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The 100 Minute Hour*†

I BEGAN WORK WITH LSD as a facilitating agent in psychoanalysis in 1959. The work was exciting and interesting and led me to present a paper before this society.[1] This was soon followed by another to the Academy of Psychoanalysis.[2] These papers fell upon the psychoanalytic community with a dull thud. Reprint requests were roughly zero, but I had the good fortune to have my Academy paper discussed by Max Fink.

After a number of comments on my assertions as to what LSD would do, Dr. Fink noted that certain elements I had mentioned were testable by psycholinguistic analysis. He went on to say, "Dr. Dahlberg asserts that the effect of these changes in verbalizations is salutary. This is a most tenuous conclusion, since neither the criteria of efficacy, nor the methods of evaluation, nor the relation to control data are presented. There are at least two major changes in this treatment—the interpolation of a drug, and the extension of sessions . . . to many hours. Perhaps this latter element alone may be as potent in altering behavior as the administration of the drug." He then congratulated me on my enthusiasm and commended the techniques of experimental psychiatry to my attention.

In a later publication,[3] I cleverly answered one of Fink's points by saying, "The extension of time of the analytic session has been singled out as possibly accounting for some of the effects. However,

* Presidential address to the William Alanson White Psychoanalytic Society, May 17, 1967.

† The research reported was supported in part by PHS Grant # MH-11670-02 from the National Institute for Mental Health to the William Alanson White Institute, New York City.

early analysts who worked with variations in time and frequency did not find that prolonging the session gave them results comparable to these, so this does not seem to be a serious criticism."

Since then I have begun working with a research team, have subjected my findings to rigorous analysis by disinterested persons within the team, have utilized some of Fink's suggestions, and have been considerably humbled.

This paper will not deal with the specific effects of LSD upon psychoanalysis because our project has not progressed sufficiently for me to say anything of consequence. I shall, however, have certain things to say about changes in the life of a psychoanalyst when he turns to research, and also some observations upon psychoanalytic technique as well as psychoanalytic research which have come up in the course of this project. The title of the paper, of course, refers to the fact that Fink was right. A change in the length of the session changes the nature of the session remarkably with some patients. I shall make a number of comments on this point during the course of this paper. In addition, I would like to commend you to the rigorous examination of your own therapeutic techniques, particularly toward the point of view of examining under what circumstances time adjustments can be made and how their usefulness can be predicted.

A colleague suggested "Serendipity" as a subtitle to this paper and, though it would be appropriate, I rejected it on the grounds that he had a penchant for sesquipedalianisms, which are unnatural to me. At any rate, this paper will deal with the late introduction of a practicing psychoanalyst into the life of research, what he has learned outside of the strict hypotheses of his research project, what it has taught him about working with colleagues, and the personal rewards he has reaped from this work.

The double session, or 100 minute hour, has turned out to be particularly interesting with retarded, depressed patients. In this day when everyone is trying to abbreviate psychotherapy, I may go down in history as the man who lengthened it.

I have often had the experience of spending an hour with a depressed, uncommunicative patient only to find that the last 5 or 10 minutes of the session were productive. On the patient's return to his next regularly scheduled hour, it was as if nothing had happened; the session was essentially a repetition of the preceding one. With the 100 minute hour (allowing a 10 minute break in the middle),

such a patient has frequently been able to use his second 50 minutes highly productively and make real progress.

When I was an analytic candidate, one of my teachers told me she thought that the actual length of a session was not important. "If a patient has something to say, he'll say it." This is distinctly not true in some cases. There are people who need an extended contact before they can come out of their withdrawal and be able to know and tell you what is going on. It is surprising how patients look forward to these longer sessions. I think they feel a specialness and extra warmth, such as a child feels when he gets extra time alone with a parent. These are combined with a pressure to produce and make use of the time while knowing the conditions are propitious.

It was Ferenczi, I believe, who, when seeing patients five times a week, spoke of the "Monday crust" which formed over the weekend. This crust can often be softened in the first 40 or 50 minutes of the 100 minute hour. Judicious use of this alteration in technique can break through stubborn defenses, if not used too often; the opposite can occur with a generally unproductive patient, who may become even more depressed by his exaggerated inability to produce.

In the normal course of psychotherapy, patients become conditioned pretty rapidly to the 45 or 50 minute session, and they consciously or unconsciously prepare for this amount of time. As the session nears its close, the patient usually knows it, whether he looks at his watch or not; and a similar readiness to end is communicated by the movements and possibly vocal gestures of the analyst. Patients have told me that they feel disappointment at the approach of the end of a session, but that this is mixed with a sense of relief and release if the work is hard and laden with anxiety. "To return to the swamp again," as one patient put it on beginning the second 50 minutes, "is difficult"; yet, one can explore new ground since the prepared session has been passed and the patient is thrown on his own. On a few occasions, when the patient seemed ripe for it and I had the time available, I have suggested the second hour on the spot and not scheduled it in advance. With a cooperative patient at a time of transferential ambivalence, I have found this to be of great value. One must be cautious in using this approach, as defenses that are not ready to be given up may be breached, and the subsequent rebuilt defense may be stronger. The scheduled 100 minute hour permits the patient to be better defended but, if not used too often, still allows a breakthrough.

Patients occasionally ask for an extended session, and this is an entirely different matter. A busy schedule infrequently allows it, but I am sure we have all accommodated such a request at a time of crisis. If the patient asks for it in advance, it can be much the same as when the analyst requests it, although I think it is highly unlikely that a patient will make such a request unless his analyst has previously brought it up as a possibility. I shall not go into the pros and cons of satisfying dependency needs with the 100 minute hour, because I think it is both too obvious and too individual to be dealt with here.

One patient discovered that the analytic relationship produced an intimacy that was completely unfamiliar to her, and therefore caused anxiety and distance. In a session before a long one, she said that she was in a cocoon. She told me that she was clinging to the 100 minute hours because she could not isolate the 50 minutes, as she usually did, and therefore the analysis became more a part of her life—richer and more fulfilling. It was true that in the longer session she was much closer to her feelings, which she expressed fully but undramatically. She had been quite productive in the hours leading up to one of these longer sessions but had been waiting to get into the essence of the qualitative changes in her mode of relating to me and other father surrogates. She was able to define this as now trying to satisfy *herself* rather than pleasing others so as to make them like her, and she experienced me as a part of herself. It would be ridiculous to imply that this could not have occurred without the long session; however, I am convinced that the longer session brought her feelings of unity into focus.

There have been similar reports, and I have since used the double session profitably with patients outside of the research project itself.

Since our project lasts about a year and a half for each patient and is connected with the hottest thing around—LSD—, it was to be expected that the specialness of being in a research project would have an effect. Indeed it did. Initially, patients experienced excitement and pride in being chosen and "in on the beginning" of what they felt might turn out to be quite an important advance. I shared these feelings, of course, and had my own grandiose fantasies. Soon, however, a second response set in. Most of the participants felt I was in danger of losing interest in them, that I was primarily interested in the project. While such feelings grew out of previous experiences

with indifferent and rejecting parents, there was enough semblance of reality in their present situation to make this a potentially serious problem. Reassurance that what we were trying to do was to facilitate the analysis, and that this was still the central focus, served to resolve these doubts and turn this material to analytic usefulness, especially when subsequent events in the analysis corroborated my sincerity. I think that the longer sessions and other evidences of concern for the patient's welfare also assisted.

The old saw that "everything is grist for the mill" exemplifies how the right thing can happen for the wrong reason. One of my patients, with many years of ineffectual analysis, had, among his other controlling, passive-aggressive maneuvers, chronic lateness for analytic sessions and almost everything else. My efforts to influence this pattern were as fruitless as those of his previous analysts.

Since we tape many sessions (and these must be full sessions to conform to the research design), I was being castigated by my colleagues for failure to get filled tapes. I hesitated to say anything about this to my patient for fear of giving him a weapon over me, but I eventually did so. He responded by coming promptly for all sessions, taped or not. Later he told me that, in addition to his usual lateness, the analytic tardiness had been compounded by hopelessness. "Thirty minutes of nothing in analysis was no different than 50 minutes of nothing." This had changed when I emphasized my research interest: at least the research was worthwhile. His fantasies of disappointing people emerged, in particular, one of being in an accident on the way to my office, thereby frustrating the entire research team. He thought this was funny, but for the first time he brought out in the analysis his hopeless relationship with his bitter, isolated, selfish father. I had reversed the ordinary analytic situation with him by expressing my interest in my own work. He could cooperate with my work, whereas he could not do something useful for himself since he had no faith in any man being responsive to his needs. He has worked harder and with some gain, though it is much too early to say how profitable that will be for him. At least he turns up on time and makes greater efforts than before.

The simple fact of being in a research project affects both patient and analyst—the latter probably more profoundly, but the patient is certainly affected. Food and Drug Administration's "informed consent" rule—that the subject must be told what is to be done to him—

has caused great consternation among researchers. It has been said that putting this rule into effect will signal the end of research on human subjects.

Fortuitously, we anticipated the "informed consent" problem. Because we were primarily concerned with the treatment of the patients, it was important that they be fully cooperative and aware of what was going on. We were also concerned about what might happen to us if something went wrong, since we were dealing with an experimental drug. Without revealing the experimental design. I briefly told each patient about the drug and my experience with it. Then I did so in writing and requested that a witnessed release be signed by the patient and also by the spouse, or by the parent in the case of a minor. The release read, in part:

> . . . in consideration of your making such use of the drug and recordings I hereby remise, release and forever discharge you and your heirs, executors and administrators from any and all causes or actions, claims and damages whatsoever I or my heirs, executors, administrators or personal representatives ever had, now have or may hereafter have against you or them by reason of your use of Lysergic Acid Diethylamide and recordings in connection with my psychoanalysis.

In addition, we asked the patients to fill out a questionnaire. They were asked if they had ever had adverse reactions to drugs, had taken certain drugs, or had suffered from eczema, asthma, hay fever, high blood pressure, heart disease, liver or kidney ailments, diabetes, chronic eye disease, tuberculosis, or epilepsy. What was sought was information that might be relevant to dose levels or rule out certain patients for whom the drug might be hazardous.

One might think that the release and medical questionnaire would frighten patients away. This was not the case. These patients, whom I knew well (and I shall say more about this when I deal with the "research alliance"), generally considered these documents to be evidence of our seriousness, not only in the research itself but in all aspects of work—including their psychoanalysis. Does this contradict what I said earlier about patients thinking that the research took priority over the analysis? I think not. I believe that the reason patients were so easily convinced that they came first was that they had already become convinced I was not "holding out" and not attempting to put anything over on them. The one thing that is hardest to convince patients of is that I don't know how much LSD they are getting on any given occasion. However, since it is so obviously true,

they believe it after a few sessions. I really *don't* know; therefore I behave quite convincingly.

I am not suggesting that these techniques will always work; but I think that sincerity of purpose and conviction about the validity of one's work, when coupled with adequate attention to the patients' welfare, will produce sufficient numbers of experimental subjects. There are psychiatrists in hospitals who can get patient volunteers for projects, and others who cannot. The psychiatrist's attitude is the important variable in these cases and, if a conscientious attitude is to be successfully communicated, there is no substitute (as any analyst knows) for time spent with the patient.

Two examples of how patients behave before the LSD session may be of interest. One young man, who was quite successful in his career, nevertheless stalled about taking certain steps that were, in effect, commitments to his work. He misplaced documents, neglected to fill them out for months on end, and even forgot to mail them for weeks. This happened with respect to two separate matters while he was in analysis. Some months passed before this problem came into the analysis; even after its discussion, no action occurred until just prior to an experimental session. As yet I am not fully aware of the dynamics involved, but I think that it has something to do with the committing aspects of these sessions, which made him more serious about himself.

The second example, somewhat different, concerns a person with a very low self-regard who was exceptionally cut off from his feelings and quite bland about taking LSD. He is one of those people who usually pays his bills late. After some time, he noticed what I had not—that he always paid what he owed just before his experimental session. This brought out an unconscious fantasy that he might die from LSD, and that I was the only creditor to be paid. I believe that the death he was afraid of was the death of his rigid defensive armor; he wanted me to be paid up and on his side when this took place.

Having one's analysis recorded when you know technicians are going to listen to the tapes and turn everything into little square holes on IBM cards can be inhibiting. Studies show that simple recording doesn't interfere much with most patients but frequently bothers the therapist. Since we will have about 900 tapes in our project, I rapidly decided that I could not afford the luxury of being sensitive about living in a goldfish bowl. I was greatly helped by my team-mates, who

were very supportive. We have gone to great lengths to preserve anonymity and juggle session and patient sequence to confound the technicians, but we are still concerned about patient response. The first group of patients we worked with were uniformly cooperative and unconcerned about being recorded. One, who had something more to say after I had turned off the recorder, asked me to turn the machine on again so that her last words would be on the tape.

I have requested that patients not use last names when talking about companions so as to avoid identification. During an initial period of testing our equipment, one patient mentioned surnames and bedroom secrets of a number of people in her life. It turned out that she was acting out hostile fantasies of revenge by telling on them. Interestingly, this behavior was not repeated. My clear impression is that even those who are initially conscious of the microphone do not censor themselves because of it. This is confirmed by the fact that they may strike it with a gesturing hand, apparently having forgotten its presence.

Two patients have commented on the way I behave while being recorded. One asserts that I say more for the listening audience, the other that I say less. An interesting subtlety was revealed by the first patient. He said that he had on several occasions noted my garrulousness while being recorded, but that it usually came clearly to mind only when he was at home. Since there were many unrecorded sessions when he could have said something about it, it seemed significant that he did so when we were taping and was therefore likely to embarrass me. I have not noted any deliberate use of the recorded session to act out aggressive impulses against the analyst; but there is no doubt that it can be an instrument for aggression as well as revelation and embarrassment. This patient, I should add, is not one who conceals his embarrassing moments.

We have a pattern for the analyst to listen to the tapes. I listen to "productive" and "unproductive" sessions indiscriminately, and compare my impressions with previously recorded notes. I shall speak later of the varieties of disappointment that I have noticed in patients, so one example will suffice at this point.

A woman droned on and on in a voice which grew softer and flatter during the second hour. She called the session dull and disappointing, and I silently agreed. When I listened to the tape shortly thereafter, I was startled to hear that she had spoken early in the ses-

sion about what she called the turning point in her life—when she had shifted from living in reality to fantasy and had stopped *caring* about people. She had become passive and depended upon fate to manage for her. She also expressed the most clear, sustained anger towards her father that I could recall ever having heard from her, as well as brutal, violent fantasies about the governesses who had raised her. Her later dullness was interpreted by her as disappointment at what she considered to be a minimal drug effect; but it was actually a reactive depression, which came out so strongly that *both* of us forgot the earlier communications until I listened to the tape. I have since wondered how often a patient's emotional state has succeeded in inducing this kind of amnesia in me.

I would like to see some controlled studies done on this phenomenon, particularly from the point of view of how it affects therapy. It is clear to me that the kind of "empathy" I exhibited with this patient would have adversely affected her analysis if I had not heard the tape. How many other such sessions escaped my attention I have no way of knowing. Perhaps rigorously scrutinized recordings of analytic sessions could reveal something to us about the therapeutic limitations of empathy. In this case, response to her emotional communications overrode the content of what she had said. Is it possible to understand this more fully and deal with it adequately through teachable techniques?

Greenson[6] has defined the "working alliance" as a part of the patient's desire to emulate and learn from the analyst. This desire is fostered by the analyst's attempt to understand all that goes on in the patient, thereby stirring up the patient's curiosity about himself. It is the "working alliance" which helps carry the patient through apparently unproductive or hostile periods in the analysis.

Offer and Sabshin[10] have recently compared the "research alliance" with the "therapeutic alliance." I think that their therapeutic alliance is mostly Greenson's working alliance. The research alliance, they say, must include trust and respect, stimulate the intellectual process, and permit the subject to satisfy some curiosity about himself. Motivation can be stimulated by encouraging a feeling that the subject is helping others. Since Offer and Sabshin are dealing with "normal" control subjects, they emphasize that few psychodynamic interpretations should be made.

They find that three main difficulties may arise in the subject-re-

searcher interactions: (1) *excessive detachment* which, in the name of scientific objectivity, may create too much distance and thereby result in a lack of trust; (2) *seduction,* in which the researcher, in order to engage cooperation, promises help that he cannot deliver, thereby causing disappointment and bitterness; (3) *resistance,* in which case negative feelings are overlooked, not explored, and therefore allowed to interfere with the subject's interest in the relationship. It must not be forgotten that the researcher asks the subject for cooperation and help, which is the reverse of the therapeutic situation.

In our project we are using patients who are paying for their analysis, both as objects of the study and as controls, so that our problems are somewhat more complicated than those outlined above. The patients develop great interest in the study and are frequently eager to learn our results, hypotheses, and how their own reactions compare with those of other patients. Inasmuch as I am not privy to all the details of the psychological work, I have had to be silent. My expressed attitude—"we'll have to wait and see"—is convincing and not felt as a rejection. It seems to induce in the patients a similar professional detachment, which is encouraged by my promise to let them in on the study when it is completed. Their curiosity should help us with our follow-up.

While we promise nothing from LSD, there is no doubt an implied promise because of the publicity that the drug has received and the mere fact that we consider it worthwhile to invest so much time and money in the work. Although I make no attempt to deny my hopes, the attitude that "if we knew the answer we wouldn't be doing the research" tends to minimize the promise of something we can't deliver. Nevertheless, we do have to deal with disappointments although, as yet, not with serious bitterness.

The resistance problems of which Offer and Sabshin speak are obviated by our subjects being patients; we have the usual analytic means of dealing with their defenses. There is another problem relevant to the special problems of psychoanalytic longitudinal case studies; that is, a stable patient population is needed. The investment in the patient is so great, and the number to be studied is so small, that we can ill afford to lose a half-completed case. We were not too worried that our patients might get well too quickly and leave, but we did have to consider whether they were likely to leave town, get pregnant, and so forth. (Teratogenic studies have not been done with

LSD.) These matters were easier to assess with patients already in treatment when we started the research, but new patients could be accepted only tentatively for evaluation. One person with an exceptionally obstructive, passive-aggressive personality was turned away after a number of incidents had convinced us that he was unlikely to be able to cooperate even minimally, much less collaborate over a period of 18 months.

It is this attitude of collaboration that we feel we have been particularly successful in getting. I have already mentioned the patient who wanted the recorder turned on again because she had more to say. There are many examples of patients who arrange business trips so as to conform to the recording date. This has been done without my making a special request. The patients figure out our recording schedule and, after a few sessions, enter a collaborative relationship.

Some of this may be due to positive transference, but this factor is fluctuating and undependable. I am particularly impressed with the collaborative attitude of these patients. They seem to feel that they are engaged with me in an important enterprise which is not only their analysis but also the research. As with research subjects generally, they are paid. The payment is the drug and the additional free time I give them during experimental sessions. This is explicitly stated in the release as the "consideration" for which I am freed from responsibility for ill effects and allowed to record and use their communications for research.

It is possible to produce an LSD effect in almost anyone with a large enough dose. We, however, are studying the effects of small doses and, especially, how they affect verbal communication. Since individual response to LSD varies even in the same person because of several unknown factors, we have been able to observe many different aspects of disappointment. To fully appreciate this, it is necessary to realize that the patients must devote an entire afternoon and evening to each experimental session; therefore, they expect something to happen. When they do not notice a change, they are naturally disappointed and sometimes also relieved. I have spoken of the patient who reacted with such depression and withdrawal that we both forgot the earlier, very important material. She was quite apprehensive about taking the drug and, in her first sessions, seemed to have no response. She experienced these times with great relief, as free time during which she had no calls upon her from an ordi-

narily hectic life. This guilt-free state could not last, but did serve to let her know what equanimity could feel like.

A common experience among these middle-class progeny of striving parents is the fear that they will disappoint me and that I will reject them if something dramatic doesn't happen. Not infrequently, the hostility that is evoked expresses itself in subtle ways. Acting out, through infractions of the simple rules we have had to make governing the patient's activities when he leaves the office, or through excessive silence, is the usual expression. Other patients try harder, and the most common response to disappointment has been "better luck next time." Since this is an experience of shared disappointment, it is often a new experience; and the feeling of disappointing me can become a new aspect of the collaboration and thereby contribute to maturation.

I had decided not to speak about LSD-facilitated psychotherapy before preparing this talk, but in the process changed my mind. The LSD-type drugs have become controversial and have been used illicitly, I am afraid, by some therapists as last-ditch attempts to do something with resistant patients, so I decided that something must be said. What I shall say will, I hope, be relevant to all research in psychotherapy.

According to Luborsky and Strupp,[8] the ideal attitude for the researcher in psychotherapy should be, "Work as though everything depended upon it, but don't care about the result." Psychoanalytic training assists one to adopt this attitude.

This injunction is particularly true in any pharmacological research which is not merely a matter of giving pills and placebos to ward patients. The so called "placebo" or "psychotherapeutic" effects, which are so difficult to define, measure, confirm, or verify, cannot, as any psychotherapist knows, merely be disregarded as random variables. It is exactly at this point that the longitudinal (or idiographic) study becomes crucial to psychopharmacological research. The longitudinal study offers the possibility of distinguishing between drug-specific effects and patient or therapist-specific effects.

For example, a patient early in her psychoanalysis responded to small doses of LSD with feelings of physical movement towards her analyst. As this felt intimacy was becoming an integral part of her personality, she experienced the drug-induced distortions of her tactile sensations as dirtiness and tried to wash herself clean in the ana-

lyst's bathroom. This was followed by months of the depressive symptoms and the kind of regressive behavior (though more exaggerated) that had brought her into therapy; but it was accompanied by the psychotherapeutically elicited intimacy that had been lacking in her childhood experiences in her family. A period of maturation followed in which a relatively mature love relationship with orgiastic potency developed. Given during this latter period, LSD produced the usual symptoms but little or nothing in the way of important perceptual distortions or special insights because (I presume) she was developing psychological autonomy.

This case shows the necessity of knowing where the patient is in his development if one is to assess the effect of a drug or any other agent. I know of no other way of doing this than by seeing a patient frequently over a long period of time. A double-blind design, objective clinical notations, and well-designed psychological instruments confirm or confound the findings of the longitudinal study. These latter modalities are particularly important, since it is as ridiculous to expect a therapist to be so neutral and objective as it is to disregard his observations. The conclusions of the participant-observer are simply not enough if a study is to be convincing. Therapeutic bias is a fact of life, as I think I have shown in the example where I was amnesic for my patient's early communications as well as in my more optimistic observations of patient behavior.

What we are trying to do in our study[9] is observe process while not neglecting outcome. Process is the more difficult but more fruitful study. The LSD-type drugs lend themselves to process studies because of the dramatic, short-lived effects they produce; but non-drug process studies can also be designed. It is a misfortune of psychoanalytic research that controlled process studies are so rare and, indeed, that so little has been done along these lines. However, I am pleased to report that process observations are bearing fruit in developmental psychology.

The surprise or novelty effect of LSD, along with its apparent enhancement of suggestion, make the factors I have just noted especially important to consider. Are there really effects that facilitate psychoanalysis? Does the novelty merely impress the patient (and therapist) the first few sessions? Is the suggestibility countered by familiarity and suggestibility in general? These are questions which the case I have outlined above hint at.

I shall conclude my remarks with some observations on what happens to the middle-aged clinician when he turns to research. Briefly, I can report delight, frustration, confusion, exhilaration, boredom, exhaustion, apprehension, and excitement.

Psychoanalysis is an isolating profession, and we all search for ways to alleviate our insularity and increase communication with colleagues. We form organizations such as this one, where we can exchange ideas and renew social contacts over food and drink. We get on hospital staffs to have an interchange with persons having different clinical approaches and to keep in touch with a variety of case material. We teach, lecture, get involved in community activities, and read the literature, while always devoting our principal efforts to what Freud[4] called an "impossible" profession—intensive psychoanalytic work with individual patients.

I am sure I have not exhausted the range of diversity of our solutions to the problem of isolation, but I will confine myself now to my particular solution. To my delight, when Joe Jaffe and I went to NIMH to talk about our project, I discovered that they were almost as happy at our meeting as I was. Research psychiatry has become a subspecialty, and a clinical psychoanalyst who wanted to work with experimental psychologists and professional psychiatric researchers was a welcome find. Our association with NIMH has been instructive, helpful, encouraging, and remunerative.

After the initial anxiety of having to produce something, once we were awarded our grant, we set to work. My clinical experience has been based on the axiom "strike while the iron is hot." This can be translated into "treat your patient when he is motivated" and "interpret when he is ready to listen." In research I learned: "Test your instruments before you start to use them"; and "Be sure you know what you are trying to discover before you start gathering data." In retrospect, I think there is not much difference between the two approaches; it is largely a matter of different disciplines confronting the same problems. But at first my frustration-tolerance was sorely tested by the problems of instrumentation and experimental rigor and what seemed to be intolerable delay.

To the clinician who feels he must break through his isolation, the interpersonal tensions of working with a team can be a bit more than he asked for. They were, however, compensated for by the exhilaration of finding that there was a reputable idiographic approach to the single case study wherein repeated observations of the same subject

could be treated as respectably as single observations on a large number of subjects. There was also the excitement (for me) of being introduced to new techniques, which led to greater understanding of my psychotherapeutic processes and held promise of being useful to my patients.

At a research workshop of the Academy of Psychoanalysis, I said that Stanley Feldstein, my co-principal investigator, kept me honest and I kept him human. I don't know how he feels about this but, as far as I am concerned, this witticism has turned out to be quite true. The necessity of recording observations rather than relying on memory has taught me what I previously only knew from theory.

Erich Fromm[5] has recently said that "psychoanalysts have failed to develop a sufficient body of research." The experimental psychologist, he went on to say, still follows an old-fashioned model of scientific research: "... he experiments, and the experiment either 'verifies' or 'disproves' the hypothesis. This kind of thinking... denies the validity of all theories which are not thus 'proved' or their opposite 'disproved'."

Fromm suggests rather that the "proof" of the hypothesis is better thought of as referring to its "plausibility" and that the main emphases should lie on "the force of theoretical thinking, controlled imaginativeness and inference making. The experiment is one step in the development of theoretical thought—not the end of the line indicating truth or falsehood."

He then states that the methods of experimental psychology do not lend themselves to psychoanalysis because we are dealing with unconscious forces which "must be inferred from manifest phenomena and which are strongly influenced by the experimental situation itself" (as if all experiments did not influence the experimental phenomena). The psychoanalyst, Fromm says, "has only the alternative between being lax in his scientific criteria, or of finding a scientific method which fits his data . . ."—implying that we must eschew the methods of experimental psychology.

While I agree with much of what Fromm says, I think it is a mistake to disregard the solid work of experimental psychology, particularly with reference to methodology, instrumentation, and analysis of data. Jaffe, Dahlberg and Feldstein,[7] in discussing multi-disciplinary research, have stated:

> ... the systematic phase of research ... usually involves the therapist in interaction with a variety of other specialty disciplines with whom he

must share control and whose techniques he must necessarily respect and adapt to if the research is to succeed. This becomes especially interesting when there is a mixture of participants from the clinical, so-called "soft" approaches, and others from the more statistical, or "hard" approaches to research. The latter are, of course, concerned with questions of controls, experimental design, and statistical evaluation. Such concerns must at many junctures clash with the traditionally understood therapeutic attitude. It is important for would-be clinical researchers to realize that the "hard" approaches to their problems are lifetime specialties in themselves, and that there now exists a considerable corps of such specialists who are conversant with and extremely sympathetic to the psychotherapeutic enterprise. It is surprising how many therapists will muddle along with ill-conceived or insufficiently informed research enterprises without realizing that a simple paid consultation is available in many quarters for aid in more rigorous formulation of their ideas.

... it has been our experience that the more rigorous disciplines, although fully aware of the realities of clinical research, properly adopt an excessive posture of scientific precision with the intention of compromising these where absolutely necessary. This should not be seen as a "holier-than-thou" attitude, nor as condescension. It often serves to provoke the therapist's examination of which aspects of his technique are absolutely necessary to the therapeutic enterprise as opposed to those which may bear modification, if necessary, in order to further the research goals. An attitude of mutual respect is salutary. This can only occur when the group works together long enough to become relatively undefensive with each other. For example, the necessity for controls may often require the therapist to have less knowledge of particular aspects of the research than his colleagues.

Specifically, this last statement refers to the kind of psychopharmacological-psycholinguistic study that we are doing, but it may equally refer to other psychoanalytic research.

I think it is time that psychoanalysts renounce their chauvinism and accept, in a spirit of scientific collaboration, the discipline that related fields can offer.

Of course, the work I am speaking of is hard, frequently tedious, and often exhausting. One decides what must be done to gather relevant, replicable data, and then one has to do it. As a college friend of mine said about religion, "It interferes with your social life."

Perhaps what is most important is the element of novelty or surprise. One learns many things from every patient, but when one tries to fit them into a research pattern one learns more. One also

learns what is going on in other fields, and that all committee work is not group psychotherapy. I think that for a group to work together, it must have frequent regular meetings. Our group meets for two hours weekly. This time is spent not only in dealing with specific questions related to the study but in teaching me techniques of psycholinguistics and statistical analysis. I hope I teach them something, too. I cannot hope to master these new fields without spending more time than I am willing to devote, but I can at least become familiar with the operations. Not to be overlooked are the interpersonal processes in the group. Hope, competition, respect, and envy are combined with devotion to a single goal (our project) transcending these anxiety-producing emotions. It would be false to say that all has run smoothly, but from the friction has come a sharpening of wits and out of the anxiety has come a mutual respect that is humbling and rewarding.

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