February 1970, pp. 195–200) suggests several implications to me. First, despite the fact that Freud recognized the role of constitutional, developmental and current factors in influencing the formation of neurotic symptoms, contemporary psychoanalysts tend to emphasize the first two factors exclusively and to ignore the third. One of the few analysts who took the current reality situation seriously was Herzberg with his concept of 'tasks', but this notion has not found acceptance by most therapists. His belief in the value of self-esteem acquired through acquisition of skill has received detailed examination in the past few years in America as a result of the work of Robert White on 'competence.' A psychotherapeutic approach which emphasizes infantile urges, personal weakness, incompetence, helplessness, dependency and impulsiveness cannot help build realistic self-esteem. This is especially true for the increasing numbers of people who nowadays enter into analysis with low self-esteem and low ego-strength at the very outset.

A second point implied by Dr. Schmideberg's paper concerns the nature of professionalism. The basic question is: Is the current system of private practice for the delivery of mental health care (1) efficient, (2) effective, or (3) ethical? Although it may be argued that the few cases presented by Dr. Schmideberg are isolated exceptions and untypical, the fact remains that there is no good evidence on this point. We do not, in fact, know what per cent of analytic patients are failures in the senses described in the paper. We have few data on the actual effectiveness of psychoanalysis for 'curing' symptoms or for producing long-lasting personality changes. It almost seems, from a perusal of the contemporary scene, that the therapeutic gains expected from psychoanalysis go down as the length of the treatment goes up.

Another aspect of this question concerns the efficiency of the treatment. By this I mean the benefits in relation to the costs. Analysis has become so expensive at the present time that it is perfectly fair to ask whether the presumed benefits are worth the time and money which will be expended. Any prospective patient is surely entitled to ask (himself at least) whether five or ten thousand dollars plus five or ten years of therapy time could not be more profitably put into such things as getting an education, providing a dowry, going on an ocean cruise, buying a new car, taking tennis, golf, skiing, painting, or sculpture lessons, or loafing on a Caribbean island.

My final point concerns the ethics of private practice. We are all aware of the abuses of the relationship that can and do occasionally occur in the privacy of the analyst's office. But here too we have no way of knowing just how untypical these exceptions really are. In addition, the analyst's fallibilities, his biases and his tendency to play God can remain unobserved and unchecked in the private practice setting. It would be better to remove the source of temptation than to assume all men can remain saintly.

What I should prefer to see, in contrast to private practice, are small and large clinics widely distributed among communities. The advantage is obvious. Costs can be reduced, more adequate records kept, and there would be constant professional interaction to prevent the blindspots which each analyst has from exercising an undue influence.

I think we should feel grateful to Dr. Schmideberg for directing our attention to the kinds of issues raised here.

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THEMES IN A THERAPEUTIC COMMUNITY

DEAR SIR,

I was interested in the article by D. H. Clark and Kenneth Myers in the October 1970 issue of the *Journal*, pp. 389–95. I would like to make a couple of points, not so much criticism but perhaps enlargement.

I was fortunate enough, while recently seconded to Fulbourn, to spend a few weeks in Hereward House, and it was generally a very stimulating experience. I would certainly agree, firstly, with the authors that the 'flattening of the authority pyramid and blurring of roles' is a very worthwhile goal—but in practice I felt that a certain degree of leadership is still needed, and I think that the tension and uneasiness that was present in the 'ward' while I was there was, at least in part, attributable to the absence of this. Residents, when unable to solve problems collectively, still seem to look to the 'professional' for certain guidance and modification. This group anxiety seems to accumulate and, ironically, to interfere progressively with genuine attempts to reduce it.

Again, though the therapeutic community has indisputable assets, it seems to have limited rehabilitative value.

In the first place, the kind of frank and immediate expression of feelings which the therapeutic community seems to involve is not often possible in the normal community. Invariably—perhaps unfortunately—for the sake of tact and generally successful interpersonal relationships, greater suppression of

emotion and maybe less honesty is usually needed. In many ways, the therapeutic community then is a 'luxury', and very stimulating and perhaps therapeutic in its constant examination and analysis of one's own and other people's actions, motivation, and attitudes—but it is arguable whether residents are being conditioned to meet realistic situations. In a similar vein, the community is a source of understanding and support which is a strongly attractive force—especially for those without satisfactory homes—and I feel that many can become too dependent on it; which, of course, also makes rehabilitation difficult. Maybe 'a therapeutic community within the community' (rather than within the hospital), with a gradual tail-off of meetings for those who have left, might be an answer, rather than complete and immediate ending of support after discharge.

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THE CLINICAL DISTINCTION BETWEEN AFFECTIVE PSYCHOSES AND SCHIZOPHRENIA

DEAR SIR,

In the September, 1970, issue of the *Journal* (p. 261) Kendell and Gourlay report no clinical distinction between affective psychoses and schizophrenia, and further imply that these conditions should be considered as opposite poles of a continuum and not as separate diseases. Their conclusions rest on a discriminant function analysis which demonstrates a trimodal distribution rather than the bimodal distribution characteristic of two distinct illness populations. I submit that these conclusions are based on methodological artifact and do not reflect clinical observation.

Patients described by Kendell and Gourlay were involved in a cross-national study and were given diagnoses by physicians associated respectively with a London and with a New York State mental hospital. The diagnostic criteria of these physicians are not described, but the authors suggest the use of 'inconsistent diagnostic criteria' by the New York group, with one of the New York physicians, 'by his broader concept of schizophrenia', effectively eliminating the concept of affective psychoses from his consideration. If diagnostic criteria are inconsistent, discriminant function analysis of patients selected by these inconsistent criteria could not possibly discriminate two relatively homogeneous groups.

Nevertheless, the authors state they attempted to maximize the possibility of bimodality, yet they include involutional paraphrenia with the schizophrenics, dismissing this inclusion as statistically insignificant. Most discussions include involutional paraphrenia among the affective disorders (Slater and Roth 1969), and I would have been more inclined to accept their maximizing had they too kept to this established classification. Also included among the schizophrenic group, and I suppose other examples of maximizing efforts, were: one acute schizophrenic (an illness clinically distinct from process schizophrenia (Robins and Guze, 1970), four latent schizophrenics (an illness based on highly questionable psychodynamic concepts), 17 schizo-affective schizophrenics (an illness demonstrated by Clayton *et al.*, (1968) to be a variant of the affective disorders), and four 'unspecified' schizophrenics (an illness with which I am unfamiliar, nor one to which I can find any reference). So we have 26 patients who perhaps should not be included among 'Kraepelinian' schizophrenics and whose inclusion may have resulted in the reported trimodal distribution.

Finally, if maximizing a bimodal distribution was their goal, why did the authors choose for analysis such statistically confounding and often irrelevant items as: loss of insight, difficulty in relaxing, insomnia, time in hospital, and 'schizophrenic speech' (is that like a criminal face)? Would not Schneiderian first-rank symptoms or other precise psychopathological terms have been more relevant and more likely to have resulted in a distribution reflecting homogeneous groups?

In summary, lack of rigid diagnostic criteria, the inclusion of questionable schizophrenic sub-categories and the choice of less than optimal items for analysis could explain the authors' result, perhaps statistically sound but too far removed from basic clinical observation.

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