A Step Forward in Research on Psychotherapy of Schizophrenia

by Philip R.A. May

Abstract

The Boston group have devoted 10 years to a thoughtful, comprehensive, and much needed study that will undoubtedly become a classic reference in the field. If the emphasis is placed on differences that reached statistical significance, the major finding is minimal outcome differences between exploratory, insight-oriented (EIO) and reality-adaptive, supportive (RAS) therapies with, if anything, significant advantages to RAS in recidivism and performance. Selective beneficial effects for EIO gain only very weak support. The results should contribute to a climate wherein we may move on to study how psychotherapy and pharmacotherapy can be combined to achieve optimal results and minimize toxic effects.

John Gunderson and the Boston group (Stanton et al. 1984; Gunderson et al. 1984) deserve our thanks for devoting 10 years to a thoughtful, comprehensive, and much-needed study that will undoubtedly become a classic reference in the field. They are also to be congratulated on surviving to tell the tale, and praised for designing and executing a study that contradicts their original prejudices about the relative merits of the two different types of psychotherapy for schizophrenia.

There were three main conclusions:

• In general, minimal outcome differences between exploratory, insight-oriented (EIO) and reality-adaptive, supportive (RAS) therapies, regardless of the type of outcome measure.

• If anything, in the areas of recidivism and role performance (occupational functioning, hospitalization, and social adaptation), RAS exerts preferential and specific action compared with EIO.

• In contrast, EIO appears to exert preferential, albeit more modest, action in the areas of ego functioning and cognition (adaptive regression and thought disorganization).

For reasons that I will go into later, there is less than solid support in the data for even the “albeit more modest action” postulated for EIO. Let us for the moment, however, leave this on one side and consider the study itself. In general, the confidence that we may place in the conclusions of a study depends on its aims, on the design to achieve these aims, and on the effort expended to adhere to that design. From this viewpoint, the investigators aimed to compare EIO and RAS given for 2 years by experienced therapists, added to a good basic program of pharmacotherapy and group activities. Assignment to treatment was randomized; outstanding efforts were made to establish and monitor the process of the two forms of therapy, to investigate areas of psychological functioning in which insight-oriented psychotherapy might be expected to exert specific effects, and to assess the therapists themselves. Provisions were made to equate other potentially confounding forms of treatment, particularly pharmacotherapy, which was supervised by a highly experienced and much respected consultant. Evaluation was comprehensive and multifaceted; data management was sophisticated. Finally, the investigators’ sober account of their difficulties and problems in implementing the design (difficulty in obtaining patients, therapists, and raters; attrition;

Reprint requests should be sent to Dr. P.R.A. May, UCLA/NPI, 760 Westwood Plaza, Los Angeles, CA 90024.
quality of records; nursing data; masses of data; staff changes; suspicion; and time pressures), instead of detracting from the findings, creates an impression of integrity and thoroughness: They are clearly not persons who gloss over difficulties and unwelcome data points. In my own experience, if anything can go wrong in research, it will—at the most inconvenient time and in the most aggravating way. The authors’ account will acquaint future investigators with some of the practical problems that they will undoubtedly encounter.

No study can be perfect or answer all possible questions. Despite the limitations that the authors themselves describe, their study is, in my opinion, far and away the best evidence that we have today on EIO-RAS issues. Indeed, it is likely to be the best we may get for a long time, given the statement that “Of the many institutions approached about possible collaboration, most of those that had the strongest identification and tradition with the practice of intensive psychotherapy still felt they could not randomly withhold this treatment from patients admitted to their institutions” (Stanton et al. 1984, p. 524). Note, however, that the study cannot be taken as providing any indication of the results that might be obtained when either form of psychotherapy is given alone, without concomitant pharmacotherapy and group activities.

We can be confident that while some will accept the results of this study with equanimity, others will aver that 2 years is not enough; that the therapists were lacking in analytic training; or that the “Boston amalgam” is not deep enough, some other form would be better, etc. That the differences in favor of RAS were most clear-cut at McLean, where the therapists were more experienced and most committed, should mitigate much of this criticism. There can, I think, be little realistic criticism of the study on the basis of the therapists, and their experience range (5–35 years) dovetails nicely as a contrast to that in the Camarillo study (6 months–6 years). However, a dark aside: Much has been made of the need for therapists in a study to be “committed.” Granted that a poorly motivated therapist can make a mess of any kind of treatment, is there no place in the research world for uncommitted therapists? For those who have an open mind? Are open-minded persons to be banished forever from the psychotherapy research scene, except as investigators?

It is possible that some of the differences in favor of RAS might be due to better or more appropriate use of medications in that group, a notion that gains some support from the fact that long-term RAS patients tended to get more medication and lithium was used more often. This was only a trend, however, and one would expect that the expert consultation would even things out. Indeed, one might even follow Freud’s suggestion and propose that the more intensive understanding of subjective experience provided by EIO might have led to more appropriate adjustment of dosage in that group.

The diagnosis of schizophrenia may be picked on as less than certain by currently fashionable research criteria (27 percent schizophreniform or schizoaffective). Personally, I agree with Lehman (1984), who questions the need, even the justification, for preoccupation with foolproof diagnostic criteria. As he observes, what matters for a clinician is not a “true” diagnosis of schizophrenia but the prevention and treatment of its symptoms and making prognoses for their future disappearance or development. There is, in fact, no evidence that homogeneous diagnostic groups are an essential requirement for successful research into therapy; substantial progress in research has been made without it. Indeed, response to treatment may, in itself, lead to meaningful reform of diagnostic systems. And, on the battlefield of clinical reality, persons identified by “at least one modern criterion” are commonly treated as if they are schizophrenic and by the methods employed in this study.

Others will pick on the representativeness of the patient sample: Does it truly represent the middle prognostic range when 2,000 persons are screened to yield 186 suitable candidates (9.3 percent) and only 164 (8.2 percent) are finally accepted into the study? The answer is, of course, “Yes!” Stringent entry criteria are necessary to conduct an uncontaminated study, and this requires a massive screening effort. (In the Camarillo study, we had to screen 4,816 persons who were officially, and often erroneously, designated as first admissions to obtain 640 suitable candidates (13.3 percent), of whom only 247 (5.1 percent) were accepted into the study.) However, the authors’ statement that “our sample included the vast majority of schizophrenic patients for whom the possibility of psychotherapy could reasonably be under consideration” (Stanton et al. 1984, p. 529) overstates the case. I would suggest a more temperate version—it was a representative sample of patients who might reasonably be diagnosed as schizophrenic, in the middle prognostic range for that illness, and for whom it was felt that psychotherapy was a reasonable proposition.

Then there is the matter of
dropouts. Since the objective was to study only those who were treated for a minimum of 6 months, the early dropouts do not bother me nearly as much as those that occurred after 6 months, which might introduce substantial bias. (One wonders, of course, whether the outcomes for the early dropouts, not just their baseline characteristics, were any different from the outcomes of the remainers, but this, I gather, is to be the subject of a future publication.) The authors’ figures for attrition are based on the initial n of 164—56 percent at 12 months, 69 percent at 2 years. However, if you recalculate on the basis of the 95 persons who successfully completed the required minimum of 6 months of treatment, things look somewhat better—24 percent at 12 months, 46 percent at 2 years (a little greater for completed assessments). As I see it, attrition is a fact of life; the investigators did the best they could: they tried hard; I’m not so sure anyone else in psychotherapy practice could do any better; and their planned analysis of the reasons for dropping out should yield important clinical information.

Turning to the statistical analyses, I applaud a detail—the decision to use only two-tailed significance tests. It has always seemed to me thoroughly dishonest to double the odds in favor of calling something significant merely because it happens to go the way you hoped it would. I must, however, express serious reservations about some of the analyses, or rather about some of the conclusions that are drawn from them. These reservations center around the authors’ position that “While recognizing the importance of significance testing, we believed that it would be a mistake to overemphasize this aspect of the data analysis since we knew that the size of our sample would make the detection of statistically significant differences difficult” (Gunderson et al. 1984, p. 566). Granted that significance measures say nothing about the relative magnitude or clinical importance of an observed effect. Granted that every cloud has a silver lining—the decision to rely heavily on effect-size analyses makes it less likely that differential treatment effects would be overlooked.

Nevertheless, significance tests do say something very definite about the likelihood that a result might have occurred by chance. To “pay more attention” to effect size seems to me to be inflating the importance of the nonsignificant. A more conservative approach is to pay attention to effect size only when the finding is statistically significant. Nonsignificant results may be interesting, even suggestive for further research, but they are a very weak basis for forming conclusions. I would certainly hope that readers will not convert them to teaching dogma for uncritical students.

If you place the emphasis on differences that reached statistical significance, there can be no quarrel with the authors’ statement that the most obvious and striking finding was “minimal outcome differences between . . . EIO and RAS . . ., regardless of the type of outcome measure” (Gunderson et al. 1984, p. 571). Their further conclusion that RAS had selective beneficial action is supported at 12 months by statistically significant results (for the full sample) on 3 of the 32 variables (paranoid expansiveness, primary process thinking, and days in hospital). At 24 months, the first two were not significant: days in hospital, days dependent, and household responsibilities were borderline. (For remainers only, the 24-month advantages of RAS reached somewhat higher significance levels.) However, the suggestion that EIO had selective beneficial effects has very weak support indeed. For the group as a whole (table 2), EIO was not significantly better than RAS on any variable (borderline at 12 months on optimistic/integrated attitude and anxiety-depression; not even borderline at 24 months). For remainers (table 3), there was only one statistically significant advantage (retardation-apathy) at 12 months, and this had disappeared at 24 months—not even borderline.

In the final overall interpretation of the data, it is concluded that EIO may have had specific effects on ego functioning and some core aspects of schizophrenic psychopathology, "hardly an insignificant finding" (Gunderson et al. 1984, p. 582). Unfortunately it was nonsignificant, statistically speaking. Ego functioning was assessed by three variables: adaptive regression, ego weakness, and subjective experience, for which none of the group contrasts reached even borderline statistical significance at any time period, either for the whole sample or for remainers only. When the three were combined into a single measure, "a moderate-sized effect favoring the EIO treatment was found" (Gunderson et al. 1984, p. 579). Appendix F, table 1, shows that the effect-size ratio (.17) does not meet the authors’ own criterion for a moderate-sized effect (.20) and that it was not statistically significant or even borderline.

Thus, the attribution of specific value to EIO rests on tenuous ground. One cannot accept the conclusion that it is “equally important” to the finding that RAS emerged as “the preferable form of treatment” (Gunderson et al. 1984, p. 582) from the point of view of hospital stay and full-time employment—particularly since there
was also some evidence of a specific EIO toxic effect—"Throughout the followup period, RAS patients spent considerably more days . . . in full-time employment than EIO patients . . . [raising] the possibility that, for schizophrenic patients, remaining in an intensive psychotherapy is somehow incompatible with working full-time . . . " (Gunderson et al. 1984, p. 575). As the authors observe, this has "important cost-benefit implications . . . [A] treatment which has documented, positive effects on occupational performance very quickly pays for itself by cutting into the enormous costs associated with the lack of productivity characteristic of most schizophrenic patients" (Gunderson et al. 1984, p. 582).

Times change, and so do the problems that need to be addressed. In the late 1950s when we started the Camarillo study, the issues were different from those that prompted the Boston study in the early 1970s.

Its results will, I trust, contribute to a climate in which we can move on to the next question: How can psychotherapy, drugs, other forms of treatment, and rehabilitation be combined to achieve optimum results and to minimize toxic effects? There is a widespread belief that if some is good, more must be better. This has been shown to be false for drug treatment. I suspect that it may be false for psychotherapy and social and rehabilitation therapies also.

I look forward eagerly to future publications—and future research—by the Boston group.

References

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The Author
Philip R.A. May, M.D., is Della Martin Professor in Psychiatry, Neuropsychiatric Institute, University of California at Los Angeles. Dr. May is also at the Camarillo State Hospital and the Veterans Administration Medical Center, Brentwood, Los Angeles, CA.