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## It's Official

The name of the Division is now The Society for General Psychology: Division One of the American Psychological Association.



Inside this issue you will find a hopefully controversial article on the treatment of schizophrenia written by **Bert Karon**. Old friend and Division mainstay **Greg Kimble** was interviewed on the occasion of a number of things including his receiving two awards, one by APA and one by the Society. His responses are included within. An invited address at the last APA convention by current Society President **Kurt Salzinger** is added as an extra treat. Then there are a number of business items including info about candidates for the upcoming Society elections and the minutes of the last Society Executive Committee meeting. Enjoy.

# **THE TRAGEDY OF SCHIZOPHRENIA**

**Bertram P. Karon, Ph.D.  
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The real tragedy of schizophrenia is not the severity of the symptoms and the suffering that results for patients and families, but that we know treatments that work and are not using them. Families and patients are settling for treatments that aim at making the patient a lifelong cripple who is not too disturbing. Even when medications are maintained, two-thirds of patients will be re-hospitalized within two years. Psycho-educational programs, which potentially could be helpful, usually give false information which make worse the burdens of both patients and their families.

Eugen Bleuler described schizophrenia as consisting of: autism (that is, impaired relations with other people); thought disorder (that is, the inability to think logically when you want to); and seemingly no affect or inappropriate affect; and the more dramatic symptoms which might or might not be present: hallucinations, delusions, and/or catatonic stupor. DSM-IV simply spells this out in a way that permits reliable diagnosis, but with scientific shortcomings.

The most important research on schizophrenia in the last twenty years are the long term follow up studies.

Kraepelin (1907), Eugen Bleuler (1911), and Manfred Bleuler (1971; 1978) taught that schizophrenia was a chronic disorder, that it might have remissions, but that the outlook was poor in the long run. But as Manfred Bleuler pointed out, they were misled because they followed only hospitalized and re-hospitalized patients. When Ciompi (1980) published 40 year follow-up data for patients in Switzerland, from 1900 on, the course of

schizophrenia was found to be highly variable, seeming more like the vicissitudes of life than the course of a disease or several diseases. This and the other four studies which followed schizophrenics for more than 25 years, from Switzerland, Germany, and the United States, were summarized in 1987 by Harding, Zubin, and Strauss (1987), who noted that 30% fully recovered in the long run, and that 60% to 70% became self-sufficient. This was not due to any modern treatment. Moreover, re-diagnosing the patients using DSM-III diagnoses, which are essentially the same as DSM-IV, in place of earlier diagnostic criteria, made no difference in predicting the long-term outcomes in Harding's study.

Harding (1988) pointed out that, even though professionals who have encouraged patients to take their medication have been well meaning, in this study all of the patients who fully recovered were among the 50% who had stopped taking their medication. This could mean either that the healthier patients feel freer to stop, despite their doctor's advice, or that the medication, helpful in the short run, prevents full recovery.

More recently, Harding (1995) has summarized five more studies, for a total of ten, with somewhat more variability,

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leading to the conservative recommendation that professionals tell patients and families:

"You have a serious illness but results from 10 worldwide studies show that you have a 50-50 or better chance of significant improvement and perhaps recovery. It may take a long time, but we will be here to help you maximize the return to functioning."

Nonetheless, DSM-IV (American Psychiatric Association, 1994, p. 282) states that "Complete remission (i.e., a return to full premorbid functioning) is probably not common in this disorder." a statement not only denying the existence of patients who have recovered with appropriate treatment, but contradicting the findings of every long term follow up study. Professionals regularly tell patients and their families that the patients will never get better, robbing them of hope.

Patients are told they must never have children. But as Manfred Bleuler first reported (1978), the data are that 80% of children of schizophrenics raised by their schizophrenic parent never become schizophrenic. And the 20% rate of schizophrenia can be lowered by preventive counseling.

At the end of the eighteenth century, before modern psychotherapy or modern medication, Phillipe Pinel (Bockoven, 1972) and the other practitioners of moral treatment innovated treatments that worked, contradicting the earlier myth of incurability. In France, England, Scotland, and the United States, moral treatment produced startling results: 60 to 80% of patients were discharged.

Its elements were simple: first, no cruelty, no humiliation, use physical force only to prevent the patient harming him or herself or someone else, but not for punishment nor, to use modern jargon, negative reinforcement. Second, get as accurate a case history as possible (you might learn something about this patient or even about such patients in general). Third, encourage work and social relations. Finally, most powerful and seemingly unscientific do your best to understand the patient as an individual human being. But from the records of hospitals that continued in existence, not only were the results better than those previous, they were better than the middle and end of the nineteenth century and the beginning of the twentieth century when moral treatment was abandoned, and the discharge rates dropped to 20 to 30%.

Moral treatment was succeeded by physical treatments claiming to be more scientific, that did not require understanding the patient, and which had economic advantages for the public sector as long as one was not worried that patients were not getting better. The myth of incurability was reassuring (Alexander & Selesnick, 1966; Bockoven, 1972; Whitaker, 1992).

But Eugen Bleuler noted that when he read Freud and applied psychoanalytic ideas to his schizophrenic patients at Burgholzli, three times as many were discharged (Federn, 1943). By the middle of the twentieth century, psychodynamic psychotherapy and other psychological treatments were developed. When they were

most frequently used, patients benefited. Today they have largely been abandoned, the patients have stopped getting better, and psychotherapies have been succeeded by physical treatments that claim to be more scientific, which do not require understanding the patients, and which have economic advantages for everyone except the patients. And the myth of incurability has been re-created. Comprehensive surveys of outcome studies in the United States show 28% favorable outcome before 1925; 49% from 1956-1986 when psychotherapy was most likely to be offered; and 36% from 1986 to 1994 (Hegarty, et al., 1994).

In the 1930s, Harry Stack Sullivan and Frieda Fromm-Reichmann consistently helped schizophrenics. The treatment was arduous, but patients improved (Fromm-Reichmann, 1950; Sullivan, 1953). The well-known novel, *I Never Promised You a Rose Garden* (Greenberg, 1964), described that early treatment. The author, who had been a patient of Fromm-Reichmann, used an assumed name. It was only after later novels under her own name became popular that she finally attached her name to this book, which not only describes such treatment, but demonstrates the kind of recovery that allowed the patient to write so well.

To understand schizophrenic persons is to grasp painful facts about the human condition that we would rather not know. The sociological data about schizophrenia remind us of unpleasant realities. For example, a disproportionately greater incidence and prevalence of schizophrenic disorders is associated with low socioeconomic status (Hollingshead & Redlich, 1958), which cannot be accounted for by downward drift. While this is disputed, current data are still consistent (Cohen, 1993). This suggests, and psychotherapeutic experience makes vivid, the physical and psychological pain, humiliation, and physical danger associated with being very poor in our society create realities which those of us who are not very poor do not like to perceive or remember. Similarly, schizophrenic disorders are more common among those who are the victims of prejudice and discrimination (Karon, 1975). Thus, the psychotherapist, to be effective, will often be confronted with the ugliness of the economic, racial, ethnic, and religious discrimination which have contributed to these disorders. The fact that the long-term prognosis for schizophrenics is better in nonliterate cultures (Sartorius, Jablensky, & Shapiro, 1978) reminds us of the relative lack of kindness in our civilization.

Many schizophrenics have talked about incest, sexual abuse, and physical abuse as problems; but such talk nearly always has been dismissed as the ravings of lunatics (Rieger, 1896). When Freud, referring specifically to conversion hysterics, reported that the incest memories they related in psychoanalysis were revealed more often to be fantasies than real events, although in many cases, according to Freud, they were undoubtedly real events (Freud, 1917, p. 370), psychology, psychiatry, and psychoanalysis (but not Freud) falsely generalized that all such memories of all patients were only fantasies, because it was believed incest was a rare event (e.g., J. Strachey and Jones as cited by Masson, 1984, p. 213).

Therapists and researchers who worked with

schizophrenics (e.g., Lidz, 1973), however, reported that the incest "fantasies" related by those patients more often reflected real events rather than fantasies, as did their memories of child abuse. The ugly realities of child abuse—psychological, physical, and sexual (including incest) in general in our society—are only now evident to most mental health professionals. It is now known, for example, that one out of six women, and perhaps one out of three, have been sexually abused (Finkelhor, 1979; Gagnon, 1965; Russell, 1983).

### Common Misconceptions About Schizophrenia

What happens when a therapist talks to a schizophrenic? Usually the therapist feels uncomfortable, depressed, and/or angry, because the patient doesn't react the way the therapist wants him or her to react; the patient often does not show the therapist respect. What the therapist knows does not seem to work. Moreover, the therapist feels scared and isn't sure why.

The reason for these uncomfortable feelings is that these are the patient's feelings. Eugen Bleuler (1911) made the mistake of assuming, because schizophrenics look as if they have no feeling, that they have no feelings. In fact, schizophrenic persons have very intense feelings, although they may mask or even deny them. The most basic affect is fear, actually terror. Sometimes the therapist may, all too successfully, empathize with the schizophrenic patient's terror and withdraw from the patient.

Human beings are not easily able to tolerate chronic, massive terror. All of the symptoms of schizophrenia may be understood as manifestations of chronic terror and defenses against terror. Thus, withdrawal from people diminishes one's fear, and the severity of the thought disorder varies with the severity of the terror. Chronic terror tends to mask other feelings. Nonetheless, the schizophrenic frequently experiences, in addition to fear—chronically or intermittently—anger, hopelessness, loneliness, and humiliation.

Apparently inappropriate affect is usually socially inappropriate affect, not inappropriate to the patient's inner experiences.

If it is believed that schizophrenics have no affect, then it becomes a puzzle why major tranquilizers and similar drugs, which greatly diminish affect, should be helpful. But once the centrality of terror in schizophrenia is understood, then their utility makes sense. All the medications that are of some use to schizophrenics are drugs which, among other things, damp down the affect system and, therefore, diminish the fear. (It was a public relations coup to relabel major tranquilizers as "antipsychotic medication," implying that they are as specific and effective for psychosis as vitamin C is for scurvy. Unfortunately, there is no "antipsychotic" medication in that sense.)

But someone living with a loss of affect has some handicaps in adjusting to life. For example, in most big cities, there are teenage gangs who prey on medicated patients who are unable to be alert and self-defensive. Schizophrenic patients die of the effects of multiple muggings.

Fundamentally, we do not want to know about schizophrenia because we do not want to feel terror at that intensity. All of us have the potential for schizophrenic symptoms if there is enough stress; the only differences seem to lie in the quantity and qualitative nature of the necessary stress. The severity of the stress is usually determined by the conscious and unconscious meanings of that stress. In 1959, I published a description of the specific meanings of giving birth for a woman whose postpartum schizophrenic psychosis lasted for over 12 years before she was successfully treated with psychoanalytic psychotherapy (Rosberg & Karon, 1959).

The myths of the lack of meaning of schizophrenic symptoms, the irrelevance of understanding, and the "incurability" of schizophrenic disorders are still with us. A psychologist at a state hospital in another state consulted me about some problems in the treatment of a schizophrenic patient who had been hospitalized for 15 years. After a year of hard and insightful psychotherapeutic work by the psychologist, the patient left the hospital. The staff psychiatrist said, "I guess the medication finally took hold."

Deikman and Whitaker (1979) instituted a regimen of almost purely psychological treatment on one "experimental" ward of a psychiatric hospital. Despite dire warnings that their failure to medicate constituted malpractice, their ward program resulted in decreased need for rehospitalization and there were no suicides, suicide attempts, or elopements during the 11 months in which the ward was fully operative. A comparison ward that was more fully staffed and practiced expert psychopharmacology had three suicides in the same period, despite sending its more disturbed patients to a long-term state hospital. The experiment was discontinued and never imitated in spite of its success.

Among the problems with DSM-IV is its rationalization of bad treatment and preservation of the myth of the incurability of schizophrenia. The very same symptom picture is diagnosed as "brief reactive psychosis," "schizophreniform psychosis," or "schizophrenia," solely dependent on whether the patient recovers in less than one month, more than one month but less than 6 months, or more than 6 months, irrespective of type or adequacy of treatment (American Psychiatric Association, 1994, pp. 273-304).

In the 1950s, as Senior Clinical Psychologist at a reformatory for male adolescents, I instituted a policy of psychotherapy for all psychotic reactions. Patients had daily psychotherapy sessions without medication for 5 days before transfer to the state hospital was considered. Psychotherapy was continued at a minimum of one session per week. During a 6-month period, no state hospital transfers for reason of psychosis were necessary. The usual procedure before and after that 6-month period was to transfer psychotic patients to the state hospital; the usual length of stay in the 1950s was approximately 2 years before they were returned to the reformatory. But DSM-IV makes that comparison seem irrelevant. According to DSM-IV, as it were, only "brief reactive psychoses" occurred during the 6 months when psychotherapy was

available, whereas before and after that period, the state hospital treated true "schizophrenics" from the same population.

Clinical experience, without control groups, can nonetheless be convincing. Thus, the patient who had spent many years at the most expensive hospitals in the United States to no avail, who had been treated unsuccessfully by electric shock and insulin comas as well as so-called psychotherapies, and had been catatonic for years at his latest hospital, responded to an intensive psychotherapeutic effort (10 hours a day for 10 days with 2 energetic psychologists) without medication by beginning to talk, then responded to five day per week therapy with ordinary sessions, and progressed to outpatient therapy; or the patient who had been sick for 16 years and responded to two years of inpatient psychotherapy, and went on to a successful independent life (one of the few cases where long term follow-up was obtained); or the 10 year old paranoid schizophrenic, who after three years of therapy for him and his parents, twice a week, recovered and whose mother was kind enough to call the therapist to let him know when the ex-patient graduated college. Or the hopeless schizophrenic, according to several psychiatrists, despite being on a number of medications, whose only hope was shock treatment, which probably would not cure him, but it was his only hope. Luckily, he was brought for therapy, taken off medication, worked with, and six months later was back at work. Choosing increasingly high life goals, he opted for a long term psychoanalysis which helped him realize his potential as a distinguished scholar and a successful husband and father.

There has never been a lack of treatments which do more harm than good. They have in common that they do not require understanding the human condition. In this light, one can grasp why Freud, who laid the basis for most modern psychotherapy, never was awarded a Nobel prize. Instead, the Nobel prize was presented to the neurosurgeon Egas Moniz, the pioneer of prefrontal lobotomy, who we now know faked some of his data (Valenstein, 1986). Lobotomy allowed one to treat these people without having to understand them at all. It got them so they would not bother anyone.

Electric shock treatment is still practiced despite clear evidence that it, too, produces brain damage (cf. Breggin, 1979; Morgan, 1985). But if a psychiatrist who has given shock treatments becomes a patient, the first thing he does is beg not to be shocked; and the more people he has shocked himself, the stronger he begs.

The predominant treatment which does not require understanding today is medication, which does reduce disturbing affect and some of its immediate consequences. Patients' behavior improves, and they become more compliant, which is sometimes very helpful because other people are almost always afraid of schizophrenics. People tend to be cruel when they are afraid. Because cruelty makes schizophrenic people more schizophrenic, there are advantages to making schizophrenic people less frightening.

However, there are many problems with psychiatric

medication. Breggin's (1983; 1990; 1997) reviews of the literature on brain-damaging effects of psychiatric medication are troubling because they suggest that the mental health system is creating a population of brain-damaged people. Not only are the patients given medication, but they are told they must take the medication for the rest of their lives. There are professionals who think psychotherapy with schizophrenics is finding out why they do not take their medication; but if they are good patients and take their medication, in the long run at least 40 % of them are going to be demonstrably brain-damaged (Breggin, 1990; Breggin, 1991, pp. 68-91).

Suddath, Christison, Torrey, Casanova, and Weinberger (1990) studied the brains of 15 pairs of monozygotic twins discordant for schizophrenia, and found brain abnormalities of unknown origin in the schizophrenic twin as compared to the nonschizophrenic twin. Though they concluded that this could not be due to the medication because the correlations of lifetime medication dosage with measures of brain damage did not reach the conventional 5% level of statistical significance, in fact the data show that they reached the 6% level; the correlations of abnormalities (e.g., enlarged ventricles) with lifetime medication dosage were as high as .50 within the schizophrenic twin sample, all of whom had been medicated; and as any statistician knows, the correlations of brain abnormalities with medication would be higher if the range of lifetime medication dosage were extended downward to 0, as none of the nonschizophrenic twins had been medicated. Examination of the data reveals that the findings of brain abnormalities in the schizophrenic twin as compared to the nonschizophrenic twin would disappear if these differences were statistically corrected for medication dosage. In other words, the simplest explanation of their findings is that the medication produced the brain abnormalities.

Enlarged ventricles result from the medication. And, the excess dopamine receptors found in the brains of schizophrenics have been demonstrated by animal studies to be the result of the medications (Porceddu, Giorgi, Ongini, Mele, & Biggio, 1986; Porceddu, Ongini, & Biggio, 1985).

The only thing that is keeping us from doing more damage is that patients sometimes lie. The medication is unpleasant: Men are often made impotent by it, women often cannot enjoy sex, and the feeling of not having feelings is unpleasant. Some studies (Boczkowski, Zeichner, & DeSanto, 1985; Irwin, Weitzel, & Morgan, 1971; Willcox, Gillan, & Hare, 1965), as well as clinical experience, suggest that up to 60 % of patients who are believed to be taking maintenance medication lie about it. Why patients lie when they stop taking their medication is illustrated by a patient interviewed as part of an in service training session for psychiatric residents. When asked why he was in the hospital, he replied, "Because I stopped taking my medication."

This seemed to be a chance to teach him something useful. So the patient was told, "Now that's nonsense. Nobody would put you in the hospital because you stopped taking your medication. What happened is that

you stopped taking your medication, and then you did something. The something you did upset people so much that they put you in the hospital. What did you do?" The patient did not say. Afterward, several members of the staff said that the patient had told his psychiatrist, "I stopped taking my medication," and that the patient was put in the hospital for that reason alone. The message is, "How dare you think you can make decisions about yourself (unless you lie to me)."

The current generation of psychiatrists have been trained almost exclusively in treating patients by means of medications, and have neglected their training in psychotherapy. Unfortunately, the medications do not live up to their advertisements: they are only partially effective, they habituate, and they have serious side-effects (Breggin, 1991; Fisher & Greenberg, 1989). Thus as the disadvantages of medication are finally being learned, some psychiatrists are going back to administering shock treatments, without considering psychotherapy. If a psychiatrist instead changes from medicating people to practicing psychotherapy, he or she has three problems: (a) psychotherapy is a difficult skill; (b) it requires experiencing all kinds of unpleasant feelings; and (c) the psychiatrist's income is going to drop dramatically. A study funded by the American Psychiatric Association reported that psychiatrists who practice psychotherapy cannot make much more than \$ 100,000 per year, but that a practice confined to medication and evaluation will yield \$ 300,000, certainly a strong incentive (Moran, 1993).

Sometimes it is argued that research shows psychotherapy is not helpful. But when the Michigan State Psychotherapy Project (Karon & VandenBos, 1981) randomly assigned schizophrenic patients to (a) an average of 70 sessions of psychoanalytic psychotherapy per patient, (b) medication, or (c) a combination of the two, blind evaluation showed that psychotherapy alone, or with initial medication that was withdrawn as the patients could tolerate it, led to earlier discharge from the hospital, kept the patients out of the hospital, and improved their thought disorders more than medication did, and the patients lived a more human life in a variety of ways. Psychotherapy with maintenance medication was better than medication alone, but not as good in the long run as psychotherapy alone or with initial medication that was withdrawn. Because of the hospitalization and particularly rehospitalization findings, psychotherapy was less expensive in the long run, saving nearly half the usual (medication) treatment costs in a 4-year period.

Unfortunately, decision makers are not interested in saving money over 4 or more years. In that length of time there will be a different political administration, a different head of this hospital, or of this department of an insurance company. The decision makers have always wanted to save money within 6 months, or at most a year or two, and that was unfortunate. But in this age of managed care, if patients can be denied benefits entirely, or as a result of inadequate treatment lose their job, and consequently lose their insurance, the managed care company considers it a successful treatment.

The Michigan State study included experienced and inexperienced therapists. The experienced therapists had over 10 years of experience in treating schizophrenics with psychoanalytic therapy, were knowledgeable about treating African-American and lower socioeconomic patients (characteristic of most of the inner-city patients in this study), and were considered effective by their colleagues. The inexperienced therapists wanted to learn how to do this kind of therapy, valued their supervisors, were paid for their time, and were given careful training and supervision.

The Michigan State study is different in these respects from the widely cited controlled studies conducted in this country (i.e., Grinspoon, Ewalt, & Shader, 1972; May, 1968) which found that psychotherapy was not as effective as medication. (Understandably, a drug company distributed widely free copies of May's book.) While these studies had many methodological flaws, the most important was that they involved so-called psychotherapists and so-called supervisors neither of whom had ever treated a schizophrenic patient by psychotherapy before. They either had little training in any psychotherapy or had training in treating a different kind of patient with a different kind of therapy, like a psychoanalyst experienced only in treating upper middle class neurotic outpatients on a couch.

If the therapist does not know how to do therapy, it is true that medication works better than psychotherapy. If bicycle riding were studied the same way, using only people who had never ridden a bicycle before, it would be concluded that human beings clearly cannot ride bicycles.

The McLean study (Gunderson, et al., 1984) is often cited as evidence that dynamic therapy is not useful, since supportive therapy was more helpful in obtaining work. But only 25% of the patients in either group finished treatment and were evaluated (Karon, 1984); therapists were required to insist that the patients take their medication. Most patients simply avoided the study doctors.

My review (Karon, 1989) of all available studies found by a computer search, found that the effectiveness of psychoanalytic therapy is supported by empirical data. Thus, for example, Benedetti and Furlan (1987) reported from Italy and Switzerland a series of 50 severe schizophrenic cases treated with intensive psychoanalytic therapy (2-5 sessions per week) for 3 to 10 years by supervisees, who were psychiatrists, with very good results in 80% of the cases. Alanen (1991) in Finland demonstrated what a real community mental health system can do. In each community there is a four person psychosis team. At least one of them has training in the appropriate use of each modality— individual psychotherapy, family therapy, and medication. The first session is always a family session to which all members of the family are invited. It begins with the invitation: "Six months ago your son, daughter, husband, wife, father, mother was not psychotic, now they are. Something must have happened. Can you help us try to figure out what might have happened?" They have developed sensitive theories of helpful family interventions. The treatment

team decides which option or combination of options—individual psychotherapy, family therapy, medication, hospitalization—are most likely to be helpful. Treatment is not decided on a national basis or a general local policy, nor by diagnostic category. A decision is made for this individual and family for this week, with the difficult clinical decisions shared by four mutually respecting colleagues. Since this system has been in effect, the amount of medication used has decreased and even more dramatically the necessity for hospitalization. The data show that it is cheaper than the previous American style community treatment emphasizing medication without meaningful psychotherapy. More importantly, the patients are restored to a more human and productive life.

Psychotherapy has been discouraged by the supposed evidence from adoption studies which claimed there is a strong, genetic basis for schizophrenia. However, the Danish adoption studies conducted by Kety, Wender, and Rosenthal (Kety, Rosenthal, Wender, & Schulsinger, 1968; Wender, Rosenthal, Zahn, & Kety, 1971; Wender, Rosenthal, Kety, Schulsinger, & Welner, 1974) suffer from fatal scientific flaws and misleading reporting, as revealed in the critiques by Lidz, Blatt, and Cook (1981), by Lidz and Blatt (1983), and in the book, *Not in Our Genes*, by Lewontin, Rose, and Kamin (1984). For example, biological relatives were reported to have higher rates of schizophrenia than adoptive relatives. But the data for biological relatives was inflated by half-siblings of schizophrenics, who had a higher rate of schizophrenia than full siblings or than parents. There is no genetic model that would account for that data. Adoptive parents whose adoptive child became schizophrenic were reported as not different from adoptive parents whose adoptive child did not become schizophrenic, despite the fact that many of the former had been hospitalized for psychiatric disorders. Margaret Singer (Wynne, Singer, & Toohey, 1976) was able to pick out blindly, with absolutely no errors, on the basis of the Rorschach communication deviance scores, adoptive parents whose adoptive children became schizophrenic from adoptive parents whose adoptive child did not become schizophrenic. The researchers did not report this, instead reporting that psychological tests did not differentiate the two groups of adoptive parents, even though they had sent the protocols to Singer, and knew the results. But there is a careful, extensive adoption study from Finland, reported by Tienari (1992), which found that the most potent predictor of schizophrenia in adopted children is Communication Deviance (cf. Wynne & Singer, 1963), measured in the interaction between the adopting parents without the child being present, so it is not a reaction to a sick child; that children of schizophrenics are more vulnerable to other disturbing interactions; but that children of normals or of schizophrenics only become schizophrenic in disturbing adoptive families. No study is perfect, and this study included adoptions as late as 4 years of age. (See also Tienari et al., 1985, for an earlier preliminary report.) But the Tienari data are by far the best available.

The most impressive genetic experiment was one no sane researcher would have carried out (Binder, 1938; Breggin, 1994; Proctor, 1988). For several years all

schizophrenics in Nazi Germany were sterilized. Then the annihilation gas chambers were designed by psychiatrists, originally not for Jews, but for mental patients. Schizophrenics were annihilated. But a generation later, the rate of schizophrenia was not affected.

Clinical experience also leads one to be skeptical of genetic factors. Even when there seems no basis for the disorder, if one listens carefully, the disorder always makes psychological sense, and seems inevitable in terms of the life as experienced.

A favorite example was provided by the residents in psychiatry at a state hospital who endured a seminar with me which made them uncomfortable because they were told that shock treatment was destructive, psychosurgery was destructive, and medication was of limited benefit. They were encouraged to talk to their patients. That was not what the rest of their supervisors told them. The residents, in reaction, asked me to interview a patient.

Most schizophrenics are not dangerous, but the residents chose someone with a history of repeatedly assaulting strange men, who was big, muscular, and moved very fast. I insisted that the residents sit in the same room during the interview, knowing that they had never been that close to anybody who moved that fast or was that dangerous.

The patient had been hospitalized for 10 years, but there was nothing in the case records which would account for his disorder. The only apparent major stresses were that he was poor, his father was an alcoholic, he had developed a speech disorder (stutter) as an adolescent which did not respond to speech therapy, and he had reported to sick call in the army with a venereal disease, whose site was his mouth, just before his first assault on a stranger.

He was grossly incoherent and, when he became coherent, he stuttered very badly. All the residents could have done to choose a more difficult psychotherapy prospect would have been to choose someone who didn't speak English at all.

In my value system, which most patients share, one deals first with homicidal danger; secondly, suicidal danger; and thirdly, anything else. This patient would creep up behind other patients and choke them. The attendants would see feet waving in the air. The patient had not killed anyone (he dropped the victim when the victim was unconscious), but the attendants were worried that he might.

Therefore, I kept bringing up this symptom during the first session. Finally, the patient and I worked out what seemed to be going on—that when the patient was a little boy, his mother, for minor offenses like not eating, would put a cloth around his neck and choke him. After that first session he stopped choking other patients. (It is a useful clinical rule of thumb that when you get a dramatic improvement in a symptom, you are probably doing the right thing.) Now this is not the kind of difficulty with which even people with difficult mothers have had to cope.

A second fact came to light in a transference reaction.



The patient began a therapy hour by yelling, "Why did you do it to me, Dad?" It is not difficult to recognize a transference reaction when a schizophrenic patient calls the therapist "daddy" or "mommy."

"What did I do?"

"You know what you did!"

When asked how old he was, he said, "You know I was 8 years old." Bit by bit he revealed that "you" had come home drunk and anally raped him. This was not an ordinary alcoholic father.

The patient's terrible stutter was also revealed to have an extraordinary cause, very different from other stutters. In the middle of his stutter there were words in Latin. When asked if he had been an altar boy, he said, "You swallow a snake, and then you stutter. You mustn't let anyone know." He was extremely ashamed and guilty. Apparently, he had performed fellatio on a priest.

He was reassured that it was all right, and it was interpreted orally: "Anyone as hungry as you were would have done the same thing." (With schizophrenic patients much of what seems sexual really has to do with orality, that is, infantile feelings, survival, and the early mother-child relationship. A penis, for example, may represent a mother's breast, and the breast represent love.) At that point the stuttering stopped. When he started to stutter in later sessions, it was only necessary to repeat the interpretation, and the stuttering immediately ceased.

But look at this poor man's life. He turned to mother, and mother was terrible. If mother is terrible, one ordinarily turns to father, but his father was terrible. He turned to God, and the priest was destructive. Would not that drive anyone insane? Yet examination of 10 years of ordinary hospital records revealed no basis for his psychosis.

Of course, this was an unusually hurtful family. Most parents of schizophrenics are not consciously destructive people, but often admirable people who will go to great lengths to attempt to get help for their children. Sometimes the destructive life experiences have nothing at all to do with the parents; in other instances hurtful parenting is the result of bad professional advice, or the repetition of bad parenting that they endured from their own idealized parents, or the result of unconscious defenses which like all unconscious defenses are uncontrollable until brought into awareness, and consequently not an issue of good or bad, but simply a problem to be solved.

With schizophrenics, the treatment of choice is psychotherapy with a competent therapist, who has relevant experience or training. If the patient, the therapist, and the setting can tolerate it, the psychotherapy is best conducted without medication. If the patient asks for it, or the therapist is uncomfortable talking with disorganized patients, or the setting requires it, medication can be used but it should be withdrawn as rapidly as the patient can tolerate. Medication as an adjunct

makes behavioral control easier to attain, but slows down the rate of underlying change. This is because medications damp affective responses, which is helpful to the patient. But affective responses during the psychotherapy session are also a helpful part of the process of change.

The therapist must help the patient create a livable world. As in any therapy, forming a therapeutic alliance is essential; but with psychotic patients, it is more difficult and forms a more persistent part of the therapist's work. The severity of the symptoms generally means there have been more bad things to transfer, and hence the transference to the therapist will tend to be negative. When there is ambiguity (or sometimes even when there is not), the therapist may be perceived as hostile, dangerous, shaming, belittling, and/or conspiring against the patient. This makes the therapeutic alliance harder to create and maintain. The therapist should try to be unambiguously helpful; the blank screen will inevitably become a monster.

Frequently, the patient does not communicate even what they understand because they do not trust you. It is important to tolerate not understanding; the moment you decide you will not abandon the patient no matter how confused or uncomfortable you are, you are already doing good therapy.

Patients cannot tolerate examining themselves except within the confines of a dependable relationship with a warm, strong therapist. By strong, we mean simply a therapist who will deal with anything and will not abandon the patient just because the therapist does not understand or the material is painful or the patient is hostile.

As in any therapy, what changes the patient is the internalization of the therapist as well as the insights gained. The patient internalizes the therapist into the superego so that the patient treats him or herself in the kindly rational way the therapist would instead of the rigid, punitive way that most patients treat themselves (based on their early identifications). The patient internalizes the therapist into the ego as a model for how a human being might be, discarding those quirks of the therapist which are not useful. The patient internalizes the therapy relationship as a model of what a human relationship might be. This process of internalization is central to effective therapy, particularly with psychotic patients, but it goes on without explicit attention as an automatic part of the patient-therapist interaction.

The therapist must repeatedly distinguish between thoughts and feelings vs. actions. Only actions have consequences. All thoughts and feelings are permissible; and actions can best be controlled if the patient dares to allow him or herself freedom of feeling and of thought.

The role of insight is the same as in any psychoanalytic therapy: making the unconscious conscious, changing the defenses in part by awareness, making the connec-

tion between the past and the present. Understanding the transference is central. The more severely disturbed the patient, the more obvious the transference reactions. Schizophrenics are constantly trying to solve their problems, but they are too frightened to deal with the real problems directly; they deal with symbols. Only when the symbolic act (or symptom) and the original traumatic experience are re-connected in consciousness can the person overcome it.

### Understanding the Dramatic Symptoms of Schizophrenia

Let us consider the most bizarre symptoms of schizophrenia. Take the catatonic stupor, in which the patients sit in the corner and do not move; and they are either absolutely rigid, or they may be waxy flexible. They may stay in one position for hours or for days.

Fromm-Reichmann (1950) reported a long time ago that catatonic patients see and hear everything that is going on around them, even though they do not react. They look like they are in a stupor, but they are not: They feel as if they will die if they move. Fromm-Reichmann understood this because the patients told her when they finally came out of the stupor.

Some years ago, Ratner (Ratner, Karon, VandenBos, & Denny, 1981) investigated animals and discovered that the catatonic stupor is a life- and species- preservative mechanism effective when in danger of being killed by predators that is built into just about all living animals, including human beings. The biological evidence is consistent with the clinical evidence from Fromm-Reichmann. The therapist should talk meaningfully and communicate to the patient that they are safe. This should be continued until the patient is able to talk to you.

Schizophrenic patients as well as professionals like to say that nobody understands hallucinations. But hallucinations are entirely understandable by Freud's (1900, 1917, 1933) theories of dreams, with a few additions. Today, the concept of the collective unconscious seems scientifically untenable; it was based on the then-accepted biological theory of the inheritance of acquired characteristics, no longer acceptable to biologists. There is no evidence of universal symbols; there are only symbols which are used frequently with a given meaning. But there are always people who will use any symbol with an entirely different meaning.

Unlike most people, schizophrenics hallucinate while they are wide awake. Everyone hallucinates when asleep. Dreams may take any sensory modality, but the predominant experience is visual. Schizophrenics also may hallucinate in any sensory modality, but the predominant modality is auditory. Whatever other hallucinations they have, they almost always hear voices. This is different from toxic psychoses, in which the hallucinations are primarily visual.

Why predominantly auditory hallucinations? Because basically schizophrenia is an interpersonal disorder. If

someone is blind, they are more physically incapacitated than someone who is deaf, but in terms of the probability of emotional disorders, deafness is more likely to cause emotional problems because it tends to cut an individual off from other people (Corbin & Eastwood, 1986; Gelder, Gath, & Mayou, 1989, pp. 457-458; Thomas, 1981).

But is the capacity to hallucinate while wide awake restricted to schizophrenics? Not at all. It is well known that starving people start seeing food. It is a human capacity if the motivation is strong enough; luckily, most of us will never be desperate enough to have to hallucinate. A trivial example illustrates the meaning of hallucinations. In the middle of a therapy session, a patient asked, "What's that bell?"

"I didn't hear a bell."

"Well, I did."

"It may well be. There are a lot of funny noises in this building. I work here all the time and maybe, like a lighthouse keeper, I just don't pay attention to them anymore. What did the bell sound like?"

"It sounded like a telephone bell, only very loud."

"That's surprising. A telephone bell I would have heard. What comes to mind when you think of a telephone bell?"

"Trying to get through to somebody."

"I think I know what's happening. I've been talking about what I thought was important, but you know I'm off somewhere; and you wish I would get through to you and talk about what is really going on here."

And then the patient smiled. She was too intimidated to tell the therapist he did not understand and ask why he was talking about irrelevancies when there were some things that were important. The most she could do was wish that somehow he would get through to her; and even that was too frightening to deal with consciously, so she had to have it come through in disguise, as an hallucination.

There are four major bases for delusions. The most important is transference (Freud, 1912): reliving feelings, fantasies, and experiences from the past with no awareness that it is the past. Of course, Freud thought schizophrenics did not form a transference; he was mistaken because he did not talk to schizophrenics. According to people who knew him, Freud said schizophrenic patients scared him. He certainly had enough work to do without schizophrenic patients, but even Freud's inferences are unlikely to be accurate unless they are based on clinical observations.

Freud originally thought of transference as a phenomenon occurring only in psychoanalysis, as the chief resistance, which by understanding he was able to transform into its most potent therapeutic tool. Ferenczi (1909/1950) first pointed out, and Freud accepted, that transference, like other resistances, was a defense used to cope in ordinary life. What was unique about transference in therapy was not its occurrence, but that it was studied.

But schizophrenics, if listened to, are not subtle in their transferences.

A young woman alarmed the hospital staff, despite their trying 20 different medications, by repeatedly cutting and burning herself. When I asked about her religion, she said, "I was raised a Catholic."

"Oh, you were raised a Catholic, but you're not now."

"Actually, I'm a Satanist."

"Why don't you tell me about it."

"I used to feel I had to save people. I had to save all the people in Beirut."

"That's a marvelous image. Beirut, that's a marvelous image. You know who the people in Beirut are, don't you?"

She started to say yes and then she said, "Well, no."

"What's Beirut? Beirut is a city where people kill each other, and then they declare peace. But when you look, they are still killing each other. Then they find out why they are killing each other, and try to deal with those problems and solve them; but they go on killing each other. Then they have a truce, but still go on killing each other. What a marvelous image—your family must have been like that."

She became very interested at that point. "Satan says that if I hurt myself, he'll keep me with him. That's what he says."

She was very scared. She described Satan's voice and his appearance. She described his face in considerable detail. When asked whether she knew anybody who looked like that, she thought and said, "Yes; he doesn't look like it now, but he used to."

"Who?"

"My father."

Indeed, according to later information from the family, her father used to beat her mother, and her mother eventually left the house. One can understand a little girl's belief that pain is the price of not being abandoned.

That hallucination disappeared. All one had to do was to ask the patient to describe her experience, and ask what it could possibly mean.

The second source of delusions was described by Freud (1911) on the basis of insights derived from his reading of Schreber's (1903/1955) book. As widely cited, Freud derived many paranoid delusions from the fear of homosexuality, viewing them as different ways of contradicting the implicit guilt-producing feeling (for a man), "I love him." Thus, (a) I do not love him, I love me—megalomania; (b) I do not love him, I love her—erotomania; (c) I do not love him (using projection), she loves him—delusional jealousy; (d) I do not love him (using projection), he loves me—the delusional threat of being endangered by homosexuals; (e) I do not love him (using reaction formation), I hate him—irrational hatred; or, most common, (f) I do not love him (using reaction formation), I hate him, but I cannot hate him for no reason, so (using projection) he hates me, which is why I hate him, and if I hate him, obviously I do not love him—delusional feelings of persecution.

However, secondary sources almost never mention the part of Freud's insight that is most meaningful and essen-

tial for therapeutic effectiveness. In the language of libido theory, Freud (1911, p. 70) said that the patient with schizophrenia feels withdrawn from emotional relatedness to everybody. Consequently, he wants to be able to relate to someone again. In addition to the hunger for approval from the same sex parent, people of the same sex are more like us than are those of the opposite sex, and, in growing up, it is usual to feel comfortable in relating closely to peers of the same sex before becoming comfortable with the opposite sex. When one feels withdrawn from everybody, there is a strong urge to get close to people of the same sex. Unfortunately, the patient fearfully interprets this self-curative tendency as "homosexuality."

It is usually helpful to let schizophrenic patients with symptoms based on the fear of homosexuality know that their fear of being homosexual is unfounded (if, as is usually the case, it is unfounded), that they are simply lonely, that their loneliness is normal, and that we all need friends of both sexes. Unless they have had a meaningful and benign homosexual relationship, schizophrenics are not helped by reassurances concerning the increased acceptability of homosexuality, but they always feel understood when their therapist talks of loneliness.

Of course, Freud's views on paranoid delusions have been criticized, fairly and unfairly. The fair criticism is that they account for only some delusions, not all. The unfair criticism is that persecutors in the delusions of women are usually men. But the first to point out this apparent contradiction was Freud (1915) who noted that when a woman is first psychotic, the persecutor is female, and is changed to a male persecutor as a later development of the delusion, illustrating the general human condition that feelings about men are not necessarily based on experiences with men, nor are feelings about women necessarily based on experiences with women.

The third basis for delusions is that some families actually teach unusual concepts (Lidz, 1973). Human beings depend on their families to teach them the categories of thought and the meaning of those categories. Children (and adults) assume that other people use concepts in the same way that they do, unless confronted with understandable contradictions. For example, if a person believes that "I love you" includes in its meaning "I hurt you, physically assault you, occasionally even try to kill you," that person is unlikely ever to be able to relate closely to another in a loving relationship.

It has been noted (Lidz, 1973; Searles, 1965) that families with disturbed children have a tendency to discourage the use of people outside the family as sources of information and corrective identification. Patients from very disturbed families who do not become schizophrenic are inevitably found to have remedied the defects in their nuclear families with relationships outside that nuclear family. This is the normal mechanism. Nobody ever had a perfect mother or father. Most children, as well as adults, use people outside the family to correct any problems in their family.

When parents interfere with this mechanism, any problem in the family is enormously magnified in its destructive impact. The parents, of course, do not do this to be hurtful; they are unaware that it has any harmful consequences. Indeed, they may even believe that it is good for the child.

The last basis for delusions is the general human need for a more or less systematic explanation of our world. Most people share similar systematic understandings. One who believes the world is flat is normal if the year is 1400, and is suspect if the year is 1998. The belief is the same; it is the relationship to others' beliefs that makes it normal or suspect. Schizophrenic people have had strange experiences. In part, their symptoms are strange experiences by ordinary standards. In addition, their lives often include unusual real events. Therefore, their systematic explanations of their world seem strange. But they demonstrate a need to be as realistic as their anxieties permit. Insofar as discrepancies between their understanding and reality become apparent to them, and as dynamic balances change, the patients continually revise their understanding.

The more intelligent patients are more apt to develop a systematic understanding that is adequate enough to obviate the need for more deteriorated symptoms and, hence, to be diagnosed as paranoid or paranoid schizophrenic. The less intelligent are less likely to develop as functionally adequate a "paranoid system."

Because the paranoid system is not an abnormal process, but a normal process used to cope with unusual problems, it is possible for a non-frightened, non-humiliating therapist to share the patient's systematic understanding, to respectfully call attention to inconsistencies, and to helpfully supplement the patient's understanding with the therapist's knowledge of the world, of other people and, more importantly, of the workings of the human mind.

The best description of what it feels like to be schizophrenic came from a catatonic man whom it took 8 weeks of psychotherapy (without medication) to get out of the hospital and back to work. One of his symptoms was bowing. When asked why he bowed, he said, "I don't bow."

"Yes, you do."

"No, I don't bow."

"Wait a minute. You do this [the therapist bowed]. This is bowing; you bow."

"No, I don't bow."

"But you do this."

"That's not bowing."

"What is it?"

"It's balancing."

"What are you balancing?"

"Emotions."

"What emotions?"

"Fear and loneliness."

That is, when he was lonely, he wanted to get close to

people (so he leaned forward). When he got close to people, he got scared and had to pull away (so he straightened up). But then he was lonely again.

Balancing between fear and loneliness is the best description of what it feels like to be schizophrenic. That we do not want to understand this is a tragedy.  $\Psi$

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# An Interview with Greg Kimble

Gregory A. Kimble is emeritus Professor at Duke University, a Past-President of Division 1, a teacher, a textbook writer, an administrator, a gourmet cook and oenophile, and the next recipient of both the APA's Career Award for Distinguished Contributions to Education and Training in Psychology and Division One's Hilgard Award for a Distinguished Career Contribution to General Psychology. Bowing to pressure, he agreed to field some of the Editor's questions for this issue.

*How did a kid from Iowa get interested in psychology at a small college in Minnesota in the thirties, interested enough to go on to a masters at Northwestern and to a Ph.D. at Iowa in 1945?*

Although I was born in Iowa my family moved to Minnesota when I was about 2. When I graduated from high school, we lived in Northfield. That's part of the reason I went to Carleton. The other part was that the college gave me a scholarship. Remember that was 1936; the nation was in the depths of the Great Depression; nobody had any money. The thought of college never even crossed my mind but my mother dragged me over to Carleton to discuss possibilities. There I found out that the college had two scholarships for every high school in the state of Minnesota: for the boy and girl with the highest grades. Although two of my female classmates had better grades than mine, I was the highest boy. The scholarship covered almost all of my expenses. My total cash outlay for a 4-year Carleton education was about \$100.

The interest in psychology was the path of least resistance. I went to Carleton with the intention of preparing to become a high school teacher. I took the necessary education courses and did a double major—in English and psychology, with honors theses in both subjects. I went on to Northwestern in psychology, chiefly because my teacher, Herbert F. Wright, suggested that I do so. Until he talked to me about it, I was so naive that I had never heard of graduate school and I didn't know where college teachers came from.

Northwestern was a mistake, mostly because I was too immature to deal responsibly with the program. After finishing a master's degree, I dropped out, returned to Northfield, and served as a clerk on a construction crew that was building munitions factory near there. By then World War II had begun. I was drafted but rejected because of a hernia.

At some point during that period Herbert Wright got back into the picture. He informed me that there was a research assistantship in psychology at Iowa that I could apply for. I did and was accepted.

*Who were some of the major figures that influenced your decisions to stick with psychology as a career as well as your perspectives on the field that have remained with you? What are these perspectives?*

Well, first there was Herbert Wright who showed me that theory can be fun. The theory he promoted was Lewinian Field Theory. I never totally abandoned that position because it was full of factual truth. When he suggested that I apply to Iowa, Wright warned me that I would be moving into the camp of the enemy but suggested that I do so on the general philosophy that, to be a responsible enemy of a theory, one had to understand what that theory was about.

At Iowa, the strongest influence came from Kenneth Spence. He was as near to a perfect psychologist as I have ever known. When he found out that I knew about Lewinian theory, he took that as a positive, for reasons that were similar to Wright's reasons for thinking I should know about Behaviorism. He reacted in the same way to the fact that I also knew about William McDougall's instinct doctrine—Wright, from whom I learned that, was an early 1930s Duke PhD. When he found out that I knew Rorschach methodology which I learned from Samuel Beck at Northwestern—he got me an assistantship doing testing at the Iowa State Psychopathic Hospital.

Spence also insisted that Iowa students be very well grounded in the positions of Guthrie and Tolman, his main competitors. And, of course we were expected to know Hull's theory in detail.

Spence's most impressive attribute was clear thinking about issues in psychology. I am sure that this came down

to insisting that both he and his critics be clear on the objective bases of the concepts they were using. Too often in his critics' case he found these bases lacking and served public notice of that fact. That, as much as anything, is what made Spence so unpopular among the majority of psychologists who don't think clearly about such matters.

Finally Spence was an extremely effective writer. He went over manuscripts time after time, determined that every word he used was exactly the one he meant to use and not some other. He tended, similarly, to go over students' dissertations with a very careful eye. All of us turned in dissertations that were better as a result.

Another important influence came into my life at Iowa when I met Lucille Laird shortly after I arrived there. Within three weeks we were engaged, and within three months we were married. That was almost 57 years ago. We are still together and looking forward to the next 57!

***You worked several places, but spent longer periods at Brown, Duke, and Colorado. Was there anything specific—people, attitudes, opportunities, problems, and so on—at these places that influenced your career choices?***

At Brown, the most important influence came from Walter S. Hunter, Chair of the department. He had a series of conversations with young faculty, which I am sure were carefully planned. Today they would be called "faculty development." The first one with me had to do with research versus teaching. It went something like this:

"In academic life, young man, there are first things and most important things. The most important thing is research. How your colleagues and the field regard you and your own self-esteem all depend on it. Eventually research will make or break you. But, here at Brown, the first thing is teaching the undergraduate students. So get your courses under control and do a good job at teaching because that buys you the privilege of doing the most important thing: research."

The second had to do with career planning:

"I've been watching what you're doing in the lab and your work on learning seems to be going well. But, if I were you, I'd be looking 10 years ahead and deciding what you might best be doing in research then. From what I see happening in psychology, I think that a good choice would be for you to get into the field of child development."

Remarkably perceptive, given that the date must have been 1947.

Hunter's other influence had to do with my perception of myself as a psychologist. He was a committed generalist. He insisted that every graduate of Brown University in Psychology be able to carry on an intelligent conversation with psychologists in every other branch of the field. The most important impact of that on my career at Brown was that he had me give a graduate seminar in projective methods. Largely as a result of Hunter's influence, I have always been fairly charitable in my judgments of the subject matter of various specialties in psychology. But,

at as a result of Spence's influence, I have been critical of their methodologies.

***I believe that you have attended more than 50 consecutive APA Conventions. There must be major differences between conventions in the forties and now that are striking. Would you care to comment upon changes in the Convention and changes in psychology that impress you as interesting and important?***

Actually it's 54 conventions in a row, if I make it to this year in Boston. The most conspicuous change has been the change in size—from a few hundred registrants to 10s of thousands. In parallel, there have been changes in emphasis. In the 1940s, they were mostly about science. Highlights were the symposia in which the giants in the field debated current theoretical issues. With time that emphasis has lessened and, in addition, the surviving emphasis has splintered, just as the field of psychology has broken up into a collection of specialties. Two themes that have gained greater strength have been commercialism—now the exhibits are a much more important part of the convention—and politics—particularly on Council there is a great deal of unseemly jockeying for political advantage and campaigning for positions in the governance of APA.

***How about changes in the APA? Other psychology organizations? Any comments that have not already been made before or not emphasized enough?***

When I was a graduate student at Iowa in the 1940s Bob Sears, who was on the faculty in Child Welfare there, called all of us together to report on the outcome of a meeting that he had attended, probably in DC. He said that with WW-II coming to an end, psychology would be playing a much more important role, nationally, than it had before the war. He told us that playing that role effectively would require APA to have a headquarters building in the nation's capitol. Eventually the association bought a house in 16th street in northwest Washington, which served that purpose for several years. I visited there once or twice, for drinks after meetings sponsored by APA. As I recall it, we each contributed 50 cents to help defray expenses. Filmore Sanford was Executive Secretary and he, along with Mrs. Sanford and most of the APA staff, were always there. Fil was a great story teller. I still remember his doing Hamlet's soliloquy in a southern accent: "To be or not to be. Boy that's what I want to know." With time, of course that easy informality gradually disappeared, although some of it—now without the fifty cents—was still there when Michael Pallack and Leonard Goodstein were Executive Officers.

Early on, the biggest change in APA was the development of a malignant friction between the groups that in my 1984 paper I called "Psychology's Two Cultures"—scientific and humanistic. Those disagreements had been there from the beginning, of course, but by about 1960, they were threatening the unity of APA. There was talk of reorganizing APA into separate societies that would cater to the interests of those constituencies. In the APA Council of Representatives the friction reached an unpleasant apex in the establishment of what were called "Section A" and "Section 1"—to avoid suggestions of priority. All that



ploy accomplished was to allow the politicians on Council three opportunities to express their prejudices. They joined both sections, thus gaining the opportunity to champion their causes at each meeting and once more in a plenary session.

Fortunately, now the rancor has subsided and Council meetings have turned civilized.

***You entered psychology in the heyday of behaviorism, and now it is said that the cognitive revolution overthrew behaviorism. But psychology in some ways is much different from what it was then. What have we gained, and what have we lost, if anything?***

In my opinion, there never was a "cognitive revolution." What went by that name was just a clouding of the issues, a failure to see psychology's scientific situation in the proper light. Paraphrasing the way I put it in a recent contribution to an "e-mail dialog" on the demise of animal learning and behaviorism initiated by Frank Logan:

At the risk of boring everybody, I want to add my predictable tidbit to this dialog. It strikes me that the issues we should be facing are the decline of behaviorism and the misuse in psychology of the concept of agency.

As I've already said in print too many times, if psychology wants to be a science, it must be some species of stimulus-response behaviorism. Science begins and ends with the public observation and the only behavioral observables available are the things that organisms do (responses) and the situations in which they do them (stimuli). Once it honors its commitment to base its science on stimuli and responses, psychology can use those data to draw inferences that are as biological, cognitive, or even as humanistic as it wants to. E. C. Tolman demonstrated 70 years ago that those inferences may include animal cognitions (expectation and purpose).

One inference that is not legitimate is the inference of agency (causality): that the animal does this or that because of its knowledge or intentions. In my discussions of psychology's scientific and humanistic cultures, I have made the point that, for scientific psychology the causes of behavior are in the environment, the genes, and gene environment interactions. For the humanists causality is in personal dispositions such as motivation and morality. The problem with both of those assertions of agency is that, in science, explanation is not in terms of causes but in terms of laws relating the dependent variables of that science (behavior for psychology) to independent variables. For psychology the independent variables, and thus its laws, are of the two different kinds that are the stock in trade of the two different branches of scientific psychology that Lee J. Cronbach called experimental and correlational. The distinction is the same as Kenneth Spence's distinction between experimental psychology's S-R laws, where the independent variables are environmental events, and psychometric psychology's R-R laws, where the independent variables are behavior assessed by tests of other measures. In different contexts this type of independent variable includes physiological assessments. Looked at in those terms, psychometric psychology, when it predicts from test scores, and biological psychology, when it predicts from brain waves, are both behavioristic.

In my opinion, many psychologists, along with the general public, find this way of explaining behavior offensive. It clashes with a deeply held sense that people are personally responsible for their actions. That led them to reject behaviorism, animal learning and, I would say, straight thinking in psychology.

***Where does psychology seem to be going? Is this a good thing? Will it work?***

Psychology is moving on several fronts, although whether those movements are all properly called "forward." is debatable. I would emphasize these:

1. Toward new scientific specialties identified by such titles as "cognitive-neuropsychology," "connectionism," and "artificial intelligence." The good thing about these developments is that they capitalize on powerful new methodologies, such as fMRI and computer simulation. The danger is that they tend to lose sight of the mission of psychology, the understanding of behavior.

2. Toward greater political involvement. Most conspicuous has been the recent explosion of lobbying activities carried on by all of the professional associations. The good thing about this development is that, from it, psychology may get a larger share of all the benefits that flow from Washington. The danger is that, along the way, psychology may make promises on which it can't deliver.

3. Toward a greater emphasis on application. This is one development on which I am mostly positive. I have long admired the practice of the English and Canadian laboratories of including basic science and application in the same program of research. It cannot possibly hurt to have psychology constantly aware of the importance of keeping in contact with the lives of real people.

4. Toward greater concentration on the professional concerns of scientists, educators, industrial organizational psychologists, and health service deliverers.

5. There appears to be a strong felt need for a unity in psychology. Since the heyday of the classical "schools"—behaviorism, functionalism, Gestalt theory, and the rest—psychology has become increasingly segmented. Some of the friction we discussed earlier comes from the splintered condition of the field. This unattractive situation has led to several attempts to find coherence among the diverse specialties of psychology. Recently, plans to make scientific psychology and practice partners in the discipline have figured conspicuously in the agenda of the APA Council of Representatives. The literature contains a fair amount of discussion of the importance of bringing the discipline back together. I have published some of it myself, attempting to describe the laws that apply generally in psychology, and Arthur Staats has done even more. Whether this where psychology is going will depend on everyone's ability to recognize that what all the branches of psychology have in common is their interest in behavior. If they can keep that point at the forefront of their thinking, the contributions of the several specialties will become much clearer. Bottom-line, the obligations of all of them are to work toward an understanding of behavior and to employ that understanding in programs aimed at solving the problems of the world. Every version of psychology can contribute.

***What of your specific contributions to the field are you particularly pleased to have made, and why?***

Over the years, the contributions that I like best have changed. At please first, they were my experimental work on classical conditioning and the acquisition of skill. Later on, they became my books, all of which have had the purpose of presenting psychology as a natural science. Throughout, I have treated psychology as an operational science that draws heavily on logical positivism, an approach that has gained a bad reputation in the past decade or two. By and large that bad reputation is undeserved. Positivism has a great deal more going for it than the undisciplined subjectivism that sometimes has replaced it.

' The first of these books was *Principles of General Psychology*, an introductory textbook that went through six editions, 2 and 3 with Norm Garnezy, 4, 5, and 6 with Garnezy and Ed Zigler. Then there were two books on conditioning and learning, *Hilgard and Marquis Conditioning and Learning* and *Foundations of Conditioning and Learning*, which described knowledge in those areas and made a stab at showing their relevance to complex behavior, with treatments of personality, social motivation, and the effects of cognition and attitude on conditioning. Finally, there was *Psychology: The Hope of a Science*, so far the most important of the efforts mentioned earlier to present the laws that hold all of psychology together. It is my, no-doubt presumptuous, effort to identify the psychological equivalents of Newton's laws of motion.

Throughout my career, I have also enjoyed teaching—with students as young as the 13-year-olds in college-level courses that I taught in the Duke University Talent Identification Program, to postdoctoral fellows. My most important journal article came from work with a postdoctoral fellow: "The Problem of Volition" with Larry Perlmutter, which appeared in *Psychological Review* at a time (1970) when, in psychology, "volition" was still a dirty word. In that article we pointed out that views like that of William James on volition contain ideas that are amenable to experimental study. Since then, many psychologists have picked up one of those ideas (although not necessarily because of Kimble and Perlmutter): that with long practice, voluntary acts may become automatic and occur without conscious attention. In that sense they are involuntary

Finally, I should mention that, among the contributions that have brought me great satisfaction, have been those which people tell me have made a difference in their lives. Some of it has come from my courses which revealed to scientifically-minded students a side of psychology that they didn't know existed and encouraged them to go on in the field. But most of that influence has been through my books, including *A Departmental Chairperson's Survival Manual* and my home-style statistics book, *How to Use (and misuse) Statistics*. In the first of those books, sponsored by CoGDoP (Council of Gradu-

ate Departments of Psychology), I used the results of a survey of CoGDoP members as to select topics and problems on which I offered recommendations of procedures that chairs can use to manage a department without losing their sanity. The statistics book expanded on an idea that I got in courses with E. F. Lindquist many years ago at Iowa: Statistics is not so much a branch of mathematics as it is a way of reasoning, for which most math (most of it below the level of junior high-school algebra) is simply a useful tool. Moreover, the subject can be fun and has applications that extend far beyond the analysis of data.

***If you had to do it all over again, what, if anything, would you have done differently? Or if you don't like that question What specific lines of endeavor would you have devoted more time to if you had had the opportunity to do so.***

I'd have acquired the quantitative and computer skills required to make me a better theoretician.

***Do you have any suggestions or comments to make to young people just entering the field?***

For undergraduate students, I have just one message: In the end it all comes down to passion. If you've found something that you love enough to work at it for nothing, whether it's psychology or gourmet cooking, that's the field for you. Go for it! Now the only problem is to find someone who is willing to pay you for doing it. For those who have decided on psychology and, let's say, have finished their PhDs and are moving to their first jobs, my only informed thoughts are for people going into academics. For them: Congratulations! You have chosen a great career. Salaries are better than they used to be—when I started at Brown, mine was \$2,200 a year—and you probably will be teaching the subjects you like best. Once you have those courses under control, it will be important get started on research. These days research is essential for securing retention, promotion, and tenure. Good teaching counts, but, almost everywhere, less than research. "publish or perish" is a fact of academic life but you shouldn't see it as a threat. Some of your greatest thrills will come from interactions with your colleagues that grow out of what you publish. So get things into print—in time to help in getting reappointed, a decision that will be made in only a year or two. In the long run, the quality of your publications will be more important than their number. Stick to the refereed journals; the others count against you.

Until you have a secure position, avoid entanglements with the textbook publishers and the politics of APA and other professional organizations. The rewards of such involvements can be great, but not great enough to risk your future on. Wait until you're sure you have a future.

As a last word, let me leave you with the hope that you find your academic career as satisfying as I have. I can't imagine any other way of life. Ψ



# Words, Words, Words: The Psychologist's Dilemma

Kurt Salzinger  
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**Much more than the other sciences, psychology is dependent on the words it uses when it describes, analyzes and modifies its subject matter.** We do not have the luxury of physics which allowed the use of the neologism, "quark" coined by James Joyce in Finnegans Wake to describe basic particles of matter. Physicists could, I suppose, have called the particles "souls" or "züks" (one of my favorite nonsense syllables) without affecting the state of the subject of physics the least bit, but we don't have that freedom.

For everyday language, our colleagues in advertising demonstrate for us quite regularly just how wrong Shakespeare was when he maintained that "a rose by any other name would smell as sweet." Imagine, if you will, a man courting his prospective love with a flower named stinkweed. Picture Heinrich Heine's poem, beginning not "Du bist wie eine Blume"—"You are like a flower"—but "Du bist wie eine verstunkene Blume"—"You are like a stinking flower."

In psychology, use the term "soul," and most of us get upset; use the term, "mind" or "id" and many (but not all) behaviorists would look askance; say "conditioned" and many psychologists under the influence of cognitive theory scoff at the concept. The point is that words affect us as psychologists, as they do in our other roles. For psychology, words immediately and directly move us to a particular theory or approach. We have no neutral words in our field. Now, bad as all this is, the worst development in psychology has been our embrace of lay language. Some of this is simply a continuation of early development; some of it is due to our mixing of roles: clinical psychologists who spend a great deal of their time conversing with their patients or clients (the use of these different words, of course, imply different political as well as scientific and therapeutic assumptions) wind up speaking "client" or "patient" speech at psychological

conventions. At a recent convention (Salzinger, 1997) when I was asked to comment on the behavioral analysis of some cases, it occurred to me that we were listening not to applied but to implied behavior analysis. Even while espousing a behavioral approach, speakers were carefully avoiding the use of technical terms; they implied the use of the theory rather than actually using it.

What is my point? We do not pay enough attention to the most important tool in our armamentarium, namely the words we use or is it "abuse." We employ words loosely with the consequence that all agree with what we say, all the while agreeing on quite different things. Let us examine a small number of words quite common in our field to exemplify our "word" problem.

A word-turned-concept, to take but one example, "mindfulness" has recently increased in popularity and has been used extensively by Ellen Langer (1997). Langer provides the following description: "a mindful approach to any activity has three characteristics: the continuous creation of new categories; openness to new information; and an implicit awareness of more than one perspective. Mindlessness, in contrast, is characterized by an entrapment in old categories; by automatic behavior that precludes attending to new signals; and by action that operates from a single perspective. Being mindless, colloquially speaking, is like being on automatic." (Langer, 1997, p. 4). Being "mindful" of one another or of one's environment is something praiseworthy in our society. "He is always mindful of others" or "She is mindful of all possibilities before she makes any important decision" are the kinds of uses to which the word "mindful" or "mindfulness" is put. It might even persuade people to be mindful and as such have a very important use in society.

Unfortunately, when it comes to explaining this concept

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in scientific terms, we find it necessary to engage in much handwaving and "you know's" rather than empirical explanations. Nevertheless, Langer treats us to some interesting examples. When she describes how people perform tasks in a creative way, she essentially describes how they vary their behavior. We find a chapter cleverly headed "when practice makes imperfect" (p. 9). And what she rightly points out is that learning to do something in a rote manner breeds mediocrity; injection of doubt into instructions, she tells us, produces more effective understanding.

In behavior-analytic terms, we would say that learning in a stereotyped manner, as is often produced by strict rules, makes the learner incapable of changing with changing conditions. We explain this in behavior analysis by means of the concept of variability of behavior. Behavior analytic experiments (e.g., Catania, Matthews, & Shimoff, 1982; Hayes, Brownstein, Zettle, Rosenfarb & Korn, 1986; Joyce & Chase, 1990) have demonstrated that stereotyped or unvarying behavior (often produced through following rules) remains relatively untouched in the face of changing conditions; on the other hand, learners acquiring the original behavior in a more varied way, change effectively in response to changing conditions. Here, I prefer the simpler, more objectively definable concept of variability to the one of mindfulness.

In another series of experiments that Langer presents as examples of mindfulness, she shows that subjects viewing a poster in a way different from their habitual manner, promotes better memory than when doing it the way they always do. It again demonstrates the effectiveness of variability of behavior. The more variable the observers' viewing behavior, the better their recall. Unquestionably, variability of behavior does not sound nearly as romantic as does mindfulness, but clearly, it is easier to define scientifically because we can easily demonstrate what we are talking about when we speak of variability. The concept of variability of behavior (or mindfulness, if you still wish to follow Langer) can also explain how operators respond to accidents with changing conditions (Salzinger, 1991). When things go wrong as in an accident, resorting to old rules is likely the wrong reaction; if the operator learned what to do only through some strict (stereotyped) rules, then one should not be surprised that he or she will not react to the changed conditions in a new way. On the other hand, if the operator learned what to do, not through following rules, but through behavior slowly shaped to the appropriate reaction, then the changed conditions of the accident will evoke a new and potentially appropriate response.

Let us next examine the use of lay terms for categories of study. Sometimes they can be translated without too much difficulty into scientific concepts which have the advantage of objectivity, reliability, and precision of description. More often than not, however, we find that lay categories describe behavior in terms that do not correspond to empirical categories of behavior. Instead, they do so partially and in that way, they result in misleading us. In a recent attempt to discuss this problem with respect to the concept of anger (Salzinger, 1995), I noticed that Spielberg and Sydeman, (1994) found some

items from the trait version of his anger scale to fit better with his famous anxiety scale. Perhaps what we need is not concepts of anger and anxiety but a concept of "Anxiety." The point is that the mixing of emotions happens often, as when we talk of "anger out" as opposed to "anger in" to describe still another emotion, namely depression. Should we perhaps substitute for all of this some category of intensity of behavior, going from aggression at one extreme all the way down to depression at the other? We desperately need to be mindful (to use Dr. Langer's concept) of alternative categories, especially when we find ourselves hedging and twisting and patching up the categories that we inherit from our lay environment.

We should add here that when we speak of lay language, we are talking of lay language of a particular period or in a particular area of the world. Danziger (1997) reminds us of both kinds of effects. Thus terms or categories like "passion," "will," and "reason" are no longer very popular, neither in our lay language nor in our scientific reports in psychology. But at the time of their popularity, they no doubt seemed quite natural. Danziger (1997) begins his book by relating a personal experience of teaching in Indonesia. An Indonesian colleague and he were both teaching psychology but they had each categorized the field in such different ways that they were unable to share the teaching. Danziger also cites I. A. Richards who found that the Chinese philosopher, Meng Tzu used a term meaning both feeling and propensity, clearly not a category that we recognize. And that, of course, is exactly my point. We can categorize behaviors in a great variety of ways and we ought to study those categories or perhaps simply vary them sufficiently to give new discoveries in psychology a chance to occur.

What about our category of memory? Has anything given us more trouble in recent years than this concept? A woman recalled suddenly, and apparently with the aid of her therapist, that her father had killed a childhood friend of hers. A trial occurred and her father was found guilty on the basis of that recovered memory. The Queen in Lewis Carroll's "Through the Looking Glass" said it best: "It's a poor memory that only works backwards." Indeed, some "memories" defiantly "reorganize" (or put more descriptively, are a function of variables other than events in the past) to accommodate new conditions. The clear assumption with respect to recovered memories is that events are somehow stored in —where else — one's mind, to be eventually dislodged by some fluke incident or more recently through a psychotherapist's treatment. This idea of storage — about which more later — demonstrates our inability to contemplate action at a distance. This was a problem that physicists struggled with. When physicists first discovered the attraction of bodies for each other, they needed something to transmit the force from one body to the other. They invented an ether for the job until they discovered that it was unnecessary.

Memory presents us with a similar problem. If we recall something then we again have an effect that comes to us through an ether. Watkins (1990), in an article entitled "Mediationism and the obfuscation of memory," maintains that "the sorry state of memory theorizing is a direct

result of adopting the mediationist doctrine" (p. 329). All theories seem to assume three stages of memory, namely an encoding stage (information is registered), a retention stage (information is somehow stored) and a retrieval stage (information is somehow elicited to effect behavior). He maintains that the complexity of these three stages is too great for experimental psychology's power to use. Referring to Newton's law of universal gravitation, Watkins shows us that there is no need for mediationism, any more than Newton needed an ether for his law. No behaviorist, Watkins nevertheless suggests that we forget mediationism and instead investigate the recaller's environment — outside and inside plus his or her history. It is the positing of a memory ether that makes us believe in the so-called recovered memories. There is, of course, always a delay between a stimulus and the response we make to it. When we respond in different ways to a physically constant stimulus at different times in our lives because conditioning or learning has taken place, we are talking about memory. But would we not be better off conceptualizing memory as simply a response to stimuli affected by their association with stimuli presented to us earlier in our experience? If we think of memory that way, we have the opportunity to include among the stimuli that control our behavior those that are affecting us right now as well as the stimuli that first impinged on us some time ago.

Would it not be simpler to think of memory as a form of stimulus control? Or is stimulus control simply a case of memory? When we read, we naturally rely on our memory for deciphering the letters, words, and sentences. We also depend on knowing the meaning of those words and sentences, still another case of memory. Thus, we can consider reading, or for that matter any response we make in the presence of a stimulus, a case of memory, except for one fact: Stimulus control is the simpler concept and, therefore, the concept that we should use to gain an understanding of people's behavior. When we respond, as in recalling something, we are responding to stimuli that, as I have already said, are related to the stimuli that had affected us some time ago. Such an approach makes it easier to study memory and makes it easier to anticipate that sometimes our memories would not be "accurate," that is, when we recall something, we respond to stimuli that differ from those that had originally affected us. In that sense there is no such thing as an accurate memory only verbal responses that society accepts as, or characterizes as, accurate. It can never be the same as the original situation.

### Systems of categories

Categorization is a natural way of beginning any science; take biology and Charles Darwin's classification system or chemistry and Mendeleeff's Periodic table. Mendeleeff invented the Periodic table, a classification system of the elements that allowed him to predict elements that had been unknown when he made that prediction. Darwin's system of classification of animals and plants also went beyond simply making up categories; he used his classification system to arrive at a theory of evolution according to which the elements classified developed by a process of gradual continuous change

from previous forms. In other words, both of these systems of classification went beyond simply categorizing to offering a description of a theory that told us about the interrelationship among the categories.

By way of contrast, psychiatry has for many years tried to establish what one might consider to be the analogue of these systems in its field but has been unsuccessful. A recent posting by Dr. Larry Beutler in the Society for a Science of Clinical Psychology electronic network was sufficiently interesting that I have gotten his permission

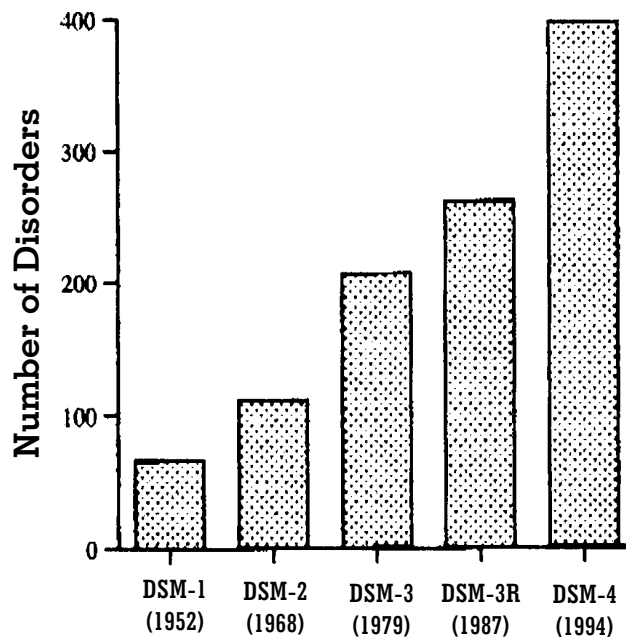


Figure 1. DSM-O-Mania

to graph it and present it here. He headed it: "DSM-O-Mania." (DSM=the *Diagnostic and Statistical Manual* used by the American Psychiatric Association). I quote it in full:

"The *DSM-1* (1952) was fewer than 100 pages and had 66 disorders. The *DSM-2* (1968) was 117 pages and had 111 disorders. The *DSM-3* (1979) was 493 pages and had 206 disorders. The *DSM-3R* (1987) was 563 pages and had 261 disorders. The *DSM-4* (1994) is 885 pages and has 397 disorders."

Dr. Beutler went on to indicate that the number of pages since 1979 is negatively correlated with the size of the membership of the American Psychiatric Association.

Unfortunately, there is no discernible theory to associate the diagnostic categories with one another and thus each diagnosis has to stand on its own feet without suggesting anything about the other diagnostic categories. Many psychologists have for years opposed this diagnostic system on the basis of a great many reasons; one is the intrinsic continuity of behavior, or put otherwise, the ranges of behaviors among various people, with too little of some behavior or too much of other behaviors constituting abnormality. But what has been most wanting, at least in the most recent incarnations of the *DSM*, is an overall scheme like Darwin's or Mendeleeff's, although to be fair what the *DSM* used

to have in earlier editions, was a psychoanalytic scheme underlying the categorization of abnormal behavior. Said to be empirical, the *DSM* since Number III has, in fact, suffered from following the medical model in a purely empirical manner; it seeks out symptoms and, determined to describe them "objectively," provides minimal time intervals of pathology and such as criteria for a particular diagnosis.

Top all of this off by talking about comorbidity, that is, a combination of entities which themselves suffer from poor definition, and you confront serious problems in our field. Recently, Lilienfeld, Waldman and Israel (1994) examined the concept of comorbidity in psychopathological research and found it both revealing and wanting for a number of reasons. They found it revealing because it showed that the "iclassical" model of categorization, in which indicators are both singly necessary and jointly sufficient for a diagnosis" does not apply to the *DSM*. They found it wanting because extensive co-occurrence and covariation of diagnostic categories shows that the classical model of classification does not apply. Basically, the authors concluded that comorbidity which assumes an association between disease entities in fact only refers to an overlap at the descriptive level and might simply reflect inaccuracy of categories.

### What should we do?

After all this criticism, it is incumbent on me to suggest a different way of running a science of psychology. The categories into which we carefully place the various units of behavior must be more basic than the ones we have been using. I believe we must begin with the simplest description and build up to the complex behavior that we are trying to explain. Our categories must be basic and simple. I suppose you might say, no matter how you slice it, it is still psychology. My point is that if you slice it into chunks too variable, you might well wind up with baloney.

Let us look at the concept of memory first. The use of the word, "memory" and recall and the like is common in society. We remember where we put something, what we had done before, what we were supposed to do, what had happened before, whether it is safe to go somewhere, etc. In the more recent past, memory has been given special meanings in psychology. Some memories have come back to haunt us unbidden and unwelcome—whether it be scenes of the holocaust, the Vietnam War, or of more private mishaps. Sometimes, psychologists have been paid to keep people (as in PTSD—posttraumatic stress syndrome) from remembering awful things; sometimes psychologists have been paid to remind people of the awful things that happened to them, in order to make them feel better, or just to set things right. Secret Service men have been called by a grand jury to recall the whereabouts of the President of the United States and of "that" woman. Witnesses are called all the time to report what they had seen and their words often decide the fate of an accused person in court. We have to remember how to operate some machine or whom to give a particular

message and so on. For all these uses and many, many others the words "memory" and "recall" and "remember," etc. are necessary and useful. The question that I am raising here, however, is whether these words are useful in a science of psychology.

What if we started with the following basic categories: Stimulus—that which impinges on a person, response—that which the person emits (does, if you prefer) and which includes both verbal as well as nonverbal behavior, and finally the consequence of the response, that is, what happens after the person responds. So, when we ask a man to tell us what he had for breakfast, we are presenting a stimulus to him which is related to the stimuli presented to him when he was having breakfast. In our society, we have good reason to believe that the information we will get in response to our request will have veracity because we have evidence for the match between what he now says and what he then did. Now, when you describe this situation in this way, the word stimulus comes up several times. Once to describe my question, and many times to allude to the stimuli that impinged on the man when he was eating breakfast. In addition, there are other stimuli that intervene before the man answers our question. There are the stimuli with which the word "breakfast" has been associated in his experience, that is, the aversiveness or pleasant value of the stimuli that accompanied breakfast; we must also take into account in evaluating the man's response what the reinforcement contingency is with respect to his answer.

It would obviously make a difference if the person asking the question were a friend at a cocktail party or an attorney in a courtroom, if the man was trying to prove that someone had tried to poison his food or if he was simply making idle conversation while waiting for an elevator. Now, you might say that I have complicated what society sees as a very simple conversational transaction but by analyzing this transaction into its components, I have made it possible to identify the variables that governed the man's response and that's very important, especially if it's coming up in court or if we are trying to gain an understanding of what the lay person calls memory.

Another advantage, in the study of memory, of beginning with the stimuli that control the verbal responses, is that we have less at stake to demonstrate a relationship to one kind of stimulus or another. The great strength of stimuli that ostensibly ask about past events has been well illustrated by Loftus' experiments (Loftus, 1975) with the red barn, that is, she was able to show that people would "recall" seeing something that actually never existed by simply asking her question (about a "red barn"). Now, we must realize that being influenced by a question stimulus does not mean we are stupid or lying or easily swayed or any other insulting condition; rather, such experiments simply demonstrate the power of various stimuli, including especially the great and surprising strength of "current" stimuli.

What we are still trying to do is to discover how to ask

questions to obtain responses primarily controlled by stimuli that occurred some time ago (e.g., Croyle & Loftus, 1993). We need to experiment to discover how to phrase questions to find out whether a man had employed a condom, for example, the last time he had sexual intercourse. It is obviously a very important question we are asking, but we have precious little information about how to best "remind" people. I dare say we cannot even entirely predict what people will say if we ask them to describe what they are looking at or are smelling or hearing, etc. I believe that to answer questions about the effect of stimuli from the past, we had better start working on finding out what control current stimuli exert over responses. Once we know that better, we can slowly increase the delay between exposure to stimuli and verbal responses to them.

Where then does all of this bring us? To a call for a return to basic concepts, to concepts that do not depend on lay language or lay assumptions about behavior. We should eliminate the requirement that the way we communicate with the lay public or patients must determine the manner in which we conduct our science. In that sense we must have a dual personality that allows us to speak one way to the public and the patients we treat, and another way to our fellow scientists and to the science which we use to increase our understanding of the behavior of organisms.

In conclusion, I have a final request of my fellow psychologists: I ask that we all spend some time every day abandoning lay categories, that we explore relations among our empirical categories that defy our lay language habits, that we spend an equal amount of time making up scientifically based categories; that we slice behavior anew and examine it all empirically. Remember, we have nothing to lose because old categories, like old soldiers, never die, they just fade away.  $\Psi$

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# General Stuff

*The General Psychologist*, as the Newsletter of Division One (now) AKA The Society for General Psychology, reports on the Society and its business, the metabusiness of General Psychology. In each issue there will be a section devoted to such matters. In this issue we have included a message from current President Kurt Salzinger as well as biographical and programmatic blurbs from the candidates for Society offices in the upcoming APA election process for which you will be receiving ballots from APA about the time that this issue reaches you. This issue also contains a discursive commentary by Secretary-Treasurer Lee Matthews of the actions and discussions that took place during the meeting of the Executive Committee of the Society in Washington, DC, at the end of January.



# A Message from the Society's President

## From Lab to Life

## Kurt Salzinger

"So, you're the President of the Society of General Psychology ... sounds like the president of vague knowledge," she said.

"No, no, general does not mean vague," I protested.

Well, you know what William Blake said, don't you?" She continued her harassment, despite my clear lack of interest: "To generalize is to be an idiot. To particularize is the alone [sic] distinction of merit—general knowledges are those knowledges that idiots possess."

That was clearly too much for me, "Hegel," I rejoined, "said: 'An idea is always a generalization, and generalization is a property of thinking. To generalize means to think.' That's what we do in general psychology," I squelched her.

All right, so I made it up. It's not what anybody said in a conversation but I have heard our division described as the default division in the American Psychological Association—the division that is appropriate if you have no specific interest as described for you by all the other 50 or so divisions. Indeed, story has it that at one time an enterprising Division 1 officer informed APA members belonging to no division that having joined no specific division they were automatically general psychologists and ought to ante up their membership fees.

Well, our membership fee is not as low as it used to be and I'm not sure this would work these days. I would prefer to present the positive arguments that validly argue for a general psychology. I am aware that we have to do this in the face of continuous splintering of knowledge into a greater number of smaller areas of "unequivocal truths." We live in particularities not generalities. Even those who describe themselves as psychologists—a general term after all—view their use of that word to refer only to what they are doing, such as comparative psychology, psycholinguistics, memory study, behavior therapy or psychoanalysis.

Only a small number think of themselves as truly general psychologists. Most of us just believe that what we are doing makes up the most central aspect of psychology or at least the most significant one. For those reasons, it is really a pleasure to look at the articles in our division journal, *Review of General Psychology*, for here we find unabashed examples of how one can go from simple fundamental principles, from generalities, to specific interesting cases of behavior that we are trying to understand.

An article by A. W. Logue (1998, 221-238) does this admirably. Taking the concepts of what she calls self control and impulsiveness, she explicates an operant conditioning paradigm in which a subject has a choice between working for an immediate low value reinforcer or for a delayed but higher value reinforcer.

This paradigm was earlier employed by H. Rachlin, and also by Mischel, Shoda and Peake (*Journal of Personality and Social Psychology*, 1988). The latter called it delayed gratification and found that they could predict performance of adolescents based on using this paradigm. Those preschool subjects who demonstrated self control, that is, waited for the larger reinforcer while giving up a smaller reinforcer they would have gotten immediately became adolescents rated more competent academically and socially and as having a host of other attributes bespeaking good adjustment.

Logue applied this paradigm to administrative decisions. To take but one example she supplies, a dean quite often has the following choice: Hire a seasoned star professor who would produce immediately both in work and in reputation but, being at the end of his or her career, soon reduces his work schedule, as opposed to hiring a young but promising faculty member who, although just starting, continues to produce and add to the luster of the department and school for a longer time.

She then asks whether the stewardship of a dean is usually long enough to engage in such long range planning and finds that their terms in office are in fact typically too short to expect them (and other administrators) to make decisions that take long range consequences into account.

This paper is but one of many demonstrating the effectiveness of general principles. There are others and I invite interested readers to turn to our Division 1 journal and partake of those interesting papers themselves.

As for our executive committee's activity, we busy working on the various awards that our division gives out each year; we are evaluating potential fellows; we are busy reshaping the web page; the revived new old newsletter is going great guns as you can all see for yourselves; we are making an effort to increase our student membership (I hope you will encourage your students to join); and I am giving thought to my presidential address. I hope that each of you will spread the word about general psychology and about where to find it in the APA. Ψ

# Candidates for Division Offices

Ballots will be sent to members by APA the middle of May for the election of officers for APA itself and for its various divisions and state associations. The members of Division One according to Bylaws will be electing this year a President-Elect and two Members-at-Large of the Executive Committee. To help you decide on your choices for these offices, the candidates have submitted biographical information and a platform statement, all of which follows. The candidates for President-Elect are Linda Bartoshuk, Don Bersoff, Lyle Bourne, and Tiffany Field. For Executive Committee, the candidates are Morton Ann Gernsbacher, Lynn Hasher, Duncan Luce, and Sam Turner. The candidates are presented in alphabetical order.

## Candidates for President

**Linda Bartoshuk** is a Professor of Surgery (Section of Otolaryngology) at the Yale University School of Medicine. In spite of that title, she is an experimental psychologist who got her PhD with Carl Pfaffmann at Brown in 1965. She works in the human psychophysics of taste and has taught courses on sensory processes and food behavior in the Psychology Department. Her early work included the study of plants that modify taste. One study on the artichoke (it makes water taste sweet to some lucky people) led to a brief period during which she was known as Linda Bartichoke. Fortunately the era ended when one of her friends who is vegetably challenged introduced her at a lecture as Linda Bavocado to the puzzlement of those in the audience. She subsequently discovered supertasters, individuals born with an unusually large number of taste buds and is currently interested in the health implications of food preferences influenced by genetic variation in taste.

The niche she has found for herself in medicine (she thinks there is room for a lot more psychologists there) has allowed her to use experiments of nature to learn about taste. On lucky occasions, her work leads to treatments. An anatomical link between taste and pain has led her to use capsaicin (the hot material in chili peppers) to desensitize pain receptors in the mouths of cancer patients and she is currently interested in the treatment of taste as well as oral pain phantoms (e.g., burning mouth syndrome). Although active in specialized associations (Chair, 1978 Gordon Conference on Taste and Olfaction; President, Association of Chemoreception Sciences, 1980-81; recipient, first award for Outstanding Achievement in the Chemical Senses, 1998) she sees herself as a generalist in psychology. She is a member of Divisions 1, 3, 6, 25, and 35 and was President of Division 6 in 1988-89 (and of EPA in 1990-91). She was elected to the Society of Experimental Psychology and the American Academy of Arts and Sciences in 1995.

**Bartoshuk's Statement** I love being a psychologist. I don't think there is a better way to be trained in science. The difficulties of studying behavior

have made us sophisticated about experimental design and statistical analysis. We study the real world but we know how to look beneath the surface to explore mechanism. The results of our work have impact on the lives of real people. We have low tolerance for nonsense in science. As far as I am concerned, it doesn't get any better than this.

I value Division One because it stands for the unity of psychology. Although my work might seem very specialized to some, I think as a generalist. I care very much about making what I do accessible to people in both directions on the intellectual food chain. I like being around psychologists who make their work accessible to me. If I were elected president of Division 1, my first task would be to work with the Executive Committee and Program Chair to help insure that the annual meeting continues to be a source of professional pleasure to us generalists. Beyond this, I am especially interested in disseminating the work of psychologists not only to the public but also to colleagues in other disciplines.

A few years ago I attended a press conference held by the AAAS to announce the results of a survey on the decrease in morale among young scientists caused by difficulties in getting research funding. To my dismay, about halfway through I realized that the scientists surveyed were mostly physicists and chemists. A few biologists were surveyed (if they were molecular enough) but even most medical researchers were ignored. Needless to say, psychologists, sociologists, anthropologists, etc were invisible. At the end the speaker asked for comments. I heard myself say, "If you would expand your concerns to the behavioral sciences, we would be able to treat more social ills, increase the gross national product, and have enough funding to take care of the research you value in physics and chemistry." I thought I would be asked to leave; to my amazement people applauded. We have many allies out there and one function of Division 1 is to find them. Ψ

**Donald Bersoff** is both a lawyer and a psychologist. He received his Ph.D. in 1965 from New York University and his J.D. in 1976 from Yale Law School. As a psychologist, he has taught at the Ohio

State University, the University of Georgia, and the Johns Hopkins University. He has also worked as a school, counseling, and clinical psychologist in both public and private settings, including a three-year stint as a military clinician during the Vietnam War. In 1979 he became the first General Counsel of the American Psychological Association and in 1981 helped found the law firm of Ennis Friedman Bersoff & Ewing, later merging with Jenner & Block, where he served as a partner.

In his capacity as APA counsel, Dr. Bersoff wrote 50 briefs for the U.S. Supreme Court and lower courts on scientific and professional issues related to psychology. He has worked on such cases as *Watson v. Fort Worth Bank* in which he argued on behalf of APA that "subjective" employment devices may be objectively validated, and *PANE v. Nuclear Regulatory Commission* in which he argued that the psychological impact of reopening a flawed nuclear power plant is measurable. In January 1990,

Dr. Bersoff became Director of the J.D./Ph.D. Program in Law and Psychology jointly sponsored by Villanova Law School and the Department of Clinical and Health Psychology of the Medical College of Pennsylvania-Hahnemann University and is a tenured full professor at both institutions. The joint program is one of four in the United States to offer students the opportunity to pursue a law degree and a Ph.D. in psychology simultaneously and the only one to focus on the interaction of law and clinical psychology. It is cognitive-behavioral in orientation and its goal is to produce scientist-professionals who will produce situation-specific, ecologically-valid research that will, it is hoped, produce better judicial decisions and sounder social policy.

Dr. Bersoff has served as President of the American Psychology-Law Society and recently completed a three year term (1994-1997) as an elected member of the Board of Directors of the American Psychological Association. One of his responsibilities was to act, at his request, as liaison to the Board of Scientific Affairs. He is currently the: (1) Chair of the Policy and Planning Board; (2) chair of the Blue Ribbon Panel investigating how APA can carry on its work without the proliferation of new task forces and ad hoc committees, and (3) a member of the Council of Representatives. He previously served on the Committee on Psychological Testing and Assessment. He was also appointed by Pres. Salzinger as Program Chair of Division 1 for the 1999 convention. He has written over 100 articles, chapters, and books concerning law, ethics, and psychology. In 1993 he served as counsel of record for an amicus brief submitted on behalf of a Group of American Law Professors in *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, a case involving the admissibility of scientific evidence in federal trials.

In 1995, the American Psychological Association published his text entitled *Ethical Conflicts in Psychology*. The second edition is in press and will be published in July 1999. Another of his texts for APA, *Law and Mental Health Professionals—Pennsylvania*, was published in 1999 as well. He is also the contributor of two entries in the forthcoming APA-Oxford Press *Encyclopedia of Psychology*.

**Bersoff's Statement** To my mind, our division, General Psychology, should exemplify and promote the integration of knowledge from a variety of subfields. It should also, where appropriate, communicate that knowledge to other components of our social system. It would be my intent, during my three-year term on the Division 1 Executive Committee (president-elect through past-president) to foster that integration. My own particular interest is in the application of social science data to legal decisionmaking. Research from a wide variety of fields, including social psychology, developmental psychology, measurement and evaluation, memory, sensation and perception, and cognitive psychology, are all applicable to scrutinizing common misperceptions about human behavior currently held by the legal system. The law is primarily populated by people who are uncomfortable with science and choose to rely on the "pages of human experience" (as the late Chief Justice Burger once put it) and their own "gut feeling" (as one judge once told me) rather than the findings from valid and methodologically sound research. As a well-known social psychologist colleague has written, lawyers are smart people who can't stand statistics. I would like to see us, as a Division, stimulate the communication of our combined and extensive knowledge, to policy makers of all stripes. Although our Council of Representatives promulgates social policy statements, based in part on research data, quite often those statements languish without further action. I would hope to see our Division become more actively involved in seeking ways, where appropriate and where there is truly supportable data, to critique existing flawed social policy and foster sound social policy. We have an abundance of talent within our Division. Our members come from far-reaching and extensive fields of psychology. I would hope to see us use that knowledge, not only to speak to ourselves at conventions and in journals, but to others outside our fields of endeavor. If I were to develop a slogan-like theme to undergird my presidency, it would be "Fostering Sound Social Policy Through Sound Social Science Data." Ψ

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**Lyle E. Bourne, Jr.**, received a Bachelor's Degree at Brown University in 1953 and a Ph.D. in psychology from the University of Wisconsin in 1956. He has been a member of the faculty of the Departments of Psychology of the University of Utah (1956-1963) and of the University of Colorado, Boulder (1963-present), where he has been professor of psychology since 1965.

Professionally, Bourne is a member of the Rocky Mountain Psychological Association, fellow of the American Psychological Association, and charter member of the Psychonomic Society, the Cognitive Science Society, and the American Psychological Society. He was elected to membership in the Society of Experimental Psychologists in 1972. Bourne is the author of over 130 journal articles, a 27 book chapters, and 14 books including most recently *Psychology: Behavior in Context*, with Nancy Filipe Russo (1998, Norton), and *Foreign language learning: Psycholinguistic experiments on training and retention*, with Alice Healy (1998, Erlbaum).

During his career, Bourne has served on the Editorial Board of a number of journals and is presently co-editor of the Science Watch Section of the American Psychologist. Bourne has made various service contributions to the American Psychological Association, as member of the Council of Representatives (two terms, from Division 3), Publication and Communications Committee (two terms), Council of Editors, and Board of Scientific Affairs (two terms, Chair in second term). He has also been a member and chair of the Publication Committee (1980-1985) and Governing Board of the Psychonomic Society. He was the Chair of the Science Seminar Series of the Federation of Behavioral, Psychological and Cognitive Sciences (1987-1990). Bourne has been President of the Rocky Mountain Psychological Association (1988), Chairman of the Society of Experimental Psychologists (1987-1988), President of Division 3 (Division of Experimental Psychology) of the APA (1991-1992), Chair of the Governing Board of the Psychonomic Society (1981-1982), and President of the Federation of Behavioral, Psychological and Cognitive Sciences (1995-1997). He was a member of the Psychobiology Panel of the National Science Foundation from 1972 to 1976. He received a Research Scientist Award from the National Institute of Mental Health, 1971-1976. His scholarly interests reside largely in the area of human learning, memory, and cognitive processes. His laboratory has received continuous funding from Federal and private sources since 1957 and is currently funded primarily by the Army Research Institute through 1999.

**Bourne's Statement** There's a bad rap on general psychology. It's been around for as long as I've been a psychologist. I remember being told in graduate school to align myself with those of a similar stripe, because much of what passes for psychology really isn't science. From the beginning, the pressure was on to specialize on a well-defined "scientific" problem; to be general was to be weak and possibly unscientific. Partly, I think, the bad reputation of the "generalist" arises out of an inferiority complex shared by many psychologists. Some of my colleagues have wondered out loud in an unguarded moment whether psychology is really a science? They have confessed to being concerned about how to impress colleagues in the "real" sciences? "If I focus on some small problem that no one else knows much about, maybe I can pull it off." In a recent discussion of these issues with a bright and promising new assistant professor in my Department, I learned that his aspiration was to be able to do one thing really well and to be honored and recognized for that contribution by his colleagues. He claims to feel safe in his own territory. Being "general" is presently furthest from his mind. But far more than feelings of inferiority are involved when it comes to being a generalist in psychology. It is truly difficult to build a reputation in the field as a general psychologist, although some have managed to pull it off. What serious Journal publishes "general psychology" papers? Even our most general prime journals, like the *Psychological Review*, publish only quite specialized articles these days, that relatively few of us can read once and understand. We can only hope that the *Review of General Psychology* will encourage

significant thoughtful pieces with wide appeal. Do academics get promoted for their work in general psychology? And, has anyone ever gotten a research grant from NIH or NSF or any other funding agency for a project in general psychology? It might be difficult, but is it really impossible to be a general psychologist? I don't think so. Consider the membership of this Division. Largely being a generalist is a matter of definition and breadth of interest. To me it means a willingness to look beyond my specialty, to be sensitive to the work of others, and to try to find a way to contribute in several areas. Sure, the field is specialized, the available knowledge even in the narrowest areas is often vast, and the methods demanded by state-of-the-art research are highly technical and tailored to the area. Even so, we are all psychologists. We all have certain interests in common. There is a glue that holds the field together. And that is our shared desire to know how the mind works—to discover the general principles of behavior and mental life, any interesting exceptions to those principles, and the applications of those principles to significant real-world problems. If that's not what you are working on, then you are not a psychologist, general or otherwise. Most of us will admit that there are intriguing questions about the mind and behavior that lie outside of our area of specialty. In addition, many methods concocted for specific problems turn out to have broad utility or high generalizability. Further, other specialists working in a different area sometimes are quite receptive to joint or collaborative efforts. Now for a confession; early in my career, I was a specialist. For me, little existed outside of the study of college students solving concept problems. But at some point along the line and for various reasons I came to take a much more general view of our field. Partly, it derives from trying to write a credible general text book, partly from the prodding of good students who didn't share my narrow perspective, and partly from a great deal of help from my friends and colleagues. So in recent years I have found myself publishing more broadly in journals and on psychological topics that, at an earlier time, I hardly knew existed. These efforts include work on brain potentials during cognitive tasks with my German colleagues, Paul Pauli and Niels Birbaumer, published in *Cognitive Brain Research and Psychophysiology*; psycholinguistic studies of foreign language learning, with my Colorado colleague, Alice Healy, published in a recent book from Erlbaum; experiments on gender difference in international peace and war decisions with Alice Healy and Frank Beer (a political scientist at Colorado) published in the *Journal of Peace Psychology* and the *American Political Science Review*, and an examination of rape myth acceptance by men and women with my student Colleen Sinclair published in the *Psychology of Women Quarterly*. I recite this litany not to demonstrate my credentials, but rather to let you know that these heady experiences have taught me a lot about myself and more importantly about our field. I think I see better now how small special problems fit into the larger, general picture of psychology. My identity has changed from specialist to general psychologist. I recommend this venture to any who would try it. If you elect me President of Division 1,

I will try to bring this message, about the importance and the rewards of thinking generally, to any audience of psychologists who will listen. I might try also to write a paper showing how cortical brain potentials, mental calculation, second language learning, date rape, international decision making, and gender differences are all pieces of one coherent general psychological puzzle. ♣

**Tiffany M. Field** is director of the Touch Research Institutes at the University of Miami School of Medicine and Nova Southeastern University and the Dean of the Family and School Center at NSU. She is recipient of the American Psychological Association Distinguished Young Scientist Award and has had a Research Scientist Award from the NIMH for her research career. She is the author of *Infancy, Touch, Advances in Touch*, the editor of a series of volumes on *High-Risk Infants*, and on *Stress & Coping*, and the author of over 350 journal papers. At the APA she is a Fellow in Divisions 1 and 7, She also and is currently the President-Elect of Division 7 and has been the secretary/treasurer of Division 7 and representative to APA Council and has been on several Division 7 committees and several committees in the APA Council. She has also been active in the Society for Research in Child Development and International Society on Infant Studies, having been Program Chair for two international conferences for ICIS and President of ICIS for two years.

**Field's Statement** When I was nominated as a fellow of Division One of the APA, I imagined that Division One was a place where leading psychologists in the different divisions would gather together and talk about the trends of their various divisions and the annual conferences as reflected in the trend seen in the program. I think the newsletter and the new journal are attempting to include commentaries on those trends. And, I think the division is making a concerted effort to include leaders from the various divisions. But perhaps the division needs to be more proactive than reactive in this particular mission. Instead of retroactively trying to capture trends of the annual conference in the newsletter and inviting papers for the journal, Division One members could do some prospective research on the trends as they are emerging and then formulate a conference program sponsored for the APA meeting by Division One to feature representative trends from different divisions in single sessions where members of APA can come and talk about the significance of these. Even if trends are not emerging, important issues that should be shaping trends can be targeted, for example the growth of violence amongst our youth in the United States and the effects of the rapid growth of the Internet.

In addition to the importance of thinking about Division One as general psychology and an umbrella division that looks over all the divisions, I think the membership should reflect the significant leaders from the various divisions. An attempt to increase the membership should be focused on recruiting fellows for Division One from fellows listings of other divisions as well as seeking those fellows' nominations for exceptional young members for membership status in Division One.

Thirdly, I think Division One might be more proactive in asserting themselves on the APA media list to provide, perhaps, more well-rounded perspectives on media events. Similarly, we may be more vocal regarding congressional policy, NIH and NSF funding as an example of having more influence on the history and the future of general psychology.

These are some of the issues and efforts I would target as a potential president of Division One. Coming from a rather eclectic psychology background with an early career in clinical work and a more recent career in developmental and behavioral medicine research, I think I have at least a strong empathy for the divergent fields in our discipline and the confidence that a more integrated perspective can come from more collaboration across the members of the different divisions. ♣

## For the Executive Committee

**Morton Ann Gernsbacher** received her Ph.D. from the University of Texas at Austin in 1983, was an assistant, associate, and full professor at the University of Oregon, from 1983 to 1992, and then joined the faculty at the University of Wisconsin-Madison, where she is the Sir Frederic C. Bartlett Professor of Psychology. She is a fellow of the APA (Division 1 and 3), APS, and AAAS. She has received a NIH Research Career Development Award, a Fulbright Research Scholar Award, a James McKeen Cattell Foundation Fellowship, and a Professional Opportunities for Women Award from the National Science Foundation. She is President of the International Society for Text and Discourse, a member of the Governing Board of the Psychonomic Society, and was the co-organizer of CogSci98, the annual meeting of the Cognitive Science Society. She has served as Member-at-Large on the executive committees of Division 1 and Division 3 (Experimental Psychology). She is an award winning teacher, and recently received the University of Wisconsin's highest award bestowed by its own faculty, the Hilldale Award for Distinguished Professional Accomplishment. She edits the journal *Memory & Cognition* and serves on five editorial boards. She wrote *Language Comprehension as Structure Building* (Erlbaum, 1990); edited *The Handbook of Psycholinguistics* (Academic Press, 1994); co-edited *Coherence in Spontaneous Text* (Benjamins, 1995), has two books in press, and has published over 90 journal articles and invited chapters. She is also the proud mother of a soon to be three-year old. Her research investigates the general cognitive processes and mechanisms underlying language comprehension.

**Gernsbacher's Statement** I view General Psychology as a goal for all psychologists. I believe that we should all strive to be general psychologists. By that I mean making our research accessible to those outside of our own interest areas, capitalizing on (learning and borrowing freely from) the literature and methodologies used by our neighboring colleagues, and carrying our message beyond academic psychology into the public forum, or as many have said, "Giving psychology away."

My research has been characterized by a generalist approach. In the mid-1980s I first began touting my message that we could best understand language comprehension by exploring the general cognitive processes and mechanisms that underlie it. In those days I was influenced strongly by the contemporary theories and discoveries in the fields of attention, perception, and memory. For example, I explained how it is that we (as skilled language users) understand the correct referent for an anaphor (such as a pronoun) by drawing on theories of selective attention (including inhibitory processing); I predicted that the information presented first in a sentence (which is typically the syntactic subject and semantic agent) would be more accessible than the information presented later in a sentence because traditional models of memory predicted this; I explained when listeners would perceive the topic (or importance) of a discourse by referring to gestalt principles of perception.

Although this message was not initially met with open arms (if I had a dollar for every reviewer who responded with the opposition, "But this is not psycholinguistics" I could pay for my university parking spot), the position is now becoming vogue (my small contribution to encouraging the often myopic field of psycholinguistics to become more integrated among the broader field of psychology).

In more recent years, I have extended beyond the traditional boundaries of experimental psychology to embrace the theories, literature, and methodologies of social psychology (for example, to predict when readers will adopt the viewpoint of the narrator); personality psychology (for example, to identify the personality correlates of language users that allow the attenuation of interfering information during comprehension); and developmental psychology (for example, to explain why adults have acquired more facility with active, affirmative, declarative sentences than sentences with syntactic forms with which they have had considerably less experience and exposure). I have recently begun using the methodologies of behavioral neuroscientists (e.g., functional magnetic resonance imaging) because I continue to believe that the best psychology is general psychology - psychology that bridges traditional boundaries and values integration over specialization.  $\Psi$

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**Lynn Hasher** received her A.B. from Smith College in 1966. She went to UC Berkeley for graduate school and received a Ph.D. in 1970. She stayed to do a postdoctoral year in cognitive development, persuaded that it was a mistake to try to understand human memory by looking (mostly) at college student participants. Her academic career started at Carleton University in Ottawa, Canada. She then went to Temple University in Philadelphia and she is now on the faculty at Duke University. Her work has centered on the relationship between attention and memory and on how attention constrains mental life.

**Hasher's Statement** My theoretical and empirical work has always cut across the classic domains of experimental and cognitive psychology to include ideas whose origins lie in other fields. Over the years, I have considered the development of memory, age differences

in cognition over the lifespan, the role that personal values, stereotypes, and affect play in cognitive functioning and most recently, the influence of circadian arousal patterns (and age differences therein) in determining performance. This eclectic mix of interests explains why I have appointments at Duke in Psychology: Social and Health Sciences, Psychology: Experimental, and in the Fuqua School of Business. I am also a Fellow of the Center for Developmental Sciences, at UNC, Chapel Hill and of the Center for the Study of Aging and Human Development at the Duke University Medical Center, and of the newly formed Cognitive Neuroscience Center at Duke University. Division 1 of APA is one place in professional and academic psychology in which it is possible for broad interests to be brought together and examined and it is critical for our understanding of behavior that this division be supported and nurtured.  $\Psi$

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**R. Duncan Luce** is Distinguished Research Professor of Cognitive Sciences and Research Professor of Economics at the University of California, Irvine. In addition, for 10 years he was the founding director of the Institute for Mathematical Behavioral Sciences. Earlier positions included professorships of psychology at Harvard, The Institute for Advanced Study (visiting), the University of Pennsylvania, and Columbia (assistant) as well as being a fellow three times at the Center for Advanced Study in the Behavioral Sciences. His formal education involved a B.S. in aeronautical engineering and a Ph.D. in mathematics from MIT. He is the (co)author of about 200 scientific papers, the (co)author of the following books: "Games and Decisions," "Individual Choice Behavior," "Foundations of Measurement" (3 Vols.), "Response Times," and "Sound & Hearing." Currently he is finishing a monograph on his contributions to utility theory. He has co-edited some 10 volumes including "The Handbook of Mathematical Psychology" (3 Vols.) and the "Stevens' Handbook of Experimental Psychology" (2 Vols.) He is a member of the American Academy of Arts and Sciences, the American Philosophical Society, the National Academy of Sciences, and the Society of Experimental Psychologists. The APA honored him with a Distinguished Scientific Contributions Award. And he is on the editorial boards of six journals.

**Luce's Statement** Although I am new—very new—to Division 1 and am not really aware of the issues it faces, I do bring a good deal of general experience with committee activities and, I like to think, a certain wisdom that comes from such experiences plus broad reading. I was a member of the APA's Board of Scientific Affairs, was a member of and later chair of the National Research Council's (then) Assembly of Behavioral and Social Sciences, and have co-chaired two groups that attempted to assess progress in the behavioral and social sciences broadly (for the NRC) and for basic science in psychology in the series on Human Capital supervised by a consortium of psychological societies. My knowledge of scientific psychology is fairly broad and includes some of the interfaces with other social sciences, and some of my research—measurement theory and analyses of rationality in decision making—can be construed as contributing to the philosophy of science as well as to science itself.  $\Psi$

**Samuel M. Turner** received his Ph.D. from the University of Georgia in 1975 and joined the faculty of the University of Pittsburgh School of Medicine where he advanced to the rank of professor. In Pittsburgh, he directed the Psychology Internship Program for 10 years. In 1992, he joined the Department of Psychiatry and Behavioral Sciences at the Medical University of South Carolina where he directed the Anxiety Prevention and Treatment Research Center. He currently is Professor of Psychology, Director of Clinical Training, and Co-Director of the Maryland Center for Anxiety Disorders at the University of Maryland.

Turner's program of research primarily has been in the anxiety disorders, including obsessive-compulsive disorder, social phobia and panic disorder. Studies have included children and adults, treatment outcome, treatment development, studies of phenomenology and psychopathology, high risk and longitudinal designs, and manifestation of anxiety in minority populations. He is the author or co-author of over 160 professional publications. Included among his books are *Treating Obsessive-Compulsive Disorder*, *Shy Children/Phobic Adults: Nature and Treatment of Social Phobia*, *Psychopathology and Diagnosis*, and *Diagnostic Interviewing*.

Turner's career is highlighted by an active interest in training at all levels, policy issues, and psychology at the national level. He has been on numerous national advisory committees including the NIMH Extramural Scientific Advisory Board. His APA service includes

the Board of Educational Affairs, Board of Scientific Affairs, Committee on Ethnic Minority Human Resources, and APA Council of Representatives. He currently serves as co-chair of the APA Task Force on Test User Qualifications and as a member of the APA College of Professional Psychology.

Turner served as editor of *The Clinical Psychologist*, currently serves as associate editor of the *American Psychologist*, and has served on the editorial board of virtually all of the major clinical journals. He is a diplomate in both clinical and behavioral psychology, fellow in Divisions 1, 12, 25, and 45, the 1997 recipient of APA's award for Distinguished Contributions to Professional Knowledge, and recipient of the Association of Medical School Psychologists' 1997 award for Distinguished Contributions to Medical Research.

**Turner's Statement** Despite the tremendous diversity of our discipline, the glue that holds us together as one is the sharing of a common background in the science of behavior, shared theoretical perspectives and similar methodological approaches. Division 1 is the single Division within APA where the focus is on this commonality with the goal of promoting a general science of psychology. As a clinical scientist, a large part of my career has been devoted to the integration of scientific principles into the clinical arena and the training of clinical scientists dedicated to the use of psychological science in the pursuit of solving clinical problems. It would be an honor for me to serve the Division that embodies the principle of psychology as a general science. Ψ

## Getting Down to BUSINESS

### Executive Committee Minutes

The Division One Executive Committee (EC) meeting was held January 30 and 31, 1999 at the APA Central Office in Washington, D.C. This review is meant to give you, the members of Division One, an overview of the activities at that meeting.

Our president, Dr. Kurt Salzinger noted the numerous accomplishments since the last Executive Committee meeting and the numerous exciting new changes that are in progress for the Division. Perhaps the most significant of these is the proposed name change for the Division to The Society for General Psychology, Division One of the American Psychological Association. The proposal has been sent to all division presidents, and if there are no objections, the change will be sent to Council for approval.

Dr. Frank Farley, our Past President, gave the 1998 Convention Program report and noted that the VIP Miniconvention may have been one of the highest attended miniconventions in APA history. Most sessions were full. Dr. Alan Boneau provided Dr. Farley with an updated membership brochure, which, along with a one page membership application, and issues of *The General Psychologist* were handed out at the miniconvention session, as well as place at the APA Divisional Booth.

Dr. Donald Bersoff, as 1999 Program Chair noted that we have 24 hours of programming scheduled for the 1999 convention. In addition, we will be the inaugural sponsor for the first plenary session, in which E. O. Wilson will be the first plenary presenter. The bulk of your Division's activities will be Friday, Saturday and Sunday of the convention. In addition, the Division will also be co-listed for a number of other sessions. Collating does not require that we give up hours, as cosponsoring an event would.

Our journal, *Review of General Psychology* with Dr. Peter Salovey as editor, continues to be productive and to have a short editorial "tag time". Dr. Salovey has also prepared an institutional mailing list. Various ways to increase subscriptions were discussed including the possibility of a "library adoption" form that members could give it to their universities, Look for more information in future issues of either the journal or the newsletter.

Dr. Gregory Kimble, as our representative, reported on highlights from the August 1998 APA Council of Representatives Agenda, and noted no critical issues for the Division. However, several items on the February 1999 agenda appear to be of relevance to the division. These include requests for funds to support advocacy to gain access to the Graduate Medical Education (GME) funds, so that psychologists would be eligible for financial support for education and training.

As part of the discussion of the February Council agenda, several EC members voiced concerns about the cancellation of some board and committee meetings, especially the cancellation of the Division Leadership Conference. Alternative ways for APA to save money without the loss of this vital interdivisional activity was supported by the EC. This issue, in relation to other concerns voiced by several members of the apparent high level of the compensation package of several APA executives was discussed in light of salary freezes for staff and executive directors, as well as, programmatic "belt tightening".

Priorities for future APA budgets need to be focused on avoiding the severe cutbacks in activities and funding which occurred in 1998. The two alternative motions seem to be: a) either policy

changes by Council (as recommended by the Finance Committee), or what appears to be the preferable alternative, the Board of Directors plan to refer the issue to the Policy and Planning Board's Blue Ribbon Panel. This group is chaired by our own Dr. Donald Bersoff. It is charged with looking at alternatives to the functions of all boards, committees, task forces, and presidential task forces. The Panel is developing both a 5-year retrospective and 5 year prospective view on these activities,

Although various members of the EC expressed concerns about the proliferation of new divisions, which impacts available convention time and members' interest in existing divisions, no major opposition was directed toward the pending applications for the February meeting.

In the area of Membership, there is good news, and some areas that we all need to work on as Division One members. The good news is that Dr. Nora Newcombe has accepted the position as Chair of the Membership Committee, and Dr. James Butler agreed to serve on the committee (Dr. Salzinger is an ex-officio member of the committee). Dr. Newcombe will add other members to the committee, as she needs them. We had approximately 2,700 members for 1998. About two-thirds of us are male, about 25% of the members are dues exempt (most because of members over the age of 65), and less than 10% of the members are fellows. While data on the total number of student members are not yet available (although it appears to be around 100) increases are needed in this area, Dr. Salzinger sent a letter to APAGS, describing advantages of affiliation with Division 1, making them aware of the journal and newsletter, to encourage them to become involved in its governance.

All of us in the Division should make a commitment to recruit women, minorities and new student members from our departments, while encouraging our longtime friends/colleagues to apply for fellow status. Also, ask your colleagues if they are a member of the Division, as some folks just join APA without any division affiliation. We have a great Division (some might call it Number 1) and each of us can make our Division stronger.

Are you a dues exempt member? Do you know what you are missing when you do not subscribe to the journal? Special issues on topics such as new directions in research on emotions, articles on Gardner Murphy's Double-Aspect psychology, Arthur Staats on a unifying theory of psychology, and a taxonomy of rule-based "rational errors" to name just a few! Think about purchasing a subscription.

Drs. Gregory Kimble and Michael Wertheimer presented the table of contents for Volume IV of the *Portraits of Pioneers in Psychology*. Eight of the 18 to 21 chapters are in first-draft or later revisions. The EC authorized the purchase of a new printer for this project, Dr. Wertheimer noted that he found the teamwork with Dr. Kimble very positive and rewarding. Just to add credence to the saying that "No good deed goes unpunished", the possibility of a Volume V was also discussed, with literally dozens of names having been mentioned either in the meeting or prior to the meeting in written communication between members. Perhaps a focus on underrepresented individuals in the next volume?

The President's report by Dr. Salzinger included information on our Division 1 Web Site, which is 3 or 4 years old, and completely out of date. Dr. Salzinger had gotten two colleagues at Hofstra University, Duong (Dennis) Ba Nguyen and Marc Carter to serve as OUT Web Masters and to update the web site. Ideas being considered include: placing the newsletter on the web site; having the web site address in the newsletter each issue; having access for membership applications on the site; making recommendations for fellow status, as well as, building links to other divisions and to the National Psychology Archives in Akron. So look to the future for announcements (or as my students might say "Surfs Up") about this soon-to-be new source of information about our Division.

Dr. Salzinger's report also noted that Dr. Arthur Staats had sent a detailed description of what he felt the criteria should be for the Staats Lecture for Unifying Psychology Award. These were reviewed and the EC voted on the nominees. At the 1999 APA convention, an announcement will be made at the Focus on Science Plenary session (tentatively scheduled for Saturday, August 21 from 11:00 - 11:50) that the first annual Arthur Staats Award will be made to Dr. Edward O. Wilson. The tentative plan is to have a conversation/presentation hour (from 5:00 to 6:00 on Saturday, after the Business Meeting) for Dr. Wilson and also to announce the winners of the other divisional awards,

The Council apportionment ballot results were received and we acquired just enough votes to have one seat on Council,

A request for the nomination of one member from each division to the Committee on Division/APA Relations (CODAPAR) was received, and Dr. Kimble was nominated by the EC from our Division,

On the Nominations and Election Committee, Drs. Frank Farley and Gregory Kimble were working on the potential slates of officers for President and the two Member-At-Large positions and these will appear in the newsletter.

Dr. Alan Boneau has agreed to return as the Editor of the Division's newsletter, *The General Psychologist*. The previous issue was completed under Drs. Farley's and Lyon's direction, with Volumes 2 and 3 in a single issue.

Dr. Morton Ann Gernsbacher has accepted the position as Chair of the Fellows Committee, with Drs. Wertheimer and Butcher serving on the committee. She presented a report of the current applicants for Fellow, as well as, written listings of the past recommendations for fellow for the years of 1996 and 1997. This year approximately 50 members have expressed some interest in applying for Fellow status. About half are Current Fellows (those who have previously achieved Fellow status in another division) and the other half are New Fellows (members who are applying for their first Fellow status). Two new Fellows, Wendy Williams and Judy Hall were nominated and accepted, and Kaye Coleman was accepted as a Current Fellow. There were 32 Current Fellows accepted in 1998, as compared to 26 in 1997. In 1996, coincidentally, a total of 96 Current Fellows were accepted. Do you know someone who should be nominated for Fellow status in the Division? Are you aware that as a member, you can self-nominate?

D. Alan Boneau has agreed to serve as the Chair of the Awards Committee for the next few years. A new timetable for awards has been developed and a press release was sent out for the 1999 Awards competition. Beginning next year, the new timetable will have a deadline for submission of all awards to March 15. In that way the final 3 or 4 entrants can be selected as soon as possible by mid-June, but no later than July 4. The final decision date for the winner of each award is to be August 1, so that the results can be announced at the annual APA convention, and winners have a year to prepare their address for the following year at APA. Currently, the procedures for all awards are being reviewed, so that the methods used will be consistent both in the Divisions' Operations Handbook and in the By-Laws. The following are the individuals responsible for reviewing applications for the awards to be selected in 1999, to be presented in 2000:

William James Book Award	Lewis Lipsitt
Hilgard Lifetime Achievement	Frank Farley
Miller Outstanding Article	Kurt Salzinger
Lindzey Dissertation	Wendy Williams
Soneau Award	Kurt Salzinger
Staats Lecture for Unifying Psych	Arthur Staats

Several issues were raised regarding the Lindzey Dissertation



award including: the small number of submissions in recent years; the problems in determining if a dissertation was in general psychology; the need for continuing this category of award; and whether some other area might be used for the award in place of dissertations, Dr. Wendy Williams will review these issues and make recommendations to the EC about the Continued status of this award.

As the 1999 William James Book Award Chair, Dr. Salzinger reported that the award winners are: Steven Pinker's *How the Mind Works*. (W. W. Norton & Co.); Stuart A. Vyse's *Psychology of Superstition*. (Oxford University Press).

Several motions were made and approved by the EC regarding the issue of Awardee registration fees, so that each year, all awardees will have their convention registration fee paid, for the year in which they give their presentation-, and this policy will be included in the revision of the Operations Handbook.

The Divisions's new Secretary /Treasurer is Dr. Lee Matthews, The Division's total assets, based on the Preliminary December (1998) Financial Statement are approximately \$70,500, As noted in the earlier membership report, we currently have a large number of dues exempt members in the Division, while at the same time, we have few student members, so membership needs to be increased. The need to more actively seek institutional subscriptions was again mentioned.

The revision of the Division By-Laws and The Operations Handbook are underway. Drs. Boneau and Wertheimer reported on the draft update of the Operations Handbook. They are working on the bylaws revision with the assistance of APA legal counsel, and various members of the EC are working on sections of the document.

A draft of a Calendar of Responsibilities for the year was distributed for review by the EC. Several areas were identified as needing updates on the calendar. These included the schedules for: fellows' applications; membership renewal mailing dates; submission dates for apportionment ballots; convention program deadlines; and various journal and newsletter deadlines.

New business items included renewal of the Division's annual membership fee of \$200.00 for 1998-1999 in the Federation of Behavioral, Psychological and Cognitive Sciences. The EC also made a \$100.00 donation to the psychology archives at the University of Akron.

Drs. Salzinger and Farley noted their thanks to Dr. Mary Ann Lyons for taking over the newsletter and revitalizing it this past year.

Dr. Salzinger announced that the next EC meeting will be on Thursday evening, August 19, 1999 at the APA Convention in Boston. The starting time will be determined later, If you have any issues you wish for your officers to discuss, please feel free to contact any member of the EC,

The EC meeting was adjourned at 2:38 PM on January 31, 1999.

Respectfully submitted, and looking forward to meeting all of you at the Division Business meeting

*Lee*

Lee Matthews, Secretary /Treasurer

**The Society for General Psychology**  
Division One: American Psychological Association

To become active in Division One, please determine the category that best describes your intended relationship with the Division, submit the needed information and dues, if any, to the Secretary-Treasurer of the Division at the address below.

**A P P L I C A T I O N F O R M**

If you are a member (**Mem**) of APA (Fellow, Member, or Associate), there are no dues required at this time. APA will apply any dues and assessments on your next Dues Statement (currently \$20 which includes \$12.50 for *General Psychology Review*).

If you are an APA Affiliate (**Aff**) (International, High School, or Student), we need a \$7.50 dues payment to initiate your affiliation with Division One. Students (**Stu**) who are not Student Affiliates of APA may also affiliate with the Division but the application must be signed by a psychology instructor.

An interested individual who does not qualify for one of the above categories may still qualify as an Individual Affiliate (**Ind**) of the Division upon payment of \$7.50 for current dues. Please check the following category(ies) that best describes your status:

- Member APS  Eligible for APA membership
- Other qualifications

If you are not eligible for APA membership, please submit a letter that describes your qualifications and interest in General Psychology.

Name \_\_\_\_\_

Mail Address \_\_\_\_\_

City/State/Prov. \_\_\_\_\_ ZIP \_\_\_\_\_

Country \_\_\_\_\_

Membership Category (Circle One): **Mem Aff Stu Ind**

APA Membership Number (if applicable) \_\_\_\_\_  
Instructor's Signature  
(when applicable) \_\_\_\_\_

Instructor's Affiliation \_\_\_\_\_

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# The **GENERAL** *Psychologist*

## **C o n t e n t s**

Karon: <i>On Schizophrenia</i> . . . . .	1
Kimble: <i>Interviewed</i> . . . . .	13
Salzinger: <i>On Words and Theory</i> . . . . .	17
Salzinger: <i>President's Message</i> . . . . .	23
Society Election: <i>Candidate Blurbs</i> . . . . .	24
Executive Committee Minutes. . . . .	29

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