VOLUME 31, NUMBER 1, SPRING 2003 ISSN 0090-3604

THE JOURNAL OF THE AMERICAN ACADEMY OF PSYCHOANALYSIS AND DYNAMIC PSYCHIATRY

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Special Issue
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The Role of Psychoanalytic Theory and Practice in Understanding and Treating Schizophrenia: A Rejoinder to the PORT Report's Condemnation of Psychoanalysis*

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Abstract: The Schizophrenia PORT report rejects, in recommendation 22, the use of psychodynamic therapies. It also rejects, in recommendation 26, a crucial point of psychoanalytic theory by rejecting family interventions based on the assumption that the family of origin can make a causal contribution to schizophrenia.

Both recommendations are based on level "C" evidence, which is defined by the authors as a: "Recommendation based primarily on expert opinion, with minimal research-based evidence, but significant clinical experience" (Lehman et al., 1998, p. 2). Conclusions based on low-level evidence would be better formulated in tentative rather than categorical statements as is now done in the PORT report.

More substantially, I report on a number of empirical studies that contradict the claims of recommendations 22 and 26. I therefore urge that the two PORT recommendations be revised. I argue that the recommendations can be rewritten so that a useful point in each of the recommendations can be preserved and even strengthened. With reference to recommendation 22, it is wise to stress that psychoanalytic-inspired therapies with schizophrenics should not simply apply the same methods used in the treatment of neurotics. With reference to recommendation 26, it is important to stress that imposing or augmenting guilt is not a good therapeutic technique. Similarly, it is not good therapeutic practice to deny the truth. To tell the truth and not create guilt requires artful interventions.

If recommendations 22 and 26 were to be rewritten as suggested, they would become scientifically defensible and would make a positive contribution by stressing the contributions that many approaches, including psychoanalysis, can make to the treatment of schizophrenia. As they stand now, recommendations 22 and 26 are contradicted by the scientific evidence I will submit.

Key Words: schizophrenia; psychodynamic; psychoanalysis; evidence

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*In this article I make use of research I did for a book coauthored with Alphonse De Waelhens: *Phenomenology and Lacan on Schizophrenia, after the Decade of the Brain*. I am grateful for editorial assistance received from both Mark Nowacki, my Teaching Assistant, and Lynn Ilene Poss, who was supported by a Georgetown Undergraduate Research Opportunities Program (GUROP) grant from Georgetown University.

Journal of The American Academy of Psychoanalysis and Dynamic Psychiatry, 31(1), 11–29, 2003

INTRODUCTION

Twentieth century philosophy discovered the importance of language for human beings. Philosophy already knew the importance of intersubjective relations. Therapies using psychoanalysis or psychoanalytic insights are built upon these two principles: They use transference (an interpersonal relationship–even if it is one of a special kind) and speech as their main tools. Given the difficulty of managing transference with schizophrenics and the equal difficulty of talking to such patients, it is understandable that some theories about psychoanalytic approaches to schizophrenia are deficient and also that some (even many) therapies fail. There is, however, empirical evidence demonstrating that psychodynamic approaches are successful. There are also plenty of case reports describing successful psychodynamic treatments of schizophrenics. However, the Schizophrenia Patient Outcome Research Teams (PORT) report in two of its recommendations (22 and 26) categorically rejects psychodynamic therapies and also categorically rejects a crucial point in psychoanalytic theory.[1] I will criticize the two PORT report recommendations that reject either psychoanalytic therapy or psychoanalytic theory and then defend, on empirical and theoretical grounds, the promise of wise psychodynamic approaches to schizophrenia because such an approach aims, more than any other approach, to restore a sense of full agency to the patient.

THE PROBLEM

The Schizophrenia PORT report has the stated goal of "develop[ing] recommendations for the treatment of persons with schizophrenia based upon the best scientific evidence, with the ultimate goal of improving the quality and cost-effectiveness of care for persons with this diagnosis" (Lehman et al., 1998, p. 1).

The report formulates recommendations about "antipsychotic agents, adjunctive pharmacotherapies, electroconvulsive therapy, psychological interventions, family interventions, vocational rehabilitation, and assertive community treatment/intensive case management" (p. 1).

Of the 30 recommendations, 18 are about pharmacotherapies; three concern electroconvulsive therapy; and 9 are about psychosocial approaches. Of these 9, two are devoted to psychological treatments; three to family treatments; two to vocational rehabilitation; and two to service systems.

One recommendation about psychological treatments and one of the three recommendations about family treatment are of direct relevance to psychoanalytic theories and treatments. The two recommendations about psychoanalysis (either its practice or its theoretical insights) are both negative. Recommendation 22 opposes the use of psychoanalytic and psychodynamic therapies in the treatment of schizophrenia (p. 7). The recommendation reads as follows: "Individual and group psychotherapies adhering to a psychodynamic model (defined as therapies that use interpretation of unconscious material and focus on transference and regression) should not be used in the treatment of persons with schizophrenia." The authors of the PORT report give the following as their rationale: "The scientific data on this issue are quite limited. However, there is no evidence in support of the superiority of psychoanalytic therapy to other forms of therapy, and there is a consensus that psychotherapy that promotes regression and psychotic transference can be harmful to persons with schizophrenia. This risk, combined with the high cost and lack of evidence of any benefit, argues strongly against the use of psychoanalytic therapy, even in combination with effective pharmacotherapy" (pp. 7–8).

The rejection of a basic insight of psychoanalysis is contained in recommendation 26, which reads as follows: "Family therapies based on the premise that family dysfunction is the etiology of the patient's schizophrenic disorder should *not* be used." The authors of the PORT report give the following rationale:

> Research has failed to substantiate hypothesized causal links between family dysfunction and the etiology of schizophrenia. Therefore, therapies specifically designed from this premise are not empirically founded. Although there has been little or no randomized, controlled research on the impact of family therapies arising from this orientation, experts in the field have expressed strong caution against the use of these techniques. The presumption that family interaction causes schizophrenia, especially as an alternative to biological risk factors, has led to serious disruption in clinician/family trust without any evidence of therapeutic effectiveness. The repudiation of the theoretical premise of these therapies, the lack of empirical studies, and the strong clinical opinion raising concerns about the potential harm caused by these approaches lead to this recommendation. (p. 8)

I will restrict my critical remarks about the Schizophrenia PORT report to a reflection on the two recommendations that are critical of psy-

[1] Lehman, Anthony F., Steinwachs, Donald M., and the Co-Investigators of the PORT Project (1998), At issue: Translating research into practice: The schizophrenia *Patient Outcome Research Team* (PORT) treatment recommendations," *Schizophrenia Bulletin, 24*, (1), 1–10.

choanalytic theory or practice: recommendations 22 and 26. First, I will comment on the quality of arguments presented in the report against psychoanalysis. I call this a formal criticism of the PORT report. Second, I will show that the two recommendations against psychoanalysis each make a valid point, but that the valid insight justifies the rejection of neither psychoanalytic theory nor psychoanalytic therapy of schizophrenics. On the contrary, I will present scientific evidence contradicting the claims of the PORT report about psychoanalysis.

FORMAL CRITICISM OF PORT REPORT RECOMMENDATIONS 22 AND 26

As part of the argument to reject psychoanalytic therapy the authors of the report write that: "there is no evidence of the superiority of psychoanalytic therapy to other forms of therapy" (p. 8). Why demand superiority from psychoanalysis? To demand proof of superiority for one approach is to raise the bar of statistical evidence for that one approach. In order to reject a therapeutic approach should one not demand statistical evidence that it is inferior? But that is in fact not the case according to a meta-analytic review article by Mojtabai and his colleagues of 141 sources reporting on 106 individual studies in which the authors state that "therapies based on various psycho-dynamic principles were not significantly less effective than verbal treatments based on other theoretical rationales" (Mojtabai, Nicholson, and Carpenter, 1998, p. 583).

More seriously, the authors of the report violate their own standards when making recommendations against psychoanalysis. Indeed, the authors of this report praise themselves for giving only recommendations "based on existing scientific evidence" and "focus on those treatments for which there is substantial evidence of efficacy" (Lehman et al., 1998, p. 1). The authors also claim that their recommendations are "evidence-based" (p. 1). However, the *categorical* rejection of psychodynamic therapy or an important element of psychoanalytic theory are rejected on the basis of "level C evidence," the lowest level of evidence reported in the article. Level C evidence is defined as: "Recommendation based primarily on expert opinion, with minimal research-based evidence, but significant clinical experience" (p. 2). For the record, evidence of level A is defined as "Good research-based evidence, with some expert opinion, to support the recommendation" (p. 2). Evidence of level B is defined as: "Fair research-based evidence, with substantial expert opinion, to support the recommendation" (p. 2).

Only psychodynamic/psychoanalytic approaches and their theoretical insights are evaluated and rejected—and rejected categorically—on the basis of such a low-level of scientific evidence.[2] The two recommendations against the use of psychoanalysis as theory and practice are, indeed, both based solely on level C evidence. For recommendations about pharmacotherapies, 15.78% of the recommendations are based only on level C evidence. For recommendations about electroconvulsive therapy (ECT), 33% of the recommendations are built upon level C evidence. For psychological, family, and vocational approaches, 42.85% are based on level C evidence. This group includes the two recommendations against psychoanalytic approaches. If we disregard these two recommendations against psychoanalysis, the group on psychological, family, and vocational approaches has only 20% of its recommendations based on level C evidence (this constructed statistic is included in table 1). For recommendations about systems of care, no recommendation is based on level C evidence. Table 1 summarizes the level of evidence used in the different domains in which the authors make recommendations. Note that the report has only 18 recommendations about pharmacotherapies. Table 1 has 19 because recommendation 5 reports two different levels of evidence for the two parts of its recommendation.

Let me underline that, despite the fact that the evidence is only of level C, the authors of the report make strong claims and *categorical* recommendations against psychoanalytic theory and practice.

In recommendation 22, the authors recommend without qualification that "psychotherapies adhering to a psychodynamic model . . . should *not* be used" (p. 7). In recommendation 26, the authors recommend that no family therapy should be used that is "based on the premise that family dysfunction is the etiology of the patient's schizophrenic disorder" (p. 8).

As a philosopher, I am very sensitive to the limits of any research method. Admirably, the Lehman report of the PORT project shows awareness of the limits of its own method. Thus the authors of the report write that the

> requirement that recommendations be based on substantial scientific evidence means they are silent about or may appear to understate the importance of other aspects of treatment that have not been evaluated adequately. Therefore, there are many more recommendations about pharmacotherapies than about psychosocial treatments. This does not mean that psychosocial treatments are less important than medications, but reflects the fact that we know much less about which psychosocial treatments are helpful (Lehman et al., 1998, pp. 1–2).

[2] If the level of evidence is low, then it might be appropriate to formulate the conclusion in a guarded way.

Table 1. Level of Evidence for Recommendations in Different Approaches to Schizophrenia

	Pharmaco-therapies		Electro-convulsive Therapy		Psycho, Family, Vocational		Systems of Care		Psy, Fam, Voc. minus Psychoanalytic		Psychodynamic Psychoanalytic alone	
	Cases	%	Cases	%	Cases	%	Cases	%	Cases	%	Cases	%
As	7	36.8	0	0	1	14.3	1	50	1	20	0	0
Bs	9	47.4	2	66.7	3	42.9	1	50	3	60	0	0
Cs	3	15.8	1	33.3	3	42.9	0	0	1	20	2	100
Total	19	100	3	100	7	100	2	100	5	100	2	100

However, in their concluding discussion section the authors omit the very appropriate warning put in their introduction when they write: "the Treatment Recommendations should stimulate close examination of practices at both the aggregate and the individual patient levels to ensure that treatments are offered in the most effective manner" (p. 9). But what is most effective: the treatments for which the method used has documented evidence or the psychosocial treatments which, according to the warning in the introduction might be no "less important than medications" but about which we know less? (pp. 1–2). A warning in the introduction should remain a warning in the conclusion. After all, recommendations are presented rhetorically so as to be followed. Following the recommendations without paying attention to the proper warning from the introduction is unjustifiable.

MERIT AND DEMERIT OF THE PORT REPORT'S ARGUMENTS AGAINST PSYCHOANALYSIS

Recommendation 22 rejects psychoanalytic/psychodynamic therapy. Recommendation 26 rejects one of psychoanalytic theory's basic insights. Many therapies are grounded in theories. I will therefore begin with a discussion of the attack made in recommendation 26 on a point of theory in psychoanalysis. Afterward I will address recommendation 22's rejection of psychodynamic therapies.

Recommendation 26 combines several ideas in one recommendation. The recommendation points to the "serious disruption in clinician/family trust" created by "family therapies based on the premise that family dysfunction is the etiology of the patient's schizophrenic disorder" (p. 8). One may assume that such therapies create *needless guilt*, which in turn creates clinician/family distrust. I agree with the concern of the authors of the report that the creation of needless guilt is undesirable and therapeutically counterproductive. However, that concern is valid whether or not one believes that family interaction is part of the etiology of schizophrenia. On the other hand, clinicians will lose the trust of the family if they deny a truthful admittance of (partial) guilt by family members. Parents of schizophrenics are humans. All humans make mistakes. Sometimes parents of schizophrenics believe that they might have contributed to the condition of their child and they want to know what they can do to help their child. I like the answer of Bertram Karon, an experienced and successful therapist, to such a difficult question. He rejects the creation of guilt to assure cooperation of parents with the treatment of schizophrenics. He also rejects lying to parents about their own in-

volvement. When parents initially cooperate but later disrupt the therapy, Karon claims that "either parents and the therapist colluded to avoid the issue, or the therapist blatantly lied to the parents about their involvement" (Karon and VandenBos, 1981, p. 129). When parents ask about their own role in their child's situation, Karon proposes to say: "I don't know. It may take us a long time to know exactly what caused your child's symptoms; however, it is our job to find out. Even if your child had no symptoms, being a parent is very difficult. When your child has problems, it is even more difficult, and I would like to be of use to you" (pp. 129–30). Recommendation 26 is correct in trying to avoid guilt. It is wrong in supposing that avoidance of guilt demands denial of the possible parental involvement in the etiology of schizophrenia.

I now turn to the explicit claim of recommendation 26: "Research has failed to substantiate hypothesized causal links between family dysfunction and the etiology of schizophrenia" (Lehman et al., 1998, p. 8). This claim rejects the basic insight of psychoanalysis that one becomes what one is through a developmental process in which genetic factors together with environmental ones, namely, interactions with family members, play a crucial role. The authors of the PORT report thus flatly reject a basic tenet of psychoanalytic theory.[3] However, at least some recent empirical research does not confirm the extreme statement of the PORT report. Instead, several studies confirm the statistical relationship between schizophrenia and psychological factors in the family of origin or in the rearing family (adopting family).[4]

First, in the Finnish adoption study, under the direction of Pekka Tienari (Tienari et al., 1990), all women admitted for schizophrenia or paranoid psychosis to a hospital from January 1, 1960 until December 31, 1979 were selected as study subjects. The researchers checked the civil registers for all the children of these women and selected the children given up for

[3]I am referring here not to the theory for psychoanalytic practice but to the developmental theory of human beings based on psychoanalytic experience and insights.

[4]My argument should not be construed as implying that psychodynamic approaches are only useful if one believes that the psychic environment is the crucial causal factor in the creation of a problem to be treated. Psychodynamic approaches are also useful in dealing with the psychic consequences of physical handicaps (cancer, accidents) and thus, by analogy, they can be argued to be potentially useful even if one is convinced that biological causes (that is, genetic predispositions) are crucial for developing schizophrenia. Remember that a biological researcher on schizophrenia writes that schizophrenia is "a disease of the brain that is expressed clinically as a disease of the mind" (Andreasen, 1999, p. 646). Psychodynamic approaches give patients the opportunity to restructure themselves psychologically, whatever the cause which created the psychic structure deemed inappropriate.

adoption before age four to non-family members in Finland. The researchers then studied the adoptive families and divided them into the categories of psychologically "healthy," "neurotic," and "severely disturbed" families. Of a sample of 144 adopted children of schizophrenic mothers, 13 were schizophrenic. *Not one* became schizophrenic if the adoptive family had been classified as psychologically "healthy."

Second, in the Finnish study by Antero Myhrman and co-workers (Myhrman, Rantakallio, Isohanni, Jones, and Partanen, 1996), all children in Northern Finland born in 1966 were studied. Those alive and living in Finland at age 16 constituted the sample and were studied until they were 28 years old. During the first visit to the perinatal clinic, a questionnaire was given to the pregnant mothers with the intention of separating the children into wanted, mistimed (but still wanted), and unwanted children. In the sample of 11,017 children there were 76 cases of schizophrenia up to the age of 28 years (0.7%) (Myhrman et al., 1996, p. 638). The cumulative incidence of schizophrenia for the three groups of children was 0.6% for the wanted children; 0.5% for those of a mistimed but wanted pregnancy; and 1.5% for the unwanted ones. Thus the unwanted children had more than double the incidence of schizophrenia than the wanted children.

Third, in an experiment in the U.S., Denise Fort (1990) selected normal sons and their parents and schizophrenic sons and their parents as subjects. Obviously, the parents of schizophrenics are not necessarily schizophrenic. The subjects were alternatively speakers and listeners. As speakers they were asked to make a tape recording describing the meanings of four proverbs. They were told that the listeners would be asked to select the correct proverbs from a list provided to them.

Before listening the subjects of the experiment were told that they would be given three tapes. Parents would receive the tape of their son and sons would receive the tape of their parents. Parents would also receive a tape from each of two boys who were not their own sons, a normal and a schizophrenic one. Sons would also receive two more tapes of parents of other children. One tape was from parents with a schizophrenic son and the other tape was from parents with a normal son. Thus two tapes were specific to the son/parent pair—the tape made by the son for his parents and the tape made by the parents for their son. The other tapes were standardized and no one received the same proverbs twice.

The following results were obtained: In proverb identification, parents performed less well after listening to a schizophrenic boy and sons performed less well after listening to parents of a schizophrenic son ($p < .01$). These results confirm Fort's main hypothesis about schizophrenics and their parents: There is communication deviance in *both*

directions. However, schizophrenic sons were more impaired when responding to parents of schizophrenics than normal sons were and *schizophrenic sons responding to parents of a normal son increased their level of performance on the proverb identification task to the point at which there was no difference between their performance and that of normal sons* (Fort, 1990, p. 87). These results support the theory that parents of schizophrenics have defective communication. The results also prove the *curative possibilities of good communication* for solving the task given in this experiment to schizophrenic children.

Fourth, the connection between psychological family trauma and schizophrenia is supported by the observed increase of incidence of schizophrenia in families in which the husband died during pregnancy (Huttunen and Niskanen, 1978) and in which a grandparent died within about two years of birth (Walsh, 1978; Teixeira, 1997, p. 5; for a case see: Silver, 1992, p. 116).

Turning now to the mechanisms of the etiology, we can make use of the research of Karon and of the Tienari group (Tienari et al., 1990), this time under the leadership of Karl-Erik Wahlberg (Wahlberg et al., 1997).

First, Karon and his various co-workers (Karon and Widener, 1994) constructed a pathogenesis score. Pathogenesis is described as follows: "When the needs of the parent and the needs of the child are in conflict, the parent of a preschizophrenic child, more often than other parents, acts in terms of the parent's need without regard to the conflicting need of the child. Of course, all parents do this some of the time; it just seems more frequent in parents of preschizophrenics" (Karon and Widener, 1994, pp. 52–53). A pathogenesis score was created by analyzing Thematic Apperception Test (TAT) stories. A story was scored pathogenic if a dominant person, whose needs conflicted with those of a dependent person, did not take the dependent person's needs into account in his action (p. 53). Mothers of normal children average pathogenesis scores of about 35%, while mothers of schizophrenic children have pathogenesis scores of about 75%. Using Singer and Wynne's TAT data from their National Institute of Mental Health study (Singer and Wynne, 1965), Mitchell found that pathogenesis scores differentiated mothers of schizophrenics from mothers of normals (Karon and Widener, 1994, p. 53; Mitchell, 1974).

Walhberg and his colleagues (1997) developed a follow-up analysis of Tienari's Finnish adoption study. Their analysis of gene-environment interactions led to some unexpected results regarding causal relationships in schizophrenia. A subsample of 154 adoptive families (58 index and 96 comparison or control ones) was chosen from among the original sample with the stipulations that at least one adoptive parent be available

for study and the adoptee not be older than 36 years. The reasoning was that older adoptees would have older adoptive parents whose communication habits would possibly differ from the communication used during child rearing (Wahlberg et al., 1997, p. 357). The putative adoptee vulnerability was measured by the Rorschach Index of Primitive Thought which was independently scored by two research psychologists with an intraclass correlation coefficient of 0.91, confirming interobserver reliability (Wahlberg et al., 1997). "Environmental risk was measured by using frequency of communication deviance as a continuous variable, scored independently from Rorschach assessments of the adoptive parents" (p. 355).

The study found that the putative risk for schizophrenia as measured by the Index of Primitive Thought[5] was almost evenly distributed among the high risk adoptees and the control adoptees. Among the high-risk adoptees (children of a schizophrenic mother), 41.4% showed no evidence of the putative schizophrenic genotype. At the same time, "56.2% of the comparison adoptees showed at least some evidence of this rather extreme form of schizophrenic thought disorder" (p. 358). As for the communication deviance scores, there is a wide range of scores in the rearing parents of both the index and comparison adoptees, with the scores of index adoptive parents slightly higher. Thus support for the hypothesis that offspring influence rearing parents was not statistically significant (pp. 359–60).

When analyzing the *interaction* of high genetic risk (being the biologic child of a schizophrenic mother) with the communication deviance of the adoptive parents, the study made two interesting findings. First, *increasing communication deviance on the part of the adoptive parents steeply increased the positive scores on the Index of Primitive Thought for adoptees with a high genetic risk* (the scores go from 40% to 90%), but does not at all increase the positive scores for control adoptees (these scores stay in the range between 40% and 60%, and *actually decrease* with increased scores in communication deviance of adoptive parents; pp. 358–9). Second, *when exposed to adoptive parents with low levels of communication deviance, the high genetic risk adoptees "have a lower proportion of positive scores than do the comparison adoptees"* (pp. 359, 361). Thus, *high genetic risk adoptees are hurt more by a defective environment than comparison adoptees, but they end up with a better measure on the Index of Primitive Thought than the comparison adoptees when both kinds of adoptees have a "protective" environment*. It is as if the comparison adoptees develop more according to their own scheme whereas *high genetic risk adoptees ei-*

[5]Eugen Bleuler defined thought disorder as a primary symptom of schizophrenia (De Waelhens and Ver Eecke, 2001, p. 127 ff; Karon and VandenBos, 1981, p. 431).

ther drown or flourish depending on the environment, here defined as high or low communications deviance in adoptive parents.

The above studies demonstrate that family environment does matter for the emergence of schizophrenia. The part of recommendation 26 which denies all influence of the family in the emergence of schizophrenia is therefore contrary to at least some available scientific evidence.

Recommendation 22, similar to recommendation 26, is partially accurate, but because it fails to make the distinction between psychoanalytic techniques that are appropriate for neuroses and those that are appropriate for psychoses, it too is defective and should be corrected.

Recommendation 22 is right in arguing that classical psychoanalysis as used for neurotics is not useful and is sometimes even dangerous for schizophrenics. Current theoretical psychoanalysts know this distinction and make a theoretical point of it. Thus Bruce Fink writes in his book *A Clinical Introduction to Lacanian Psychoanalysis*: "An analyst cannot treat psychotics in the same way as neurotics" (Fink, 1997, p. 13), and "Lacanian theory demonstrates that certain aims and techniques used with neurotics are inapplicable with psychotics. And not only are such techniques inapplicable—they may even prove dangerous, triggering a psychotic break" (p. 75).

Also, psychoanalysts who successfully treat schizophrenics stress the differences in the psychoanalytic technique used for treating schizophrenics.[6] Furthermore, such important clinicians as Frieda Fromm-Reichmann, Harry Stack Sullivan, Harold Searles, Bertram Karon, Ann-Louise Silver, Yrjö O. Alanen, Gisela Pankow, Françoise Davoine, Jean-Max Gaudillière, Piera Aulagnier, Gaetano Benedetti, Isidro Vegh, Contardo Calligaris, Willy Apollon, and Palle Villemoes have reported on successes obtained in treating schizophrenic patients. The claim that there is a clinical consensus that psychodynamic approaches to schizophrenia should not be used is therefore unfounded.[7] Recommendation 22, however, could be useful in stressing the need to change classical psychoanalytic techniques for neurotics in order to adapt them for the treatment of schizophrenics.

[6]Psychoanalysis knows about and studies regression. Psychoanalysis does not demand the creation of regression; rather, it knows about techniques to handle or prevent psychotic regression.

[7]McGlashan concludes his review of long-term follow-up studies of schizophrenia with the statement that schizophrenia "does not appear to be progressively dementing as originally thought" (McGlashan, 1988, p. 538). Help for schizophrenics might therefore be considered promising. McGlashan is favorably disposed towards psychotherapy "if [it is] applied in a supportative, rehabilitative mode in the context of *stable and unlimited continuity of care*" (p. 538). He is negative about psychodynamic approaches (Dingman and McGlashan, 1989). However, Birgitte Bechgaard argues that McGlashan's studies were designed to study the question of the putative natural course of schizophrenia and that "[m]ethodologically the study does not fulfill any of the demands for a study of outcome of treatment" (Bechgaard, 1994, p. 18; for published version in Danish see Bechgaard, 1990).

There is at least some empirical evidence in favor of psychodynamic approaches to schizophrenia and severe mental illnesses. First, in a meta-analytic review, Mojtabai and his colleagues use 141 sources reporting on 106 individual studies. That meta-study concludes that adding psychosocial treatments of schizophrenics to somatic ones produces an improvement effect of .39 in Cohen's measurement. The authors write that an effect of that size "implies that a typical patient in the experimental treatment group was better off than 65 percent of the control patients" (Mojbatai et al., 1998, p. 576). These effects proved durable (p. 580). Comparing the efficacy of different psychosocial approaches, the authors conclude that there is no statistically significant difference between the different approaches, with the exception of group therapy which has a lower size effect (pp. 579–80). They explicitly state that "therapies based on various psycho-dynamic principles were not significantly less effective than verbal treatments based on other theoretical rationales.[8] Thus, our review provides no evidence that psychodynamic therapies are harmful" (p. 583). The Mojtabai study thus flatly contradicts the research basis for the PORT report's recommendation 22 against psycho-dynamic treatment of schizophrenics.

Second, the project of Bertram Karon and associates of the Michigan State University Psychotherapy Project compared three groups of patients (Karon and VandenBos, 1981, pp. 382ff). Group C, called the hospital group, was treated mainly with medication. Groups A and B were given psychotherapy by an experienced therapist (one third of the

[8]The study also writes that the analysis does not "suggest that psychodynamic treatments are superior to other interventions" (Mojtabai et al., 1998, p. 583). In order to prove that psycho-dynamic treatments are potentially superior one would have to distinguish between different psycho-dynamic treatments and maybe even between skilled and unskilled psycho-dynamic therapists. It is my view that psycho-dynamic therapy is based on a more complex theory and makes more stringent demands upon its therapists than other psychosocial therapies. The potential superiority of psycho-dynamic treatments may be lost either because the theory guiding practice is not quite adequate or because the individual therapist is not skillful enough. Confirmation that this hypothesis is a promising way of thinking would arise if there were a greater variation in the size effect between different psycho-dynamic therapies than between other psychosocial ones or if there is greater variation in outcomes between individual psychodynamic therapists than between other therapists. Sidney Blatt has already demonstrated that there were great variations in outcomes between different psychotherapists, and he therefore argues that the individual skills of psychotherapists make an important difference (Blatt, Sanislow, Zuroff, and Pilkonis, 1996).

group) or by trainees (two thirds of the group). Group A was treated psychoanalytically without medication; group B used ego-analytic therapy with adjunctive medication given in a progressively decreasing dosage; both groups used about 70 sessions over a 20-month period (pp. 391, 399). The Michigan State project included a test (the Feldman-Drasgow Visual-Verbal Test [VVT]) specifically designed to measure schizophrenic thought disorder, which, according to Eugen Bleuler, is the primary symptom of schizophrenia. The Michigan State project used the VVT both as a major index of imputed deep-structure improvement of the patient and as a predictor of long-term condition as defined by other measures such as hospitalization days (p. 431). The improvement was measured at preset times (6, 12, and 20 months), not at the time of discharge, which is presumed to be biased against measuring improvement caused by psychotherapy. Indeed, for psychotherapeutically treated patients, discharge is a period with a higher anxiety increase than for patients treated medically, because, for the first kind of patient, discharge often means looking for another therapist or—worse yet—ending psychotherapy, whereas for the second kind of patient, discharge often goes together with a plan for continuing medication.

The findings of the Michigan study after 6 months were that, in comparing the hospital group, Group C (treated with medication), with the combined psychotherapy groups, psychotherapy contributed little (p. 425). However, if the psychotherapy group is subdivided between experienced therapists and trainees, then the patients of the experienced therapists did significantly better than the patients of both the inexperienced therapists (trainees) and those of the hospital group treated by medication (p. 425), as measured by both thought disorder tests and days of hospitalization (corrected data), with $p < .01$ and $p < .02$ respectively (pp. 423, 425).

The findings after a 12-month period were as follows: The pooled psychotherapy patients were hospitalized less, exhibited less thought disorder (VVT), and were judged to be functioning more healthily (Clinical Status Interviews [CSI]) than the hospital patients treated by medication, with $p < .03$, $p < .001$, and $p < .05$ respectively. Looking specifically at the patients treated by inexperienced psychotherapists (trainees), the authors of the Michigan State project noted imbalanced improvement of these patients as a group (substantial improvement in one test, little improvement in another test). Indeed, patients of Group A's trainees had longer hospitalization and much improvement in thought disorder; whereas patients of Group B's trainees had much less hospitalization but their thought disorder remained similar to that of the control Group C treated by medication (p. 428).

The findings at 20 months were that the patients of the psychotherapy group had 31% to 51% less hospitalization days and had less thought disorder than the control group using medication only, and that the patients of the experienced therapists had improved more than the patients of the trainees (pp. 430–3).

Karon and VandenBos (1981) confirm their conclusion that psychotherapy is more helpful than medication alone by quoting the results of Wisconsin and Massachusetts studies that reported more hospital days for patients on medication than for psychotherapy patients the year after the termination of therapy (p. 440). The results of the Michigan State University Psychotherapy Project are contradicted by the study of P. May (1968) and associates at the Camarillo State Hospital in California. In that study, five methods of treating schizophrenics were compared: (1) Psychotherapy without medication; (2) Psychotherapy with medication; (3) Medication alone; (4) ECT; and (5) Milieu therapy. Each of the psychiatrists practiced each the five methods (Karon and VandenBos, 1981, p. 376). May and his associates concluded that: "medication was the treatment of choice, that improvement on their criteria up to the day of discharge showed an advantage to patients receiving medication over those not receiving medication, and that all other differences were trivial" (p. 377). Karon and VandenBos reconcile the apparent contradiction between their findings and those of the California study by pointing out that at the day of discharge patients treated psychotherapeutically experience greater anxiety, that the psychiatrists in the project and their supervisors had little or no experience in doing psychotherapy with schizophrenics and that all participating psychiatrists were asked to do psychotherapy even if they did not believe in it–which is detrimental to the practice of successful therapy. Karon and VandenBos conclude:

> The California project has little to say about the effectiveness of psychotherapy, with or without medication. It does answer the question: "Is psychotherapy provided by inappropriately trained but medically qualified residents of much use?" The answer is "no." The Michigan State Project asks the question: "Is psychotherapy provided by appropriately trained professionals (psychiatrists *and* psychologists) useful?" The answer is "yes." The fact that the trainees in the Michigan State Psychotherapy Project really did learn to do psychotherapy effectively, as evidenced by the actual progress of their patients during the project, clearly indicates that psychotherapy with schizophrenic patients is a skill that is both teachable and learnable. (p. 460)

One statistical result in the Michigan study worth noting is that the VVT statistic (measuring thought disorder) indicates a greater efficiency of the approach by Supervisor A (psychoanalytic therapy with minimal

or no medication) than the approach by Supervisor B (ego-analytic approach with adjunctive medication) after 6, 12, and 20 months of treatment and in the two-year follow-up study with $p < .01$ or $p < .001$ (pp. 423, 427, 430, 434, 438) and that the same VVT statistics for patients of the trainees of Supervisor A (psychoanalytic therapy) show that these trainees' patients do better than those of Supervisor B, the trainees of Supervisor B, and those on medication alone after 12 and 20 months of treatment and in the two-year follow-up study, but they do worse than all of them in the statistic for six months of treatment, with $p < .001$ or $p < .01$ (pp. 423, 427, 430, 434, 438).

Third, studies by Sidney Blatt and his co-workers also provide evidence for the efficacy of psychodynamic approaches and of psychoanalysis. In their first publication, they demonstrate the therapeutic efficacy of psychodynamic treatment for severely disturbed young adults in the Austen Riggs Center (Blatt and Ford, 1994, p. 197; pp. 149–57 for the statistics) and, in a second publication, they demonstrate the greater efficacy of psychoanalysis than brief pharmacological and psychological outpatient treatment for the subgroup of introjective pathologies (paranoid schizophrenia, overideational—or guilt-ridden, self-critical—borderline personality disorder, paranoid and schizoid personality disorders, obsessive-compulsive disorders, introjective—guilt-ridden—depression, and phallic narcissism; Blatt, 1995, pp. 1015, 1013). Thus, at least in some cases, there is an indication that psychoanalysis is more effective than other forms of psychotherapy.

Blatt's conclusion is supported by one of the reports from the Stockholm Outcome of Psychotherapy and Psychoanalysis Project. In that report the authors study the "capacity to prevent the return of symptoms after treatment" rather than "the sheer reduction of acute symptoms" (Blomberg, Sandell, Lazar, and Schubert, 1997, p. 1). The report documents the finding that both "psychotherapy and psychoanalysis produce highly stable effects" (summary), but "patients who—for whatever reason—did not undergo the recommended *psychoanalysis* were significantly worse off than patients who did not undergo the recommended psychotherapy" (p. 3).

Finally, in their book, Karon and VandenBos (1981) analyze the claim that psychodynamic therapy is too costly. After adding costs of rehospitalization and welfare support for chronic illness, Karon and VandenBos argue that psychodynamic approaches to schizophrenia are substantially cheaper than medication alone (pp. 442–453).

Recommendation 22's rejection of psychodynamic approaches to schizophrenia should therefore be revised in light of the above empirical evidence which contradicts the claims of the PORT report.

CONCLUSION

The Schizophrenia PORT report rejects, in recommendation 22, the use of psychodynamic therapies. It also rejects, in recommendation 26, a crucial point of psychoanalytic theory by rejecting family interventions based on the assumption that the family of origin can make a causal contribution to schizophrenia.

I have pointed out the formal deficiency of these two recommendations: both are categorical recommendations based on level "C" evidence, which is defined by the authors as a: "Recommendation based primarily on expert opinion, with minimal research-based evidence, but significant clinical experience" (Lehman et al., 1998, p. 2). No other group of interventions is judged on only level "C" evidence. Furthermore, conclusions based on low-level evidence would be better formulated in tentative rather than categorical statements.

More substantially, I have reported on selected empirical studies that contradict the claims of recommendations 22 and 26. I therefore urge that the two PORT recommendations be revised. I argued that the recommendations can be rewritten so that a useful point in each of the recommendations can be preserved and even strengthened. With reference to recommendation 22, it is wise to stress that psychoanalytic-inspired therapies with schizophrenics should not simply apply the same methods used in the treatment of neurotics. With reference to recommendation 26, it is important to stress that imposing or augmenting guilt is not a good therapeutic technique. Similarly, it is not good therapeutic practice to deny the truth, as has been statistically proven by the many studies I have reported. To tell the truth and not create guilt requires artful interventions, examples of which I have presented. If recommendations 22 and 26 were to be rewritten as suggested, they would become scientifically defensible and would make a positive contribution by stressing the contributions that many approaches, including psychoanalysis, can make to the treatment of schizophrenia. As they stand now, recommendations 22 and 26 are contradicted by the scientific evidence I have submitted.

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