Most of the patients who do not participate verbally actively involve themselves in the session. This is shown in the accounts these patients give of the tape-a-drama session during small group therapy meetings held later in the day or on subsequent days.

Finally, there is much less tendency for three or four members of the group to dominate the therapy hour. One can make various speculations as to why this is true, such as presuming that the subject matter is concrete and vivid enough for almost everyone to participate. The important thing is that this widespread participation has consistently been observed to take place. Therefore, this technique appears to be particularly useful for group therapy with a large group.

ABSTRACT

A useful group therapy technique employed at the Florida Alcoholic Rehabilitation Center is "tape-a-drama." A conflict situation (e.g., marital or employment problems) is chosen and acted out by patients to a group of patients circled around the actors. This is recorded and then played back to the group for discussion in which the actors themselves do not participate. In this way the problem is brought out into the open and examined and solutions are discussed. A special advantage is the ability to stop the taped record at significant places and to demonstrate nuances of expression by the actors which would otherwise go unnoticed or be forgotten. The majority of patients participate verbally. The technique is recommended for use with large groups.

Treatment of Alcoholism with Lysergide

Comment on the Article by Smart et al., with Special Reference to Issues of Responsibility in Research Reporting

J. Ross MacLean, M.D.[1] and W. E. Wilby[2]

The report by Smart et al. (1) on the role of lysergide (LSD) in the treatment of alcoholism, as published, will perhaps not evoke much comment since it reached the fair and innocuous conclusion that "Lysergide, as used in the present study, failed as an effective adjunct to psychotherapy. . . ."

The published text bears evidence of care apparently exercised in revising an original report (2) which was circulated by the authors before publication of their article. To those with an adequate perspective of the "psychedelic controversy," and to the few aware of facts seldom

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correlated in public view, such technical defects as are readily apparent in the published article become secondary to a course of action that raises serious questions of responsibility and integrity in reporting by scientists. We therefore limit our critique of the published text to only a few observations which seem to us especially pertinent, and address ourselves chiefly to the wider issues raised by the distribution of the prepublication report.

The psychedelic perspective dates from Hofmann's synthesis of lysergic acid diethylamide and the subsequent observation of its dramatic impact upon consciousness and perception, variously labeled hallucinogenic, psychotomimetic and, later, psychedelic. Although first investigated for its presumed value in evoking a transient "model psychosis" and later for its apparent equivalence to delirium tremens, there emerged a group primarily concerned with the experience in terms of religio-philosophic or "mystical" insights. Responsible scientific inquiry became overshadowed by evangelism and ritual bordering on cultism and, within a remarkably brief period, Pandora's box was opened to unknown thousands of users seeking new sensation, escape, or something else not readily definable.

A counterreaction was to be expected, and in fairness to the most vociferous critics of lysergide, many of the early works purporting to present findings of competent investigations were notably deficient in abiding by minimum research standards. Studies were often inadequately designed or documented, and conclusions were at best highly speculative, sometimes fanciful, hypotheses.

The broader perspective, however, has roots in the current and often ill-founded reverence for anything spawned of the computer, test tube or erudite exposition.

Research has brought society to the brink of understanding the very essence of life itself, while placing in man's hands the tool of total annihilation. It is a term and pursuit that has attained frightening sanctity, and it is a paradox of an enlightened society that work frankly represented as research, or accorded that status by its manner of presentation, tends to be widely and uncritically accepted as "authoritative." It is precisely this naive reliance upon "authority" that imposes an awesome responsibility upon those who compile and disseminate the products of inquiry. The following case in point is offered primarily as a plea for reason and responsibility.

So far as the published version (1) is concerned, one might well take exception to the research attitude: "An impression gained from some earlier reports is that trials were carried out by personnel committed to a belief in the value of lysergide, or at least convinced that earlier papers had demonstrated some value. In the present study, the investigators were skeptical about its value and no one was committed to a belief in its efficacy." The implication here is that a "negative" bias in research is somehow more virtuous than the presumed deficits of a "positive" one. Skepticism as a motive for inquiry is laudable, but it offers no more assurance of the prime research requirement—objectivity—

than does the approach they criticise. A project conducted in doubt must inevitably color the entire team's outlook, and it is conceivable that an incautious remark or the general skeptical demeanor of any worker could filter to the client (subject) level. Of course, precisely the same effect may apply in the opposite direction, and subtle expectations of success might well be a factor in the therapeutic outcome of other studies. The net conclusion is that the question of "placebo response" remains unanswered by this study despite the attempt in project design to test such an influence.

An interesting sidelight to the attitude of Smart et al. is the statement that "The orientation of the professional staff is to see alcoholism as an illness which can be cured or alleviated by medical and psychiatric treatment. . . ." The term "cured" is anathema to most North American workers for practical, if not factual, reasons. However, any suggestion or expectation of cure is notably absent in the pro-lysergide reports that are subject to the authors' ire.

A further criticism of the Smart et al. study centers upon their demand and failure to fulfill the stringent requirements of a controlled double-blind research design. In an earlier paper (3), Smart and Storm were incisively critical of pro-lysergide findings because such procedures had not been followed. Not only were their misgivings and demands clearly recorded in their article, but in review form they were given even wider circulation. For example, Jordy's resumé (4) was circulated to some 2,000 members of the medical profession throughout the Province of British Columbia, and to many thousands of others in Canada and the U. S. A.[3]

Questions may be raised concerning the appropriateness of the demand for the controlled double-blind approach in this case, but of greater pertinence is the fact that Smart et al. failed in their attempt to fulfill the self-imposed standard. As they say (1), "complete double blindness was not achieved but a modified sort of single blindness was."

They also conclude that their results demonstrate "that such [beneficial] effects are not associated solely with the pharmaceutical properties or with the procedures used here." This will come as no surprise to most workers since there is no ground upon which to believe, or lead the patient to believe, that lysergide has any magical curative powers. Apparently Smart et al. allowed such an erroneous luxury to persist since they write, "In nearly every case the patient continued to believe that he had received a 'magic' new drug." Any sort of tolerance for the expectation of a "cure" for alcoholism via the action of a "magic new drug" would appear to offer little ground for confidence in the pursuit of scientific inquiry.

Putting aside other possible criticisms of the design, attitude and conclusions of the study by Smart et al., the critical issue we wish to raise is that of irresponsible conduct in the circulation of a prepublication

[3] Via syndicated distribution, in the *Alcoholism Treatment Digest,* by the Journal of Studies on Alcohol, Inc.

report (2) that is in many respects unrecognizable in terms of the published version (1).

The late Dr. E. M. Jellinek, who was closely associated with the agency sponsoring the Smart et al. study, was quite explicit in his attitude toward the use of provisional research reports. "I believe one should be very careful with working papers and memoranda as to their circulation outside the agency from which they emanate. It is highly desirable to designate any ideas and sketchy analyses for staff information only and restrict it to that. There are, of course, memoranda that go beyond the sketchy stage, and these may receive wider distribution, but they should still be fairly restricted to people who can readily understand the limitations of a working paper."[4]

For the studies published by responsible scientific journals a measure of protection is afforded by editorial consultants and specialist boards of review who exercise discretion as to content. Authors wishing the prestige of a particular vehicle of communication must submit to the standards, scrutiny and recommendations of their peers. For those unable or unwilling to meet high standards there is no dearth of lesser outlets including private publication by the author. In the latter case there is complete freedom from restraint and anything, regardless of merit, may find its way into the hands of those who will unwittingly or deliberately reach unwarranted conclusions, perpetuate myths or chart unsound courses.

In the case of "prepublication reports" we are faced with an awkward dilemma. It can become unreasonable to expect research findings to languish in storage until polished for journal acceptance or other public viewing. However, where a study has not been exposed to competent impartial assessment, the use to which a prepublication report may be put becomes a critical issue. Individual workers, sponsoring agencies and all associated with a project have a singular responsibility to be qualitatively selective, both as to material and recipients, when distributing so-called "confidential" preliminary drafts.

In making the comparison here, there is need to disregard the specific admonition (2): "This manuscript comprises unpublished material probably not in final form suitable for publication. Please do not quote from it or use it in any way without the written permission of the author." A violation of this admonition may itself be the subject of attack, but the remarkable circumstances of the present case seem to render the paper's confidential status void. The draft was widely distributed and was so cited and employed as to be in the public domain, and the authors apparently believed it would be published in essentially the same form.

The failure of responsibility in this instance is not only in the prepublication distribution per se, but in the release of conclusions so fundamentally in error as to be offensive.

First, it is necessary to contrast the published conclusion, "lysergide,

[4] Jellinek, E. M. [Private communication, 13 Feb. 1963.]

as used in the present study, failed as an effective adjunct to psychotherapy" (1) with the prepublication statement that "The results of this study fail, completely, to show that LSD is a useful adjunct to psychiatric treatment of alcoholism. The conclusion is, therefore, that LSD was not shown to be an effective adjunct to the existing clinical treatment of alcoholism. These findings represent a strong indictment of the previous unwarranted assertion that LSD is effective in the treatment of alcoholism" (2).

The latter conclusion, in our opinion, exceeds the most liberal interpretation of the evidence presented, and its use within the agency could not fail to establish a negative attitude toward the possible merits of psychedelic adjuvants to therapy. Had the distribution of this "memorandum" been confined to internal use a certain degree of containment of error might have resulted. Unfortunately, this draft was not used with discretion, and at least one State authority, faced with the problem of framing legislative control of lysergide, was to some degree influenced by the report. The Governor of California was apparently impressed by the fact that ". . . recent work with LSD at the Alcoholism and Drug Foundation in Toronto, Canada, has been reported using the double blind controlled study design which is recognized in scientific circles as the most rigorous method of testing drug effects."[5]

Confidence in the scientific authority of design obviously precluded recognition of human fallibility in conduct and conclusion. Under the very real pressures of illicit abuse of the drug, with its inherent dangers, it is not surprising that drastic criminal laws have been enacted relative to the use of lysergide in California. Some reservation as to the effectiveness of such legislation may be expressed, despite the good intent, but the fact remains that a selective use of "research" has in all likelihood influenced a legislative precedent.

Since distribution of the prepublication report does not appear to have been adequately guarded, attention may be directed to some of the basic procedural defects tolerated in the reputed effort to replicate the work of others. The design of Smart et al. was patterned on the "single large dose" approach reported earlier by MacLean (5) and others, and it is clear that they believed their procedures were as similar as possible with the exception of the introduction of double-blind features and controls. At this point they were clearly in error. Rather than "a number of important similarities between this study and earlier studies of lysergide and alcoholism," there were in fact gross differences.

On the one hand, set and setting were stressed as critical to the outcome by MacLean and those of similar belief. The lysergide experience was conducted in a highly permissive, supportive environment and allowed to continue its natural course uninterrupted by chemical or other intervention. In the Smart et al. investigation the experience was conducted in a single hospital room with the patient strapped to the bed. "After drug administration patients were attended throughout a 3-hour

[5] Governor Edmund G. Brown. [Private communication, 3 March 1966.]

interview by doctor and nurse cotherapists. The three-way psychotherapeutic interview attempted (*1*) to discover an insightful alternative to the patient's habitual anesthetic use of alcohol and (*2*) to define the patient's attitudes in the following areas: (*a*) transference feelings toward doctor and nurse; (*b*) displacement feelings toward the act of drinking; (*c*) child–parent relationship; (*d*) suicidal propensity; (*e*) displacement attitudes toward alcohol; (*f*) genital–sexual and urethral–sexual behavior; and (*g*) coordination between verbal and nonverbal behavior." The contrast here is startling, and that which is euphemistically termed a "three-way psychotherapeutic interview" bears a striking similarity to a psychiatric inquisition. Considering the physical restraint and presumed patient–therapist dialogue, it is small wonder that a further departure from previous methodology was deemed necessary, i.e., the administration of an anticonvulsant (250 mg of phenytoin).

In one instance, to our knowledge, a patient exposed to treatments of this type exhibited near total amnesia for the experience. The same patient later underwent the more permissive, supportive approach advocated by MacLean et al., and not only consciously reappraised his conflicts with full recall but appears to have been placed on a firm course toward rehabilitation.

A single-case contrast of this type is not offered as proof of methodological superiority, but it does tend to support the concession by Smart et al. (1) that their results "do not preclude the possibility of finding some effects of lysergide on drinking, given some very different procedures or personnel." The fact is that very different procedures and personnel were used, and to reach the damning prepublication conclusions that were circulated was totally unwarranted.

Critical as one may be of the published report, and appalled as one must be with the prepublication abuse, the real dilemma is faced by those responsible for screening works to be accorded the prestige of publication in scientific periodicals. The referee system as used by the QUARTERLY JOURNAL OF STUDIES ON ALCOHOL will probably remain the most practical defense against publishing work of doubtful merit. But since the referees cannot be expected to know of the prior use or misuse of materials under consideration, the issue of circulation of prepublication drafts remains to be resolved.

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ABSTRACT

A report by Smart et al. [Quart. J. Stud. Alc. **27**: 469–482, 1966] is criticized as an abuse of research responsibility. (*1*) Smart et al. in a previous paper have criticized clinical studies of lysergide for not using adequate control groups, and yet in their study they failed to achieve a controlled double-blind condition. They claim a number of important methodological similarities between their study and previous ones, but on many essential points their study differed, particularly in the personnel and setting used. (*2*) A report of the Smart et al. study was widely circulated prior to publication and before editorial referees had evaluated it and apparently recommended important changes. The conclusion in the prepublication report was that the study had demonstrated that lysergide was of no use in the treatment of alcoholism and that it represented "a strong indictment" of previous studies. These conclusions were unwarranted by the data but were uncritically accepted by some readers. In the published version this had been modified to conclude that lysergide, as used in the study, failed as an effective adjunct to psychotherapy.

Treatment of Alcoholism with Lysergide

Comment on the Article by Smart et al.

James Fadiman, Ph.D.

The recent article by Smart, Storm, Baker and Solursh (1) appears to be a first step towards evaluating this controversial drug in the treatment of alcoholism. It is hoped that the generally nonsignificant differences between drug and control groups will focus greater attention on what appears to be the central variables in using these materials. Too much time has already been wasted attempting to isolate the "effect" of the chemical independent of the setting, as well as the preparation and expectations of the patient. Hopefully, the enormous rise in unsupervised use of psychedelics has awakened researchers to the differences between lysergide, a drug, and psychedelic therapy, a method of treatment using lysergide or other psychedelics as part of a total milieu designed around the unusual effects of these substances.

Since Smart et al. have undertaken the difficult role of evaluating the work of others, it is hoped that their study will encourage them to go on and compare the results of psychedelic therapy done with persons trained in the use of the total environment (music, nondirective techniques, etc.), with clinical studies using lysergide as a chemical adjunct to a therapy program. While it is all too easy to criticize others' work, several specific points should indicate possible differences between the