The Fear of Reducing Medication, and Where Have All the Patients Gone?

by Bertram P. Karon

Abstract

In spite of the enormous effort made by the Boston group, the findings were few. The high rate of dropouts and refusals to be assessed raise serious questions. One significant point that should be raised is that there was no difference in the use of medication between the treatments, and there should have been to foster underlying change with expressive psychotherapy. Also, treatment was not attempted without medication. Did patients drop out to avoid medication? Would a more flexible technique such as multiple crisis intervention have reduced dropout? Did selecting subjects with normal Wechsler Adult Intelligence Scale scores eliminate severe thought disorders, making differential improvement impossible? Was eliminating the sickest third of patients a mistake?

The most striking features of the Boston psychotherapy study (Stanton et al. 1984; Gunderson et al. 1984) are the extraordinary effort and care that the experimenters have exercised, and the field must be grateful to them. But it also exemplifies the extraordinary difficulty in carrying out meaningful psychotherapy research. Despite their careful work, the attrition of subjects is enormous, not all of these subjects take the evaluations, and the number of conclusions is not great. Undoubtedly, the most important lessons from this study will be learned at a future time when the investigators have had time to winnow out what is most meaningful.

At first glance, it seems as if the exploratory, insight-oriented (EIO) and the reality-adaptive, supportive (RAS) treatments are not greatly different in effects, with time out of the hospital and in full-time employment seeming to be in favor of RAS and with some areas of psychological functioning showing an advantage to EIO. But the high dropout rate raises serious questions about even these conclusions. It is worth raising a few critical questions.

Comments on the Michigan State Psychotherapy Research Project

Probably the least important issues are some of the misstatements about our study. Nonetheless it is not true that the psychotherapeutic approach used was "direct analytic (a la John Rosen)" treatment. Although there is a historical connection (I worked with him for 1 year, during which I resigned repeatedly), there is little relationship between the technique used in that study (e.g., Karon 1963, 1976; Karon and VandenBos 1981) and the technique actually used by John Rosen (Brody 1959). The medication was not "uncontrolled" but adjusted on the basis of clinical judgment. That the medicating psychiatrists had excellent clinical judgment is indicated by the fact that the "medication-only" group in our study did as well as the "medication-only" group in the May (1968) study—the difference being that their inexperienced therapists, supervised by therapists inexperienced with schizophrenic patients (although very experienced with other kinds of patients), were not as effective as medication, while our relevantly experienced or supervised therapists were more effective. One "psychotherapy alone" patient in the final tabulation of our study had received

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medication for less than 2 weeks; deleting that subject made no
difference. For further discussions of
the frequent misunderstandings of
our work, see Karon and VandenBos

Disadvantages of the Large-
Scale Research Design

The advantages of large-scale
research, of large numbers of
carefully specified measures, and use
of several institutions seem obvious,
and the analogue with drug evalu-
ation studies is also obvious. The
disadvantage, however, even in drug
evaluations, is that so much has been
anticipated that one may not notice
what was not anticipated in advance,
as, for example, unanticipated
negative side effects of a drug. In
psychotherapy research in particular,
one is almost inevitably studying an
at least slightly different population
than one intended, because of
features of the research design. Thus,
in the Michigan study, we intended
to study the responses of acute
patients, and somewhat expanded
that definition to ensure sufficient
patients. What we did not know was
that even first admissions to center
city treatment facilities include
almost no acute patients, that our
sample was entirely chronic, and that
lower socioeconomic patients and
their families tend to lie about
anything that they believe will
prevent them from getting adequate
treatment (and, sadly, it is an appro-
priate response on their part) such as
previous hospitalization, alcohol, and
drug histories until they have reason
to believe that you are trustworthy
and the information will not be used
against their best interests. We made
the decision that studying the
response to psychotherapy of such
chronic patients was as valuable as
studying the response of acute
patients, and continued the study.
Drugs do not learn, but psycho-
therapists do, and need to be able to
act on what they learn. Some of the
critical further learning in our study
will be discussed below.

The Vanishing Subjects

One is struck by the high rate of
disappearance of subjects from
treatment (remaining in treatment: 58
percent at 6 months, 44 percent at 12
months, and 31 percent at 24
months) and reluctance to be
assessed even if in treatment (of
those in treatment, assessed at 6
months: 95 percent at 6 months, 88
percent at 12 months, and 76 percent
at 24 months). There is much to be
said for analyzing only patients who
have received a critical amount of a
treatment. But there is also
something to be said for analyzing
data on all patients to whom a
treatment is made available. Both
analyses should be made. Patient
noncompliance is an important and
meaningful issue. Thus, in evaluating
kidney dialysis, the well-being of
patients who choose to die rather
than continue the treatment is also
relevant, but the percentage of
patients who choose to die rather
than continue the treatment is also
relevant. Similarly, research on
psychiatric drugs and on psycho-
therapy must include noncompliance
and its reasons. Patients may leave
treatment because they have obtained
what they want (even if their
therapist thinks they should want
more). Patients may leave treatment
because it isn’t helpful, or because it
is unpleasant, or makes them worse.
Patients may leave treatment because
their remaining problems are more
urgent, and they do not believe the
treatment will help with those
problems. Patients may leave because
relatives tell them to stop. Patients
may leave because treatment is too
economically demanding, not only in
direct treatment costs, but also time
away from work (which generally
will get most patients fired), or the
costs of babysitters and transpor-
tation. Whatever the reasons, it will
be instructive to explore them, and to
find out what benefits the patients
obtained in the time they were in
treatment.

It is instructive to compare this
with the Michigan study. While we
had only 36 patients (or perhaps
because we had only 36 patients),
every patient to whom treatment was
offered was included in the treatment
group, and every living patient was
evaluated at the end of the project.
This is unique, but whenever there is
a selective bias, one has to ask what
determined who allowed themselves
to be evaluated. (Our experience
suggests it is the worst functioning
patients who are hardest to get to
come in for assessment.) Even in the
May (1968) study, one of these
investigators reported that “when we
lost track of a patient, we started a
new patient in the group in the
hospital to replace him” (A.H.
Tuma, personal communication).

Even more important, we
 discovered that economically poor
patients frequently are so
overwhelmed by “reality” issues that
they will only accept psychotherapy
on a crisis intervention or multiple
intervention basis, as opposed
to regular appointments. The patients
forced us to change our views about
this during the project, and we found
that they greatly benefited from such
help, often as much as those who
kept more regular appointments.
Such learning, and the willingness to
adapt technique to the previously
unknown aspect of the patient,
would have been harder if we had
had an even more elaborate research
staff, overly specified manuals of
treatment, and two other hospital staffs whose agreement would have been necessary.

The Fear of Withdrawing Medication

It is noteworthy not only that there was no attempt to treat schizophrenic patients without "antipsychotic" medication, but that the amounts of medication were the same for the EIO and RAS patients. This attests to the degree to which the May study and drug company advertising have scared professionals. Professionals frequently do not dare to treat schizophrenic patients without "antipsychotic" medication, and are afraid to reduce medication once the patients are started on it.

One question about the dropout rate is whether the project psychiatrists were seen by the patients as unequivocally committed to medication, and whether they were avoided in order to avoid taking medication (which is frequently experienced as unpleasant).

It is clear that in the absence of psychotherapy, medication, at least in the short run, is helpful. When combined with psychotherapy, it makes behavioral control easier to attain. However, the finding of the Michigan study was that it slowed the changes with psychotherapy, and diminished the improvement in the thought disorder with psychotherapy.

"Antipsychotic" medications, among other things, dampen down the affective system. One view of their therapeutic action is that by dampening down anxiety (terror) and aggression, they make unnecessary the symptoms caused by high levels of terror (characteristic of schizophrenia), aggression, or the defenses against such terror or aggression.

But the affective reactions of the patient during a psychotherapy hour are one of the mechanisms by which psychotherapy produces change. Medication, which interferes with affective reactions outside of psychotherapy, also interferes with them during psychotherapy, thus removing one of the curative mechanisms.

One of the findings of the Michigan study most frequently cited is the long-term comparisons for the 2 years after the project period, i.e., the 20- to 44-month followup. The differences between psychotherapy-treated patients and medication-treated patients did not disappear, but became most striking in the followup period. Patients treated by medication alone averaged 99.8 days in the hospital in the followup period, and those treated by psychotherapy (by both experienced and inexperienced therapists), 27.2 days. But relevant here are the data concerning the conjoint use of both psychotherapy and medication. Inexperienced therapists using adjunctive medication maintained the medication, and their patients averaged 112.3 days in the hospital. The experienced therapist using adjunctive medication told the patients that understanding, not medication, was curative and withdrew the medication as rapidly as the patients could tolerate it; these patients averaged only 10.8 days of hospitalization. Thus, the withdrawal of medication should be an integral part of insight-oriented psychotherapy with schizophrenics, and the failure to do so will diminish the long-term effectiveness of such psychotherapy.

Recently, there is evidence that long-term use (e.g., 10 years) of antipsychotic medication produces tardive dyskinesia in perhaps 30 percent of patients, as well as neurological changes that make sudden withdrawal traumatic (Breggin 1983). This is true despite evidence that suggests that more than half the patients believed to be taking maintenance antipsychotic drugs have stopped taking them and lie about it unless they are rehospitalized.

Recent data (Ciompi 1980) suggest that the long-term spontaneous full recovery rate in schizophrenics is 35 percent. There is reason to be concerned as to whether long-term use of antipsychotic medication prevents those spontaneous recoveries.

Selection of Patients

The Boston study repeats the procedure used by May of selecting the "middle third" of patients. This procedure is a mistake, because it eliminates the very sick. If there is anything we mean by schizophrenia, it certainly includes the very sick. It is true that one must be more careful about gross organic conditions misdiagnosed as schizophrenia among the very sick, but psychotherapy often makes possible a more accurate physical and neurological diagnosis, even if the initial diagnosis is in error.

Startling is the fact that the mean Wechsler Adult Intelligence Scale (WAIS) score of the patients before treatment was 98.2. (Obviously, the average nonschizophrenic score is 100.) It is well known that WAIS scores are affected by schizophrenia (Rabin and King 1958), as is the ability even to take the WAIS. Again, they have eliminated the sick patients. It would be difficult to show an improvement in thought disorder (of which the WAIS is one measure) if you select schizophrenic patients whose thought disorder in some ways is minimal before treatment. In the Michigan study, improvement in full-scale WAIS scores was one of the measures most responsive to psychotherapy as compared to medication. If the
investigators wished to screen for IQ, they should have used a measure less sensitive to schizophrenic disorders, such as a multiple choice vocabulary scale.

Finally, one has to wonder about the criterion for selection specifying that patients should not have had more than 20 treatments with electroconvulsive therapy (ECT) (table 2). The authors do not refer to this issue again, and perhaps none of their patients had received ECT. However, the inclusion of patients who had any ECT makes no sense in a study designed to examine the effectiveness of psychotherapy. It is not that treating such patients is absolutely impossible, but that the process of change with psychotherapy is enormously slowed. The inclusion of patients with any ECT in a research study makes it very difficult to discover the helpful effects of psychotherapy. This mistake was made by Grinspoon, Ewalt, and Shader (1972); at least 55 percent of the psychotherapy subjects in their study had histories of ECT, insulin comas, or both.

**Increasing the Number of Patients Who Comply With the Evaluations**

This is a difficult issue for all researchers. A simple suggestion (made by Paul Bakan, Ph.D.) which helps greatly is to pay the patient for the time involved in being evaluated. They frequently have added expenses, time off work, etc., and payment helps motivate them even if they have not continued treatment or are hostile to their therapist.

**Statistical Quibbles**

One could raise issues about factor analysis, about whether unique variance is really "error" variance, about the possible advantage and disadvantage of the use of regressed scores, and about using initial score as a predictor vs. analysis of variance, but such differences rarely make any difference in the meaning of data. One question that can be raised is the direction of causality in the correlations with time in treatment (e.g., it is plausible that a better patient may stay in treatment longer). Within-patient correlations, i.e., improvement with number of sessions for the same patient, would be decisive; however, the authors' interpretations of the data are probably correct.

**Issues of Technique**

All psychoanalytic psychotherapy is not the same. The real learning from this project will occur in future analyses. Two issues of technique that concern me have already been alluded to. The first is the failure to withdraw medication, which should be a planned part of insight-oriented psychotherapy, discussed with the patient, and integral to the process. The second is the use of multiple crisis interventions if the defenses of the patient require it. One can reasonably ask whether any of the dropouts would have been amenable to such an approach.

There is a tendency in psychotherapy research funding these days to demand "manuals" of specified procedures. Yet psychoanalytic psychotherapy is complex, and involves a process of continual learning from the patient. Freud's analogy of a chess game which can never be entirely specified is still relevant. He argued that one can describe the opening game, the closing game, and principles of strategy. But each game is unique because these two players are a unique combination. It is just as much a mistake to think of schizophrenic patients as being alike as it is to think of any other group of patients as being alike.

It would be presumptuous to make any comments on the technique used here, but there are questions that one would like to ask, and that, one hopes, will be answered in later analyses.

The principal question is whether the EIO therapists perhaps abstained too much from dealing with practical realities. In some settings sharp distinctions are made between supportive and insight therapies, and insight-oriented therapists in those settings sometimes pay insufficient attention to practical matters. But optimal work with schizophrenic patients requires a strong relationship within which insights are safe, the giving of information (which schizophrenic patients often lack), the analysis of why the patient can't make use of what he now knows and wants to make use of, the examination of every practical problem, and the unraveling of the complex intertwining of reality and fantasy.

It is interesting to note that on the Visual-Verbal Test (VVT) the RAS therapists seemed slightly more effective at McLean, where they probably were more influenced by the EIO atmosphere, and the EIO therapists were more effective at BU/VA, where they were probably more influenced to deal with practical matters.

In summary, the field owes these researchers a debt for their careful work. It is an important building stone in our knowledge. Undoubtedly, their future analyses will be the true yield of this study to the field.

**References**

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