Letters to the Editor

Risperidone-Induced Tardive Dyskinesia

TO THE EDITOR: A few reports of tardive dyskinesia occurring while a patient was receiving risperidone have appeared in the last few years (1–5). However, these cases are fraught with complex factors that might have caused the dyskinesia—i.e., previous exposure to other neuroleptic drugs, a high propensity for developing tics, old age, and systemic illness that affected the central nervous system. Here we report a case of tardive dyskinesia that developed while a patient was on a regimen of risperidone.

Ms. A, a 21-year-old, single, female college student, came to the Samsung Medical Center Outpatient Clinic with complaints of auditory hallucinations. She had had ideas of reference and difficulties in social adaptation for the past year or so. She did not have any major illness or movement disorders. Her motor, sensory, and cerebellar functions were intact; no focal signs were detected. Results of an EEG were normal. After being diagnosed with schizophrenia according to DSM-IV criteria, Ms. A was placed on a regimen of risperidone, 1 mg/day, and trihexyphenidyl, 2.5 mg/day. Her symptoms subsided gradually during the first 2 months, and there were no adverse effects such as extrapyramidal symptoms. Her dose of risperidone was increased to 1.5 mg/day, and trihexyphenidyl treatment was discontinued by the third month. At this point, her psychotic symptoms had subsided completely, and her medication dose was maintained.

In the fifth month of treatment, Ms. A began to show involuntary perioral and tongue movements. Chewing motions and vermicular movements of the tongue were noticeable even when her mouth was closed. No other involuntary movements were seen in the rest of her body. Neither cogwheel rigidity nor tremor was observed. One month after the onset of dyskinetic symptoms, risperidone treatment was replaced with sulpiride, 100 mg/day. Two months of sulpiride treatment did not cause any significant improvement in Ms. A's tardive dyskinesia symptoms; therefore, her medication was changed to a regimen of clozapine. She soon switched back to sulpiride because she could not tolerate the sedative effect of clozapine. Presently, Ms. A is on a regimen of sulpiride, 100 mg/day, which maintains her remission from psychotic symptoms. Her dyskinetic symptoms are held to a minimum. We wish to change her medication to olanzapine in the near future.

In conclusion, unlike previous reports, this one clearly demonstrates the cause-and-effect relationship of risperidone and tardive dyskinesia in a patient whose clinical course was well documented. We want clinicians to be aware of the possibility of patients developing tardive dyskinesia when they are given the supposedly safe neuroleptic risperidone.

REFERENCES

1. Addington DE, Toews JA, Addington JM: Risperidone and tardive dyskinesia: a case report. J Clin Psychiatry 1995; 56: 484–485
2. Buzan RD, Thomas MR, Firestone D, Franklin DA: Risperidone-induced tardive dyskinesia (letter). Am J Psychiatry 1996; 153:734–735; correction, 851
3. Woerner MG, Sheitman BB, Lieberman JA, Kane JM: Tardive dyskinesia induced by risperidone? (letter). Am J Psychiatry 1996; 153:843
4. Feeney DJ, Klykylo W: Risperidone and tardive dyskinesia. J Am Acad Child Adolesc Psychiatry 1996; 35:1421–1422
5. Gwinn KA, Caviness JN: Risperidone-induced tardive dyskinesia and parkinsonism. Mov Disord 1997; 12:119–121

KYUNG SUE HONG, M.D.
SEONG SIM CHEONG, M.D.
JONG-MIN WOO, M.D.
EYONG KIM, M.D.
Seoul, South Korea

Air Disaster and Posttraumatic Stress Disorder

TO THE EDITOR: In their prospective study, Richard S. Epstein, M.D., Carol S. Fullerton, Ph.D., and Robert J. Ursano, M.D. (1), cursorily mention preexisting mental traits, the quality and degree of exposure to trauma, the level of social support, and posttraumatic stress as having been identified as risk factors for posttraumatic stress disorder (PTSD) in previous research.

However, they ignore important aspects of the 1988 Ramstein disaster. Ramstein Air Force Base is located in Germany. The subjects examined were not part of the affected community and represented a minority of the German population; they were U.S. military personnel living in a ghetto-like situation with limited social intercourse, and they were not unequivocally accepted by the German civilian population. Could the impact and subjective meaning hereof be irrelevant to pre-traumatic self-perception and mental status?

The majority of spectators and casualties were German civilians—i.e., members of a sociocultural entity different from that of the U.S. health care workers. There were considerable problems in coordinating the efforts of local rescue forces with those of the U.S. military during evacuation. These factors certainly influenced the perception of, and reaction to, the traumatic event.

Subsequently, local media condemned the operation. Instead of being regarded as highly competent professionals whose "heroic efforts" saved many lives, the U.S. health care workers were confronted with criticism within the German community, thus more likely than not imposing specific demands on posttraumatic coping strategies.

REFERENCE

1. Epstein RS, Fullerton CS, Ursano RJ: Posttraumatic stress disorder following an air disaster: a prospective study. Am J Psychiatry 1998; 155:934–938

EDWARD J. GUERIN, DR.MED.
Wittlich, Germany

Dr. Ursano and Colleagues Reply

TO THE EDITOR: Edward J. Guerin, Dr.Med., draws our attention to the complexity of disasters—in particular, international disasters. Although we are not clear about what is meant by his construct "pre-traumatic self-perception" and we would disagree with his construct of "ghetto-like" living, we agree that community response and community cohesion can be very important to disaster recovery (1, 2). Our study did not directly address the community variables that Dr. Guerin refers to. However, in previous studies, it has been shown that emotional closeness to disaster victims and identification with the dead (3, 4) both affect the "metabolism" and "digestion" of posttraumatic stress symptoms and the risk of PTSD. Studies (5) suggest that the recovery environment can either facilitate or prevent recovery from PTSD following traumatic events. PTSD after exposure to trauma and disasters can be acute or chronic and potentially disabling.

REFERENCES

1. Steinglass P, Gerrity E: Natural disasters and post-traumatic stress disorder: short-term versus long-term recovery in two disaster-affected communities. J Applied Social Psychol 1990; 20:1746–1765
2. Bartone PT, Wright KM: Grief and group recovery following a military air disaster. J Trauma Stress 1990; 3:523–539
3. Jones DR: Secondary disaster victims: the emotional effects of recovering and identifying human remains. Am J Psychiatry 1985; 142:303–307
4. Fullerton CS, McCarroll JE, Ursano RJ, Wright KM: Psychological responses of rescue workers: fire fighters and trauma. Am J Orthopsychiatry 1992; 62:371–378
5. Ursano RJ, Fullerton CS, Norwood AE: Psychiatric dimensions of disaster: patient care, community consultation, and preventive medicine. Harv Rev Psychiatry 1995; 3:196–209

ROBERT J. URSANO, M.D.
RICHARD S. EPSTEIN, M.D.
CAROL S. FULLERTON, PH.D.
Bethesda, Md.

The "Very Old"

TO THE EDITOR: I was surprised to learn from the article by Anna-Karin Berger, M.Sc., and associates (1) that I have reached "very old" age. It is true that an older cousin told me a few years ago that after 75, it is all downhill, but at 77, I am still waiting for that to happen. I could point to my own activities after age 75 (a new career, my first competitive orienteering, among others), but perhaps it might be more convincing to recall that Sophocles wrote *Oedipus Colonnus* at 90, Bernard Baruch continued to be an adviser to presidents at the same age, and Bernarr MacFadden died swimming a river in his late 80s.

I suggest that the very old must be defined operationally rather than by an author's fiat. In a previous article, *Journal* authors defined the very old as those over 85 (2). In fact, neither 75 nor 85 is a number based on observed changes. The assumption that a period of human life can be termed very old age and that this period can be defined by a number of years needs to be validated before it can be used as a basis for statistical research.

Over a century ago, Otto von Bismarck defined retirement age as an arbitrary number of years. That number has frustrated legions of healthy men and women and is now liable to bankrupt our Social Security system. I hope geriatricians will resist efforts to create another arbitrary standard that may turn out to be equally destructive.

REFERENCES

1. Berger A-K, Small BJ, Forsell Y, Winblad B, Bäckman L: Preclinical symptoms of major depression in very old age: a prospective longitudinal study. Am J Psychiatry 1998; 155:1039–1043
2. Skoog I, Aevarsson Ó, Beskow J, Larsson L, Palsson S, Waern M, Landahl S, Östling S: Suicidal feelings in a population sample of nondemented 85-year-olds. Am J Psychiatry 1996; 153:1015–1020

OLIVER FRENCH, M.D.
Ithaca, N.Y.

Dr. Bäckman and Colleagues Reply

TO THE EDITOR: We accept the premise of Oliver French, M.D., that the definition of very old age is far from an exact science. Like many other classificatory labels, determining which individuals fit the category and which do not is a relativistic matter. Neugarten (1) was among the first to differentiate between specific segments of the aging population, making the distinction between the "young-old" (55–74 years) and the "old-old" (75-plus years). More recently, authors have referred to individuals above 85 years as the "oldest-old" (2), and centenarians, those who are 100 years and older, have received some empirical attention (3). Thus, terms such as "old" or "very old" are not static entities. Their meanings vary across time and cultures. As the life expectancy continues to increase because of changes in medical technology and lifestyle behaviors (4), and with the advent of possible means to extend the human life span by means of chromosomal modification (5), the definition of very old age is likely to be altered in the near future.

Even finding a clear-cut operational definition of very old age at a given point in time may be difficult, because this segment of the population is characterized by such large interindividual variability—biological, cognitive, and social (6). Although our use of the term "very old age" largely reflects conventions within our specific domain of research, we take some comfort in noting that the average life span for men varies between 69 and 77 years in the industrialized countries at present; the corresponding figures for women range between 77 and 82 years (7). The mean age in our study group was 83.2 years. Thus, the term we used would not seem to be completely out of bounds.

We finally note that Dr. French's cousin carries a much more pessimistic view of late life than is scientifically justified. It is not "all downhill" after age 75! It is true that various biological and cognitive functions decline as part of the normal aging process and that several diseases known to be detrimental to psychological functioning (e.g., dementia) increase dramatically in advanced age (8). It is equally true, however, that many individuals who live into their 80s and above are not fundamentally different from what they were 30 years earlier. Studies of memory in individuals carefully screened for health indicate minimal losses from the 70s through the 90s (9); the prevalence of depression does not increase in late life (10); the majority of healthy elderly adults benefit greatly from cognitive interventions (11); and life satisfaction appears to remain stable or increase rather than decrease from midlife through old age (12), to mention a few among several possible examples. Thus, although the remarkable accomplishments in late life by Sophocles, Picasso,

and Rubinstein may represent extreme points on a continuum, the relative maintenance of functioning in old age exhibited by these individuals is shared by a substantial proportion of the population at large.

REFERENCES

1. Neugarten BL: Age groups in American society and the rise of the young-old. Annals of the Am Acad of Political and Social Science 1974; 415:187–198
2. Lindenberger U, Baltes PB: Intellectual functioning in old and very old age: cross-sectional results from the Berlin Aging Study. Psychol Aging 1997; 12:410–432
3. Poon LW, Sweaney AC, Clayton GM, Merriam SB: The Georgia Centenarian Study. Int J Aging Hum Dev 1992; 34:1–17
4. Suzman RM, Willis DP, Manton KG: The Oldest Old. New York, Oxford University Press, 1992
5. Bodnar AG, Ouellette M, Frolkis M, Holt SE, Chiu CP, Morin GB, Harley CB, Shay JW, Lichtsteiner S, Wright WE: Extension of life-span by introduction of telomerase into normal human cells. Science 1998; 279:349–352
6. Baltes PB, Baltes MM: Successful Aging: Perspectives From the Behavioral Sciences. New York, Cambridge University Press, 1990
7. Ebrahim S, Kalache A: Epidemiology in Old Age. London, BMJ Publishing Group, 1996
8. Ott A, Breteler MM, van Harskamp F, Grobbee DE, Hofman A: Prevalence of Alzheimer´s disease and vascular dementia: association with education. BMJ 1995; 310:970–973
9. Hill RD, Grut M, Wahlin Å, Herlitz A, Winblad B, Bäckman L: Predicting memory performance in optimally healthy very old adults. J Ment Health and Aging 1995; 1:57–67
10. Wittchen HU, Knäuper B, Kessler C: Lifetime risk of depression. Br J Psychiatry 1994; 165:16–22
11. Neely AS, Bäckman L: Effects of multifactorial memory training in old age: generalizability across tasks and individuals. J Gerontol B Psychol Sci Soc Sci 1995; 50:P134–P140
12. Latten JJ: Life course and satisfaction: equal for everyone? Social Indicators Res 1988; 21:599–610

LARS BÄCKMAN, PH,D.
BRENT J. SMALL, PH.D.
YVONNE FORSELL, M.D., PH.D.
BENGT WINBLAD, M.D., PH.D.
ANNA-KARIN BERGER, M.SC.
Stockholm, Sweden

Skills Training or Occupational Therapy for Persistent Schizophrenia

TO THE EDITOR: It was with dismay that I read the article by Robert Paul Liberman, M.D., and colleagues (1). Occupational therapists have worked in the mental health system for over 75 years, and many of the current psychiatric rehabilitation practice models incorporate occupational therapy theory and practice. It was therefore a matter of great concern to read about research that questioned the efficacy of psychosocial occupational therapy. I believe there are several flaws in the research design and the article that should be acknowledged.

With no apparent literature or references, the authors state that occupational therapy treatment "[consists of] expressive art and crafts and recreational activities [used to] build self-esteem and productivity." While these media are used in occupational therapy programs, they are not the only media used. In a 1997 survey of 243 occupational therapists practicing in mental health, the following interventions were regularly used by more than 50% of those responding: training in daily living skills (62%), assertiveness training (64%), behavior management (58%), coping skills (56%), self-awareness exercises (60%), social skills training (74%), and time management (66%) (unpublished American Occupational Therapy Association Mental Health Special Interest Section Survey of Mental Health Practitioners, 1997).

It is unclear when the authors' research occurred. They mention that the program began in 1981 but not when the study was conducted. There have been significant changes in mental health service delivery since 1981—e.g., managed care, more effective psychotropic medications—which have affected modalities used by occupational therapists.

While I applaud the study of the efficacy of interventions with people with persistent mental illness and Dr. Liberman's significant contributions, it does not seem appropriate to evaluate the whole of occupational therapy by only one of its modalities. While constructive and expressive activities are a part of occupational therapy, according to the American Occupational Therapy Association's Practice Guidelines for Adults With Schizophrenia (2), occupational therapy "uses activities which are meaningful and relevant to the clients to improve functional performance…facilitate the client's engagement in the desired performance area… and experience success and gain confidence in his or her performance abilities." Occupational therapy "emphasizes…development of skills, environmental or task adaptations, compensatory strategies, and family/caregiver training to support function in desired and needed roles in activities of daily living, work and other productive activities and leisure" (2).

Occupational therapy continues to have significant value to individuals with serious mental illness in helping them adapt to their disability and recover their ability to function. As members of a profession and association, occupational therapists continue to be concerned about the outcome of our services to individuals across the continuum of life. A recent study demonstrated the positive effect of occupational therapy on a variety of measures of social functioning and mental and physical well-being (3). It is my hope that research will continue to explore efficacious interventions without encouraging inappropriate bias toward any of the disciplines that have the skills and technology to benefit individuals with serious mental illness. Please contact me if you or your readers would like more information about occupational therapy.

REFERENCES

1. Liberman RP, Wallace CJ, Blackwell G, Kopelowicz A, Vaccaro JV, Mintz J: Skills training versus psychosocial occupational therapy for persons with persistent schizophrenia. Am J Psychiatry 1998; 155:1087–1091
2. Kannenberg KR: Occupational Therapy Practice Guidelines for Adults With Schizophrenia. Bethesda, Md, AOTA, 1997
3. Clark F, Azen SP, Zemke R, Jackson J, Carlson M, Mandel D, Hay J, Josephson K, Cherry B, Hessel C, Palmer J, Lipson L: Occupational therapy for independent-living older adults: a randomized controlled trial. JAMA 1997; 278:1321–1326

JEANETTE BAIR, M.B.A., O.T., F.A.O.T.A.
Bethesda, Md.

TO THE EDITOR: In examining the article by Robert Paul Liberman, M.D., and colleagues, I found many inaccuracies regarding the definition of occupational therapy and in the research design. First, I was surprised that no occupational therapist who contributed to the study was listed as coauthor. Second, the title of the study is misleading. The authors' definition of psychosocial occupational therapy, "in which expressive art and crafts and recreational activities are the

media" (p. 1087), is outdated. Psychosocial occupational therapists today use a holistic approach and purposeful activities to increase the individual's repertoire of social and self-care skills, work behaviors, and leisure activities and use biofeedback, stress management, social skills training, exercise, and vocational exploration (1).

The next important issue deals with research methodology. Although 80 patients were assigned randomly to two groups, there were no assessments to determine that the groups were evenly matched regarding major variables of diagnosis or functional performance. We cannot assume that they were comparable groups without testing for equivalency. This would affect both internal and external validity.

Can another researcher replicate this study? In the psychosocial group, little detail was given regarding how the patients participated in the groups or individually. The skills training group did consist of modules and a trainer's manual that can be replicated.

Another research problem that was identified by the investigators was the bias inherent in the treatment of case managers. The case managers were different for each group, and their personalities may have affected the results. This remains an uncontrolled variable. The patients did not have an opportunity to rate the interventions and to determine if they were helpful. Having the clients assess treatment gives them an opportunity to evaluate its usefulness and relevancy. We do not know how sensitive the instruments were for patient improvement. We do know that many of the tests used have moderate reliability and validity that introduces measurement error. An examination of the data reveals that many of the standard errors of measurement were as large as the means; that can be interpreted as a large discrepancy between scores. A case-by-case analysis should have been done to determine which of the patients improved significantly on the variables measured. A major flaw in the study was the conclusion in the abstract, which is most damaging when many investigators examining studies may only look at the abstract. The authors conclude that skills training conducted by paraprofessionals is more effective than providing occupational therapy services. Skills training was provided by an occupational therapist in this study. This study shows that independent living skills training is effective with individuals with schizophrenia—something we have known for 40 years. The conclusion tells us nothing about psychosocial occupational therapy.

REFERENCE

1. Stein F, Cutler S: Psychosocial Occupational Therapy: A Holistic Approach. San Diego, Singular Publishing Group, 1998

FRANKLIN STEIN, PH.D., O.T.R./L., F.A.O.T.A.
Vermillion, S.D.

TO THE EDITOR: Although it is quite evident from the article by Robert Paul Liberman, M.D., and colleagues that the authors made considerable efforts to control extraneous variables and conduct a well-controlled experiment, there is one major flaw in this study—that is, a confounding of the independent variables.

The authors' abstract concludes: "Skills training can be effectively conducted by paraprofessionals, with durability and generalization of the skills greater than that achieved by occupational therapists who provide their patients with psychosocial occupational therapy" (p. 1087). There are two independent variables involved in this statement: namely, the treatment method and the provider characteristics. These two variables are confounded, and the unique effect cannot be isolated. If the research objective is to compare the effects of different types of providers, identical treatment should be used for both control and experimental groups by varying only the type of provider for each group. And if the research objective is to find the difference in the effects of two treatment methods, the same type of provider should be used for both groups.

The impression gotten from reading the article is that the authors' primary interest was to compare the two treatment methods in terms of durability and generalizability of the skills acquired during the treatment sessions. The treatment conditions and durations were comparable, except for the provider characteristics. Three certified occupational therapists provided the treatment for the "psychosocial group," whereas one occupational therapist (certification status not reported) and two paraprofessionals provided the treatment for the "skills training group." The information on the role of the occupational therapist in the latter group was lacking, as was the background information of the so-called "paraprofessionals." In order to compare the clinical skills of paraprofessionals and occupational therapists, many additional variables should be taken into consideration, such as educational background, hours of training, and job experience. These were not addressed in the article.

Two different assumptions can be made in regard to the provider groups, with alternative conclusions. Since occupational therapists are trained and qualified for both types of treatment approaches, it is possible that the occupational therapist in the "skills training group" helped develop the protocols and participated in the training of the paraprofessionals. If such was the case, this experiment could be interpreted to have compared two different treatment approaches used in occupational therapy.

According to the article, the paraprofessionals received extensive training for the treatment implementation. Both groups of providers, then, can be considered equivalent in regard to the qualifications and skills needed for particular therapeutic intervention. If the equivalency of the two groups of providers can be assumed, then the study compared two different treatment approaches provided by equally competent groups of providers.

In summary, the observed effect cannot be attributed to either treatment method or the difference of the treatment providers because of the confounding of the two independent variables. If the equivalency of the providers is assumed, this is a valuable comparison of treatment approaches, and the conclusion should address only the difference of the treatment approaches and not the provider characteristics. The authors' abstract conclusion is misleading, giving readers the impression that the authors are questioning the occupational therapist's clinical abilities.

SADAKO VARGAS, ED.D., O.T.R.
Union, N.J.

TO THE EDITOR: It is with great interest that I read the article by Robert Paul Liberman, M.D., and colleagues. As an occupational therapist concerned with the functional abilities of patients with chronic disorders, I am well aware of the need to attend to skills acquisition. I was not surprised that the subjects who received skills training, as compared to those receiving "expressive, artistic, and recreational activities," fared significantly better in domains associated with social skills development and retention.

However, the authors made several assumptions that were not only inadequately delineated but clearly erroneous. These assumptions misrepresented the conceptual and operational

definitions of the variables and the practice of occupational therapy and led to flawed methodology and conclusions.

Dr. Liberman and associates began their article by defining occupational therapy as "expressive arts and crafts and recreational activities…through which therapists build self-esteem and productivity" (p. 1087). This definition, and the subsequent study's conclusion, is deceiving. Inherent in occupational therapy practice is occupation, which is synonymous neither with arts and crafts nor with activities. Occupation reflects the participation in tasks and roles that allow for productivity within a personally relevant context, such as self-care/maintenance, work, or leisure. Modalities used within occupation are regularly subjected to a complex process of activity analysis in order to determine and monitor their therapeutic potential. This includes attention to specific skills that support adequate role function and skills that fall within the physical, cognitive, perceptual, psychological, and social arenas. Although some occupational therapists may occasionally use the types of activities described by Dr. Liberman and colleagues, many do not. Most occupational therapists who work with individuals with persistent forms of schizophrenia may address the psychological issues that affect performance, but they primarily structure assessments and treatment programs along a rehabilitative model oriented toward the development and maintenance of social and living skills.

The authors' second assumption suggests that social skills training is distinct from occupational therapy. Attention to social skills is and always has been a critical component of occupational therapy practice. In fact, one of the study's social skills module trainers was an occupational therapist. This research was more appropriately a study of expressive craft and art activities versus social skills training; it was not a study of the effectiveness of one group of mental health professionals versus another, as misstated by the authors.

Finally, the study's design assumes that "expressive art and crafts and recreational activities" oriented toward self-esteem facilitate the building of social skills. Fundamental to any research assumption is evidence, either empirical or theoretical, as to why such an assumption is appropriate. The authors failed to provide evidence indicating that these activities are appropriate modalities for social skills development. Interventions that focus on the psychological arena are very different from interventions that focus on rehabilitation and should be viewed through distinct lenses. The authors clearly neglected to make this distinction and consequently never defended the appropriateness of the independent variables.

In summary, social skills training, which is vital to the reintegration of clients with schizophrenia into the community, is an essential component of occupational therapy assessment and intervention. Expressive, artistic, and recreational activities, while not central to the development of social/living skills, are also not synonymous with the practice of occupational therapy.

JANET FALK-KESSLER, ED.D., O.T.R., F.A.O.T.A.
New York, N.Y.

TO THE EDITOR: We are writing to register our disapproval of the article by Robert Paul Liberman, M.D., and colleagues. The first objection we have is that the authors' definition of psychosocial occupational therapy—"the prevailing psychosocial treatment is occupational therapy in which expressive art and crafts and recreational activities are the media through which therapists build self-esteem and productivity" (p. 1087)—is too narrow and does not accurately describe occupational therapy as it is practiced today.

A more accurate definition of psychosocial occupational therapy includes 1) the development of cognitive, social, and organizational skills; 2) the ability to perform self-care skills such as personal hygiene and grooming; 3) the skills of home management, such as budgeting, cooking, and cleaning; 4) the skills of successful community living, such as the use of public transportation and community resources; and 5) the recognition of stress indicators and how to cope with stress.

The failure of social skills paraprofessionals to recognize "the learning disabilities of schizophrenic patients" is an example of a case in which professional occupational therapists can recognize a disability, while knowing methods of treatment to overcome it. A paraprofessional does not have the knowledge base to respond to unique situations in logical, scientific ways.

The title of the article does not represent the fact that both groups were essentially supervised by occupational therapists. We believe that this article misrepresents the occupational therapy profession.

MARGARET DRAKE, PH.D., O.T.R./L., A.T.R.-B.C., F.A.O.T.A.
CAROL TUBBS, M.A., O.T.R./L.
JOYCE TITUS, M.H.S., O.T.R./L.
LORRAINE STREET, M.O.T., O.T.R./L.
PETER GIROUX, M.H.S., O.T.R./L.
BETTE GROAT, M.A., O.T.R./L.
DONA LEE ANDREWS, A.B.D., M.SC., O.T.R./L.
ROBIN DAVIS, M.S., O.T.R./L.
ROSEMARY TENNANT, O.T.R./L.
AMELIA BARBARA EDWARDS, O.T.R./L.
PATSY HESTER, MED. O.T.
Jackson, Miss.

Dr. Liberman Replies

TO THE EDITOR: My colleagues and I appreciate being edified by the many letters that we have received regarding the broad scope of psychosocial occupational therapy, but we would find even more informative the actual use of specific occupational therapy modalities that are currently in use at a representative sample of psychiatric facilities. The unpublished 1997 survey from which Jeanette Bair, M.B.A., O.T., F.A.O.T.A., extracts self-reports may confuse the interventions used with the goals of the interventions; for example, at many psychiatric facilities, expressive arts and crafts as interventions are viewed as vehicles for improving cognitive capacities, daily living skills, coping skills, self-awareness, time management, and social skills. Until more reliable data are published, we will continue to use our personal observations from more than three decades of visits to hundreds of mental health facilities to assert that expressive arts and crafts are the modal form of psychosocial occupational therapy being used with psychiatric patients. We were pleased to see support for our findings from a recent evaluation by occupational therapists of 6 weeks of art therapy, which found no impact on the social skills of persons with chronic schizophrenia (1).

At the same time, we are cognizant of the wide range of modalities—including social skills training—used by occupational therapists to improve self-care and vocational, social, and independent living skills. We trust that the concerns raised by those occupational therapists who have written letters to the editor (Ms. Bair, Franklin Stein, Ph.D., O.T.R./L., F.A.O.T.A., and others) will lead to a broader utilization of skills training techniques by occupational therapists. It is important for the future of psychiatric occupational therapy

that empirically validated and replicable techniques are introduced into the field.

The intent of the study was not to compare the entire practice of occupational therapy with skills training. Rather, the study was designed to contrast a single modality—skills training—with an equally frequent, interpersonally delivered treatment that was credible and representative of the practice of many occupational therapists. Given the limitations of the study's design and procedures, it would be reductionistic for any reader to conclude that occupational therapy as a discipline, with a rich array of complex treatments, is less effective than a limited application of modularized skills training.

The question by Sadako Vargas, Ed.D., O.T.R., regarding the educational backgrounds of the therapists can be answered simply: the paraprofessionals who led the skills training groups had associate's degrees with a minimum of 10 years working with the mentally ill and 2 years of using the modules. One of the trainers was a registered occupational therapist who had 3 years of experience using the modules. Paraprofessionals and nonprofessionals have been shown to be effective in teaching social and independent living skills to individuals with serious and persisting mental disorders, by using the modules, in various field studies in the United States and overseas (2–4). None of the skills trainers designed or supervised the training procedures. All dependent measures were administered before random assignment to the treatment conditions, and the mixed-model repeated measures analysis of covariance controlled for any baseline cohort differences. In summary, we encourage the development, refinement, and empirical validation of interventions conducted by occupational therapists for improving the social, vocational, and independent living skills of persons with disabling forms of mental disorders. We hope that our study has contributed to this impetus, which will greatly benefit the clinical outcomes of our patients.

REFERENCES

1. Tam GWC, Tse PLC, Yu KKY: The effect of art therapy on interpersonal skills of persons with chronic schizophrenia. Hong Kong J Occupational Therapy 1998; 9:10–17
2. Eckman TA, Liberman RP, Blair K, Phipps CC: Teaching medication management skills to schizophrenic patients. J Clin Psychopharmacol 1990,10:33–38
3. Wallace CJ, Liberman RP, MacKain SJ, Blackwell G, Eckman TA: Effectiveness and replicability of modules for teaching social and independent skills to the severely mentally ill. Am J Psychiatry 1992; 149:654–658
4. International Perspectives on Skills Training for the Mentally Disabled (special issue). Int Rev Psychiatry 1998; 10:5–89

ROBERT PAUL LIBERMAN, M.D.
Thousand Oaks, Calif.

Posttraumatic Stress Disorder in Children of Holocaust Survivors

TO THE EDITOR: In the introduction to their article, Rachel Yehuda, Ph.D., and colleagues (1) wrongly concluded from Solomon et al.'s study (2) of Israeli soldiers who developed posttraumatic stress disorder (PTSD) as a result of combat-related stress that "that study provided…empirical demonstration of a greater vulnerability of offspring of Holocaust survivors to…stressful events." This conclusion is not supported by Solomon et al.'s data. Solomon et al. may only have demonstrated that some of them were more vulnerable to PTSD. They did find statistically significant differences between their index and control groups in the number of symptoms reported. Inspection of their findings, however, suggests that there was considerable overlap between the groups. Furthermore, the mean frequency of any of the five symptoms for which data are reported is less than one per participant, raising some question about the clinical significance of the findings. Finally, because the study was based on questionnaire self-reports, the results may represent nothing more than differences in proneness of the respective groups to report symptoms and not a difference in vulnerability to PTSD.

By contrast, two community-based studies that used probability sampling techniques and widely differing measures of functioning failed to find differences between adult children of survivors and appropriate comparison groups (3, 4).

Dr. Yehuda et al.'s studies (1; R. Yehuda et al., unpublished data, 1998) help us understand the significance of Solomon et al.'s finding. Dr. Yehuda et al. (1) selected a group of Holocaust survivors with PTSD and assessed their adult children. In limiting their study to this population, they reduced the risk of biased findings that might result from their nonrandom sampling method. They found that the children of these survivors were at greater risk than their comparison group for developing PTSD.

What can we conclude from the studies already mentioned about the intergenerational effects of prolonged, potentially traumatic stress? The community studies cited found no evidence for it under normal circumstances in the adult children of people exposed to such stress. (They did not show that there is no difference; one cannot prove the null hypothesis.) Solomon et al.'s study may have shown that some of these children are vulnerable in severe, life-threatening circumstances. Dr. Yehuda and colleagues' studies demonstrated that having a parent with PTSD may be one of the factors predisposing children to this vulnerability.

That troubled families have vulnerable children is not a new discovery. It is not unique to families of survivors of the Holocaust or of ex-prisoners of war from the various wars in the Far East who have been shown to suffer long-term morbidity and mortality rates similar to those of Jewish and non-Jewish survivors of the Nazi extermination camps. What may be unique, but is yet to be established, is whether this occurs with greater frequency or in different ways in the families of survivors of these atrocities or in other conditions of prolonged, severe adversity. Neither Solomon et al.'s study nor Dr. Yehuda et al.'s studies can answer this question, nor have they claimed to do so.

REFERENCES

1. Yehuda R, Schmeidler J, Giller EL Jr, Siever LJ, Binder-Brynes K: Relationship between posttraumatic stress disorder characteristics of Holocaust survivors and their adult offspring. Am J Psychiatry 1998; 155:841–843
2. Solomon Z, Kotler M, Mikulincer M: Combat-related posttraumatic stress disorder among second-generation Holocaust survivors: preliminary findings. Am J Psychiatry 1988; 145:865–868
3. Schwartz S, Dohrenwend BP, Levav I: Nongenetic familial transmission of psychiatric disorders? Evidence from children of Holocaust survivors. J Health Soc Behav 1994; 35:385–402
4. Sigal JJ, Weinfeld M: Trauma and Rebirth: Intergenerational Effects of the Holocaust. New York, Praeger, 1989

JOHN SIGAL, PH.D.
Montreal, Que., Canada