Criteria for Evaluation of Results in Psychotherapy

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As all of you know, we are undergoing a tremendous revolution in the field of the psychoses. Whether the revolution has spread to the neuroses is for you to tell me. This revolution is perhaps not so noticeable to the men who have received their M.D.'s and Ph.D.'s in the last decade. Anyone who has worked in the field of psychopathology through the last two or three decades can't help but agree that we have undergone a revolution which has changed the entire face of psychotherapy. Things are going on now we never dreamed of 20 years ago. When I speak of a revolution, the first thing that comes to mind is, of course, a political revolution. Let me say something about that. In most political revolutions, the status quo is challenged, law and order are, at least temporarily, overthrown; prison doors are unlocked and thrown open; newcomers, previously belonging to the outgroup, come to the fore; the national treasury is confiscated; and finally, the revolutionists settle down to become conservatives only to be eventually overthrown in turn themselves.

The revolution in psychopathology has not been so drastic, but certain parallels do exist. Unlike political revolutions, the psychopathologic revolution occurred quietly, subtly, without public furor. But its effects are no less telling. Law and order, as represented in our diagnostic classification, have undergone a considerable change. The diagnostic nomenclature has undergone a serious face lifting in the mental diseases, and radical extirpation in the mental deficiencies—as you know, the term mental deficiency no longer exists in our classification system. Whether these operations were a success is still debatable. The prison aspect of our institutions is gone. The hospital doors have been thrown open. Release rates for mental patients have been doubled and admission rates have risen. Clinical psychologists, sociologists, anthropologists, statisticians, biometricians, biochemists, geneticists, pharmacologists, behavioral scientists, and others—the "outgroup" in the thirties, hardly ever heard in the halls of psychopathology then—are now, if not entrenched, at least safely inside the door. Funds for research, rather scarce in the thirties, are today, if not ample, at least available from the national treasury. The separation of mind and brain, a firm tenet of the thirties, is no longer a stumbling-block in integrated attacks on mental disorder.

How much of this has affected the neuroses, I don't know. Perhaps the revolution in psychopathology I just mentioned, with twice as many former hospital patients in the community as there were 20 years ago, brings to the fore the problem presented in our follow-up clinics for released patients. These outpatients are no longer the neurotic outpatient, because he has clinics and our problem there is almost parallelize that the underlying psychotherapy although how far apart they are remains to be seen.

What have we learned from the things that may be carried over into the future? Our colleagues, I believe, have come to the fore; the national treasury is confiscated; and finally, the revolutionists settle down to become conservatives, only to be eventually overthrown in turn themselves.

Let me first point out what happens when evaluating outcome, which has been the casuistry. We find as we look over our data third of the patients got well enough to go home in the hospital, and one-third died. The remaining third outcome was really the result of psychodynamic reflection of a theorem of human judgment.

When faced with a quarrel, we tend to sort of grouping. The natural thing to do is to divide, and then to divide, and then to divide, with anything that was done by questioning how we divide things up with such a division. The only difference between these and the fact that we have pushed out the middle third so that there is no one one time, with a considerable proportion of the patients.

The second point I think we might raise is: How do we know that there is no one-third who is not improved after the one-third said to have improved actually know it either intuitively, when we try to reason that when he came in, or more directly by making use of the objective baseline for evaluating the patient when he left again before he went out, and letting the dimensions into which we see an improvement.

One of my colleagues, who has helped me forecast the prognosis of psychopathologic conditions, in a rather valuable monograph on prognostic Charles Windle, has come up with the fact that if you like it or not, still makes good in impression that this one-third, one-third, one-third is the reflection of the unreliability of our initial at
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ough how far apart they are remains to be investigated.

What have we learned from the things we have accomplished in the
as the psychoses that may be carried over to the neuroses? After
ing all the answers from my colleagues, I cannot add anything else to
dicate what the criteria for outcome are, but I can perhaps tell you how
has worked out in another area.

Let me first point out what happens when you don't have criteria for
ulating outcome, which has been the case ever since the days of Es-
rol. We find as we look over our data that, since 1812, about one-
rd of the patients got well enough to go home, one-third improved but
ed in the hospital, and one-third deteriorated or maintained the
us quo. I have often wondered whether this one-third, one-third, one-
rd outcome was really the result of psychiatric evaluation or nothing
the reflection of a theorem of human judgment.

When faced with a quandary, we tend to divide up things into some
rt of grouping. The natural thing to do is to put one-third over here,
-third over there, and one-third in the middle. It may have had
thing to do with anything that was done therapeutically; it may be
question of how we divide things up when we don't have a basis for
a division. The only difference between Esquirol's day and ours is
we have pushed the middle third so that two-thirds are out of the
al at any one time, with a considerable number relapsing.

The second point I think we might raise in connection with this
blem is: How do we know that there is improvement, how do we know
at the one-third said to have improved actually did improve? We only
ow it either intuitively, when we try to remember what the patient was
when he came in, or more directly by means of setting up a specific
jective baseline for evaluating the patient when he comes in, evaluating
on again before he goes out, and letting the data dictate to us what are
dimensions in which we see an improvement or a change.

One of my colleagues, who has helped me in writing some reviews on
 prognosis of psychopathologic conditions, and who has recently writ-
 a rather valuable monograph on prognosis in mental deficiency, Dr.
Windle, has come up with the following suggestion, which, with
you like it or not, still makes good scientific sense. He is of the
ion that this one-third, one-third, one-third ratio is nothing else but a
ction of the unreliability of our initial and our final judgment about
the behavior of the patient. If you assume that the reliability of psychiatrists' judgment is of the order of 0.57 and since they deal only with the sick who constitute only one per cent of the general population, then you may ask: How reliable would the judgment be if it were spread over the entire population, including both normals and abnormal? And the answer, statistically, is: It would jump to about 0.96. When the reliability of judging normality versus abnormality is 0.96, how often would one find a person who was really down at the bottom, in the lowest 1 per cent on the initial evaluation, shift by chance into the upper 99 per cent or well people. The answer is one-third of the time. In other words, you can regard this one-third, one-third, one-third division of outcome as nothing else but a reflection of the unreliability of the initial and final psychiatric judgment.

That is a harsh judgment to make, but although Windle's hypothesis is far from proven, there is no evidence to contradict it at the moment; these are the kinds of issues that we are faced with when we try to evaluate the outcome of therapy or the outcome of illness without having good criteria and good methods for making the initial judgment and the final judgment. What ways do we have for making the initial judgment? Dr. Stevenson has already pointed out ways of doing it, and so has Dr. Franz Alexander, and so has Dr. Wölberg. I would like to point out briefly the methods we have tried to develop in our own laboratory.

First of all, we try to find out, as far back as possible, including the first nine months of life, all we can about the patient's early development, his adolescence, his early adulthood—all the data up to the point when he becomes a mental patient, that is, his premorbid characteristics.

Secondly, we obtain by means of a newly developed mental status examination schedule, which is objective in nature, the actual clinical status of the individual when he arrives for treatment. We no longer depend on the unsystematic history-taking methods of the psychiatric resident. Instead, we cover in a systematic fashion the possibly existing psychopathology through an objective, controlled interview. Then we have a whole series of psychologic tests, emerging in various laboratories, which tap the physiologic, sensory, perceptual, psychomotor, and conceptual areas. We go on into the sociologic milieu, family relations, family structure, and other sociologic and anthropologic aspects, in order to be able to get a picture of the entire individual from beginning to end, so that, with these data available, we can try to predict what the outcome will be. Finally, the course of the illness, after admission, is recorded in a specially developed Ward Behavior Inventory by the nursing staff.

Let me give you two examples of how we use, intuitive, clinical material, and make it with particular goals, in order to be able to the whole literature, in which we found a place to prognosis in schizophrenia, I am in the way, these 150 traits have stood the test and in our day. It was bad to have flatness of today. About 80 per cent of the traits have forer throughout recorded psychiatric history and gone in these last 70 years, but what a p. seems to be far more important than the kind of it is so important to find out the nat the for treatment and the kinds of assets have.

The first trait I have selected is flatness of affect, at least when you went through your triad stay, had to assess the level of affect, we don't mean to say that the measure replaces it in the last analysis, when you really want to know the patient, you have to use many more clinical methods. Here is one aspect of his behavior which is measurable, and recordable, but also seems to be important. We conduct the interview in the usual fashion. It brought him to the hospital and other groups interview into three parts, the first ten minutes, the second ten minutes, and the third ten minutes.

During the first ten minutes, the conversation is set, and for all you could tell, it is an ordinary to have it so organized that if the patient stutters for ten seconds and only then ask the next question.

During the next ten minutes, every time he has some reference to self-referred affect, like "I am embarrassed," "I was worried," "Uh-huh," "I see," "I understand." Finally, in the third period, we stop reinforcing the recording and carefully count the number of times they are defined as being specifically related to affect.
Let me give you two examples of how we have taken the rather amorphous, intuitive, clinical material, and made it into sharpshooting rifles, zing at particular goals, in order to be able to get objective facts. Out of the whole literature, in which we found approximately 150 traits that ate to prognosis in schizophrenia, I am picking two as an example. By the way, these 150 traits have stood the test of time well. It was a sign to have a sudden onset in the year 1900, and it is still considered today. It was bad to have flatness of affect then, and it is still bad today. About 80 per cent of the traits have retained their prognostic value throughout recorded psychiatric history. Therapies have come and gone in these last 70 years, but what a patient brings to the therapy seems to be far more important than the kind of therapy he gets. That is why it is so important to find out the nature of the patient-material for treatment and the kinds of assets and liabilities these patients are.

The first trait I have selected is flatness of affect. Many of you, I am sure, at least when you went through your training period in the mental hospital, had to assay the level of affect of which the patient is capable. It doesn’t mean to say that the measure replaces clinical intuition, because in last analysis, when you really want to know what is the matter with a patient, you have to use many more clinical instruments to find out. Here is one aspect of his behavior which seems to be not only objective, ascertainable, and recordable, but also seems to be related to outcome.

We conduct the interview in the usual fashion, asking the patient brought him to the hospital and other general questions, and divide the interview into three parts, the first ten minutes, the second ten minutes, and the third ten minutes.

During the first ten minutes, the conversation goes on as I just indicated, and for all you could tell, it is an ordinary clinical conversation. That is, we have it so organized that if the patient stops talking, we wait about 30 seconds and only then ask the next question or prod him along to go talking.

During the next ten minutes, every time he makes an utterance which is some reference to self-referred affect, like the phrase “I love,” “I hate,” “I’m embarrassed,” “I was worried,” the interviewer says, “Uh-huh,” “I see,” “I understand.”

Finally, in the third period, we stop reinforcing, and then we take the recording and carefully count the number of utterances which we defined as being specifically related to affect. We count the number the first ten minutes, in the second ten minutes and in the third ten
minute interval. Here's what happens: He gives little affect to begin with, and then, when you start reinforcing, he emits more affect. When you stop reinforcing, he declines in affective utterances.

What happens to normals is the first question you might ask (2). The normal's curve to begin with is very much like the patient's. Normals do not differ significantly from schizophrenics in the first period (esperant). The second period (conditioning) also shows no significant difference. The difference here lies in the third period (extinction). While both normals and schizophrenics slow down in their rate of making affect statements, the schizophrenics slow down much more. This may be the reason why Freud said that it's no use trying to analyze a schizophrenic. You have to keep on doing it the rest of his life, because when you leave him, he'll drop back, he won't show much of the effects of learning. And in the conditioning curve is the area where we have our prognostic differential (3). The patients who do not become conditioned are by and large the ones who remain in the hospital at the end of six months. Those who show an increase in affect statements are out at the end of six months. Whether the differentiation into Ins and Outs is due to differences in affect or to differences in conditionability per se remains for further research to clarify.

Just as an aside, it may very well be that this increase in affect statements under reinforcement may be the reason why Freudsians get freudian dreams, Jungians get jungian dreams, Rogerians get no dreams at all! It may very well be that as a freudian dream starts looming up the freudian therapist knowingly or unknowingly becomes more reinforcing in his manner. Thus the process of reinforcement that takes place may be one of the most essential elements in the interaction between the therapist and patient. In fact, we have turned this thing around a bit, asking ourselves, "Can we use this technique to find out what goes on in people's minds—what concepts they have" (4), just as Piaget, for example, goes about trying to discover concepts in the minds of children at various ages. This is a technique which may be useful for our purposes.

For example, we tried the following experiment (5). We asked the subject to say three digit numbers. Every time he gave an odd number, we would say, "good"; we wanted to see whether or not he would give more odd numbers, that is, did he have the concept of odd numbers? And the same thing can be done in other areas. Children, for example, are very difficult to deal with in these situations. We like to know their concepts of mother and father and of their family relations so we made a little papier-mâché clown with a red bulb for a nose (6). We have a little song, ostensibly sung by the clown. The happy only when children speak to him. up. You can see what sort of a tool we talk toward "mother" or "father" or " of the light reinforcement and in this mental content they have.

As a second example I want to describe the question of sudden versus insidious (7). Here, we searched over the entire lifespan development which would tell us whether was already sick or not. We concentrated on patterns and discovered, to our great amazement in detecting insidious onset, normative studies on adolescent friends and poems have been written about it but friendships in the normal—we had to get feature, we discovered, was that the chronic schizophrenic has, in contrast to distribution is quite unusual. It is U-sh his own age, the rest are mostly younger (the normal, his friends are mostly around a few older.

Secondly, this chronic schizophrenic is always the one brought into the friendship It's the others who break it off. The kind of interesting features which studying the adolescent friendship pattern whether or not he was already showing the mental of a psychosis.

I would like to go on to the next topic issue with Dr. Alexander. I regard his statistical summary go on to evaluate outcome st turn to an analysis of the process of their problem—a flight into process if you will, solving the problem. You will never get the problem. It seems to me that the hit stances where, instead of facing up to the to evaluate," we turn away from it an technique whose effect we do not know. recently as a hundred years ago there was.
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Happens. He gives little affect to begin with before he emits more affect. When affective utterances use up most of his life, because when you leave him off, you're left with much of the effects of learning. And in a way where we have our prognostic difference not become conditioned are by and large at the end of six months. Those who remain are out at the end of six months. Ins and Outs is due to differences in his own age, the rest are mostly younger or mostly older. In the case of the normal, his friends are mostly around his own age, a few younger and a few older.

Secondly, this chronic schizophrenic never forms a friendship. He is always the one brought into the friendship. He never breaks a friendship. It's the others who break it off. The kind of friendship he forms has a variety of other interesting features which makes us feel that perhaps, by studying the adolescent friendship pattern, we may get an insight into whether or not he was already showing insidious signs of later development of a psychosis.

I would like to go on to the next topic about which I want to raise an issue with Dr. Alexander. I regard his statement that at the present time you cannot go on to evaluate outcome statistically, and therefore must turn to an analysis of the process of therapy, as a flight away from the problem—a flight into process if you will, rather than as an approach to solving the problem. You will never get anywhere running away from the problem. It seems to me that the history of medicine is full of instances where, instead of facing up to the issue and saying “We’ve got to evaluate,” we turn away from it and try instead to describe the technique whose effect we do not know. Let me remind you that as recently as a hundred years ago there was a procedure called phlebotomy.
In the seventeenth century, book after book was written devoted wholly to process in phlebotomy, illustrating and discussing the various methods and locations for cutting into different veins for different symptoms of disease. The whole history of medicine seems to be replete with zealous movements in which the zealots redoubled their effort when the goal was lost!

There are ways of attacking the problem more directly, but I can only intimate this. We can build a scientific model, a structure, for encompassing the known facts about psychotherapy, of such a nature that we can elicit from it certain hypotheses for testing and for evaluating which perhaps will bring us out of the darkness so that we do not simply take refuge in studying the process, regardless of outcome. The process may be irrelevant to the outcome.

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FRANZ ALEXANDER, M.D.

I feel I didn't make my point, because I should be very brief, and certain briefly as the chairman would like it. I now, after I heard certain misunderstanding why I didn't make my point.

To study outcome without studying the true results achieved doesn't answer the question of Dr. Zubin's objection. To study outcome without studying the true results achieved doesn't answer the question of Dr. Zubin's objection. It is often studied what is left after a host of questions are not answered or procedures at all.

The best argument was given by Dr. Alexander about the hippocratic principle that you don't heal. There is such a tendency to take the beginning state and the end state and without attention pay you won't know anything. The patient achieved because of your therapeutic efforts primarily through your therapeutic powers. He might have recovered from psychotherapy. He might have recovered from other therapies, and other therapies might have come think often happens in very prolonged illnesses.

If you don't know what happened in the effect of what you did. Of course you begin and at the end, each patient different, however. One must study each beginning to the end. I am against a general therapeutic procedure by comparing these two end states. In order to evaluate psychotherapy and what we are doing, and how we evaluate recovery. Without this, we are simply not knowing what they mean.

Psychotherapy comprehends a most divvying from magic, suggestion, persuasion, and also systematic rational procedures, and also systematic rational procedures, of which me knowledge. One cannot even meanin
DISCUSSION

FRANZ ALEXANDER, M.D.

I feel I didn’t make my point, because the chairman admonished me at I should be very brief, and certain things cannot be said quite as briefly as the chairman would like it. But to make my point is easier now, after I heard certain misunderstandings which show me where and why I didn’t make my point.

To study outcome without studying the process by which this outcome has been achieved doesn’t answer the question. And here I can right away answer Dr. Zubin’s objection. This is far from running away from the problem. If we only study what happens to the patient before and after, a host of questions are not answered, and we don’t evaluate our procedures at all.

The best argument was given by Dr. Stevenson, who took up something in which I thoroughly believe. I am very much in agreement with him about the hippocratic principle that the doctor helps nature to heal—he doesn’t heal. There is such a thing as spontaneous recovery. If you take the beginning state and the outcome state and compare them with each other, without paying attention to what happened in between, you won’t know anything. The patient might have achieved what was achieved because of your therapeutic efforts. He might have achieved it primarily through your therapeutic effort, partially by his own self-healing powers. He might have recovered not because but in spite of psychotherapy. He might have recovered in some phases of the therapy, and other phases might have counteracted his recovery, which think often happens in very prolonged treatments. There are many possibilities.

If you don’t know what happened in between, you can’t evaluate the effect of what you did. Of course you must compare the patient at the beginning and at the end, each patient on his own. This is not sufficient, however. One must study each case precisely from the beginning to the end. I am against a general statistical evaluation of the therapeutic procedure by comparing these two points, the beginning and the end state. In order to evaluate psychotherapy, we must understand what we are doing, and how what we are doing affects the process of recovery. Without this, we are simply setting up figures blindly without knowing what they mean.

Psychotherapy comprehends a most diversified array of procedures, ranging from magic, suggestion, persuasion, consolation, reassurance, and also systematic rational procedures, based on advanced psychodynamic knowledge. One cannot even meaningfully differentiate the thera-

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pies according to these therapeutic measures, because most therapies contain all these factors. Equally heterogeneous is the total set of the conditions we treat, including psychopathologic symptoms and reactions to extreme traumatic events, chronic personality dysfunctions, behavior disturbances, chronic and transitory forms of psychosis. And to increase the complexity of the problem, the instruments of psychotherapy include not only the technical procedure enumerated above but also, a most important factor, the unique personality of the therapist.

According to our present studies, which we are carrying out by observing the process, this may turn out to be one of the most significant therapeutic factors. There is strong indication that the outcome of the procedure, be it magical incantation or standard psychoanalysis, to a large degree depends on the idiosyncratic qualities of the therapist or of the magician. There is good historic corroboration of this statement, since magicians and medicine men become medicine men because of certain idiosyncratic qualities which make them effective. This factor is even more elusive because the effectiveness of the psychotherapy seems to depend largely upon a lucky combination of the therapist's and the patient's idiosyncratic qualities. It requires a methodologic naïveté of the first order to expect meaningful answers in this field to come from statistical figures. The number of the independent pertinent variables determines the number of categories into which the whole sample must be broken down, in order to compare similar things. The greater the number of these categories, the greater is the required size of the total sample. In the case of psychotherapy, this may reach astronomical dimensions. Well, today that shouldn't be an obstacle. The correlations between this multiplicity of variables could be statistically handled by computers. We heard a very interesting paper about the use of computers a while back in the Academy. Dr. Jaffe, who uses computers, does remarkable things with them. But a meaningful coding of these variables can be hardly visualized today. Jaffe's very interesting procedure does not seem to have a meaningful coding system. Why? Because in every coding you must overlook certain differences. In the psychotherapeutic experiments, no two cases are similar, so it would be extremely difficult to code them meaningfully. One cannot help but overlook certain differences. And, not knowing the significance of the differences, one can't do meaningful coding.

Now, for example, if you put into the machine such variables as the theoretical persuasion of the therapist, the procedures that he actually uses, the nature of the case, the nature of the neurosis, its duration, and I don't know what else, but forget to put in the "lucky combination" between the therapist and the patient, comes meaningless. Fiedler maintains that the JT, Adlerian, Rogerian, or Freudian, has about the same—I don't state that it is so, but let the combination of the therapist's and the patient's idiosyncrasies be. It reason not to assume it—that the distribution is the same among all types of psychotherapists. There is no good reason to assume that the factors of the therapist can be matched less of in a therapeutically useful combination than the correlation statistical results among different factors by the parallel distribution of personality one should not try to evaluate the end; today it can only be done meaningfully by individual case, and by observing what evaluating it happened and why it happened, even with another psychoanalysis say, one jungian analysis with another junct personality factors may be different from freudian analyses.

This is only one example to show that similar cases in sufficient number effectiveness of one form of psychotherapy form of psychotherapy. What we can see is that this patient achieved a cure maybe beneficial, maybe nonbeneficial, by such and such factors. The chance put also into the calculating machine. More and more I've come to the factor has been undervalued to a very moment, meeting the right person might cure, and another chance event, but while prolongation of treatment or a failure of long, and an unfortunate event which really cannot be simply neglected.

The complexity of all the many factors turns out the way it does, is so great that, try to understand how we achieve these or how much the therapy did or, how much fortuitous event, these other fortuitous events, such as the
between the therapist and the patient, then your statistical result becomes meaningless. Fiedler maintains that any psychotherapy, jungian, adlerian, rogerian or freudian, has about the same results. Let us assume—I don’t state that it is so, but let us assume—that the lucky combination of the therapist’s and the patient’s idiosyncratic qualities is one of the powerful therapeutic factors. It is very probable—there is no reason not to assume it—that the distribution of such lucky combinations is the same among all types of psychotherapies.

There is no good reason to assume that among Jungians the personality of the therapist can be matched less often with the patient’s qualities in a therapeutically useful combination than among Freudsians. The parallel statistical results among different disciplines could be accounted for by the parallel distribution of personality factors. My point is not that one should not try to evaluate the end result of therapy. I say that today it can only be done meaningfully by treating every patient as an individual case, and by observing what happened to this patient and evaluating how it happened and why it happened. But you can’t compare it, even with another psychoanalysis, or you can’t compare, let us say, one Jungian analysis with another Jungian analysis, because important personality factors may be different between two Jungian and two Freudian analyses.

This is only one example to show that we are not able yet to match similar cases in sufficient number to compare statistically the effectiveness of one form of psychotherapy as compared with another form of psychotherapy. What we can say is merely that this physician with this patient achieved a certain change in the patient, maybe beneficial, maybe nonbeneficial, and that it was achieved by such and such factors. The chance factors, for example, must be put also into the calculating machine. Chance factors are terrifically important. More and more I’ve come to the conclusion that the chance factor has been undervalued to a very high degree. In the right moment, meeting the right person might be the difference between recovery, and another chance event, but which is not favorable, might mean prolongation of treatment or a failure of the treatment. Life is not so long, and an unfortunate event which retards recovery for a few years, cannot be simply neglected.

The complexity of all the many factors which determine why a case turns out the way it does, is so great that, at least at present, we should try to understand how we achieve those changes which came about, how much the therapy did it, how much fortuitous events did it, how much these other fortuitous events, such as the matching of the two personal-
ities, play a role in it. And there are still other variables and combinations of variables, of which we cannot even think now. We will learn them only by careful, systematic observation of what is going on.

"Now one more word about some of these criteria. I mentioned briefly that I think it may be possible, by using modern experimental methods, which psychologists are perfecting—for example those which I was talking about and which measure definite, concrete personality functions—that we may be able to substitute for the global evaluation, what the patient says, what he feels, what the relatives say, or general life performances; maybe we can replace these global criteria by more precise ones. And I think there are more and more intelligent methods. But Dr. Wolberg mentioned a very interesting criterion, the dreams of the patient. I agree with him that they are extremely valuable for the understanding of certain dynamic changes which have taken place in the personality, but again, without understanding very precisely the dynamics of dreaming, one cannot glibly draw conclusions.

Here is one example. I know of a group of patients, whom I observed during 30 years or more of practice. I was often very much impressed by the fact that these patients, when their behavior in life definitely improved—when they functioned better, had less symptoms—had weirder and weirder, more and more regressive dreams. It almost looked, on superficial inspection, as if they were schizophrenic. Now, why is that? The patient improves in life and behavior, and his dreams become more and more regressive primitive. The solution, I think, is rather simple. What takes place is that this patient has succeeded in excluding certain regressive trends from his behavior, but there are still these residues, which now have no other outlet. They could not influence the patient’s behavior, because, in that system which we call the conscious personality, which is a harmoniously integrated entity, they have no place anymore, and they have no other outlet but in dream life. Let us not forget that this is partially the natural function of the dream, the dream is the psychosis of the normal person. He gets rid of those things which he cannot get rid of in his interpersonal relationships in actual life. And this discrepancy between behavior and dreams indicates improvement. One should not generalize, however.

What is a desirable change? That is indeed a difficult question. I treated a middle-aged woman with arterial hypertension. That was why she came for treatment. Later, I met the husband. He turned to me and said, "You may have helped my wife's blood pressure, but she became a much more difficult person to live with." Was this an improvement or not? For the harmonious marriage she paid a pretty great price, increased arterial hypertension. But her treatment became much more complicated. Now if she had died very soon after that. I ask, what would I have done? Probably there would have been a suggestion in the woman, maybe a suicide. Use your best judgment in the consequence of this case. What is improvement? This is the first ratification, M.D.

On the second round I found myself much n Dr. Alexander, and this shows the evils of brevity have enough opportunity to expound his ful some. I do still agree with Dr. Zuimun, that study should not await a full or even partial understanding of life processes. We would all agree in thinking that these two k incompatible and in fact can go on simultaneously.

In our studies we are trying to separate the two. We may learn a good deal about process, if our study is semiautomatic. We hope to learn about process from the dreams. Each session what he thinks he was doing in the dream has been doing many things besides those he could not. The way we may miss those very important intangible features. And Dr. Wolberg mentioned. But still we will ha that these therapists say they are doing, and if we are undoubtedly doing. And then if we find out the outcomes of the patients belonging to different therapists or when he is using different techniques. What is something about process, at least to the extent that a particular technique (as reported by the one) a common denominator of the results we are to think we need to have any feeling of profound in the studies of outcome and of process.

Lewis R. Wolberg, M.D.

I find myself in agreement with both of my points. The effect that you cannot isolate process from outcome on the same continuum. After all, we are attempting therapy, and if we are unable to describe the process, then we may miss our goal. Here a number of value judgments are involved.
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price, increased arterial hypertension. But her marriage after treatment became much more complicated. Now for the husband's luck. He died very soon after that. I ask, what would have happened if he hadn't died? Probably there would have been a divorce, a deep depression in the woman, maybe a suicide. Use your poetic fantasy to figure out what could have been the consequence of this beautiful symptomatic care. What is improvement? This is the first thing we should know.

JAN STEVENSON, M.D.

On the second round I found myself much more in agreement with Dr. Alexander, and this shows the evils of brevity, because earlier he did not have enough opportunity to expound his full views. On the other hand, I do still agree with Dr. Zubin, that studies of outcome should not await a full or even partial understanding of the patient. And I think we would all agree in thinking that these two kinds of studies are not compatible and in fact can go on simultaneously.

In our studies we are trying to separate them but I think that we say learn a good deal about process, if our study works out satisfactorily. We hope to learn about process from having the therapist check after each session what he thinks he was doing in the session. Now he may have been doing many things besides those he checks, and undoubtedly we may miss those very important intangible features that Dr. Alexander and Dr. Wolberg mentioned. But still we will have a list of some things that these therapists say they are doing, and assuming their candor, we undoubtedly doing. And then if we find considerable differences in the outcomes of the patients belonging to different therapists, or to the same therapist when he is using different techniques, I think we will now something about process, at least to the extent of being able to say that a particular technique (as reported by the therapists) was or was not a common denominator of the results we are observing. So I do not think we need to have any feeling of profound incompatibility between the studies of outcome and of process.

LEWIS R. WOLBERG, M.D.

I find myself in agreement with both of my previous discussants, to the effect that you cannot isolate process from outcome. They are part of the same continuum. After all, we are attempting to evaluate psychotherapy, and if we are unable to describe the process through which we attempt a certain outcome, we may miss our goal.

Here a number of value judgments are involved, because what con-
stimates psychotherapy has not yet been dogmatized. We have certain notions as to what constitutes good psychotherapeutic process, but frequently we find ourselves in the position, in examining why a certain patient has improved, of concluding that the contact between the therapist and the patient was purely coincidental. I remember a patient who came to see me because he wanted me to stop him from smoking.

When I asked him about other problems, he denied having any. In the past he had, he revealed, been a schizophrenic; however, he had been cured by a psychiatrist. "How were you cured?" I inquired. He replied: "I had been to a number of doctors before, but they couldn't help me with talk. All this talk did no good. But this psychiatrist really did things that helped me." The doctor, he continued, asked him to strip off all his clothes at each interview, and to lie nude on the couch. He would then open the window, to cool off the room. The patient was forced to lie on the couch, enduring the cold without being permitted to utter a single word. The idea was that if he learned to tolerate suffering, this would produce a psychic callus and enable him to handle his inner conflicts and tensions. When I asked the patient: "What did the therapist do during this time?" he countered, "Well, he just sat there covered by a blanket."

When we attempt to evaluate what happened, we have to admit that this was no psychotherapy by any traditional standard. I do not doubt that certain things did happen to the patient. The significance to him of this situation probably had certain beneficial effects.

We know enough about psychotherapeutic process to realize that what goes on in psychotherapy requires the development of a relationship between the patient and therapist. The relationship is employed to understand some of the forces that are operating within the individual, to delineate the conflicts that are burdening the patient, to connect these with their genetic determinants, to understand and handle resistance and transference. We know that we can designate certain things that occur in good therapy.

When we try to assess any psychotherapy, we assume that the psychotherapist is sophisticated enough to understand how to utilize his techniques with a certain degree of expertness. If the therapist hasn't had a good deal of training and supervision, we are unable to evaluate the techniques he employs.

In evaluation we are dealing with tremendously complex problems. It is dangerous to oversimplify by adopting a limited criterion and generalizing from this. Take dreams for instance. We have to utilize and interpret dreams within the context of the total life adjustment of the dreamer. His dreams may be disturbed, but relationships with people may be adequate. Only from his dreams is not entirely apparent his symptoms. A patient may complete therapy in symptoms.

To say his treatment is a failure because one may find that, willy-nilly, the patient, while much better life adaptation. He is able to do with people. He is a much happier person a father, and under these circumstances, that perhaps he pays for his better adaptation, the chances, many of the problems are irremedial the best that can be expected. In another the retention of symptoms is a manifestation that he should have accomplished is a relief situation is one which is bountiful.

Another problem is the matter of controlling. It is possible to set up controls as in other kind to two patients are alike. Any attempt to match patients according to some criteria, or to age or milieu, but the errors in delimiting. I believe that every individual I that we need do is match the patient at what he was at the beginning of therapy.

JOSPEH ZUBIN, PH.D.

I think Dr. Alexander's compromise, that I agree to look at the process, may satisfy the think, however, that it will leave the audier because after all, they don't want us to be in situation really isn't as agreeable as it might be in this area. Let me raise one issue which p necessary to look into the process altogether, under might point to situations where we don't me, the outcome might be so obvious.

One situation which I think might be worth spontaneous improvement. Suppose we fin we do with the patients, some type of patieenc certain premorbidity characteristics, cert time of onset, and so on, get well, no matter
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It has been dramatized. We have certain therapeutic processes, but there is a position in examining whether a certain psychological contact between the therapy and the coincidental. I remember a patient who needed me to stop him from smoking.

He mentioned smoking problems, he denied having any. In schizophrenia, however, he had been a heavy smoker. "Are you cured?" I inquired. He said the doctors before, but they couldn't do anything. But this psychiatrist agreed. The doctor, he continued, asked him to turn off lights, and to lie nude on the couch, and cool off the room. The patient was used to the cold without being permitted to sweat, but if he learned to tolerate suffering, he could enable him to handle his inner conflict. The patient: "What did the therapist say?" The therapist: "Well, he just sat there.

What happened, we have to admit that there is no traditional standard. I do not doubt the patient. The significance to him lies in the beneficial effects.

Psychosomatic process to realize that there is the development of a relationship. T. relationship is employed to maintain operating within the individual, enlivening the patient, to connect these two. To understand and handle resistance and make designation certain things that occur in the psyche.

In psychosomatic therapy, we assume that the psychiatrist understands how to utilize his techniques. If the therapist hasn't done so, we are unable to evaluate the process. With tremendously complex problems, adopting a limited criterion and generalizing, for instance, is not adequate. We have to utilize all techniques for the total life adjustment of the patient.

A dreamer. His dreams may be disturbed, but his waking behavior and relationships with people may be adequate. To say he is disturbed only from his dreams is not entirely appropriate. Or we may take symptoms. A patient may complete therapy and still suffer from certain symptoms.

To say his treatment is a failure because of this is inaccurate. We may find that, willy-nilly, the patient, while not symptom-free, makes a much better life adaptation. He is able to develop better relationships with people. He is a much happier person all around. We might consider, under these circumstances, that perhaps his symptoms are the price he pays for his better adaptation, that under his present circumstances, many of the problems are irremediable. For him this outcome is the best that can be expected. In another patient we may feel that the retention of symptoms is a manifestation of a poor outcome. The fact that he should have accomplished is a relief of symptoms since his life situation is one which is bountiful.

Another problem is the matter of controls. I believe that it is impossible to set up controls as in other kinds of scientific experiments. In two patients are alike. Any attempt to match patients is fallacious. The may try to match patients according to so-called diagnostic categories, or to age or milieu, but the errors in this procedure are overwhelming. I believe that every individual has to be his own control. That we need to do is match the patient at the end of therapy against the way he was at the beginning of therapy.

JOSEPH ZUBIN, Ph.D.

I think Dr. Alexander's compromise, that he will look at the outcome of our treatment, may satisfy the question for the moment. However, that it will leave the audience a little bit disgruntled, because after all, they don't want us to be in full agreement when the question really isn't as agreeable as it might be. There are problems in this area. Let me raise one issue which perhaps might make it unnecessary to look into the process altogether, just as perhaps Dr. Alexander might point to situations where we don't have to examine the outcome, the outcome might be so obvious.

One situation which I think might be worthwhile considering at least, is spontaneous improvement. Suppose we find that regardless of what you do with the patients, some types of patients with certain characteristics, certain premorbid characteristics, certain characteristics at the time of onset, and so on, get well, no matter what happens. Wouldn't
it be nice to know that there are such people!

And wouldn't it be nice to say—when you get a patient of that type who doesn't get well—"what's the matter with your technique?" "Apparently, you interfered with the natural healing processes."

In this case, knowledge of the process would be important, but I don't think we need in every instance to know the full process. Ideally, science would like to know all there is to know about all the behavior that goes on, but I would say, from the point of view of the strategy of the moment, it may be that concentrating on outcome perhaps might be equally fruitful.

I'd like to raise another point—as a matter of fact, I didn't have Dr. Alexander and Dr. Stevenson and Dr. Wolberg in mind when I was talking about this matter. I had in mind the tremendous number of studies throughout the country, for which reel after reel of taped recordings and moving pictures are being collected without any rhyme or reason or focus as to what will be done with them. I suspect that a million years from now, when they dig up the ruins of our civilization, that will be the thing to remain, those untold tapes we've collected throughout our era.

As a matter of fact, I have been to places where people had to move out of offices which became filled with tapes, and had to move into new offices because there was no more room. It seems to me that there should be more planning, more focusing, in the collection of data.

I would say that focusing in your interviews on specific points, specific areas, is superior to having a free-floating interview in which you simply collect data for the future, because the amount of data collected gets to be ponderous as the years go by. Having a goal that you are aiming at at the moment when you are collecting data, and analyzing it out to see whether you hit the target, and how much you got of this particular content, may perhaps make it easier to get information, and may make the work of discovering what process means in therapy a little easier and a little quicker.

GEORGE M. LOTT, M.D., State College, Pa.

I want to ask questions concerning a method and a technique of teaching psychotherapy. Student therapists have to have some orienting impetus to carry them along so they will not become too confused.

Let us consider the therapy of a real obsessive-compulsive neurosis. Isn't it best to require the student to keep a verbatim record of the spontaneous associative material of his patient? This material is of great value, especially if key remarks are listed in sequence on the blackboard. Marginal notations can be made of the expressions of resentment, hostility, and guilt. In a series of interviews it usually is

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It is evident that hostility releases will be soon found of self-punishment, this process going on calming down with the expectation of it. We hope we have not picked a patient for psychotherapy, abreact violent or antisocially "touched up" during the therapeutic process. We techniques to pace the process so the patient.

A realization of this cyclic flow of events especially where prolonged therapy is required along with more confidence. His presence of faith which holds the patient in therapy.

A corollary of this method is, in the first instance, the spontaneously produced topic. Get a cue of what the patient is trying to say; then, by revealing the subject's "unconscious" like a parent speaks of a fight with a boy, then of mention of his father, of a severe professor, and his father again. From this the therapist can have a sibling rivalry, or oedipal conflicts, or both become to consciousness.

Have the members of the panel found the teaching and orienting tools? I have found seminar teaching, but do not have objective evaluations.

HANZ ALEXANDER, M.D.

My answer is a categorical yes. It is an answer I would go so far as to say that at the moment of substantial increase in knowledge of there it will demonstrate what we know about students. I am effectively using our records to listen to the record and then discuss the difference. For example, let us see what the effect of a next interview. We make predictions on the reasoning. For example, we expect now that anger, more guilt, more retreating, or turning feelings to the therapist, and will speak about a type of predictions can be checked by studying an excellent exercise in psychodynamic reasonin
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It is vital that hostility releases will be soon followed by a sense of guilt, and of self-punishment, this process going on until the intensity of feeling calm down with the expectation of improvement. We can hope that the occurrence of this cycle is an oversimplification. If we hope we have not picked a patient for psychotherapy who is going to explode, abreact violently or antisocially “act out” his symptoms too much during the therapeutic process. We all use our tranquilizing techniques to pace the process so the patient can stand it.

A realization of this cyclic flow of events reassures the student, especially where prolonged therapy is required, and enables him to carry on with more confidence. His presence can radiate understanding and faith which holds the patient in therapy.

A corollary of this method is, in the first interviews, to make marginal notes of the spontaneously produced topics. The therapist can often get a cue of what the patient is trying to express. The marginal notes reveal the subject’s “unconscious” like a telegraph code. The patient speaks of a fight with a boy, then of his brother, followed by mention of his father, of a severe professor, and finally comes back to his father again. From this the therapist can get cues that some phases of sibling rivalry, or oedipal conflicts, or both, are coming up, pressing some to consciousness.

Have the members of the panel found these procedures helpful as teaching and orienting tools? I have found them useful, especially in clinical teaching, but I do not have objective evaluations to support my findings.

MUNZ ALEXANDER, M.D.

My answer is a categorical yes. It is an excellent teaching tool. It would go so far as to say that at the moment I don’t know whether it substantially increase our knowledge of psychotherapy, but I am sure it will demonstrate what we know about psychotherapy to our students. I am effectively using our records in teaching my residents. I listen to the record and then discuss the different problems of therapy. For example, let us see what the effect of an interpretation was in the next interview. We make predictions on the basis of psychodynamic reasoning. For example, we expect now that the patient will show more anger, more guilt, more retreating, or turn away from expressing feelings to the therapist, and will speak about other persons. This type of predictions can be checked by studying subsequent records. It is an excellent exercise in psychodynamic reasoning for the students. My
answer is: Yes, these records constitute an excellent teaching device. Whether it is a good research device must still be seen.

THOMAS FRENCH, M.D., Chicago, Ill.

I must state an emotional bias in the beginning: I am interested in studying process, and I am not very much interested in studying evaluation. But perhaps it would be worthwhile if I state some of the reasons for my prejudice.

I think that evaluation, in so far as it can be effectively done, has obvious practical value. But it is essentially, almost by definition, a non-scientific procedure. You use the word evaluation; that means you are making a value judgment, and I don't see how a value judgment can have the objective precision that we would like to have for a real attempt to understand even the question whether one therapy is better than another.

I would like to indicate what seems to be involved if you try to understand what happens in therapy. Let's take a rather intensive case of therapy. Let's say the doctor has seen the patient for an hour or even two hours a day over a period of a year. Now you measure what has happened, that is, you compare his state of mind or his situation at the beginning and at the end. You try to obtain objective criteria for your value judgment (but still it is essentially a value judgment) that at the end of therapy there has been a considerable improvement. This is a value judgment, but still it can have a relative degree of objectivity. How was that improvement brought about? Thousands of things happened in those six months or that year of treatment. What did it?

If you really want to find out, you have to divide the therapy up into much shorter periods. Actually I have been very much interested in doing this for quite a while. However, you don't make divisions into fixed time periods. You will discover that, whatever change happened, happened in chapters. Something decisive may have happened, let's say, in the first three weeks. Something even more decisive may have occurred during the next chapter, lasting perhaps only one week, or it may take a few months before something else happens. But if you study these definite periods, you are going to learn a lot more about what your therapeutic method achieved. It may be, as Dr. Alexander indicated, that in some cases what happened was detrimental, but that is a value judgment.

The next statement that I would like to make is that, if you really want to understand what happens, you had better try to get rid of value judgments altogether. You won't say that the patient is better or worse, but precisely what the difference is by today and two weeks from today, or today at all. I will find that the changes in the patient are many more variables than you could have questions, the scientist introduces his own criteria into the equation that occurs in the patient. Different aspect of behavior than the one they are trying to judge what happened thinks of.

I also get another impression from this that may possibly take place in a patient situation, but, in a properly conducted therapy, changes concern one conflict at a time.

Let us assume that you try to find out which period and let us assume further that this hypothesis is a real chapter in the therapy. If it works correctly, you will find that there has been some focal area not a complex process.

The next chapter may be a change in another kind of study of therapy I think it is objective. You can actually tell what has been done, you think you can, will determine what has done, whether it is due to something that occurred outside of the result of the therapist's attitude or in response to the therapist did. That kind of question can be answered. But I don't see any possibility of evaluating at all, except for practical purposes, what you want to know is what happened, not evaluating. And that is something which cannot be done up into small sections, and then study.

I might add also—and this is a point on which I may not agree entirely—that I do not believe in the use of sound溪们 and so on and multiple ways to get this kind of an understanding. A verbatim record of the patient's words, with conspicuous gestures, tone of voice, and the like. The difficulty in trying to understand essentially not lack of data, but rather lack of data we have. By careful scrutiny of a relation to a great deal toward discovering exactly
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As it can be effectively done, has partially, almost by definition, a word evaluation; that means you can't see how a value judgment can be involved if you try to understand the patient for an hour or even a year. Now you measure what has been of mind or his situation at the time you obtain objective criteria for your judgment (a value judgment) that the degree of improvement is a relative degree of objectivity. Thousands of things happen in treatment. What did it have to do with the therapy up until that point? have been very much interested, however, you don't make divisions over that, whatever change happened decisive may have happened, something more decisive may have happened, perhaps only one week, or nothing else happens. But if you are going to learn a lot more about it. It may be, as Dr. Alexander pointed out this is detrimental, that the patient is better or worse, but precisely what the difference is between his behavior patterns today and two weeks from today, or today and two years ago. And you will find that the changes in the patient are much more complex, involve many more variables than you could have predicted. When he asks questions, the scientist introduces his own criteria. But it may happen that the change that occurs in the patient involves another, entirely different aspect of behavior than the one the therapist or the person who is trying to judge what happened thinks of asking about.

I also get another impression from this kind of study. The changes that may possibly take place in a patient may be of a very complex nature, but, in a properly conducted therapy at least, the really significant changes concern one conflict at a time. I call it the "focal conflict."

Let us assume that you try to find out what happened in a two-week period and let us assume further that this two-week period has been chosen as a real chapter in the therapy. If you have chosen your two weeks correctly, you will find that there has been a change which affects one focal area, not a complex process at all, just a simple change. The next chapter may be a change in an entirely different area. In this kind of study of therapy I think it is possible to become really objective. You can actually tell what has happened! And from internal evidence alone, I think you can, with considerable reliability, determine just what has done it, whether it happened spontaneously, or due to something that occurred outside. Whether it happened as a result of the therapist's attitude or in response to something the therapist did. That kind of question can be answered with a very considerable degree of objectivity.

But I don't see any possibility of evaluating—I don't believe in evaluating at all, except for practical purposes. For scientific purposes, what you want to know is what happened, and preferably without evaluating. And that is something which can be done if you divide your therapy up into small sections, and then study objectively what happened.

I might add also—and this is a point on which Dr. Alexander and I may not agree entirely—that I do not believe all the complicated apparatus of sound recorders and so on and multiple observers and so, is necessary to get this kind of understanding. You do need, preferably, a stabile record of the patient's words, with an occasional observation as conspicuous gestures, tone of voice, and the like, the patient may have said. The difficulty in trying to understand the therapeutic process is not lack of data, but rather lack of careful enough study of the data we have. By careful scrutiny of a relatively brief record, you can get a great deal toward discovering exactly what has happened in that
two-week or one-week chapter of psychotherapy, and then you can follow it through, step by step, for all the different chapters in a long treatment.

IAN STEVENSON, M.D.

I just want to say that Dr. French's comments illustrate well the difficulty in separating outcome studies and process studies, because, as I see it, a total improvement in outcome must be composed of one hundred or some other number of individual units of improvement. And what Dr. French is proposing we study is surely these smaller units. This is a kind of micro-outcome study, as I see it. He is observing some kind of changes that go on in one day or two weeks, and the smaller your unit of observation of the outcome, the closer you get to process. But I would still think this is one kind of outcome study. We are, in fact, also outlining a program of this sort in which we study changes in a patient within 24 hours of an interview. This is a kind of outcome study and also a process study.

LEWIS R. WOLBERG, M.D.

The comment I want to make relates to values. The complexities of social living are bound up with value judgments. A "good" marriage is a value judgment. "Happiness" is a value judgment. "Getting better" —the word better itself involves value judgments. We operate on the basis of values constantly, and there is no reason why we should try to avoid values in judging the results of psychotherapy. The important thing is the validity of the values we use. And that is the reason, I assume, for the organization of this panel. I believe that we are able now to define reasonable values we can use in appraising outcome. These are not as immutable as the law of gravity, but in the present time-space continuum, are reality oriented.

JOSEPH ZURIN, Ph.D.

I wonder whether the attack on the notion of values in the evaluation of outcome is entirely justified, because all science depends on value judgments. You look at an indicator, and you want to know, is it 0.95 or 0.96. That's a value judgment too. We get around the subjectivity involved by having a consensus. I see no difficulty in evaluation if you provide the objective criteria for evaluation and measure up your patient's performance against these criteria: for example, the kind of success he has on the job, or the kind of success he has in the family, or in the community. And, like Dr. Wolberg, I have no fear of being incriminated by being subjective, because there is no basic difference between

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The subjective and the objective approach to difference is that the subjectivist doesn't know he's got the real thing. The objectivist knows allowances for it!

One should realize, however, that the on Dr. French's view and mine is that he is into the time being, am interested in outcome. For interests and Dr. French and I may some day to our interests without losing an iota of scien

FRANZ ALEXANDER, M.D.

I am in complete agreement with Dr. Freud. I really compare this type of approach to the physical sciences, where natural events are units. That is the basis of calculus. The w is a total integral of these little units. You can understand what happens in these small ay more about this, which is a very interesting e operating with values. If you build a bridge, the theory of elasticity, you want to build a bridge that is very true. But the same physics can bridge collapsed and not only why the bridge i the laws of elasticity helps you to avoid a búe bridge. First comes a nonevaluative study of you can use that knowledge for good or bad. y can be used for advancement of human and I don't know what—utilization of power f can be used also for destruction. So it psychodynamics can be used for brainwashing a very legitimate point of view. The one is applied science.

Dr. French is interested in the basic science the therapeutic process, and my inclination is that it can be used for benefit in a really cont today—I am sorry to say—is a hit-or-mi for therapy which look very easy, but after better nor worse. Initially, these seem rela minor problems. Conversely, you n in a few months the patient recovers. The one case I had luck. In the other I didn't extremely unsatisfactory feeling not to know
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... and then you can follow part chapters in a long treatment.

Dr. French's view and mine is that he is interested in process and I, for the time being, am interested in outcome. Both of these are legitimate interests and Dr. French and I may some day change places with regard to our interests without losing an iota of scientific respectability.

FRANZ ALEXANDER, M.D.

I am in complete agreement with Dr. French about the small section study. I really compare this type of approach to a great advancement in the physical sciences, where natural events are studied in infinitesimal units. That is the basis of calculus. The whole treatment is a complicated integral of these little units. You can understand the process only by understanding what happens in these small sections. I don't want to say more about this, which is a very interesting methodological issue. We are operating with values. If you build a bridge, with the help of physics, the theory of elasticity, you want to build a bridge which will be stable. That is very true. But the same physics can be used to explain why the bridge collapsed and not only why the bridge is good. So the knowledge of the laws of elasticity helps you to avoid a bad bridge and build a good bridge. First comes a nonevaluative study of what is taking place, then we can use that knowledge for good or bad. We know that atomic physics can be used for advancement of human comfort, saving labor, and I don't know what—utilization of power for constructive purposes—it can be used also for destruction. So it is with psychodynamics. Psychodynamics can be used for brainwashing and psychotherapy. This is a very legitimate point of view. The one is a basic science; the other applied science.

Dr. French is interested in the basic science of the psychodynamics of the therapeutic process, and my inclination is also that this comes first before it can be used for benefit in a really controlled way. What we are trying today—I am sorry to say—is a hit-or-miss procedure. We accept cases for therapy which look very easy, but after six years the patients are either better nor worse. Initially, these seemed to be easy cases with relatively minor problems. Conversely, you may get a desperate case, and in a few months the patient recovers. The therapist may say, "Well, the one case I had luck. In the other I didn’t have luck." But it is an extremely unsatisfactory feeling not to know why you fail or succeed.

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You don't control the phenomenon with which you are dealing. And maybe it is because we felt so helpless, that French and I have spent years and years to figure out the dynamics of treatment, to make it more than a haphazard procedure where the independent variables defeat us. We wanted to know more, so that we can control these variables, and cure, not only by intuition and luck, but also by calculated and methodical procedure. For this we must know what is going on, what are the basic principles in this complex human interaction between two persons which we call psychotherapy.

George Huthsteiner, M.D., Los Angeles, Calif.

I think we might start with the here and now, to evaluate therapy on the basis of how we can learn to evaluate a conference, even one like this, a discussion, or a panel. I think that most of us, if you are like me, have a lot of stimuli that are not tape recorded, and they are really at the bottom of things. And that is one of the big problems in psychotherapy. There are a lot of stimuli that are never observed, even through a one-way mirror, that cannot be picked up even by the best video tape recorder. But, as I think everybody can vouch for right here, these stimuli are most important in determining how you feel.

If we can find a way to detect not only nonverbal, nonsemantic communication—and I don't mean just tactile communication, because there is a wide range of types of communication—if we can learn to study this problem more accurately, not only in the patient, but in the therapist as well, then I think we will start to make real headway. And I would like to ask Dr. Zubin what he means by "normal." I would say that one of the first things I realize is that I am not "normal." And I don't believe he thinks he is either.

Joseph Zubin, Ph.D.

Well, the 16 per cent of New Yorkers which Midtown declared normal are normal.