

## RESPONSE TO THE COMMENTARIES

Stephen E. Wong  
*Florida International University*

### RESPONSE TO RAKOS

I thank Dr. Rakos for the title of his commentary, which alludes to the present narrow applications of behavior analysis mainly in the habilitation of persons with developmental disabilities. While the accomplishments in that realm are extremely impressive, this was not B.F. Skinner's or other pioneering behavioral researchers' vision of a technology that could be used to address the full range of society's ills.

Dr. Rakos suggests that "belief in free will" is the underlying antagonist to behavior analysis, and this "sense of autonomy" may actually have a natural or biological foundation as a primary reinforcer. I would agree with Dr. Rakos that many people have negative reactions to "overt, discernable behavior control." This is a reaction that has made behavioral practitioners the target of severe criticism and adverse reactions that we should appreciate and respond to better. My own speculations about the reasons for this negative reaction lead me to different set of causes than those proposed by Dr. Rakos.

First, reacting negatively to overt behavioral control is adaptive and reasonable, if one examines group behavior and human history. People are likely to use whatever power they obtain to benefit themselves, often at the expense of those who they control. Given this tendency, one can see how people might react by opposing new forms of social control. Thus, people's resistance to new systems of control erected by behavior analysts, a group of obscure scientists and professionals whose jargon is cold and menacing (e.g., reinforcement, punishment, extinction), might be the generalization of that defensive response. Second, whenever behavior analysts attempt to change the operation of programs, they are likely to come into conflict with current authorities and organizational hierarchies. Whether it involves training staff on how to respond when clients act bizarrely or selecting the facility's recreational activities for the week, these decisions probably are within the domain of some other professional (e.g., activity therapist, program director, or psychiatrist) who may approach the situation differently and who will probably want to maintain independent control over his or her practice.

Third, I see "free will" and "autonomy" as ideas, and highly abstract ones at that. The influence of these ideas appears to vary across cultures and seems to be socially determined. Individuality, free will, and autonomy are all ideas that permeate American society. These notions displace alternate values, such as cooperation, equality, and social justice, which could compose a national ethic and guide social behavior and public policy, as they do in Scandinavian countries. The American independent spirit also differs from core values of the Japanese that center around obligation, loyalty to the group, and conformity. Looking at the Scandinavians and the Japanese, free will and

autonomy do not appear to be such powerful or universally held values. My sociology readings also make me suspicious of popular ideas, as these prevailing notions are often promoted by and buttress the existing power structure (Mann, 1986). Ascendancy of the former set of ideas permits powerful groups in our highly capitalistic society (e.g., wealthy individuals, financiers, corporations) to freely influence public opinion, shape government policy, exploit natural resources, and in other ways autonomously act upon the social and physical environment to advance their self-interest. Could Americans' allegiance to free will and autonomy partly be the result of over 300 years in which wealthy and powerful groups in this country glorified and sanctified these particular values, values that also happen to perpetuate a system of social relations that allow them to dominate others? I don't know. But, I would be inclined to first examine social rather than biological factors when accounting for the popular attraction to "free will" and "autonomy."

### RESPONSE TO SALZINGER

I thank Dr. Salzinger for recounting the relatively open atmosphere within the state mental hospitals in the late 1950s and 1960s during which early behavioral research was conducted. The welcome extended to Dr. Salzinger, who was willing to work in bleak conditions with seemingly hopeless patients, is similar to that described by Dr. Teodoro Ayllon (Ayllon, 2006). No doubt the easy access to and freedom to work within these institutions contributed to the productivity and many scientific discoveries of early behavioral researchers in this field.

Dr. Salzinger correctly notes that the decline of behavioral programs with psychotic disorders is not due solely to the rise of pharmacological treatments, but also to the decline of the state mental hospitals within which these programs were administered. Camarillo State Hospital, in Camarillo, California, whose Clinical Research Unit was reputedly the longest continuously operational token economy in the world, and where I did my own initial research with psychosis, no longer exists. The patients have been dispersed and the hospital grounds have been converted into the campus of a state university. The last state hospital that I worked in, Northeast Florida State Hospital, in Macclenny, Florida, was targeted for closing the year I began work there. Only through an organized campaign involving patients' families, mental health professionals, hospital workers, and community residents was this planned closing halted. But, I think Dr. Salzinger somewhat misinterprets my message in that I do not lament the passing of the state mental hospitals or even the few token economies contained within them. Rather, I sought to remind readers of the potential benefits of behavior analytic approaches to psychotic disorders, and to offset the dearth of recent literature on this topic (Dickerson, Tenhula, & Green-Paden, 2005, being an exception to this pattern).

My major divergence from Dr. Salzinger is on how to view the activities of biomedical psychiatry and the promotion of psychotropic medications by pharmaceutical companies. I am reminded of the ritualized combat of certain Native American tribes and their use of a "coup stick." Men of these tribes demonstrated their stealth and bravery,

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and gained honor as warriors by merely touching an enemy with their coup stick. This Native American approach to warfare must have resulted in disastrous initial encounters with Western Europeans who were armed with rifles and pistols and eager to use their lethal firepower. I think the same might be said for some behavior analysts discussing the work of biomedical psychiatrists and psychopharmacology researchers. These latter groups are professional competitors and not our “friends” (see Abbott, 1988, for a discussion of professions previously subordinated or eliminated by medical practitioners). Behavior analysts’ commitment to rigorous empirical data as the ultimate proof in accounting for behavior/environmental relations actually can be a *handicap* in dealing with certain competitors who merely simulate the process of scientific investigation to obtain data that will allow them to justify and promote commercial products that reap financial profits. I am not for an instant suggesting that we abandon our commitment to science and become marketers of behavioral technology; however, I do believe it is important to be aware of how our competitors operate, how they aggressively work to influence the opinion of the public and professionals, including ourselves, and that we actively resist those influences.

### RESPONSE TO MITCHELL

In the first paragraph of his commentary Dr. Mitchell writes, “...I have routinely used the DSM extensively as part of the assessment process and I have frequently referred clients for medications as a crucial means for alleviating the painful and debilitating symptoms that can accompany mental illness.” He is, of course, not alone. Most mental health professionals and educators operate within service systems and institutions that are designed around the biomedical model. I, too, was initiated into the biomedical approach and worked within this orientation for many years at the start of my career. During that time, I was puzzled by the frequent inability of medication to produce sought-after gains, and the repeated ability of behavioral interventions to surpass psychotropic drugs in producing specific behavioral improvements. My psychiatric supervisors informed me that the medications were not effective in these particular instances because the patients were rare “non-responders,” and that patients who were benefiting from behavioral programming were only receptive to these psychosocial interventions because of the drugs they were receiving. So, my observations were explained away and I was reassured that the drugs were indeed effective and essential.

The cumulative effect of being immersed in these sorts of pro-drug rationalizations while employed in medically-dominated institutions can be quite remarkable. Many non-medical human service professionals (e.g., psychologists, social workers, and teachers) eventually abandon their own principles and technology for improving human functioning in favor of biomedical treatments, typically drugs. There are psychologists who do not want to provide psychotherapy or to administer behavioral programs, social workers who are not driven to strengthen their clients’ social relationships or to enrich their living environments, and teachers who are not interested in refining their instructional methods or revitalizing their curricula. These professionals have seemingly

lost faith in the foci of their disciplines and may instead seek to mimic medical professionals by obtaining prescription privileges. Dr. Mitchell correctly notes that many people prefer drugs over psychosocial treatments for emotional and behavioral disorders because of their seeming ease of use and lessened sense of personal responsibility. I would add, however, that many of these people act on the advice of mental health professionals who themselves have been biomedically indoctrinated.

I agree with Dr. Mitchell's observation that the self-promotion of psychiatry and the pharmaceutical industry are normal professional and business activities, and not peculiar to these groups. The American Psychological Association (APA), the National Association of Social Workers (NASW), and the Association for Behavior Analysis (ABA) also engage in self-promotion. I, too, fail to see the rationale for the APA's exclusion of "organizational, community, or educational applications..." other than on the basis of arbitrary, professional practice boundaries. But, neither the APA, NASW, ABA, nor a multitude of other human service professional organizations present the kind of threat posed by the alliance of biomedical psychiatry and the pharmaceutical industry. Because psychiatry is a branch of medicine that oversees other human service professions and because the pharmaceutical industry is such an immense and profitable enterprise, together these giants have determined how we conceptualize and talk about mental disorders, how we treat them, and what sorts of service systems do and do not exist. This is a hegemonic and monopolistic control which the APA, NASW, ABA, and other lesser professional groups can only dream about, if they ever had such imperialistic ambitions.

### **RESPONSE TO BLUMENTHAL**

Many of Dr. Blumenthal's comments are in agreement with my paper, and, quite naturally, I find little in these paragraphs with which to take issue. However, there are a couple of points in her commentary that I can respond to.

Dr. Blumenthal notes that scientific approaches to behavior often seem to lose out to more "exciting explanations" involving dreams, superstition, multiple personalities, or even witchcraft. I must sadly concur. As I sit in a Borders bookstore sipping on a raspberry mocha freeze while drafting this response, I can look over at the Psychology section and see that it is filled with the works of Freud, pop psychology books, and DSM-IV-TR manuals and study guides. Clearly, behavioral psychology and psychological science in general have failed to capture the public imagination.

Dr. Blumenthal suggests that one of the reasons why "people persist in bad behavior" is that they are poorly informed about the science of behavior. She recommends early education in behavioral principles, much like mandatory health and sex education classes currently provided in public schools. This sounds like an excellent strategy for advancing behavioral hygiene and a means of possibly preventing a host of mental disorders, if only behavioral professionals could muster the political force necessary to get this content included in public school curricula.

Dr. Blumenthal's commentary has an optimistic tone and supposes that more effective behavior solutions will eventually diffuse through universities and schools into

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the larger society, analogous to the discovery of fire being passed “from cave to cave” in prehistoric times. My own cloudy crystal ball is darker, as I do not believe that modern society necessarily adopts technological advances that produce the greatest good for the greatest number. A current and conspicuous example of this is the American love affair with the SUV. In a time when we depend on oil bought at steep prices from hostile and politically unstable foreign countries, a popular form of transportation is a massive, 2 1/2 ton, gas-guzzling, air-polluting and hazardous (to other motorists) sport utility vehicle that appeals to our weekend fantasies of exploring the wilderness. Momentarily putting aside the incongruity of driving such machines into the last remaining refuges of nature, this is not a sensible form of transportation that serves the long-term welfare of our citizens or our environment. It is, however, the sort of vehicle that will bear a hefty price mark-up and is highly profitable to automobile manufacturers and petroleum producers.

Whether it is SUVs, suburban sprawl and the resultant traffic gridlock, the maintenance of a vast nuclear arsenal in a post-Cold War era, or incipient global warming, we can find countless problems that have not been resolved technologically because powerful groups within our society benefit from their existence. The “rational” behavior of elite groups working in their own interest to the detriment of the rest of the population (Diamond, 2005) is the threat with which I am most concerned. These problems will not be easily addressed by conventional public education programs because the powerful groups that benefit from the status quo also control the mass media and major institutions that craft and disseminate these educational programs.

## RESPONSE TO HANSON

The commentary by Dr. Hanson for the most part also concurs with my article, and once again I have only a few remarks to make about this review.

First, I am grateful to Dr. Hanson for referencing the NIMH-funded, Clinical Antipsychotic Trials of Intervention Effectiveness (CATIE) study (Lieberman et al., 2005). This large-scale (n = 1493), multi-site (57 agencies and medical centers) study also failed to show superior efficacy of newer atypical antipsychotics over an older conventional antipsychotic medication, despite the aggressive marketing and substantially higher prices of the former drugs. The main finding of the CATIE study was a high dropout rate (74% overall) that occurred no matter what antipsychotic drug was used. It is also noteworthy that the drug with the lowest dropout rate, olanzapine (trade name: Zyprexa), was associated with significantly greater weight gain (mean = 2 lbs per month) and increases in glycosylated hemoglobin, total cholesterol, and triglycerides, changes consistent with potential development of the metabolic syndrome and its associated health risks.

Second, I appreciate Dr. Hanson’s reference to the Soteria studies (Bola, Mosher, & Cohen, 2005, Bola & Mosher, 2003) because they demonstrated that persons with schizophrenia could achieve favorable outcomes while participating in psychosocial programs that are substantially different from the structured token economies in mental hospitals. On the face of it, the Soteria studies present a sizable challenge to the

behavioral perspective. These programs for persons with early-onset schizophrenia produced positive outcomes without structured token economies, individualized behavioral programs, or the professional staff to administer them. In contrast to the behavioral focus on contingency management, the Soteria program emphasized: treatment in small, homelike settings; frequent staff/client interactions in a social climate with minimal staff authority; expectations that clients would recover from psychosis; and unconditional acceptance of clients' personal experiences (Bola et al., 2005, pp. 369-370). While there would appear to be fundamental differences between these two programs, and the differences probably are significant, there are also common features that may partially account for their similar outcomes. For one, the Soteria program's expectation for recovery from psychosis implied that program staff encouraged the resumption of normative conduct and the reduction of blatantly psychotic behavior. The exact ways social influence might have been exerted to bring about these changes were unspecified, but it is not unreasonable to assume that these efforts included verbally prompting clients to behave in certain ways, complimenting or praising clients for desired behavior, and ignoring or mildly disapproving of bizarre and inappropriate behavior. Second, the clients in the Soteria programs were expected to assist with all the daily household tasks needed to maintain the facility (e.g., shopping, cooking, cleaning, and gardening) and to participate in activities that integrated them with the local community. These tasks are closely related to the self-care, independent living, and work skills targeted for instruction and reinforcement within token economies and individual behavioral programs. Thus, the two approaches were similar in their objectives of promoting functional and normative responses, but behavioral programs utilized *specific* performance expectations and *direct* contingencies of reinforcement, whereas the Soteria program provided more *loosely defined* expectations for improvement and *indirect* social pressure to foster these changes.

Findings of the Soteria studies suggests that the behavioral emphasis on specifying desired behaviors and applying precise consequences for performing those behaviors is not crucial for treating persons with early-onset schizophrenia living in the community as it may have been for rehabilitating chronic mental patients in the back-wards of mental hospitals. Such intensive training procedures may be required for restoring social and self-care skills to regressed, chronic patients whose skills have atrophied during prolonged institutionalization (Paul, 1969), as they are for children or persons with mental retardation who have not yet acquired those skills. The looser and more indirect supervision of clients might have greater applicability in community settings in which less supervision is possible and with clients who are less debilitated. The reduced structure and regimentation would be more acceptable to clients, and it might promote the generalization of behavior change since it more closely resembles the conditions of independent life in the community.

**RESPONSE TO HARSHBARGER**

Dr. Harshbarger humorously refers to my paper as “The behavioral blues...” and, I must admit, it is not the cheeriest of articles. Like Salzinger, Harshbarger points out that my paper presents only a partial picture of the decline of token economies in mental hospitals and it neglects historical factors such as the national movement towards deinstitutionalization. While my article does mention the deinstitutionalization movement and describes how at least one major pharmaceutical company was able to capitalize on this social movement, this criticism probably has some validity. My paper emphasized professional competition and commercial promotional strategies as key factors undermining the progress of behavioral treatment of chronic mental patients, and it downplayed other contributing conditions. However, Dr. Harshbarger seems to have overlooked that following the transfer of mental patients from institutions to community settings drug treatments for these individuals flourished, while behavioral programs withered. Yet, behavioral programs achieved early success in out-patient programs (Henderson & Scoles, 1970; Liberman, King, & De Risi, 1976) and they have been applied extensively with normal children and developmentally delayed persons living in the community. So, as a treatment modality, they do not appear to be unworkable in open settings. I believe the major obstacle of behavioral interventions for severe mental disorders in the post-deinstitutionalization era was not the treatment setting, but rather the prevailing belief that behavioral interventions could not address the *real causes* of mental disorders, namely their purported, underlying diseases or neurological defects. However, as Wyatt and Midkiff (2006) and I have discussed, scientific data to support the pivotal assumption that major mental disorders are caused by or even associated with a biological disease or neurological anomaly has never been consistently replicated, despite the biomedical establishment’s vast network of academic departments and research centers, its monopolist hold over public and private research funds, and the scientific and professional acclaim to be garnered by any researcher who discovered such evidence.

Grasping at another thread in the cloud’s silver lining, Dr. Harshbarger briefly notes that community mental health programs routinely incorporate behavioral procedures within their therapeutic milieu. Although such behavioral programs can be found, I have seen that they usually are weakly grounded in operant learning principles; they lack functional assessment or functional analysis, individualized treatment, systematic individualized program evaluation, and when needed, treatment revision; and, most importantly, they merely supplement a biomedical treatment model. This could explain why behavioral research in this area has become so sparse. Some of the rare behavioral research with chronic mental patients is directed at increasing medication compliance (Corrigan, Wallace, Schade, & Green, 1994; Eckman, Liberman, Phipps, & Blair, 1990; Mueser et al., 2002), further indicating that the remaining behavioral interventions have been subjugated to serve biomedical goals.

Dr. Harshbarger ends his commentary with an upbeat reference to the successes of behavioral technology in work with persons with developmental disabilities, such as autism. While I appreciate his effort to be positive, I would respectfully submit that most

behavior analysts are aware of our field's achievements with autism and developmental disabilities, and that this was not the topic of my paper. My article was meant to recall a time when behavior analysis was being applied with both persons with mental retardation *and* persons with chronic mental disorders, and when both applications were producing comparable successes. For reasons that are not entirely clear (but for which my paper offers some possible explanations) behavioral programs for chronic mental disorders were largely abandoned while psychotropic drugs, with their limited benefits and serious adverse effects, became the standard treatment. These events blocked further development and advancement of behavior programs for psychotic disorders, and made them unavailable or available only in a crude, watered-down form. This was not good news for persons with chronic mental disorders and their families, or for behavioral researchers or clinicians interested in working with this population. Failing to acknowledge these grave setbacks is akin to failing to report negative findings from a controlled experiment; neither provides long-term benefits to the scientific community or to the public.

### RESPONSE TO WAKEFIELD

Dr. Wakefield's commentary stands apart from the others in format and tone. I was first struck by length of his commentary—roughly equal to Wyatt and Midkiff's article (2006)—which seems to exceed the boundaries for an essay bearing that name. A sizable portion of Dr. Wakefield's commentary focused on papers *other than the ones* published in this issue of BSI. This includes a 6-page critique of the Paul & Lentz (1977) study of a token economy. Due to the length and depth of Dr. Wakefield's comments, I asked Dr. Gordon Paul for his assistance and he has written his own response to these criticisms, which has been included as a separate section in this issue. Dr. Wakefield digs deeply at faults in my reasoning or supporting evidence, but he seems to be unaware of a large body of behavioral research that negates his main criticisms. Finally, Dr. Wakefield's commentary is disappointing. I got the feeling I was talking to someone who simply wasn't listening. His commentary seems to ignore 95% of my paper while attacking the remaining 5%. I will now respond to a few of Dr. Wakefield's main points.

Dr. Wakefield scoffs at my supposition that environmental conditions, such as deprivation, poor socialization, and aversive stimulation, might play an important role in the etiology of mental disorders. I did not make this argument in the present paper, but I briefly proposed it in a book chapter published back in 1996. Why Dr. Wakefield, a social work professor, should object so strongly to this idea is curious, since it is far from new (Hollingshead & Redlich, 1958), and there is recent evidence to support it (Read, van Os, Morrison, & Ross, 2005) some provided by a social work researcher (Hudson, 2005). And, what is so groundless and illogical about the following argument?: 1) If reinforcement contingencies can create and increase psychotic behavior (as in the study by Allyon, Houghton, & Hughes, 1965); and, 2) if reinforcement contingencies can decrease or eliminate psychotic behavior; then, 3) is it not reasonable to assume that reinforcement contingencies might have contributed to the emergence of psychotic



behavior in the first place? Why is this notion so objectionable that it warrants ridicule, if we are in an era where "...there is no "extreme biological causation" model held by serious theorists or researchers..."? The opposite seems closer to the truth: on a regular basis television and radio programs, newspapers, magazines, journals, textbooks, psychiatrists, biomedical researchers, and government spokespersons proclaim that mental disorders are caused by biological diseases. As Wyatt and Midkiff (2006) and I point out, these claims are based on flimsy or non-existent evidence. Yet, my solitary assertion in a 10-year-old book chapter that environmental stimuli might generate psychotic behavior is so egregious it provokes the dire warning that behaviorism "...is in danger of being transformed into a pseudo-science." I would suggest that Dr. Wakefield's strong negative reaction to my suggestion is partly due to the fact that we *are* living in a zeitgeist of extreme biomedical causation and within that ideological climate my comment is heretical.

A central and recurring point in Dr. Wakefield's critique of behavioral interventions for psychotic disorders is that they have failed to demonstrate generalized effects beyond the treatment setting, usually the hospital ward. This criticism contains several fallacies. First, it overlooks that operant research provided the first systematic analyses of "generalization" as a behavioral phenomena, identifying its properties and variables affecting it (Guttman & Kalish, 1956; Kimble, 1961; Terrace, 1966). Second, Dr. Wakefield portrays generalization as an obstacle or barrier which behavioral interventions have not been able to penetrate. This fallacy may reflect Dr. Wakefield's own narrow reading of the behavioral literature. The importance of generalized behavior change was recognized early during the formulation of the field of applied behavior analysis (Baer, Wolf, & Risley, 1968) and this awareness guided the development of a technology for promoting generalization (e.g., Dunlap, 1993; Horner, Dunlap, & Koegel, 1988; Stokes & Baer, 1977; Stokes & Osnes, 1989). There is nothing magical about programming generalization of positive behavior change for clients in general or for psychiatric patients in particular. If Dr. Wakefield had bothered to dig a bit deeper he would have seen that even I have done a couple of studies promoting generalization of desired skills across settings and social situations (Wong, et al., 1993; Wong, Morgan, Crowley, & Baker, 1996).

Third, Dr. Wakefield's criticism of behavioral studies for their lack of generalization is extremely one-sided. If behavioral treatments are being compared with psychotropic drugs, why is the issue of generalization being brought up at all? Psychotropic drugs are not assumed to produce generalized effects (effects that carry over to times and places in which the drugs are not in the patient's body), such effects are virtually never assessed, and their absence is not reported. Generalization is a form of extended behavior change that behavioral researchers assess because they seek to give their clients functional capabilities that will make them *independent* of therapy and professional services. This is an outcome that few psychiatric and drug researchers pursue, especially the latter since it would limit product profitability. Apparently, Dr. Wakefield believes that he is being even-handed when he evaluates behavioral research by a higher standard than he applies to drug research.

Fourth, many of Dr. Wakefield's criticisms were aimed at 30-year-old demonstration projects showing that operant contingencies could reduce aberrant behavior and restore functional behavior in persons with psychoses. These preliminary demonstrations were needed because of the historic and prevailing view that these disorders were the manifestation of diseases and not modifiable by restructuring stimuli in the client's immediate environment (a view defended by Dr. Wakefield, based partly on the age and popularity of these beliefs). In the normal course of technological progress, these demonstration projects would have been replicated, refined, tailored for different subpopulations and settings, and upgraded to maximize stimulus generalization, response generalization, and maintenance. This never happened. It did not happen because behavioral interventions were suddenly found to be ineffective with psychotic disorders, but rather because the bottom dropped out of the entire enterprise. The deinstitutionalization movement transferred patients out of the state hospitals and back to their home communities, where community-based treatment programs revolved around drugs and drug management. Meanwhile, inadequate funding hobbled the development and dissemination of skills training and rehabilitation programs suitable for open settings (Lehman, 2000). Thus, Dr. Wakefield reveals his own biases by discrediting successful studies conducted three decades ago by a few dozen loosely knit researchers in crumbling state hospitals, while endorsing the widespread use of often ineffective and harmful drugs fabricated by an enormous biomedical-industrial complex that maintains a hegemony over mental health research and services.

In sum, Dr. Wakefield advocates for existing mental health services for persons with psychotic disorders while belittling the potential contribution behavior analysis might make to this endeavor. Backed up by the weight of conventional authority and institutional practices, this is a comfortable and relatively easy position to take. There are contradictions and discrepancies, however, which would keep a curious person from being totally at ease with the present situation. The poor outcomes associated with modern biomedical treatments (Hegarty, Baldessarini, Tohen, Waternaux, & Oepen, 1994) and the stable or rising levels of mental disorders in the general population despite their widespread use (Whitaker, 2005; Torrey & Miller, 2001 ) would give some people pause. The successful application of behavioral principles in the treatment of child behavior problems, autism, developmental disabilities, conduct disorders, anxiety, tics and habit disorders, phobias, panic disorder, social skill deficits, self-care deficits, depression, aggression, and numerous other social problems might cause other people to ask, "Why shouldn't these principles also work in the treatment of psychotic disorders?," as, indeed, they were *shown* to work 30 years ago. But, I doubt that either of these points or anything else that I might say would move Dr. Wakefield. I can only hope that other readers are less satisfied with the current state of mental health services and more open to alternative, empirically supported treatments than he.

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