the psychotherapists who have enlivened the North American pantheon of professional psychology. For that reason we have thought it useful to interview him to solicit his recollection of persons and currents of thought that have been so important to our common history. Most important, we have wanted to get Ray's views on himself into the record. His life is a fascinating account of a second-generation immigrant of Italian extraction, whose personal development was enmeshed in several complex ways with the nascent disciplines to which he contributed. In this interview we glimpse again the familiar figures of Moreno, Rogers, Bettelheim, Dreikurs, and numerous others, as well as Ray himself. Constraints on space have forced us to pass over many others.

We (the interviewers) formulated a set of questions that we sent to Ray via e-mail. We have re-sequenced his responses and done some minor editing, whilst respecting the idiom as well as the substance of his responses. Throughout this process we felt free to ask additional questions that were prompted by some of his responses. Our questions are in bold print. The interview follows.

Ray, what do you consider your major contributions to psychology—in particular, to general psychology?

I suppose that every one of my more than 100 articles and over 40 books have some general merit since all were accepted by editors of respected journals or publishers. Although I am a clinical psychologist, my interests are diverse. For example, I created several psychometric tests and explored the relationship of personal data such as body type, birth date, and measured intelligence to criminal career paths. (Interestingly, I found a correlation between physical attractiveness and crime type). But I consider my dictionary of psychology (Corsini, 1999) first in importance relative to general psychology. A good dictionary should be on every psychologist's desk. That dictionary took ten years of practically constant work to complete. I spent three years planning it, making visits to various university libraries as well as the Library of Congress. I spent a lot of time studying other dictionaries of psychology and psychiatry in order to get the best ideas for this new dictionary. I also attended APA congresses for the purpose of asking psychologists (I interviewed about 100 in all) what they wanted in a dictionary. Then I sent letters to 30 commercial publishers asking them to publish my projected dictionary. It was rejected by all of them. I then asked 30 non-commercial publishers such as the APA and various university presses and again was rejected—until finally Taylor & Francis, a British publisher that I had spoken to at an APA meeting, suddenly called me, and a contract was signed. Then it took seven years to complete the book with the help of a clerical assistant and more than 100 psychology experts as well as some physicians.

After this, at my request, the publisher hired six psychologists to do a recheck. Finally, Alan Auerbach and Frank Dumont edited the whole dictionary—everything to get the best product possible. The result: a volume that contains 27,000 definitions (compared to other dictionaries of psychology that typically have about 10,000 definitions) plus 10 appendixes including about 1000 mini-biographies of people who have been important in the history of psychology. A paperback edition has come out in which some minor changes were made.

I also edited two 4-volume encyclopedias and two condensations of them. Herb Reich, an executive at John Wiley, estimated it would take me about 7 years to do the first edition of the first 4-volume work. I bet him I would do it in 2 years. In fact, it took 18 months from contract to final submission, and it won several awards. In contrast, my doctoral thesis was 17 manuscript pages in length and 7 pages in print (1956). It may have been the shortest dissertation ever written as far as I know. Right now there are four books out to publishers including what I consider my most important book, *Individual Education*, which has so far been rejected by two publishers.

I am now 88, am working on a book on current religions, and have several other projects on the drawing board.

Tell us what led you into the field of psychology. What path did you follow?

This may be an unbelievable story to some. After I graduated from elementary school at age 14 (74 years ago) I decided that I wasn't smart as I was ranked 18th out of a graduating class of 35 students and had never once made the monthly honor roll. Against my mother's wishes I refused to go to high school and found a job as an errand boy in the New York dress industry. After a year-and-a-half of working, I decided (in 1929) to go to high school and eventually graduated in 1933 with average grades. Then, to my surprise, I found that I could get a free college education at the *City College of New York* because my scores on the New York State Regents Tests were so high. I didn't want to go to college, but my mother's dentist told her that I could learn at CCNY to be a dental mechanic. When I went to register I found that dental mechanics was not offered, and I had to register for English, French, and calculus. Six weeks later the dean of the school interviewed me and told me that all three of my teachers had recommended that I quit—and I did. He didn't remember that he had interviewed me and 19 other students because we had had the highest scores on the Regents Test among the 2000 entering students.

I then was employed by Civilian Conservation Corps as a laborer. After several months I tired of that and came back home, went back to CCNY, (mostly at night), and graduated after 7 years. I earned a C-minus average—that placed me in the lowest academic decile. The Depression was still going strong, and I couldn't find a job, so I registered in the *CCNY School of Education's Master's program*, intending to become a teacher. Near the end of that degree program I had to take a speech test to qualify for a course in practice teaching—and I failed. I was told it would take two years to overcome my Italian accent, a Jewish inflection, and a lisp. I didn't want to waste two years trying to speak well enough to teach in the New York City school system. I talked to my friend Stanley Lipkin who told me that the only way to graduate was to change to psychology as he had done, and so I did. I graduated with an M.A. in psychology, again with a C minus average—and again at the bottom of my class. All 26 of our graduating group in psychology took the New York State examination for *Psychologist*, and I came out first in the test of my group. I was then employed at [New York State's] Auburn prison. That's how I became a psychologist.

Later in my career, I flunked out of Ph.D. programs at Syracuse University and Berkeley [University of California]. (I later served on the faculty at Berkeley at the time of the so-called *student riots*.) This history of doing very well on tests and poorly in grades has dominated my thinking about schools as I hope to make evident later on. In school I rarely missed a class, listened carefully, did homework, tried my best on written tests, and got poor grades. If anyone reading this can explain that to me, I'll take it as a favor.

You began your career as a prison psychologist and later branched into industrial-organizational psychology, psychotherapy, and educational psychology. How did your prison work inform the other psychological specializations into which you migrated at later stages of your career?

Someone decided that Auburn Prison needed a psychologist. No one informed anyone at that prison that this was so. When I got there in 1942 I found my only duty was to give inmates IQ exams, but since Auburn was a transfer prison every inmate already had an "assigned" IQ. In other words I had nothing to do but receive a paycheck. After several weeks I was assigned an inmate-clerk to help me, and so we both had nothing to do. Meantime I combed the local public library for psychology books and read what I could find. After some while I found that I could borrow books and bound volumes of journals from a state library in Albany and consequently kept learning more about various fields of psychology. But being a person who liked to keep busy, I one day asked the associate warden whether there was something I could do. His answer was brief: "Keep the fuck away from me."

The prison ran a school that went up to high school level. I made an enemy for life of the principal by telling a student who had failed the Regents exams several times that with an IQ about 80 and with fifth grade abilities he would probably never get a high school education. This information got passed on to the principal of the school who had told the inmate that he would get a diploma. After that all communications between the school and me were broken. I was told that I had no tact.

Let me back up here. While doing my 7-year Master's program at CCNY I was employed in the Works Projects Administration (WPA) as a vocational counselor in a "Negro" organization: *The Urban League*. I was the only White person in an organization of about 20 people. There, I learned vocational counseling by reading about what I regard as a strange specialty, and so I began the process of interviewing people, giving tests, and then suggesting ways of getting appropriate training. So, at Auburn I began to do vocational counseling of inmates, and soon I had people voluntarily coming to me. I ordinarily spent a whole morning interviewing an inmate, and giving individual and group tests. On the following day I'd give more interviews and tests; on the third day I'd write out a detailed plan for the inmates and keep a copy for myself. Some time after I had completed 50 such interviews, I called in all the inmates, one at a time, and found that not one of them had

taken any of my advice. I wrote an article on this experience (1945), sent it to Carl Rogers to read, and he wrote a footnote for my article. I never again did vocational counseling.

I learned that the parole board at Auburn, which I arranged to attend, met once a month. It was interesting to see how they operated. The three members of the Board took turns, one at a time, asking inmates questions, while the other two were busy examining a set of papers of their clients that were to follow. When the inmate who had been interviewed left, the questioner pronounced his decision—either “Parole” or “No parole.” The other two always agreed. I talked to the resident Parole Board member connected with the prison, and he agreed that a report by me would be useful. So I began to interview inmates for pre-parole reports and soon this became my major responsibility.

Following this stint in Auburn my work in prison psychology continued in Elmira, New York, San Quentin, California, where I became chief psychologist, and in the prison system of Wisconsin. Because of the extensive readings I had done (including reading every word in every issue of *Psychological Abstracts* for about ten years) and the close interviewing of several thousand inmates in Auburn, Elmira, San Quentin, and elsewhere, I gained a deeper knowledge of people and, I thought, the workings of the human mind. When I was through with the prison stage of my career, I felt confident that I could handle any assignment whether in industrial-organization work or in counseling and psychotherapy, though I understood there would always be large uncharted domains of human behavior and experience to be explored.

You spent years as Chief Psychologist at San Quentin. What was the most difficult clinical decision you ever reached in your work at that prison?

Only one problem caused me difficulty and it bothers me to this day. I suppose it might be called a clinical decision. As chief psychologist I was part of the clinical team that decided whether inmates were sane and, if so, could be executed. At one time the clinical staff consisted of three psychiatrists and myself. The chief was a capable person. His second in command was an alcoholic but had his head on straight. The third was so befuddled that almost every morning I had to explain to him the difference between psychosis and psychopathy. (This doctor had been a psychiatrist in the army, but had had an accident that damaged his brain.) I was usually the first one to see condemned prisoners, and for my examination I used my habitual clinical skills as well as, at the time, the Rorschach. My reports were listened to carefully. I soon realized of course that every one I had judged to be mentally normal was eventually executed.

When it was time to see these people, I spent the previous day reviewing all the papers that were available. I then spent the next day seeing the condemned man and writing up the case—right there in what was known as death row. On the Thursday nights preceding the Friday on which they were to be executed I could not sleep well, realizing that I was part of the process that led to their death. One case

especially troubles me to this day.

Two young Black men had come in and were awaiting their day of execution. I read the account of their crime, and the more I read, the more it seemed clear to me that an injustice had taken place. Their story: Both lived in the same neighborhood in Los Angeles, and both were on the same corner looking for a cab when one pulled up to the curb. They agreed to take it together to save on fare. The cabdriver was later found dead, shot and robbed. The two men were found the next day and each blamed the other for doing the killing. The hand of one of them bore marks of gunpowder. He had a police record while the other had none. I went to see them and saw the first one whom I'll call Jeff¹ because he was the shorter of the two and somewhat overweight. He impressed me as a fine fellow and he had, according to what I had read, a good work record and a good family. When questioned he told a story that rang true. Once in the cab, the other passenger had shown him a gun and said that he intended to rob the cab driver. He pleaded with the other man, but when they arrived, the other man killed the driver and offered Jeff some money that he refused to take. My entire clinical review revealed that he was sane, so I reported him normal.

Several days later I interviewed Mutt, Jeff's cab partner the night of the crime. He was tall and thin and he told the same story as Jeff but claimed Jeff had shot the cab driver. I went beyond my professional limits and asked him how come he had powder marks on his hands and he said that he had shaken hands with Jeff. I wrote my report on Mutt: He was also sane. But the case bothered me. Though I could not feel sure, I felt strongly that Jeff was innocent and went back to talk to Mutt. I said to him something as follows: “I have looked over your case and that of Jeff and I am sure that he is innocent. If you will tell me the truth, you can save an innocent man.” His brief reply: “Jeff shot him.”

Weeks went by and whenever I would go to the condemned men's cells (I usually talked to all those that I had tested, including Mutt and Jeff) Mutt refused to discuss his case. We just looked at each other. He was obdurate. Jeff was depressed but friendly.

Several days before the execution I happened to be on the same ferry with Doug Rigg, who was the associate warden in charge of Care and Treatment. He pointed out two Black women and mentioned that they were the mother and sister of Jeff. I debated whether I should talk to them and decided it was not appropriate for me to do so. Then the next day I made arrangements to witness their execution. I stood behind Jeff who with Mutt was strapped in a chair. Jeff turned around to see who was looking at them among the people gathered at the window that separated the quick from the soon-to-be-dead. When he saw me he suddenly

¹ Bud Fisher is the creator of the popular comic strip, Mutt and Jeff, which had a wide readership in North America for the first half of the 20th century. Mutt's and Jeff's distinctive body types—Jeff short and portly, Mutt tall and lanky—gave rise to a common designation for pairs of males with similar disparate body builds (Eds.).

smiled, as though he knew that I knew. Mutt meanwhile was facing upwards either singing or praying. And then suddenly both turned to face forward and I silently prayed (something I had not done for years).

I am still bothered by the whole event and have wondered what I could have and should have done.

Beyond that case, did you have any difficulties with inmates in your prison career that particularly challenged you?

The only time I was ever threatened by an inmate was when I was sent by a psychiatrist to interview a woman at the women's prison in Wisconsin. She had been convicted of abusing two girls who were her stepdaughters and also her nieces as she had married her deceased sister's husband, an elderly man. The psychiatrist asked me to interview her because she had persistently refused to admit her guilt even though the evidence against her was strong. The parole board usually refused to release anyone who complained of being innocent.

I went to see her, and during our conversation she repeated what she had told the parole board. At one point I wondered aloud whether her elderly husband had been impotent. She denied this. Then later I administered the Rorschach test, as was my custom at the time, and after I finished I told her that the test gave evidence that her husband was impotent. This chubby, blue eyed blonde exploded in anger, but I backed out of range. This did not convince me of her innocence. I eventually wrote my report and left soon after. I have no idea what happened to her.

My experience with prison inmates has led me to this conclusion: Somewhat like O'Henry's story of *Soapy*, the man who voluntarily went to jail during the winters, many prison inmates are the same kind of people who in the Middle Ages joined armies or monasteries—places of refuge for "inadequates," where one could be sure of a place to sleep and to get fed. These are individuals who are, or who believe that they are, incompetent. Ask 1000 inmates and not one will agree with that statement. Whatever their opinions, doing stupid crimes such as taking drugs or stealing small items that led to their getting arrested and convicted are a reaction, I believe, to their unconscious desires to be taken care of. And so most convicts enter prison willingly, adjust quickly, and then try their best to get privileges. After they get out they plot to come back in again. Some inmates say of other inmates, "They've found a home." This does not apply to everyone, for example, to murderers, pedophiles, or those who commit high-finance crimes.

During your years working in prison settings, as well as since, you've made extensive use of psychodrama—and you frequently mention your association with J. L. Moreno. Describe your relationship with him and what you've drawn from his approach for your own work.

I developed psychodrama on my own and have never read anything by Moreno on this subject that helped me. I did try to read his *Who Shall Survive?* (Moreno, 1934/1953) when

I was studying for my Master's and found it unreadable. Some years later when working at Elmira Reformatory, a psychiatrist, Dr. Ralph Brancale, mentioned that Moreno was a good example of a paranoid personality. I only got to know him personally later on in my career. When I desired to start a therapy group at San Quentin another psychologist, Rudolf Lassner, told me of having seen Moreno direct a psychodrama session and gave me enough information about the procedures to allow me to begin to experiment on my own. Soon I began writing for Moreno's journal, and to my surprise he accepted everything I wrote. In every case, however, he added some remark about himself in the first paragraph of my article.

I finally met Moreno and Carl Rogers in Chicago on the same day. Both had known of me—Rogers who had instructed me in doing nondirective psychotherapy and Moreno who had published several of my articles. The meeting with Moreno was notable. He had been lecturing, and I listened to him. When he finished he came on the floor to greet people. He was surrounded by members of the audience and I joined the group. Finally when he came to me, I told him my name and after a surprised look he enveloped me in his arms and lifted me on his enormous belly, meanwhile shouting to all: "Corsini is here."

I got to know him fairly well because he came to Chicago often. He would frequently come to my home and lie down on a bed before preparing to demonstrate psychodrama. I always acted as his assistant. Once I brought him to the University of Chicago and announced his presence, and soon he was surrounded by dozens of interested students. At a meeting he offered me the editorship of his journal and I accepted. But I added that I would not allow him to add to the articles as he had done to me in the past. He seemed satisfied at the moment, but later that day he told me he had chosen a different editor for the journal.

Years later, accompanied by a number of people from Chicago, I went to Moreno's institute in Beacon, New York. We arrived on Friday night, and on Saturday morning we went to a room that was dominated by the "Wedding Cake," a set of three large circular wooden forms on which psychodrama could be conducted. We waited for about an hour—and no Moreno. Finally a messenger came and said that Moreno did not feel able to run the meeting. He announced that Dr. Corsini should take over. This started what I'd call a riot. Yells all over the place. Attacks on Moreno and me. Shouts and howls by disappointed attendees. I was asked by some to take over, but I refused. Finally, a rabbi managed to take charge and made an impassioned plea for order. He was told to "shut the fuck up," but someone then defended this man. The next thing we heard was someone shouting that the rabbi was screwing one of the women in the group, and the situation deteriorated.

Finally, someone got up and stated that we could fight all we wanted but maybe Corsini should be allowed to speak. I again refused to go on but was literally dragged to the Wedding Cake and held down by some of my friends. When they let me go I began to explain a role-playing technique I had often used. Pandemonium broke out again, so I went

back to my seat. Then the small group of my colleagues brought me back to the Cake and held on to me. Finally one person acting in my behalf said something to the effect that I was "innocent" and that all should listen to me. Again there were some catcalls, but finally some degree of order was established. My "captors" left me and I started to explain the *Behind-the-Back Technique*, (an effective procedure that I had developed) and then suggested that the loudmouth who had led most of the disturbance and who had disparaged the rabbi should come forward. He declined, and after more pyrotechnics the hall emptied.

Moreno finally arrived, and that evening he ran an interesting session with a woman who had been coming to the group for years. She recounted her experiences with a blind pianist who, she said, had had sexual relations with her under a piano. The pianist, in attendance, later denied it. Then she and a male assistant went through the "sexual act" right in front of us. I assumed that she was a weird person who enjoyed the role-playing and who was serving Moreno's instructional purposes.

I was elected the president of the *American Society of Psychodrama and Group Therapy* in 1956, and I realized that it was Moreno who had engineered this. I arranged that the next election would be fair and appointed a trustworthy person to receive the ballot envelopes. He did this only after the votes had been received and opened by Moreno. I accused Moreno of double-dealing and quit his organization. He protested that all organizations were so manipulated. The last time I saw him was in the lobby of a hotel. After we caught sight of each other, I grabbed my wife's hand, (we had attended the conference together) and sped away. Moreno started running after us, but we evaded him. I never saw him after that.

My final comment: Moreno was insane and had streaks of genius. No one who had any integrity stuck with him.

You are known to be an Adlerian though you characterize yourself as an eclectic. Though you've evolved beyond the classical Adlerian system over the decades, what basic Adlerian principles still inform your work and philosophy?

Throughout my life I looked for a system to help me understand what motivated people. I always found the theory and philosophy of Adler most satisfactory for that purpose. Adlerian psychology is not as parochial or culture-bound as Jung's or Freud's. In my *Handbook of Innovative Therapies* I chose to include several different systems of therapy written by Adlerians. So I haven't had to be sold on Adlerianism; I've long believed what Adler was all about. As I've often said to my friends and colleagues, my personal, secular religion has long been founded on his principle of *gemeinschaftsgefühl* [German for "community spirit or interest"]. I have never deviated from Adlerian principles because they always appeared to be consistent with common sense. All of my writings are consistent with those principles.

Dr. Dreikurs, in whose institute I worked, showed me the possibilities that Adlerian thinking and actions opened for

the therapist. Dreikurs and I broke our connections in 1972, due in some measure to professional jealousy, but principally because we had very different views of education—his theory and methodology dealt with making teachers' work easier by improving their understanding of children, while my intention was to change the school system completely. He aimed for one desirable goal and I aimed for a set of different goals. But his system and mine are both one hundred per cent Adlerian.

Which psychologists with whom you worked and studied were most influential in shaping the system of psychotherapy you evolved and used over the past 60 years?

I can't point to any psychologist with whom I've worked or studied who, beyond Rudolph Dreikurs, has influenced me significantly. Floyd Allport and Carl Rogers were the most renowned teachers I had, but both were a great disappointment to me—Allport because at that time he seemed only interested in his research and spent no time, as I can remember, *teaching* about personality or social relations. I will speak of my relationship with Rogers later on.

I met and had various short professional relationships with some prominent American psychologists (many became APA presidents); they include George Albee, Anne Anastasi, Albert Bandura, Carl Rogers, Donald Campbell, Florence Denmark, Robert Perloff, J. P. Guilford, Joseph Matarazzo, O. H. Mowrer, B. F. Skinner, and Philip Zimbardo. But none of them worked closely with me. Ted Blau, an ex-president of the APA, was one of my trainees when at the Elmira Reformatory. However, Dr. Rudolph Dreikurs greatly influenced and in a sense rescued me from both Carl Rogers and J. L. Moreno (the three were trying in their own way to enlist me as an acolyte). On the other hand, I have remained loyal to both Rogers and Moreno in that I have continued to use their methods. But I became an Adlerian because Adlerian psychology is nonrestrictive. It is more a philosophy than a system of psychology.

I found that I was an Adlerian even before I became familiar with Adler's writings. While working at San Quentin, (before undertaking doctoral studies in 1953 at the University of Chicago), I had successfully treated a child with a certain behavior problem—dawdling at the dinner table. I later found to my delight that Dreikurs' procedures for such a problem were identical to mine. Dreikurs accepted me as his associate in doing family counseling at the Adler Institute of Chicago—and this amounted to sitting with him while he did his magic. And only when he was stuck would he ask me to make some input. He had a number of different people assist him every year, but he asked me to counsel with him for two years. Every Saturday during the years of my doctoral program I would pick him up and drive him and his wife to the counseling center. What happened over time between us was that he distanced himself from me for an unusual reason. Dreikurs did a lot of publishing, but he never succeeded in getting an article accepted by the *American Psychiatric Journal*. His first breakthrough came when I wrote an article, gave him first authorship, and it was accepted. Then I wrote a book (Dreikurs, Gould, & Corsini,

1974) and offered it to him. He expressed gratitude each time I asked him to coauthor a manuscript, but then he declined and seemed to shy away from me. Finally the big breakup came in 1972 when I developed *Individual Education*, which overshadowed his attempt to do the same thing. I believe the basic reason was that it appeared to him that I was competing with him, whereas in fact I was only following in his footsteps. His wife had taken a dislike to me, and at a meeting at which the members of the Institute were present compared me to a younger son who was competing with him. I admit that I learned more from Dr. Rudolph Dreikurs than from anyone else I've ever worked with, but instead of making a friend, I made an enemy.

Carl Rogers was your mentor in the University of Chicago doctoral program you pursued in the early '50s. How important was he to your personal and professional formation?

I'll try to explain my relationship with him. While at Auburn Prison I read his book (Rogers, 1942) on counseling and psychotherapy, at about the same time that I gave up doing vocational counseling. I began to use his system, failed, got in touch with him, and told him the details of one client I was seeing. Surprisingly, he told me that he would modify his system in the light of my experience: He told me that I should apprise clients of the nature of his system and what my role would be. I never had trouble with inmates after that. Some seven years later at the age of 39 I decided to come to the University of Chicago because he was there and because I hoped that my former contact with him and the possibility of therapy with him might lead to my getting the PhD that had eluded me twice, (once at Syracuse and once at Berkeley).

During the course of my doctoral program, I went to his office to ask for therapy, but he said he couldn't see me at the time because he had a full schedule. He suggested however that I enter therapy in a group headed by another of his students. I had a wonderful, liberating experience in that group. I never did let Rogers know that it had transformed me. The reason was simple. I was frightened to do anything that might jeopardize my status in the doctoral program. I didn't know how he might react if he knew that I had had a successful therapy by a student. So, the sessions I had with Carl were not productive. He conducted therapy with me as he continued to write. On one occasion I criticized him, telling him that he reminded me of a statue of Abraham Lincoln I had seen at Disneyland that recited the Gettysburg Address. His words to me seemed artificial and insincere. He calmly repeated my statement and then he said that he valued me—but I didn't believe him. After our therapeutic relationship ended, I bought him the most expensive artist's brush I could find as a present as I knew he was an amateur painter.

I took one course from him and the experience was far different from any other courses I took. Rogers let the students alone and they reacted in a most unusual way. I could not tell what was real in the class process and what was acting "as if." At times the same small group in which I found myself talked to one another in a way that made no sense. And Rogers didn't take any active role that I remember. I thought of asking him, but again I faltered for

the same reason as mentioned above, I didn't want to jeopardize my status in the program. I didn't trust him, and I never was close to him in any way. Again I must say Rogers influenced me more by his writing than through our personal relations.

I doubt that Carl Rogers had close friends. Later in life, I understand that he had some kind of mental breakdown. That made me recall the one experience in which I saw him role-playing a person who was completely different from his usual, public self. He had done it so well that I wondered whether his whole life was a kind of a role-play. At one point in a psychodrama I did with him, he was cast as a son-of-a-bitch of a professor instead of with the persona he normally displayed. I was so shocked when he played that role so well that I suspected that this probably was his real alternate personality.

Over the years Rogers and I met but infrequently. Shortly before his death I recommended to a physician who lived near him to get training from him. I can mention in passing that I'd advise all therapists to begin as Rogerians as they are not likely to do anyone harm, regardless of how unskilled they may be using his procedures. Soon after making that referral I was asked to write two articles about Rogers, and I asked him whether I should write them. I told him that some things I might write could upset him. He told me to go ahead, and I did. I was about to send the information to him (that I had had good therapy by his student but not by him) when I got a phone call from the physician I had referred to Rogers, telling me he had died.

One further memory comes to mind. The psychology faculty at Chicago scheduled a softball game against the students. The faculty was on one side when two people acting as captains began picking players, and I was surprised to see how Carl Rogers reacted when the captain kept picking others before him: He seemed hurt and frightened.

You had an encounter with Bruno Bettelheim early on in your career relative to a book on psychotherapy that you were editing. What were the issues and the conclusions you came to?

I was interested in doing the first of my series of *Critical Incidents* books (Standal & Corsini, 1959), one on psychotherapy, and I wanted a Freudian commentator. Before that I had been invited to be a lecturer at the Menninger Clinic, but when I asked Karl Menninger to be a consultant to the book, he declined to participate. Later in Chicago I decided to ask Bruno Bettelheim to be the Freudian consultant to this book. I called him and made an appointment to see him about eight o'clock in the evening in his office. Of course, I knew about Bettelheim, because all reports about him were that he was highly emotional and abusive to students. When I entered, he greeted me warmly and I told him of the project and mentioned the names of various people who had already agreed to be participants. Rudolph Dreikurs, Albert Ellis, Jerome Frank, Victor Frankl, Ernest Hilgard, Ashley Montagu, Jacob Moreno, and Carl Rogers had agreed to be contributors, but I still needed a Freudian. When he heard what the book was about and who the other authors were, he suddenly changed his attitude.

"Who are you to do such a book?" It was evident he was angry. I answered as best I could.

"Who are these people who *dare* to participate?" I tried to explain that all of them were competent and highly respected, but he dismissed all of them as incompetent. We then got into an argument about treatment. Bettelheim took the point of view that medical doctors who were also psychoanalysts were the only ones who could give psychological treatments. At one point I told him that several of the people that I had already recruited as consultants were psychodynamically oriented. His reply: "So is my butcher." I remember, in the flurry of words, arguing about treatment, and I said that if one person gave another an aspirin or stanching a flow of blood, that this was treatment. He denied this and held firmly to his position. We both got more emotional and aggressive. Finally as I got up to go, he seized me by my jacket. At that point I said: "Let go of me," and I balled my right fist. If he had not let go, he would have had a bloody face. I walked out. Later, Karl Menninger and David Shakow also refused to participate. I never did get a psychoanalyst to contribute.

Incidentally, when the book came out, Starke Hathaway reviewed it and made outrageous remarks about it; he also directed some *ad hominem* remarks at me. I complained to Edwin Boring who was the editor of *Contemporary Psychology* and pointed out that Hathaway was an enemy of psychologists as therapists. (Paul Meehl, the series editor of the book, had conveyed this to me.). Boring sent me a long letter of apology. Later on, as I planned to move to Hawaii, this review by Hathaway was read by the head of the psychology department of the University of Hawaii. He told me I would never be hired by the psychology department there. I thus came to Hawaii under a cloud. Ironically, I was the first to get the *Hawaii Psychological Association Prize* for my contributions to psychology, and the first to be interviewed on videotape—tapes archived for the use of future generations—by this association. I am now on the affiliate graduate faculty of the University of Hawaii and have taught at that school in several departments including education, business, and psychology.

We understand that you were invited to the psychoanalytically oriented Menninger Clinic in Topeka, Kansas, though you were known to be an Adlerian. Would you explain that to us?

After I had written my first book, *Methods of Group Psychotherapy* (Corsini, 1957), I received to my surprise a letter from an MD in Kansas, asking me if I would be a guest of the Menninger Clinic for a week—from a Monday to a Friday. I would be allowed to observe their operations and would be a lecturer on group therapy, which they were conducting there. I accepted and waited for the time to come. I had read two of Karl Menninger's books, and one of them the *Human Mind* (Menninger, 1945) had been a favorite of mine. I found out that Carl Rogers had been invited some time earlier, and I went to see him. It turned out that the experience had been painful and humiliating to him. He told me that Karl Menninger and the rest of the

psychotherapists there attacked him, and that he never defended himself. He presented the persona that he was known for: a gentle soul. I got the impression that he, in effect, was advising me not to accept the invitation.

I went to the Menninger Clinic anyway, and a young physician was appointed to be my constant guide. My first interview was with Karl Menninger. As soon as it was appropriate I told him that I was editing *Critical Incidents in Psychotherapy*. I asked him if he would serve as a psychoanalytic consultant for the book. He replied that the psychology department of the University of Kansas had refused to accept him (or any psychoanalysts) on their faculty. He said his colleagues in psychoanalysis would not be pleased if he participated in a book edited by a psychologist. Remembering what Carl Rogers had said to me, I told Dr. Menninger I was ashamed that I had come to his clinic to be considered there as an inferior. I added that I had had a great deal of respect for him up to now. He didn't reply, and then said: "We have to go, and I am supposed to bring you to Winter VA Hospital."

We got into a sports car and he scared me with his driving. I felt sure that we would crash because he drove so fast and made dangerous turns. I realized that I had angered him. Several days later I was in a group of young physicians who were in training to be psychoanalysts, and Menninger started to discuss Kinsey's (Kinsey, Pomeroy, Martin, & Gebhard, 1953) second book. As he went on he criticized this book and made the point that Kinsey, as a biologist, was out of his league. He also began to attack others who were not physicians who dared to write on the topic of sexuality. Finally, he got to a point where he mentioned a famous German specialist, a physician whose name he could not remember. He asked the audience if anyone knew it—and the only hand that went up was mine. Glaring at me he nodded, and I mentioned the name, Magnus Hirschfeld. He nodded again and went on.

There was a final event scheduled with Karl Menninger. On Thursday afternoons all other activities stopped and the monthly speaker was scheduled to talk to the entire group. I stepped up on a small platform with a microphone in front of me and saw about 200 people in front of me. As Carl Rogers had told me, seated on the side of the room *facing the audience* was Menninger. My selected topic was the history of group psychotherapy. I was 5 minutes into my talk when I was stopped by a question from Karl Menninger. I stopped my talk, turned towards him and said in a calm and clear voice: "First, I want to tell you that I will take questions at the end of my talk, and second, if you had been listening, you would know that I already answered that question." Every one froze. Absolute silence. Then I continued my talk. I don't remember if there were any questions. The next day while walking about, I looked into various rooms of the clinic. In some cases individuals pulled me in and shook my hand. Some even kissed me.

You must have had many contacts with other notables in the field of psychology. Do you have any memories of them you could share with us?

Let me say a few words about O. H. Mowrer. I don't remember the circumstances of why we met. It was at the time I was an industrial psychologist. He was a former president of the American Psychological Association and I was pleased to meet him. I recall that we met in a restaurant. No sooner had we sat down than I was surprised to find myself in a therapy session—with him the patient and me the therapist. At that time I knew nothing of his mental illness and I played the role of Carl Rogers, doing much listening and nodding. He seemed quite content. Either that evening or soon after I attended a meeting at which he was to speak, and he impressed me as a lecturer. I recall that he outlined his talk on a chalk board, and I found it exceptionally clear and informative.

Victor Frankl also comes to mind. I attended a meeting, I believe, of group therapists. I noted that the man seated to my left did not have a tag identifying himself. During the meeting he kept writing and I imagined that he was taking notes. When the speaker stopped, this unknown person handed me the sheet he had been writing on, and I saw a drawing of a woman on a couch and a man sitting in the manner of Freud. Neatly printed below in block letters were the words: DR. CORSINI, WOULD YOU MIND KISSING ME? Surprised, I asked "Who are you?" and he replied "Victor Frankl." I asked how he had known of me and how he had located me, and he told me that I had introduced him to America in the book *Critical Incidents In Psychotherapy*. He had noted that I was on the panel and he had found me by looking for my identification tab.

A word about Fritz Perls. I was president of the *American Society of Group Psychotherapy and Psychodrama*, and we had a convention in California. As I recall, Moreno was not present, but Fritz Perls was. Part of the program was devoted to group meetings headed by some well reputed practitioners. Perls was in one group and all members were lying down on the floor. While Perls was lecturing, some were on their backs, some were lying sideways, and others like Perls, himself, were flat on their bellies, heads resting on their arms. I listened for a while and then moved on to other groups.

Later on I took over and told the group that I wished to do a psychodrama and asked for someone with a problem. I was standing on a platform with members on all four sides and a young man volunteered to tell us his problems and he came up and it turned out he was a psychiatrist. His problem had to do with his wife who objected to his spending his time trying to create a University of Peace rather than bringing money home as a therapist. At one point after he finished telling this tale, I asked him to pick a wife from the group as well as his parents. Sure enough his father was played by Perls. Incidentally, I found out that Perls later gave me credit for teaching him the use of a chair to represent a person. However, I had learned that procedure from someone else, Rosemary Lippitt (see Corsini, 1958).

One of your most creative and influential projects was the development with Bina Rosenberg of the notion of *Common Factors* in psychotherapy, which subsequently pervaded

the clinical literature. How do you view that work today?

I'll start from the beginning. I invented a method that I called *Clinical Factor Analysis* defined in my dictionary as: "A nonmathematical method for generating independent factors. Involves putting either words or phrases on cards and then seeking other cards that are logically associated; when finished, picking up another card and continuing with a new concept." We searched the literature analysing the causes of success in group therapy and found that the first factor was altruism. I never saw my name associated in the literature with the concept. I devised this method, but in my dictionary I did not want to have myself as the author of too many items, and so I credited Dr. Bina Rosenberg, although she was a physician and totally ignorant of research methodologies.

The background of the concept was the following: I had tried to read the complete literature on group psychotherapy and as a matter of fact, with a librarian (L. Putzey), we generated a complete set of references in the *Group Psychotherapy Monographs* in 1957. In the process of reading these items for my first book *Methods of Group Psychotherapy* (1957). I ran into an unusual problem. Two collaborators had written a number of articles for this book, and then later each had written separate articles on the same issue. Each had listed five basic reasons to explain the success of group therapy. I read first one set and then the second, expecting to find that they would have the same five factors. Only one factor was common to both lists. I then began to collect terms that various group therapists used for those variables to which they attributed improvement. I paid no attention to duplicates and eventually had exactly 166 different statements of "mechanisms" accounting for therapeutic success. Each of these items was written on the back of business cards and spread out on a table. The number was so great that they made no sense. The first one I picked up might have read "Care shown by others." I said to Bina Rosenberg: "Let's see if concern or consideration by others in the group is mentioned." And we found perhaps a dozen and we then gave them the name of *Altruism*. So now we had 154 items to analyze. This took several hours and eventually we isolated nine common factors, which we grouped into three superfactors:

EMOTIONAL factors (*Acceptance, Altruism, Transference*),

INTELLECTUAL factors (*Spectator therapy. Universalization, Intellectualization*) and

ACTION factors (*Reality testing, Ventilation, Interaction*)

To my knowledge no one has ever cited me for this investigation into the common factors underlying most psychotherapy, and I've been surprised to learn that this procedure seems to have been used by others under another name.

We'd like to ask you a few personal questions bearing on your family of origin. We know you have authored an unpublished book called *Mama Mia*. What was your mother's influence

on the development of your personality?

My immediate answer would be *none and everything*. I believe I was a great disappointment to my mother up to age 14. We were constantly in conflict and she whipped me frequently with a line rope. But I believe we loved each other tremendously. I don't remember her ever beating my younger brother, Harold, who was five years my junior. My father, who died when I was six, punched me occasionally in the face and knocked me unconscious a number of times. He had an explosive temper, and my mother would find me unconscious with my father looking on at what he had done. Whatever the reason, somehow I triggered his assaultive tendencies. About the only statement she made to me about my father was that he had told her that when he spoke to me I replied as though I were an adult.

I never was delinquent. But somehow I had the power of driving my mother to distraction. When beating me was not the answer, she would bite her wrist to show me her anger. Once in a while she would lie on the floor in the attitude of Christ on the Cross to express her wish to be dead or to show me that my behavior was torture to her. Twice she brought me to the parish church to tell the priests there that she could not handle me and to ask them whether I could be placed somewhere else. This frightened me, and then I would swear to behave. Now, what were my faults?

My mother only told me what she saw to be my faults when I was about fifty. I asked her, and her answer surprised me. She said that she did not want me to become like Johnny D—, a boy I hardly remembered. She told me that he had been arrested and sent away to a "facility" and she worried that because of my behavior I would similarly bring shame on my family. On reflection, as I never ran with a gang of young thieves and never stole or broke any laws, I can't explain her suspicions and mistreatment of me. In the last analysis, I think it was that she blamed me for the death of my father. This is a separate aspect of my life that I discuss in a book I coedited with Frank Dumont (Dumont & Corsini, 2000).

I was completely different from my mother. Let me give some examples. I believe I was about five when she told me that she had broken a mirror after which her father died. She implied that breaking the mirror caused her father's death. I thought that perhaps an invisible arrow had gone from Vermont (where we lived that year) to Italy. After that I never took anything she told me seriously. I recall that I was a skeptic even as a child. An early example of my research bent: About this time a neighbor asked for a glass of water and told me to run the water first as that would make it colder. After several experiments I realized that this was so, though I didn't realize why. But I want to tell the story of the *Miracle of the Quarter* because of its psychological implications.

One evening when I was about ten my mother tried to light the gas stove we had with the last match in a match box, and the gas did not go on. She realized that the meter that was out of sight in the bottom of a closet in the kitchen had run out. It took quarters to provide more gas, like putting coins in a parking meter. She searched her purse and no quarters. Then she gave me a dollar to go to a store to get

change. As I was about to go out, she opened a new box of matches. On top of the matches was a quarter!

She lifted her head to the ceiling and began to thank Jesus for the miracle that he wrought and asked me to join in. I refused. She insisted that it was a miracle and that Jesus who was her prime protector had caused the coin to appear. We got into an argument that lasted several days. Finally one evening, several days later, after we had eaten and again restarted the same argument, she announced we should go to the local church to see a priest. We approached the church, and she knocked on the door. A woman opened it, and my mother explained we had come to see a priest. We went to a weakly lit room, furnished with a *prie-dieu* with an image of Jesus above it. After a few minutes a young priest came in, made the sign of the cross, which we imitated, and then he asked why we had come. My mother told him what had happened and used the term, *miraculo* several times. After she finished he turned to me and asked me what I thought. I said something as follows: "Father, for a miracle to occur, the Catholic church has twelve conditions for it to be accepted as a miracle. First, the purpose of a miracle is to prove to people that God exists and for all people to believe in him, and since I and my mother are the only witnesses to the so-called miracle and since I don't believe it is a miracle, it has only convinced her and not me. The second condition is that the occurrence cannot have occurred in any other normal way. I think there are several other ways it could have occurred . . ." and then I went down the list to the last item that in order for a miracle to be so regarded, the pope had to approve it after a collection of cardinals had approved it. When I had finished, the priest again made the sign of the cross and said: "Your son is right. It is not a miracle."

My mother strode out of the church and rapidly headed home, which meant that she did not want me near her. After a while she stopped, looked at me, and said "If we had gone to an Italian priest he would have agreed with me." She continued a ways and again stopped and said: "I think you know more than that priest knows."

So, trying to be as honest as I can be, I must say she may have had an influence on me. She was a fanatic Catholic. I never really believed there was a God, and more than that I had a prejudice against Jesus because she had once told me she loved him more than me. And yet I loved her more than anyone else I've ever loved, and I felt sure that I was number One in her life. (My brother Harold may not agree with me.) Let someone else figure this out.

You have stated that your repeated school failures were due to your father's premature death at the age of thirty-five when you were six. At the same time you gave stellar performances on aptitude and ability tests. Could you clarify this for us?

I have already stated that on entering the University of Chicago I asked Carl Rogers whether he would see me as a client, and he could not because his personal counseling schedule was full. He suggested that I participate in a therapy group that was being run by another graduate

student. I accepted immediately. The group started with about a dozen students and by the end of the first month it had dwindled to three: the therapist, myself, and another student. As you can expect I did most of the talking. As I began to explain my fascination with the female breast I suddenly could not talk, my throat clogged, tears came to my eyes, and I held on to the arms of my chair and thought I could hear my mother saying in Italian: *I am glad your father died because otherwise he would have killed you.* Then, suddenly, instantaneously, I understood everything about myself, and the mystery of my failure to do well academically was clear to me. My explanatory set of concepts went as follows:

I was a bad boy. Everyone said so. An aunt told me that she'd find it easier to have the five children she had than me. Another aunt forced us to move into a new home because she could not stand me. I drove my mother and father wild.

My father could not stand me and knocked me unconscious at least two times.

My mother had two boys before me, and they both died in infancy. She had two more boys after me and they also died. To see her only remaining son unconscious, apparently dead, led her to decide that my father had to die. And he did die.

Consequently, I was responsible for his death and had made my mother a widow.

Further, I must suffer for my sins and for my crime. No one should like me. And I should not succeed in school.

Parricide! As was true for Cain who had killed his

brother, I could not face the world. And this moment some fifty years ago still is a vivid memory.

This event provokes many questions. Why did this method of nonintervention work? Is psychotherapy really self-therapy? Was it Rogers' procedure that did the trick? And why after this did I become an Adlerian? Smart as I think I am, I haven't been able to answer these questions.

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Integration Themes In Clinical Psychology

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Sociologists who have studied progress in science have made an important distinction—the difference between the “core” of knowledge versus the “research frontier” (Cole, 1992). The core is comprised of those findings that are accepted by the scientific community as being “true,” that is, findings about which there is a consensus. By contrast, the research frontier consists of cutting edge contributions that are still being evaluated. Interestingly enough, a study by Cole of the grant review process has found that there is just as much disagreement in judging research proposals in the natural sciences as there is in the social sciences. In an observation with which many researchers can certainly relate, Cole suggests that the luck of the draw—namely, who reviews the grant proposal—has more to do with grant approval than the proposal itself. As he put it: “There may not be significantly more consensus in evaluating new scientific ideas than there is in judging nonscientific items such as human beauty, new works of art, or Bordeaux wines” (Cole, 1992, p. 19). And while psychology as a science and profession may have just as much disagreement at the research frontier as our colleagues in the natural sciences, the place where we differ is that we have problems in agreeing on what constitutes our core.

The problem that we have in agreeing about our core of knowledge is complex and multidimensional. In this paper, I would like to touch on some of these dimensions, as well as the past, current and future efforts directed toward creating a better integration within the field—especially clinical psychology. I might add that for each of these themes, my own initial involvement began with something personal, reflecting the not uncommon notion that the personal fuels the professional. These themes are: (1) Integrating Clinical Practice and Research; (2) Integrating Cognition and Affect into Behavior Therapy; (3) Integrating the Contributions from Different Therapeutic Orientations; (4) Integrating Past Contributions into Present Work; and (5) Integrating Gay, Lesbian, and Bisexual Issues into Mainstream Psychology.

Integrating Clinical Practice and Research

When I was in graduate school, Paul Meehl visited our program, and I was fortunate enough to be among a small group of students that went out to dinner with him. This was a rare treat, especially since I read virtually everything Meehl had written, and had enormous respect for his insights on research, practice and the philosophy of science. At one point during the evening, someone asked him the question about the extent to which his clinical work was informed by research. Without any hesitation, he replied: “Not at all.”

As someone who was struggling to adopt the identity of scientist-practitioner, I left this memorable dinner disheartened. I don’t think I ever fully recovered. The challenge of how we can close the gap between research and practice has stayed with me for all these

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years. Because I am attracted to challenges—my psychodynamic colleagues would characterize this as an unresolved conflict, and my experiential colleagues would probably call it unfinished business—I have continued to be intrigued with the integration of practice and research.

Throughout most of my professional career, I have lived in both the worlds of the researcher and the clinician. Much of my teaching, research, and writing has placed me at the academic end of the spectrum. My continued involvement in clinical training and supervision, and my part-time practice of psychotherapy have all kept me in close touch with clinical reality. However, over the years, it has been an ongoing effort to bridge these two worlds.

Those who have studied the progress of science have made another important distinction—this one between

the questions to be studied and the procedures for studying them. During the initial phase—the context of discovery—we have the “problem finders,” who identify the important research questions that are likely to advance the field. Once these issues are identified, we move to the confirmation phase, where the “problem solvers” investigate the empirical status of those phenomena that have been identified by the frontline observers. In considering the relationship between psychotherapy practice and research, I have viewed clinical work as providing us with the context of discovery. Working with clients directly and discussing clinical cases with others can provide not only the challenge of translating general research findings to the individual case at hand, but also can afford us the opportunity to witness first hand the ever-varying parameters of human behavior and the change process. In one’s role as therapist, the “problem finder” can garner clinical hypotheses to be studied under better-controlled research conditions.

In 1995, a Task Force that was formed within Division 12—The Society of Clinical Psychology—published a controversial report that attempted to delineate what at the time was called “empirically validated” therapies (Task Force on Promotion and Dissemination of Psychological Procedures, 1995). The intent was to help move the field toward a greater consensus, based on what has been empirically shown to be efficacious. However, several of us with a very definite commitment to having empirical support for our interventions had serious reservations about this report, and were particularly concerned that the methodological and conceptual constraints associated with outcome research might very well turn into clinical constraints for the practicing therapist. As a strong advocate of psychotherapy research (Goldfried & Wolfe, 1996, 1998), this dilemma may be thought of as reflecting a conflict between a wish and a fear: The wish is that therapy interventions be based on research; the fear, however, is that they might.

Despite the methodological advances in psychotherapy research that have been made over the past five decades, the findings compiled to date are limited by the fact that they do not fully generalize to the way therapy is carried out in the real world. The clinical validity of this research has been compromised by several factors. One is the medicalization of outcome research, as compared with a view of clients’ concerns as involving problems in living. External validity is also limited by the use of random assignment of clients to treatment conditions, rather than a research design that matches interventions to the case at hand. The research findings are constrained by the nature of our current therapy manuals, which typically lack the clinical flexibility to alter interventions during the course of treatment when needed. Finally, our outcome research fails to reflect clinical reality by primarily investigating theoretically pure therapies, which is not how most therapists practice. To close this gap between research and practice, the field needs to foster a more productive collaboration between clinician and researcher; to study theoretically

integrated interventions; to use process research findings to improve our therapy manuals; to make greater use of replicated clinical case studies; and to focus on dimensionalized, as opposed to heterogeneous and categorical clinical problems.

Recognizing that our methodologically sophisticated psychotherapy research paradigm may very well have sacrificed clinical validity for the sake of internal validity, Division 12 has more recently instituted a standing Committee on Science and Practice (Weisz, Hawley, Pilkonis, Woody, & Follette, 2000). The function of this committee is not only to review existing findings, but also to encourage improved research designs, to work on more effective ways to disseminate findings to practitioners, and generally to forge a closer link between researcher and practitioner.

The scientist-practitioner interface is important in that it keeps us honest both as clinical researchers and as empirically informed clinicians. Without an ongoing clinical base, it is all too easy to get caught up in research trends and fads, rather than pursuing that which is useful to the practicing clinician. Not only is it important for clinicians to help foster more clinically meaningful research, but they may also serve a vital function in providing feedback to researchers regarding those empirically supported interventions that do, and do not, seem to work in actual clinical practice. Even after drugs have been approved by the FDA for clinical use, clinicians have the opportunity to feed back information about how well they fare in actual practice. I would hope that the Division 12 Standing Committee on Science and Practice develops a mechanism for providing such feedback.

Integrating Cognition and Affect into Behavior Therapy

Behavior therapy has clearly evolved over the past four decades, moving from its focus on specific actions to an increasing attention to cognitive aspects of human functioning. In the 1960s, behavior therapy was criticized for its exclusive focus on overt behavior and its reliance on classical and operant learning models that, while important and useful, had limited clinical value. This criticism led to increased openness to cognitive procedures within the field. In the 1970s, behavior therapists began to take notice of the accumulated knowledge and developments in basic cognitive science, and most began referring to their orientation as “cognitive-behavioral.”

The role of emotional arousal in behavior therapy and cognitive-behavior therapy, on the other hand, is only recently undergoing a reevaluation. In the past, emotion has typically been viewed as something that needs to be reduced, and more emphasis was placed on behavior and cognition, as well as developing techniques for managing or containing affective arousal. For example, in a study by Wiser and myself, we found that in treating depressed clients, cognitive-behavior therapists viewed **lowering** levels of emotional experiencing as significantly contributing to the process of therapeutic change. In contrast, psychodynamic therapists

considered *increasing* levels of emotional experiencing to be clinically significant (Wiser & Goldfried, 1993).

Research has shown that although emotions can be affected by changes in the cognition (and vice versa), the emotional system also has a set of unique pathways that can be used to directly activate and change a client's emotional structures. Evidence from cognitive science and experimental psychology has linked emotion to personal meanings, and has emphasized the role of implicit meanings in the process of change (Samoilov & Goldfried, 2000). For example, two different types of knowledge have been described: One has been "tacit" knowledge, which involves the emotional-affective system, and the other involves "explicit" knowledge, a rational, logical knowledge system. Whereas the explicit or logical processing may influence rational judgments, the implicit or tacit processing, by contrast, is closely linked to emotion and is considered primary in changing global experiential states. Moreover, implicit meaning, together with its emotional overtones, is often evoked by sensory input, such as familiar sights, sounds and smells.

This influence of sensory input and emotion on implicational meaning can be illustrated with an experience I had during a trip to Poland, during which time my wife and I had visited Auschwitz and other concentration camps. It was an emotionally moving and highly personal experience, seeing the camps and viewing pictures of how people were transported there. That night, when the time came for us to board the train to leave Poland, we experienced some difficulty in locating the car in which our compartment was located. Amidst the crowd and confusion, where nobody spoke English, we were finally instructed to walk toward the front of the train. As we found ourselves at the end of the platform, our luggage fell off the cart, the whistle blew, and the train started to move. At that moment, I suddenly experienced an overpowering surge of fear and helplessness, and experienced a clear, felt-sense that we were being taken to a concentration camp! Even though there was a part of me that knew the train was really going to Prague, I nonetheless believed we were being shipped off to a camp.

Recent cognitive neuroscience findings by LeDoux (1996) have revealed a neural pathway that leads directly from the thalamus to the amygdala—the "emotional brain"—which allows the amygdala to receive direct input from sensory organs and to initiate a reaction before the information is registered by the neurocortex. According to LeDoux, signals that have higher emotional significance are more likely to be responded to by the amygdala, such that events that are highly emotional are registered at subcortical—emotional—levels. Thus, knowing with the heart can occur separately from knowing with the head.

These findings have direct implications for psychotherapy: In order to restructure emotional meaning, interventions must target not only cortical, but also subcortical levels. Although space limitations do

not permit me to go into detail as to how this might be accomplished, suffice it to say that I believe that the use of experiential therapy techniques by cognitive-behavior therapists, so as to increase emotional arousal, can hold great promise. Which brings me to the next integrative theme: Integrating the contributions from different therapeutic orientations.

Integrating the Contributions from Different Therapeutic Orientations

It is hard for me to recall exactly when I began to experience the limitations of my cognitive-behavioral orientation. I suspect it was a gradual process that brought about this change. However, I do recall one pivotal event that occurred to me in the mid-1970s when I was demonstrating the course of therapy for a group of graduate students behind a one-way mirror. The point of the demonstration was to illustrate how cognitive-behavior therapy may be used to increase self-assertiveness in an otherwise very submissive woman. Although I had carefully selected a client for whom assertiveness training would be relatively straightforward, I found myself confronted with an unexpected and repeated dilemma during the course of therapy: Should I do what my best clinical sense told me to do, or should I respond the way a cognitive-behavior therapist was supposed to respond? For example, during one session, I found myself thinking along the following lines:

This client really needs to get in better touch with her feelings, and that's what we need to be working on at this time. Not only that, but she needs feedback on how she's being submissive in dealing with me right now. But I can't do that. I would no longer be practicing cognitive-behavior therapy. It would only confuse the students. However, if I were really working with this woman clinically, that's what I would do. Why do I have this strange feeling that it would be "cheating" if I did something that wasn't behavior therapy?

With some trepidation, I decided I would finally reveal my dilemma to the students. When I went back to speak to them after the session, I confessed that I was not practicing the way I usually did, as I wanted to show them what "pure" behavior therapy looked like. They were very supportive, and they assured me that they would much prefer to watch me conduct therapy in the way I believed to be most effective. I vividly recall this experience as one in which I had "come out" from behind the one-way mirror.

Thus, I became interested in psychotherapy integration, a topic that has a long past but a short history (Goldfried & Newman, 1992). The idea of creating bridges across theoretical orientations was briefly considered in the early 1930s, but it was not until the 1980s when it moved from being a latent theme to a clear area of interest. Norcross and Newman (1992) identified a number of factors that contributed to this more recent interest in psychotherapy integration. These include the confusion and fragmentation caused by the proliferation of

different schools of thought; a realization that no given approach could successfully handle all clinical cases; a growing pressure for accountability and consensus; the focus on specific clinical problems and practical ways of dealing with them; therapists' opportunity to observe and to experiment with approaches other than their own; the development of an interest in common factors that cut across all forms of treatment; and the existence of a professional network—the Society for the Exploration of Psychotherapy Integration: SEPI—that provided a context within which consideration of psychotherapy integration could take place.

The Society for the Exploration of Psychotherapy Integration (SEPI) is an interdisciplinary organization of professionals interested in approaches to psychotherapy that are not limited by a single orientation, as well as the interrelationship between research, theory and practice. The primary objectives of SEPI are to encourage communication and to serve as a reference group for individuals interested in exploring the interface between differing approaches to psychotherapy. SEPI also serves an educational function by publishing a journal; by encouraging ongoing collaborative research on the process of psychotherapy; by keeping members up-to-date concerning books and articles relevant to rapprochement among approaches; and by sharing clinical approaches and guidelines that reflect themes of convergence and complementarity.

Although SEPI provides support for colleagues in their efforts to establish a foundation for the legitimate practice of therapies based on integrative models, this is not SEPI's mission. Because it is an educational, clinical, and scientific organization, and because it is inconsistent with SEPI's purpose to sanction any one particular approach, it does not provide certification or accreditation, endorse any approach to integration over any other, sponsor any training programs, or participate in any political activities. In short, it serves as a context in which psychotherapy integration may be studied.

One of my own interests has been to study the commonalities that exist across theoretical orientations. Setting aside theoretical jargon that is associated with different therapeutic orientations, it is possible to derive from the clinical literature a handful of common principles that cut across different schools of therapy (Goldfried & Padawer, 1982). I have suggested that these principles may be thought of as existing at a level of abstraction somewhere between the observable clinical methods and the more high-level theoretical explanations that are proposed to explain why these methods might be helpful.

To begin with, there seems to be agreement that the change process is facilitated initially by clients' *expectations that therapy will help*. Another important common mechanism of change involves the presence of an *optimal therapeutic alliance*, providing a significant interpersonal context in which change can take place. Clinicians of different orientations have also written about the therapeutic importance of *increasing clients' awareness* of alternate

ways of understanding themselves and their environment. This new awareness often sets the stage for what many believe to be at the core of therapeutic change, namely the *corrective experience*, whereby clients take the risk of behaving in a therapeutically positive way despite their anachronistic doubts and fears. Much of therapeutic change then involves an *ongoing reality testing*, consisting of an increased awareness—insight—that facilitates corrective experiences—action—which, in turn, further enhances an ongoing cycle of awareness and corrective experiences.

That these commonalities can emerge despite the varying theoretical starting points suggests that they represent very significant underlying principles of change. Still, these common principles are too general to be used therapeutically by the practicing clinician, and more detailed guidelines are needed. Thus, one may use these common principles as starting points, suggesting potentially fruitful and clinically meaningful arenas in which to conduct psychotherapy process research. The more detailed guidelines resulting from such process investigations may be thought of as parameters of these common change principles. I believe that starting from principles of change, rather than from a particular therapeutic school of thought, is likely to be more fruitful in advancing the field.

In order to advance our field, we also need to deal with the issue of integrating past contributions into present work, the next theme I would like to address.

Integrating Past Contributions into Present Work

There are times when I sit in my office and contemplate my collection of journals. To the left are the old, faded ones, and I can recall the many hours I pored over them as a student. At the time, the material in them was new and exciting. Like everyone else, however, my energies are now focused on desperately trying to keep up with those journals on the right—the current literature. Although times have certainly changed since I attended graduate school, much of what is included in the literature of the past continues to be relevant today. I doubt that many people read this literature, let alone cite it. However, it pains me when I think of those researchers and clinicians that dedicated so much time, energy and devotion to producing it, especially when I reflect on the negligible impact that much of their work has made on the field.

Why is this the case? Two of the factors that make it difficult for us to draw on contributions from the past include the language barriers that characterize the field, and also the value that is attached to what is new.

Although our theoretical language allows us to readily communicate with colleagues who share our orientation, it prevents us from reaching a consensus. Interestingly enough, however, a close reading of the literature at times reveals that once we can get beyond an author's theoretical jargon, we may find that what is being said may be quite similar to what others have said, but in different ways. For example, in 1969, Bandura described the reduction of fearful behavior as follows:

Extinction of avoidance behavior is achieved by repeated exposure to subjectively threatening stimuli under conditions designed to ensure that neither avoidance responses nor the anticipated adverse consequences occur (p. 414).

Some years earlier, Fenichel (1941), well known for his opaque use of psychoanalytic jargon, had described the very same process of fear reduction. In this description, however, he was surprising clear in his use of the vernacular:

When a person is afraid but experiences a situation in which what was feared occurs without any harm resulting, he will not immediately trust the outcome of his new experience; however, the second time he will have a little less fear, the third time still less (p. 83).

There is a long history in psychotherapy of rephrasing concepts into one's own unique language system, dating back to Freud. In fact, in 1924, Pierre Janet complained that after visiting with him, Freud began to use his own jargon in writing about what Janet had been saying all along. As stated by Janet:

In these publications, he changed . . . the terms that I was using; what I called psychological analysis he called psychoanalysis, what I called psychological system . . . [to refer to associative reactions to past trauma] he called complex; he considered repression what I considered a restriction of consciousness (p. 41).

A second factor that keeps us from building on past work is the importance given to what is new. I once asked a group of graduate students to generate as many associations that came to mind during the course of one minute when they thought of the terms "new" and "old." The results were telling. Virtually every association to "new" had positive connotations, such as "bright," "good," "better," and "fresh." Only about half of the associations to "old" were positive in nature, including such terms as "experienced," "wise," "solid," and "established." The remaining half consisted of terms like "worn out," "used," "boring," and "decrepit."

According to Webster's New Collegiate Dictionary, "new" is defined as "having existed only a short time." However, it is also said to mean "of dissimilar origin and usually of superior quality" as in "introducing new blood." By contrast, "old" is defined as "from the past—experienced," but also as "showing the effects of time or use" and "no longer in use: DISCARDED," as in "old rags."

To be sure, the focus on what's new is intrinsic to scientific progress in any field; indeed, it is the research frontier described earlier. Assuming that the field has an agreed-upon core of knowledge, it is the cutting edge that clearly should receive the most attention. When this is done in the absence of a core, however, we cannot expect the field to progress in any coherent way.

In order for our field to mature, we need to make use of our creative research and clinical energies to build upon,

rather rediscover, what we already know. As documented by Staats (1983), the citation practices in psychology leave much to be desired. However, what has been done in the past is of more than "historical interest;" it potentially represents the foundation upon which a core consensus may be achieved.

What do we need to do to get there? Clinical-research collaboration is needed; theoretical barriers must be lowered; exclusionary jargon must be translated; and professional amnesia must be overcome. Among the ways this might be accomplished are by creating a better bond between researcher and clinician, facilitating dialogues among therapists of different orientations, and making use of a common language.

The final theme I would like to address is the need to integrate gay, lesbian, and bisexual issues into mainstream psychology.

Integrating Gay, Lesbian, and Bisexual Issues into Mainstream Psychology

For most of my career, my primary clinical and research interests have centered around clinical assessment and psychotherapy, particularly in the areas of behavior therapy and psychotherapy integration. However, this is about to change. After many years of silence, I have decided that it is time for me to come out professionally—not as a gay man, but as the father of a gay son. As a result, I am beginning to devote more of my professional energies to this fifth integrative dimension: integrating gay, lesbian, and bisexual issues into mainstream psychology.

A long-standing theme in the lives of gay, lesbian and bisexual (GLB) individuals has been the need to be in hiding—to be invisible. This has also characterized the field of psychology over the years, in that GLB professionals have remained closeted. It was as if GLB psychology was not telling, and mainstream psychology was not asking.

Since the Stonewall Rebellion a little over 30 years ago, which marked the beginning of a very dramatic gay rights movement, GLB individuals have been able to be more open about who they are. This trend has also been seen within psychology, in that there has been a marked increase in professional writings about GLB issues, typically by people who have decided to come out professionally. And while psychology in general has shown support for GLB concerns, the GLB literature continues to remain invisible to those outside the area, and has not been incorporated into the mainstream body of knowledge. GLB professionals are now telling, but mainstream psychology is not listening.

There are a number of issues in the GLB literature that, although having a direct bearing on topics currently receiving attention within mainstream psychology, nonetheless continue to remain "invisible." These include: teenage suicide; substance abuse; victimization and abuse; family psychology and couple relationships; adolescent development; aging; and psychotherapy (Goldfried, 2001).

Research findings have shown that gay and lesbian youth are far more likely to attempt suicide than are their heterosexual peers; approximately one out of three gay and lesbian individuals have attempted to end their lives. A reality that is also distressing is that the mainstream literature on suicide rarely mentions that gay men and lesbians are at greater risk.

In attending a conference on adolescent suicide in the late 1990s, a member of the audience was similarly shocked to learn that none of the experts that presented their work made any mention about GLB adolescent suicide. One presenter, during the course of her talk, raised the question: "What secret could be so terrible that you would rather kill yourself than tell?" As neither the presenter nor anyone else provided an answer, this particular audience member approached her afterwards and asked why she did not mention the higher risk of suicide among lesbian and gay adolescents. Her response was: "Oh, I never even thought about them."

It has been found that GLB individuals have higher frequencies of substance abuse. However, like suicide, mainstream research and reviews of the contemporary literature fail to take into account or report sexual orientation in its writings. Client characteristics reported in current research reviews of substance abuse include race/ethnicity, gender, SES, past arrests, suicide attempts, school problems, and family conflict. Nowhere is sexual orientation mentioned.

The psychology literature on physical and sexual abuse, victimization and post-traumatic stress is extensive. And while these issues are quite problematic among GLB individuals, they are typically ignored in the mainstream literature.

Family issues play a major role in the lives of GLB individuals, involving such topics as the impact of coming out on family of origin and the considerations associated with establishing long-term partnerships. Yet, mainstream research and teaching assumes everyone in a family is heterosexual. As a result, theories about the nature of intimate relationships need to be rethought once we find that interactions believed to be linked to gender are also found to occur in same-sex partnerships.

The formation of a positive identity, a developmental task associated with adolescence, can be central to one's psychological and physical well-being. Issues involving GLB identity formation, societal stigmatization, and its consequences are rarely discussed in mainstream developmental literature.

GLB individuals face all of the same and numerous additional challenges as they grow older, and often need to confront these without family support. These issues are rarely considered in the aging literature.

Although GLB individuals frequently make use of psychotherapy services, surveys of therapists indicate that they do not feel qualified to work with this clinical population.

Despite the many advances that GLB individuals have made in society and within psychology, there nonetheless continues to be a stigma associated with doing work in this area. Typically, most of what has been done is by individuals who themselves are GLB. Consequently, for them to decide to focus on GLB issues professionally involves their coming out, as well as the possibility of putting themselves at risk of being marginalized within the mainstream community.

What can be done about this? A group of us recently formed a network within psychology of family members who have come out in open support of their own GLB relatives. Included in this network of family members are: mothers, fathers, sisters, brothers, grandparents, aunts, uncles, nieces, nephews, cousins, husbands, wives, sons and daughters. One of the goals of this network—AFFIRM: Psychologists Affirming their Gay, Lesbian and Bisexual Family—is to de-stigmatize research, practice, and teaching in this area. Bibliographies of those topics within the GLB literature having relevance to, but being ignored by mainstream psychology, have been posted on the AFFIRM Web site, to be used by mainstream professionals in their teaching, research and clinical work (www.sunysb.edu/affirm). These include bibliographies on such ignored GLB topics as adolescent development, teenage suicide, substance abuse, family and couples issues, parenting, partner abuse, aging, and psychotherapy.

Concluding Comment

In a lead article appearing in the *American Psychologist* in the early 1990s, Staats (1991) provided an account of the disunity within psychology, and pointed to where he believed we needed to head. He suggested that the progression from chaos and disunity to greater unity and consensus should involve a basic shift in our scientific goal—moving from one of preoccupation with finding the *novel*, to the inclusion of efforts to find *interrelationships*. As he argued, we need to simplify and to organize that which has already been found. At present, careers in psychology are made by making history, not knowing it. This reward system needs to be changed for us to move forward. Professional amnesia must be overcome; clinical-research collaboration is needed; theoretical boundaries must continue to be lowered; and exclusionary jargon must be translated.

How this can be brought about represents the real challenge. Thus far, no organization, no task force, and no committee have been successful in integrating psychology in general, or clinical psychology in specific. Still, such attempts at reaching consensus play a very important role in raising the consciousness of the field and in encouraging workers to devote their time and energy in moving in this direction. For example, in the 20 years that the Society for the Exploration of Psychotherapy Integration (SEPI) has been in existence, no unified statement or model has been produced. Still, I would argue that it has been successful in moving psychotherapy integration from being a latent idea to it becoming a definite area of interest. One is no longer an

outsider to be interested in integration. Indeed, the term "integration" is now prized by book publishers looking for a title that is likely to sell.

As I indicated at the outset of this paper, psychology has been struggling to form a knowledge core. I have highlighted a few areas in which integrative efforts can potentially help us move in that direction. I would like to close by referring to what Garner, Hake and Erikson suggested back in 1956, when they provided a strategy for building up a body of knowledge about a very specific area of psychology—visual perception. They raised the question of how much we had learned about perception was a function of the phenomenon itself, and how much a function of the methods that were used to study it. Their recommendation was to use more than a single methodology—as they called it, "converging operations"—to investigate the nature of perceptual processes, arguing that firm conclusions could only be drawn from studies that used different methods, but arrived at comparable findings. I believe that the same can be said in arriving at a consensus in psychology. As a field, we have a long history of taking different paths to study the same phenomena. To the extent that these different vantage points and methodologies lead us to comparable conclusions, the resulting findings are likely to be quite robust. It is here that we can find our knowledge core.

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Author Note

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Come to Think of It

Wilse B. (Bernie) Webb.
University of Florida

I am very happy to be here for this award. First off one is very happy being anywhere in a reasonably coherent state when you are in your eighties. Secondly, deep in retirement, awards are deeply appreciated. There is embedded in us the need to be affirmed—and affirmed and affirmed and affirmed. And retirement is not all that affirming. Retirement has a tendency to turn a person from Who's Who to Who is He. Daniel Schorr, in one of his many retirements, tells of the time a man came up to him and said "Didn't you used to be Daniel Schorr?" Last October The *APA Monitor* had a sleeping beauty on it's cover and the cover title was Sleep: Research and Practice. I eagerly thumbed to the section. Alas, not a mention of my name. Me, the guy, who was a leading character on the topic of sleep in every Introductory Text for decades. *Sic transit gloria mundi.*

Believe me. I really do appreciate this. My particular thanks go to, Don Dewsbury, my long time colleague, who did the spade work and those expert writers of letters of support which included six APA presidents whose friendship and eminence certainly help my cause. It is particularly appropriate that this is an award of Division 1 of General Psychology, a division that frequently seems to be going through an identity crisis. When I started my career veer into sleep research in the late 1950s it was not a hot topic to say the least. Finding journals for research papers and finding places on programs was no easy task. I remember one program chairman calling me to discuss the problem. I asked him if he had a General Psychology section. "Yes", he replied, "But sleep is not General enough".

I am going to review some eight choice points that lead me to being on this podium. I do this for several reasons. The first is simply self-indulgence, and since this is my occasion that should be permissible. Second, I was to use this opportunity to give to the younger of you a glimpse of a very different psychology. But more critically, I want use these incidents as a background for stating what I think psychology is not and what I wish for it to be in the future.

Choice point 1: I went to LSU. Of the people from Yazoo City, Mississippi who went to college, the city boys went to Ole Miss, the country boys went to Mississippi State A

& M and the girls, of course, went to the Mississippi State College for Women. But this was in deep depression times and my father had lost his lumberyard and my mother, who was from Louisiana, had a brother in Baton Rouge who could furnish me free room and board.

Choice point 2: I majored in Psychology. I was supposed to go to medical school. My great grandfather and grandfather had been physicians. But I played my way through three years and my only As were in English. Then, like so many others, I met my teacher—Dr. Harry Capps. He was a most unlikely person to be in the Louisiana Cajun country. He had just received his degree from Columbia University. He was born and raised in New York of an immigrant Jewish family, and, in retrospect, he was a closet homosexual. But he was brilliant and he was intensively excited about psychology. I spent the rest of the time at LSU earning As in all the courses that the psychology department had to offer. My mentor arranged an Assistantship at the University of Tennessee.

Bernie Webb is the 2002 Winner of the Ernest R. Hilgard Award for Lifetime Achievement in General Psychology awarded by the Society for General Psychology. This is the text of Bernie's Award Address given at the convention of the American Psychological Association in Chicago, August, 2002.

Choice point 3: I went to Iowa. Capps had done his dissertation on epilepsy and I got a job in the Mississippi State Hospital for the Insane outside of Jackson, which, in those days of no available cures, was only a step up from Bedlam. I spent the weekends socializing with the lads and lasses of Jackson. A fraternity brother, a senior in the art school at Iowa, suggested I join him there with the arrangement that I become the fraternity house graduate advisor for room and board. Since this was considerably more than my assistantship at Tennessee I agreed. In those pre GRE and admission committees I simply enrolled at Iowa. Unbeknownst to me, I had entered one of the hot beds of psychology's graduate

programs. Still under the influence of Capps, I started off in clinical psychology which was growing out of Wendell Johnson's speech clinic.

Choice point 4: I went to war. In my first semester, on December 7th, 1941, Pearl Harbor happened. The Aviation Psychology Program of the Army Air Force, for months had been advertising the opportunity for enlistment and no one had taken notice of it. Within a few months half the males in the graduate program had marched off to war to join aviation psychology programs. I spent four years learning applied aviation research among some of the best young psychologists in the country and, in the process, I ended up on Okinawa flying on strike missions against Japan in search of valid combat criteria.

Choice point 5: I changed from clinical psychology to experimental psychology. I had been granted an MA on my thesis on the use of the Wechsler-Bellvue as a measure of mental deterioration that I finished while in service. However, Kenneth Spence had become chairman of the Department in our absence and his presence and the lure of neobehaviorism was irresistible in the 1940s. Furthermore clinical psychology was undeveloped and seemed to offer the bleak prospect of insane asylums.

I set out into the academic world by becoming an assistant professor at the University of Tennessee and switched to Washington University in St. Louis. In those days some three moves in your early years were typical. I was happily publishing three or four paper a year and was a swash-buckling teacher. These were the days before research grants were widely available and I began supplementing my income in the summers by doing contract research with the Air Force and Naval Aviation.

Choice point 6: I left the academic world. I became the head of the Aviation Psychology Laboratory at the Naval School of Aviation Medicine. I not sure I will ever be able to explain that great leap. But I had enjoyed the adventurous ways of applied psychology and certainly one factor was a big increase in salary which made a difference with four children. Choice point 7: I entered sleep research. The venue to my Laboratory was research on the selection and training of naval aviators. I had about a dozen research staff. My main job was to keep the staff focused on applied research. One certainty was that my six year investment in research and publishing in Hull-Spence learning theory was totally irrelevant and had to be abandoned. My own research became focused on aviation accidents.

One day, in our small library, I ran across Kleitman's remarkable book, *Sleep and Waking*, published in 1937. It summarized the extant research on sleep and waking. From somewhere my years of toiling in the vineyards of Hullian theorizing came flooding in. Why not consider sleep as a behavioral response and see if one could develop a theoretical system to predict and control sleep behavior. The physiology section of the School of Aviation medicine had a small rat laboratory. Within a year I had devised and completed an experiment which had varied drive level (time awake), habits (number of repetitions of

sleep in a marked environment) and an irrelevant drive (hungry/not hungry) to determine their effect on sleep latency. This was published in the *Journal of Experimental Psychology* in 1957.

Choice point 8: I returned to the Academic. As I had found with learning and motivation research, a research program requires a one-track orientation and I clearly could not join accident research and sleep research. Furthermore, although applied research is exciting and challenging as a problem solving activity, I didn't find it intellectually satisfying. In 1958 there was the opportunity to return to the academic world as Chairman at the University of Florida. I took that opportunity to begin my thirty-year journey into sleep research.

And now comes that inevitable moment of speeches on grand occasions, which is introduced by the phrase "But seriously...". So, I say, "but seriously" I would like to use my choice points to comment briefly on two related topics. What Psychology is not and what Psychology could and should be.

When I entered psychology in the late 1930s the introductory texts almost universally described prediction and control of behavior as the focus of psychology. This was the lingering legacy of the Watsonian revolution in pragmatic and utilitarian America. But I would point out that each of my choice points were completely unpredictable from the previous choice in point. Yazoo City did not predict LSU. My choice of psychology was dependent on an unlikely appearance of a teacher from New York. Iowa emerged from odd circumstances. The war was not predictable. This unpredictability from antecedents I believe is typical of all individual human behavior with the exception of those controlled by strong habits or physical constraints.

I do not find this surprising. When I emerged from the rat laboratories, where prediction and control is the touchstone of research, and entered accident research the unpredictability of behavior was quite apparent. It became epistemologically apparent when I examined the problem of attributions of causality in my presidential address to the Division of History in a paper entitled "Writing history and accident Reports: A metaphorical analysis" (Webb, 1998). I compared the writing of history to the writing of an accident report and to the answering of such psychological questions as "Why can't my Johnny read?" or "Why did he commit suicide" or "Why did he get a divorce?". I discussed the formidable problems of determining and attributing causality. The paper cited that notable expert in the search for the causes of behavioral acts, Sigmund Freud (1920). He acutely described the core of the problem: "So long as we trace the development (of an event) from the final outcome backwards, the chain of events appears continuous... But if we proceed in the reverse direction, if we start with the premises inferred from the analyses and try to follow these up to the final results, then we no longer get the impression of an inevitable sequence of events which could not have otherwise determined... in other words, from the knowledge of the premises we could not have

foretold the nature of the results ...” (Freud, 1920, pp 203-231)

More recently, Manicus and Secord (1983) presented a telling analysis of the problem: “On the realist view, events such as the collapse of bridges or a cancerous growth of an organ are the conjunction of causal processes operating in an open system ... in such events causal explanation ... requires retrodiction to possible causes and the elimination of alternatives ... Accordingly, we may often be in the position to explain some event once it has occurred, when it would have been impossible ... even in principle to predict it. (p. 403).”

I do not think that we can or will be able to predict individual behaviors. Although, on the basis of behavioral measures, such as test results, we can make probability predictions, most of our correlations coefficients linger well below .50, resulting in embarrassingly large numbers of individual false positives and negative predictions. As a consequence we cannot predict where or when violence will occur in the next school, which criminal will or will not return to prison, which student will graduate from college, who will die in an auto accident, which marriage will endure. That being the case to make psychology’s central goal that of predicting behavior seems a folly.

As for controlling behavior we don’t seem to be doing so well. So far as I know there we have not developed effective means of controlling individual behavior. I do not know of any effective psychologically developed methods of controlling addictions, weight problems, and recidivisms nor dispersing phobias or depressions or dyslexias. I think I know why. The exercise of control, whether in the conduct of research, the prevention of an accident or an unwanted behavior or the guidance of behavior to a desired end is effectively accomplished by one and only one means—the loss of individual freedom. And this core human virtue is clung to tenaciously.

If one is interested in the prediction of behavior I would advise them into entering such applied fields as polling or market research or personnel selection where probability figures may be useful. Or if interested in control I suggest that they enter into law enforcement or legislation.

What then is psychology? What is the common core that knits together my wandering from clinical psychology to animal learning to accident research to sleep research? What shapes the great diversities relating brains and correlation matrices, rats and neuroses, personnel selection and aesthetics? In a widely unread advanced general psychology text, *The Profession of Psychology (1962)*, I wrote that I believed that the common core that defines and binds all psychological pursuits was quite clear and simple. It is not some grand unifying theory, nor some central “natural philosophy”, nor some vital force. It is simply the belief that behavior is lawful in all of its manifold dimensions of actions, feelings, thoughts and social processes. In short, the science and applications of psychology is the acting out of the belief that behavior is lawful.

From this perspective what should be the role of psychology in society? I believe that psychology, like the other sciences, emerged as societal efforts to better understand the lawful patterns underlying the natural world. The late emerging psychology it has been assigned systematic effort to understand human behavior. It is our mandate to systematically determine the laws of human behavior and to make these available. We join the other scientists, the physicists, the chemists, the geologists, the astronomers, the biologists, the physiologists, and the neuroscientists in their search for the underlying law of nature.

It follows from this that the primary role of psychologists is to attempt to discover the laws of human behavior and to teach of these principles. In short, I am suggesting an amplification of the George Miller proviso: the role of psychology is the discovery of the principles of human behavior and giving them away. The primary place of the psychologist should be in academic institutions, at the undergraduate and graduate levels, where these principles, derived by our science, should be taught, *i. e.*, given away, and in research laboratories in graduate schools, and institutional and applied laboratories where research is extended.

I would note that this is the current academic model of our older scientific predecessors such as physics and chemistry and biology. Indeed, this format differs little from the Arts and Sciences Psychology departments of most of the aspiring research universities of today. The tenured faculty of these departments is almost entirely comprised of research psychologists who are giving psychology away to both graduates and undergraduates. The graduates of these departments would be employed in academic departments or applied and institutional research laboratories.

But what of the applications of psychology? I am all for it. These are the societally useful application of psychological principles and the practitioners the specialists in such applications. These specialists would be taught in Colleges, Schools or Institutes of Psychology that were free standing or associated with Universities (similar to law schools). The faculties would be comprised of full time and adjunct professional psychologists from the various viable specialties clinical, counseling, organizational, human engineering, forensic, educational and school, and sports psychology. Some institutions would be narrowly focussed and other wide ranging. It is likely that they would use the PsyD degree.

The development of training milieus, internships, subdoctoral training levels, and issues associate with the levels and licensing of practice, reimbursement, prescriptions and other turf wars would be determined by these programs and their graduates and the research psychologists could return to concerns about authorship, review boards and animal welfare. The graduates of these programs would interpret and utilize the developing laws of human behavior in the wide range of applied settings as well as teaching the principles of human behavior in Colleges of Medicine, Law, Business, Education, and Engineering.

But it now it is time for me to close. It is clear that my picture of the ideal future of psychology has arrived where I began and I have gone back to halcyon days of my past. I have sometimes called these the BC days ... before computers (the calculator was a big advance for us), before the central nervous system (which in my day was called the black box), before clinical psychology (in our time known as abnormal psychology). The Departments of Psychology were devoted to academics and entering the field was a pathway to shabby gentility, probably in a four year college. The path to fame was research. Most of all, giants walked the lands dreaming theories of the nature of man—Hull, Tolman, Guthrie, Kohler, Kurt Lewin, and latter day saints like Kenneth Spence, Skinner, and a young George Miller.

I count my blessings of having lived in those times to be permitted me to wander in a wonderful world unhampered by GRE's and admission committees, tutored by wise ones, tempered by a war, riding a wave of job opportunities, and finding an unexploited research field which required few technical and mathematical skills.

I thank whatever Gods that be and you for having noticed.

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Twenty-Fourth Annual Symposium Eminent Women in Psychology Historical and Personal Perspectives

Excerpts

Agnes N. O'Connell, Chair and Discussant Montclair State University

For the last twenty-four years at the APA annual meeting, the Symposium on Eminent Women in Psychology: Historical and Personal Perspectives has celebrated women's lives and strengths, their heritage in APA and in the field of psychology. The idea for presenting reflections of eminent women in psychology was generated from a series of workshops at national and regional conventions that I chaired as Head of the Division 35 Task Force on Women Doing Research in the mid-1970s. These workshops underscored the need to preserve the contributions of women to the field of psychology as well as the need to highlight women who were strong, resilient role models. The Task Force recommended that the stories of eminent women be made known (O'Connell, A. N., Alpert, J., Richardson, M. S., Rotter, N., Ruble, D. N., & Unger, R. K., 1978). The first Symposium on Eminent Women in Psychology originated an extraordinary and unusual annual tradition at the APA annual meetings.

I organized and chaired the first Symposium in 1979 featuring Mary Ainsworth, Margaret Hubbard Jones, Molly Harrower, and Mary Henle with Dorothy Eichorn and Stephanie Shields as discussants. Nancy Russo served as discussant in 1980 and continued with me as coauthor and coeditor for works on eminent women for more than a decade (O'Connell & Russo, 1980; 1983; 1988; 1990; 1991). For the APA Centennial in 1992 and the 50th Anniversary of the APA Divisions in 1996, I developed with Louise Vetter a large exhibit of more than 100 brief biographies and photographs of eminent women in psychology. The latest published work, *Models of Achievement: Reflections of Eminent Women in Psychology, Volume 3* (O'Connell, 2001), contains 429 pages of autobiographies, integrative overview chapters of the sociohistorical context of the twentieth century, and in-depth analyses of the patterns and profiles of achievement of 53 eminent women.

Cumulatively, these reflections and assessments provide a major knowledge base for the study of women's lives and the evolution of psychology at the same time as they increase the visibility of distinguished women and their contributions, act as a source of inspiration, and serve as a catalyst for transformations of psychology and society.

The participants for this 24th Symposium, like their predecessors, are senior psychologists who have been recognized for making pioneering, groundbreaking "outstanding and unusual contributions" to psychology—achieving Fellow status in APA and other professional organizations, receiving prestigious national awards, publishing significant books and hundreds of journal articles, serving as editors and on national boards and committees, making hundreds of professional presentations, and holding major leadership positions. The impressive honors they have received and the impact of their various contributions on psychology underscore their distinguished status and facilitate their serving as role models.

Nancy E. Cantor

A prominent cognitive, personality, and social psychologist, Dr. Cantor is Chancellor and Professor of Psychology at the University of Illinois at Urbana-Champaign. She was former Provost and Executive Vice President of Academic Affairs at Michigan University. Her books include Personality, Cognition, and Social Interaction; Personality and Social Intelligence; and Personality Psychology: Recent Trends and Emerging Directions. Her awards and honors include the APA Distinguished Scientific Award for Early Career Contribution to Psychology (1985), the Women of Achievement Award of the Anti-Defamation League, the US Department of Labor Women's Bureau Work-Life Family Award, Fellow status in the American Academy of Arts and Sciences, APA Division 8 and APS, and membership in the Institute of Medicine of the National Academy of Science. She has served as President of APA Division 8 (1992-1993), Vice Chair of the National Research Council Committee on Women in Science and Engineering, Vice Chair of the Board of Directors of the American Association for Higher Education, and on numerous other national boards.

My work has focused on four aspects of personality and social behavior that I see as critical to individual health and well-being. Beginning with what might be called "constructive cognition," it is important to understand the particulars of an individual's

The Symposium, sponsored by Division One, was presented at the Convention of the American Psychological Association in Chicago, August, 2002.

construal of a situation or event in order to then make sense of his or her goals, strategies, and purposes – *what he or she is trying to do*. Construals and goals, in turn, set the stage for individuals to draw upon *social affordances* (support; partnerships, unique opportunities) in the service of making progress on personal goals. Finally, as we pursue our goals across the life course, there are opportunities to engage in new tasks and to set new goals, thus fundamentally *changing* personality.

Perhaps not surprisingly, my scholarly work has very much informed my approach to the issues in higher education that I face as Chancellor at Illinois. For example, my commitment to working on diversity – intellectual and social – is clearly influenced by the belief that we will create the most vibrant intellectual environments for scholarship and teaching when we bring alternative construals to bear on the issues of debate in the laboratory and in the classroom. Similarly, I believe that the best educational environment is one that simultaneously cultivates in students the desire to try new things and think in new ways in the “protected” space of the campus, and the desire to remain socially responsible and connected to issues of pressing concern in the world beyond the campus. In a related vein, I very much hope that our institutions can develop practices that enable faculty to take on new roles and tasks over time and to work in collaborative and interdisciplinary environments that provide opportunities for professional growth.

On a more personal note, I suspect that my interest in the diversity of individuals’ life experiences, beliefs, and values, and in the benefits of bringing these differences together, derives from my upbringing in New York City, from the social activism of my family, and from the impact on me of major social movements afoot in my youth (e.g., civil rights movement, women’s movement, anti-war movement). I have also been fortunate to have received great sustenance in my career (and life) from wonderful women colleagues. Through some mix of these experiences, I have come to believe strongly in the possibility for beliefs to mobilize goals, and for individuals to work within our social environments to effect change for the better.

Jean Lau Chin

A recognized expert and leader in cultural competence, psychotherapy, community mental health, women’s issues, and Asian American issues, Dr. Chin is the President of APA Division 35. She is President of CEO Services, a clinical and consulting firm, Clinical Director of the Grief Counseling Program, and core faculty at the Center for Multicultural Training in Psychology at the Boston University School of Medicine. She is the Praeger Series Editor for Race and Ethnicity in Psychology. Her books include Relationships Among Asian American Women; Community Health Psychology; Diversity in Psychotherapy: Race Ethnicity, and Gender; and Transference and Empathy in Asian American Psychotherapy. She is APA Fellow in Divisions 12, 35, and 45; has served as Chair of the APA Committee on Professional

Practice and Standards and the APA Committee on Ethnic Minority Affairs. She is recipient of the Distinguished Contribution Award of the Asian American Psychological Association (2001), Outstanding Women Award from Patriot’s Trail Girl Scouts (1999), Leading Women Award from Women in Philanthropy (1991), and other awards. She is currently on the Center for Substance Abuse Prevention National Advisory Council and has served on numerous other national boards.

My parents were Chinese laundry owners, the occupation of probably 80% of the immigrant Chinese American families of my generation, an occupation of choice because there were no choices following the anti-Chinese legislation of the times. An emphasis on Chinese cultural values and practices was an important part of my early upbringing.

During my education, I was the only, or one of three Asian students in the schools I attended (including Brooklyn College and Columbia University—Teachers College). Most significant to my education is what I would describe as the differences between the Confucian and Socratic methods of learning. Confucian learning occurs through listening and taking in, and one benefits from the wisdom and knowledge of one’s elders and the masters. This contrasts with the Socratic and western forms of learning that emphasize vocal classroom participation, challenging the status quo, and changing the obvious. I learned of these differences, without mentors to guide me, and often found myself subject to misunderstanding and misjudgment.

What is significant about my career path is my transformation and transcendence as a professional and as a person. To all my roles, *I brought my ability to look at things from the outside* and felt I contributed to an innovative and transformational approach. The second striking feature of my career development was the tendency of others to view me in rather stereotypic ways. My small size, distinct Asian culture, and different professional training all contributed to stereotypic expectations. Negatively, it resulted in a questioning of my managerial, clinical, and professional abilities. Positively, I was able to remain anchored in the pride I had gained about who I was, and in what I believed. Not only had I learned that I thought differently, but also I had learned to advocate for that which I held dear, i.e., serving the underserved, promoting ethnic minority issues, social equity, valuing differences, and cultural competence.

I believe I have been a rare type of psychologist with a foot in clinical work, management, and scholarly pursuits. My diverse interests and work mirror my commitment to diversity and difference. The contrast between the cultures of the East and West summarize the challenges of integration for those of us living in a bicultural environment. There is much that cannot be integrated. Not only is the vocabulary and language structure of English and Chinese very different, but so is the different emphasis of social and cultural values. These differences require a

consciousness and vigilance that is unnecessary in the absence of racial/ethnic differences. Social gatherings and differences in social customs, food, and values must be contemplated. Sometimes it means leading different lives that do not always mix. I have found my sources of strength and support in the many colleagues with whom I have shared "one mind." I learned that my strategies for resolving conflict sometimes means not to seek integration, but to seek divergence in the things we do and the thoughts we have.

Another challenge has been that of being an Asian American woman. Few or no Asian women went on for their doctorates in my era, much less major in psychology. From my experiences, I have learned the importance of perseverance, and of openness to new learning. I have learned that the need for integration must be balanced by the valuing of differences and the need for divergence. The interaction of stereotypes, culture, and one's self pervades one's personal and professional life. Students and aspiring professionals should allow themselves to think out of the box, but to be true to themselves and their cultures. For students and aspiring professionals, I'll end with one of my mother's sayings. "Learning anything is good. No learning is ever wasted."

Rochel Gelman

*An internationally recognized scholar in the area of developmental cognitive science, Dr. Gelman is Co-Director of Cognitive Science and Professor of Psychology at Rutgers University and Professor Emerita of Psychology at UCLA. Her books include *The Child's Understanding of Number* that has been translated into Japanese and Italian; *Attention in Learning: Research and Theory* that is a citation classic, and *Cognitive and Perceptual Development*. Several of her hundreds of journal articles and chapters have been translated into Chinese and Japanese or become citation classics. Dr. Gelman is a Fellow in the American Academy of Arts and Sciences, an APS William James Fellow, a Fellow in APA Divisions 3 and 7, and an Inaugural Fellow in the Cognitive Science Society. She has served as President of Division 7; and is the recipient of the APA Early Career Research Contribution Award (1976), the APA Distinguished Scientific Contribution Award (1995), and the APA Division 7 Outstanding Mentor Award. She has served on the Board of Directors of the Piaget Society, the APS Governing Board, and the National Research Council Board of Behavioral, Cognitive, and Sensory Sciences and is coeditor of the *International Encyclopedia of Psychology, Developmental Sections*.*

I had no idea that I would end up an academic, let alone in this symposium. My parents emigrated to Toronto in the 20's having their education cut short in elementary school. Although there were almost no books at home, the tradition of education was omnipresent in my environment, which included a prosperous neighborhood that was overwhelmingly Jewish. Like other kids, I expected to do well at school

and listen to my teachers. The truth was that I preferred the Math and Science track because it meant that I would not have to memorize historical dates and the names of lots of plants. This left time for me to play in the band, continue music lessons, and socialize.

It was pretty much by accident that I ended up in the challenging Honours Psychology program at the University of Toronto and a whole new world, on both the cultural and educational side, opened up. I learned what it was like to have intellectual passion. My own was nurtured by the newly recruited members of the department, including Amsel, Berlyne, Mandler, Tulving, Walters. Importantly, no one gave the impression that women were less capable than men. We simply were all scientists in training, learning to do research, give presentations, and become serious scholars, bound for advanced study. The Psychology program outside requirements meant that I studied genetics and the Associationists. All of this has turned out to be deeply relevant to the gradual emergence of my views about concept acquisition and the nature of the learning involved. So did the fact that both my undergraduate and graduate education overlapped the Cognitive Revolution. As for my choice of graduate programs, another accident of luck. I, like my family, assumed I would go on at Toronto. George Mandler, the undergraduate chair had other ideas. I landed at UCLA during the early '60's. More luck. This was a great place at that time. The Mind was back in favor, the campus and the city were at the forefront of cultural developments, and political hope was in the air.

I am grateful for the support and intellectual freedom my terrific advisors (Jeffrey and Traabasso) gave me. I continued to study learning (especially as it permeated the work of Mathematical Psychology) and joined it with the field of development. I am sure that this double specialization influenced the design of my dissertation, an early successful training demonstration involving children who initially failed a battery of Piagetian conservation tasks. There was one problem. The children learned much too fast given the standard assumption that they lacked the relevant conceptual structure. So, I went in search of ways to uncover early cognitive competencies. I, and my many wonderful students, have put out a series of papers that has contributed to a major change in the empirical and theoretical landscape of cognitive development. There now is a multitude of illustrations of task settings where young children and even infants reveal pockets of competence. These demonstrations have forced me to re-examine my initial commitment to the traditional view of concept acquisition that has permeated the field, one based firmly on the Associationist account. I now am in search of a theory of learning that is compatible with a Rationalist position, hence my choice of such phrases as Rational Constructivism or Innate Learning. I know that many think such phrases are

self-contradictory. Yes, this is true for an Associationist theory. But it is not true for all theories of learning (See Gelman & Williams, 1998). There can be no question that experience feeds acquisitions, be these biologically favored or not. The issue is how to characterize supporting experiences and the nature of the learning processes that use these. My own position has shifted gradually from the associationist account of these for a straightforward reason. The cumulative data forced it.

As an academic I have had my ups and downs, including ones related to illness and difficult work settings. I am grateful for the amazing support of my husband and son, Randy and Adam Gallistel, as well as my many talented students who created the Friends of Rochel at an especially difficult time. Of all of my awards, I am proudest of the most recent one, Division 7's Outstanding Mentor award, which will be presented in Toronto.

Florence W. Kaslow

Director of the Florida Couples and Family Institute and President of Kaslow Associates, PA. Dr. Kaslow is a diplomate in forensic, clinical, family, sports psychology and sex therapy. Her many books include Voices in Family Psychology, Vols. I and II; The Military Family in Peace and War; Handbook of Relational Diagnosis; Painful Partings: Divorce and Its Aftermath; Handbook of Couple and Family Forensic Issues: A Sourcebook for Legal and Mental Health Professionals; and Comprehensive Handbook of Psychotherapy. Her many awards and honors include the APA Distinguished Contribution to Applied Psychology as a Professional Practice Award (1989), ABPP Award for Distinguished Service and Outstanding Contributions (1994), and the APA Distinguished Contributions to the International Advancement of Psychology Award (2000), and Fellow status in APA Divisions 12, 29, 37, 41, 42, 43, and 46. She founded specialized fields in psychology serving as the first President of the American Board of Forensic Psychology, first President of the International Family Therapy Association, and second President of APA Division 43. She has served as President of APA Division 46, the American Board of Family Psychology, and the Florida Association of Professional Family Mediators and as editor of the Journal of Marital and Family Therapy.

I started working at age twelve and have not stopped working ever since – as a baby sitter, sales girl, dancing teacher, secretary, high school and college teacher, University and Medical School Professor and Dean, author and editor, and individual, couples and family therapist and coach. I worked out of necessity because my parents could not afford to pay for my college and graduate studies despite their emphasis on education. My ambition, determination, high grades, and extra curricular activities enabled me to get scholarships, fellowships and a grant for my dissertation writing.

Hearing tales of the various Eastern European countries my forbears and their extended "mishpucha" (in-law families) came from stimulated my interest, as did the many books I avidly read, about

"far away places with strange sounding names." When my husband, Sol, and I got married and started our family, we became a host family for a student from a different country each year. Valuing multiculturalism and having a great deal of sensitivity to, and appreciation of, all kinds of diversity were important principles for us. After a ten-year hiatus to get married, begin my career, and have two children, I did my doctoral studies at Bryn Mawr College.

There were no mentors on my horizon. I received a great deal of encouragement but had to find my own path. Perhaps that is why during my career when I identify a gap or a need, I launch an effort to fill it. For example, most of the 22 books I have authored or edited, plus the over 160 articles I have had published, grew out of looking for something about a topic in the literature that I could not find or needing a text to teach a course that did not exist. As head of a Section of Forensic Psychology/Psychiatry at Hahnemann Medical College in Philadelphia in the mid 1970's, I established and co-directed a Ph.D./J.D. program between Hahnemann and Villanova Law School, the third such program in the country.

My fascination with families led to family psychology being another major area of professional involvement. I helped spearhead the formation of APA Division of Family Psychology in the mid 1980's and became its second President and an early recipient of the Family Psychologist of the Year Award (1987). Today I serve as the Division's representative to Council, while my daughter is serving as Division President. In 1987 at an international family therapy conference in Prague, Czechoslovakia, we formed the International Family Therapy Association (IFTA) and I was elected the first President (1987-1990). In 1990 at a meeting of IAAP in Japan I was asked to be a founding member (the only woman) of the International Academy of Family Psychology and served as its President from 1998-2002.

One of my most important contributions is the Holocaust Dialogue Group between 25 Jewish and German second and third generation descendants of perpetrators and victims, which I have led annually since 1994 and that is held in conjunction with the IFTA meetings. Each session is a moving, gripping saga aimed at healing the pain that has been transmitted; these in-gatherings also add meaning and purpose to mine.

Along the way I've enjoyed mentoring students, encouraging colleagues to write and publish and making friends in many countries, hopefully exemplifying some of the principles, values and beliefs articulated in my work. Psychology has provided me with many wonderful avenues to explore and on which to build. To be an "eminent woman" is delicious icing on a rich and ever expanding cake.