Letters

Psychiatric Diagnosis

D. L. Rosenhan's article "On being sane in insane places" (19 Jan., p. 250), while full of important observations, is seriously flawed by methodological inadequacies and by conclusions that are inconsistent with—indeed, that directly contradict—the data he presents. . . . When Rosenhan's pseudopatients faked a history and were subsequently "misdiagnosed" by physicians at the psychiatric hospitals where they presented themselves, they established nothing about the accuracy of diagnosis per se, but merely reaffirmed the critical role of history-taking in medicine. Most physicians do not assume that patients who seek help are liars; they can therefore, of course, be misled. The so-called "Munchausen syndrome," of people who fake medical illness and may be admitted to hospitals repeatedly and receive years of extended treatment, is documented in the literature, as is, of course, simple effective malingering. It would be quite possible to conduct a study in which patients trained to simulate histories of myocardial infarction would receive treatment on the basis of history alone (since a negative electrocardiogram is not diagnostic), but it would be preposterous to conclude from such a study that physical illness does not exist, that medical diagnoses are fallacious labels, and that "illness" and "health" reside only in doctors' heads.

More misleading than Rosenhan's false conception of how diagnoses are reached, and his inaccurate conclusion therefrom that psychiatric diagnoses are empty labels, is his apparent total ignorance of the definitions of "insanity," "psychosis," "schizophrenia," and "schizophrenia in remission." Insanity is a legal term. It is not a psychiatric diagnosis. Though its exact definition varies among the states, it usually entails "the inability to decide right from wrong." This is a legal definition applied by *courts*. No psychiatrist ever "diagnoses" a patient as sane or insane. People legally insane at one point in time may be in general nonpsychotic (seizure disorder), and many, in fact most, people who are psychotic are never declared insane.

Most shocking, however, is Rosenhan's conclusion that "the normal are not detectably sane," by which he evidently means "not detectably nonpsychotic." In fact, all the pseudopatients were discharged with the diagnosis of "schizophrenia *in remission*," which means that they were clearly seen by the doctors to be nonpsychotic in the hospitals where they were observed but had been psychotic during the period described by their "history." Thus, Rosenhan's study demonstrates that despite false historical data and the set of the hospital environment, 12 nonpsychotics were observed by their psychiatrists to be nonpsychotic—a record of 100 percent accuracy.

In the rest of this article, Rosenhan poignantly describes neglect and abuses in psychiatric hospitals. He rightly reopens the door to this storehouse of bad practice and demands that the psychiatric professions enter, take responsibility, and change. But faulty application of concepts does not invalidate those concepts. The concept "psychosis" (Rosenhan refers mostly to "insanity") is eminently justifiable not only by such symptoms as hallucinations, or by such responses to medication as one sees with lithium, but by the entire thrust of modern biology: how surprising it would be if the central nervous system of man —alone among all living tissue—were immune to biochemical or physiological pathology. . . .

Rosenhan might have summarized his important observations as follows: that given our current ignorance of biochemical and physiological parameters, psychiatric diagnoses may be inaccurate; that the psychiatric professions persist in overinterpretation and thereby increase the risk of type-2 errors; that given the ease with which histories of symptoms can be faked, coupled with the absence of positive chemical-biological diagnostic tests, the relationship between psychiatric diagnoses and the law needs reexamination and revision; and that the practice of psychiatry deviates disgracefully from accurate application of its concepts and ideals.

Unfortunately, through the publicity attracted by his methods, conclusions, and rhetoric, Rosenhan may have provided society with one more excuse for pursuing the current trend of vilifying psychiatric treatment and neglecting its potential beneficiaries.

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. . . Rosenhan concludes, among other things, that "it is clear that we cannot distinguish the sane from the insane in psychiatric hospitals." He does so on the basis of two experiments. In the first experiment, eight pseudopatients gained admission to 12 different mental institutions on the basis of identical fabricated histories. After admission they all behaved in a cooperative and normal manner, and their behavior was "uniformly" described in these terms by various staff members. All were discharged (on demand?), most of them within a few weeks, with a diagnosis of "schizophrenia in remission."

In the second experiment, the staff of a hospital was told (incorrectly) that at least one pseudopatient would be coming through in the next 3 months, and was asked to rate, on a scale of 1 to 10, the likelihood at admission that any given patient was a fake. An undisclosed number of judgments of undisclosed certainty were given on 193 patients by an undisclosed number of staff. No further information is given about this experiment except that apparently several people were way off. No judgments were solicited after an observation period or at time of discharge. Furthermore, this particular staff was set up, as they say, since its members were well aware that pseudopatients had gone undetected at eight other institutions.

To point out that Rosenhan's conclusion is unwarranted on the basis of his, ah, data is perhaps belaboring the obvious. A more reasonable interpretation (assuming any interpretation based on eight subjects is reasonable) would be as follows:

1) Sane people who feign an abnormal history but behave normally do not appear abnormal to hospital staff.

2) If a patient behaves normally, he is not suspected of having fabricated his symptoms to gain admission. Or, if

he is suspected, the staff makes no official mention of it.

3) If told that pseudopatients are coming through, some of the staff will, at time of admission, suspect that certain of the patients are indeed pseudopatients. . . .

Rosenhan uses his interpretation of the results as a point of departure for an attack on mental health care as currently practiced in our mental institutions. I would like to know if his position would be any different if the pseudopatients had been exposed. If so, how? If not, why were the experiments done?

And why did *Science* publish them?

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. . . Allegedly, the pseudopatients were all admitted to hospitals because they complained of hearing voices. It is hard to accept that that was the only reason, because a complaint of auditory hallucinations of the sort described would seem to be readily manageable with outpatient psychiatric care. Either more went on in the admitting offices than is reported in Rosenhan's article, or 12 admitting officers with extremely low thresholds for hospitalization were encountered by chance.

It is also reported that all but one of the pseudopatients desired to be discharged immediately. If that is so, and if the hospitalizations ranged from 7 to 52 days with an average of 19 days, presumably the patients were not on "voluntary" status. It would seem important to examine the hospitals' justifications, legal and otherwise, for detaining people whose behavior, however its interpretation may have been biased by its setting, did not apparently include anything that would constitute grounds for commitment anywhere. . . .

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. . . Each pseudopatient reported only one symptom: hallucinations of unfamiliar, often unclear voices saying "empty," "hollow," and "thud." There is not a single psychiatric textbook or journal article on the phenomenology of schizophrenia that even suggests that a diagnosis of schizophrenia can be made on the basis of auditory hallucinations alone. . . . Thus I would have to conclude that the "uncommonly intelligent" physicians who obtained histories, performed mental status examinations, and formulated the cases on the pseudopatients were not competent to diagnose schizophrenia. That the assessments were done by incompetent evaluators seriously vitiates Rosenhan's contention that "we" (psychiatrists and psychologists? members of society in general?) cannot distinguish the "sane" from the "insane." As well, a study done in 12 hospitals not randomly selected from the nation's large number of psychiatric hospitals (likely well over 100) hardly allows the author to draw any general conclusions about psychiatric hospitals. . . .

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. . . The 12 hospitals used in the study are intended, at least by implication, to be a representative sample of psychiatric institutions. We are given no meaningful data about any of these institutions —location, size, size of staff, kinds of staff, patient-staff ratio, training of staff, ward organization, therapeutic facilities, theoretical framework, admission and discharge procedures, patient involvement in ward management, off-ward activities and privileges, visiting arrangements, group and individual therapy provisions, and so on. Any one of these may have major impact on outcome of treatment or hospital experience. Without such data, we can make no comparisons of these hospitals with each other or with other hospitals. . . .

The hospital I am most familiar with is a university hospital of excellent quality, but so far as I know not at all unique in its handling of the admission process. Every patient who requests or is referred for admission is examined by a psychiatric physician who decides whether admission is indicated. No one is admitted merely because he wishes it. Anyone who presented with only the symptoms described in this article, unaccompanied by serious other manifestation of distress, and reporting good function in work and interpersonal relations, would not be admitted to an inpatient bed. Furthermore, anyone who is admitted—having manifested much more flagrant and distressing psychopathology—who the following day shows no further distress, no symptoms, and no disturbance in his interaction with his family would be discharged in 24 to 48 hours. It is necessary to add that in ours and a great number of similar institutions, the vast majority of admissions are of voluntary patients, and patients who wish to leave the hospital do so any time they decide to. . . .

"Many" of the pseudopatients (how many is "many" out of eight?) "had never visited a psychiatric hospital," yet they immediately became experts in making complex interactional observations of patient and staff behavior which would have taxed the skills of an experienced researcher. It appears that the pseudopatients gathered pseudodata for a pseudoresearch study. . . .

The article claims omniscience about what staff assumed or found—"*never* were staff found to assume that one of themselves or the structure of the hospital had anything to do with a patient's behavior." How was this "never" established? The author is a professor of law. Would he label this kind of evidence as anything but useless? I have sat in too many lengthy staff meetings and patient-staff meetings (were there none of those in the "excellent" hospital that was part of the study?) where hours were spent agonizing over the part played by staff and *their* behavior in patients' distress to give any kind of credence to the "never." . . .

I am deeply concerned about the state and fate of psychiatric care in this country. I am also deeply concerned about the destructive potential of such pseudostudies as the one under discussion. Appearing in *Science*, it can only be productive of unwarranted fear and mistrust in those who need psychiatric help, and make the work of those who are trying to deliver and teach about quality care that much harder.

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. . . Rosenhan apparently lacks clinical knowledge and knowledge of the clinical mind; thus he fails to distinguish between what may have been a poor quality of practice and the reliability of current diagnostic criteria when properly applied.

Any expert clinical psychiatrist faced with the sudden onset of auditory hallucinations in a previously well person would think first of organic psychoses such as are associated with the use of drugs (hallucinogens), drug or alcohol withdrawal, endogenous or exogenous endocrine toxicity, intentional poisoning, cerebrovascular disease, and brain tumor. After the initial mental-status examination, which is a highly formal

procedure, expert clinicians would be likely to order urine assays for hallucinogens, a neurological consultation, skull x-rays, and electroencephalograms in these cases. Careful interviews with the families and friends would have been conducted, since drug and alcohol abusers are notorious deniers.

One feature of the mental-status examination, namely, the hallucination of voices saying "thud," would have indicated intense case study. In 20 years of experience with hospitalized mental patients I have never encountered this hallucinatory content, nor read of it; experienced colleagues concur. How this unusual finding escaped interest is, as Rosenhan points out, puzzling.

If the organic work-up produced no conclusive positive findings, the clinician who failed to consider malingering in these cases would be considering schizophrenia. However, any psychiatrist trained in psychodynamics would be searching both for current precipitating interpersonal factors and severe early life traumas; the failure to find these would have created an urgent need for clinical psychological testing and, once again, interviews with family and friends.

No indication that any of the foregoing procedures were carried out is given in Rosenhan's article. . . .

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. . . If complaints of isolated auditory hallucinations are believed by the doctor, they can require neurological investigation including lumbar puncture, skull x-ray series, and radioisotope brain scans. None of these procedures is without risk to the patient, but the risk is less than leaving undiagnosed brain disorders that can give rise to isolated hallucinations. One wonders if the volunteers for this reckless experiment were informed of this risk. . . .

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. . . Simulation is a challenge to the diagnostician in every area of medicine in which diagnosis rests primarily upon reports of subjective experiences, as it does, for example, in the case of angina pectoris, chronic neuralgia, or headache. Where there is something obvious to be gained by the simulation, such as compensation for injury or disability insurance payments, the examining physician is wary, though seldom able to make the distinction between the real and the simulated definitively. Since the psychiatrist deals with disease manifest primarily in subjective sensation and secondarily in overt behavior which he seldom has the opportunity to witness directly, mental illness can easily be simulated. That an illness can be successfully simulated does not make it any less "real" than one which cannot. . . .

Rosenhan claims that the only abnormal findings presented by his experimental subjects was the complaint of having heard voices. Review of his procedure discloses the presentation of at least two other phenomena which are ordinarily signs of illness. The first is the fact that the simulators sought admission to psychiatric hospitals. It is so rarely that this is done by anyone who is not indeed mentally ill that that alone must be taken seriously by a conscientious admitting psychiatrist as suggestive of illness.

Second, the voluminous note-taking which Rosenhan describes is a common occurrence among patients in mental hospitals. He expresses surprise that the hospital staff was not made suspicious by it. If anything, this note-taking would make the simulators seem more like compulsive paranoid patients than otherwise. . . .

I suspect that had these simulators applied for private care, most psychiatrists would have observed them without hospitalizing them. . . . However, the admitting room psychiatrist is not in private practice, and an admitting examiner who refuses hospitalization to an individual seeking it and presenting at least suspicious symptoms may face legal difficulties should the patient then commit suicide or homicide. Legal complications aside, there is considerably less harm done by admitting a patient who does not require hospitalization than by turning away one who does. . . .

A mental illness is not a diagnostic label; it is a pathologic process with its own natural history. . . . There is an important difference between sleep-induced or drug-induced hallucinations on the one hand and hallucinations of schizophrenia on the other. The difference is that when the period of sleep or of drug intoxication is over, the hallucinations and the behavior alteration associated with them will disappear. Once the diagnosis of schizo-

phrenia has been correctly made, we may anticipate that during the rest of that patient's life he will be in danger of recurrent attack. . . . That the specific details of the subsequent course cannot be predicted is less important than the fact that the odds of recurrent or continuing difficulty in a schizophrenic individual are orders of magnitude greater than they are in the general population. Rosenhan may claim that it is the process of labeling which exerts such a deleterious effect upon the individual's life. The practicing psychiatrist, on the other hand, will at once be able to call to mind many patients whom he has seen in second or third attacks of schizophrenic illness whose first attacks were not recognized or diagnosed. . . .

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. . . What Rosenhan must wish to conclude is that the criteria for distinguishing sane from insane are not clear or unambiguous. This of course is a well-known fact. The question is, just how vague are the criteria? That in none of 12 cases was the phony patient spotted by the authorities gives the erroneous impression that these criteria are very, very vague. However, this is not necessarily borne out by the "data."

While it is true that psychosis is thought to "reside in" the patient (that is, the adjective "psychotic" is applied to a person), it is generally understood that the psychosis manifests itself only under certain conditions. The psychotic is not expected to be bizarre in everything he says or does. . . . The conditions on the ward are designed (rightly or wrongly) to make the patients manageable (hence to appear sane). Those whose psychosis is not suppressed are transferred from the admitting ward. Accordingly, a much more impressive demonstration of his point could be made by Rosenhan if he were to take obviously insane persons and, by giving them a new name and releasing them to a community where they were not known, successfully pass them off as sane.

That the pseudopatients were not diagnosed as sane is not surprising. These pseudopatients were not just sane persons; they were sane persons feigning insanity. From the experiment, then, the only accurate conclusion to be drawn is that presumably competent judges cannot distinguish the insane from the sane-feigning-insanity when the judges are not aware of a possible reason for faking. While malingering is a diagnostic possibility, "researching" is not. A proper "control" condition would be to repeat the study with a regular medical problem. Send pseudopatients with "low back pain" or "severe headaches" to physicians and see how many of them are detected as "researchers." . . .

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. . . The pseudopatients did *not* behave normally in the hospital. Had their behavior been normal, they would have walked to the nurses' station and said, "Look, I am a normal person who tried to see if I could get into the hospital by behaving in a crazy way or saying crazy things. It worked and I was admitted to the hospital, but now I would like to be discharged from the hospital." If the nurse displayed skepticism, it would have been quite easy to call on the telephone any number of persons who knew of the ruse and would quickly arrange for discharge.

Although I do not doubt for a moment that patients in mental hospitals occasionally receive beatings, I think it is a surprisingly rare occurrence. My association with mental institutions goes back to 1946, and for 2 years I lived in a mental institution as an extern; I have never known of a patient who was beaten. . . . In view of the extraordinarily provocative behavior of some patients and in view of the poor screening involved in hiring hospital personnel, it is surprising that more beatings do not take place. One might have given the same interpretation to the "pseudopatients" who saw beatings that in the novel *I Never Promised You a Rose Garden* (*1*) seem to have been a key factor in the patient's recovery from mental illness. The patient had witnessed another patient's being beaten. The therapist's response was: "Life is no bed of roses. There are persons in the world who will beat a weaker person. If you don't want to run the risk of being beaten and if you don't like to see others beaten, how come you are not working as hard as you can to get yourself out of this place?" . . .

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Reference

1. H. Green, *I Never Promised You a Rose Garden* (New American Library, New York, 1971), p. 256.

. . . Because the study of the phenomena of illness and its classification have been denigrated within American psychiatry, it is not surprising that individuals might be called schizophrenic even when their purported hallucinations are utterly atypical and other evidences of illness are absent. The emphasis on psychodynamic understanding of the sources of illness and on the uniqueness of the individual has so dominated psychiatric education that many American psychiatrists are scarcely aware that abnormal behavior is found in discrete, classifiable patterns. When psychiatrists differ in court, it is usually not about the diagnosis of illness, but about something as ill-defined as responsibility. In many training centers, residents in psychiatry are encouraged to write facile dynamic formulations, such as the one quoted in Rosenhan's article, but since these formulations are based on the sketchiest historical data it is understood that this is but an exercise.

The assertion that the diagnosis of schizophrenia written in a hospital record is a lifelong stigma has been repeated so often that one seldom hears scientific skepticism expressed about it. There has certainly been much question within psychiatry about the manner in which the term schizophrenia is to be defined. Those who apply the term widely do not understand what the fuss is all about, for many of their patients do quite well and appear to recover. Those who apply the diagnosis narrowly find that most of their patients do poorly, as did individuals with similar symptoms years before the label was invented, for it is the underlying illness, not the label, that accounts for the difficulties. The fact that the boundaries between normal and abnormal vary from culture to culture is cited in the article as an element of the proof that diagnosis of mental illness cannot be valid. But mental illness is diagnosed on the basis of definite criteria, not merely an impression of abnormality. Psychiatrists who have not mastered the criteria will of course make errors. . . .

It is interesting that the author adopted a misconception shared by most hospital patients. This is the narcissistic belief that what the nurses record is the abnormalities of patient behavior. Well-trained staff report, without evaluation, what they observe; the only criticism to be made of the nurse's report "exhibits writing behavior" is that it uses three words where one would suffice.

If patients "were viewed as interesting individuals rather than diagnostic entities, . . . would we not *seek* contact with them . . . ?" asks Rosenhan. To suggest that the diagnostic category so occupied the attention of the hospital staff that they could not respond to the individuality of their patients is to ignore what is known about institutions, roles, and even efficiency. People accept the fact that they can be guided along highways by traffic policemen who do not know their ultimate destination or motives for traveling, that they can be given money by bank clerks who do not know how they will spend it; but they hate to come up against the fact that in a modern psychiatric hospital they are likely to recover from a psychotic illness without ever telling a psychiatrist about their fears and hopes. The old-time doctor who sat by the patient's bed while he died was held in higher esteem than the physician of today who provides a prescription and then absents himself while his patient recovers. . . .

Are people "stripped of their citizenship, their right to vote," and so on, by psychiatrists and by diagnosis of illness? If rights are lost, it is because of laws, not medical practice. The new mental hygiene law in New York State specifically states that treatment in a mental hospital will not cause anyone to be deprived of his civil rights. If every state enacted similar laws, there might be fewer instances of a legislative problem's being misdiagnosed as a medical one.

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. . . Rosenhan overlooks some obvious and important conclusions:

1) In this country it is increasingly easy to obtain psychiatric care, including hospitalization. Often one need only ask; the request is taken seriously, and bizarre or uncontrolled behavior is not required as further proof of need. Whatever the impact of this on scientific accuracy of diagnosis, the humanitarian aspects are obvious.

2) The psychiatric hospital is not a pleasant place for "normal" people.

This is perhaps appropriate: normals seldom want to get in, and it is probably good that they should want to leave soon. Few of those who are ill enjoy their hospitalization either, but many prefer the hospital environment with all its failings to any alternative available to them. They enter and remain voluntarily. I do not think one can accept without reservation Rosenhan's allegation that the hospital environment is "undoubtedly countertherapeutic." His lone scientific point is that "normals" don't like to be in a psychiatric hospital—a point I think no one doubted before reading his article.

3) The "medical model" of mental illness is moribund, at least where patient and hospital meet. Where the model survives in hospital records and official reports, its sustenance may well come from administrators and funding agencies such as insurance companies who expect to receive the reports. Insurors are notoriously reluctant to pay bills if the patient wasn't "really sick."

Rosenhan is puzzled because his pseudopatients were retained in hospital even though they were symptom-free and exhibited no abnormal behavior. Had he looked about him (at the "real" patients) his question might have been resolved, for many psychiatric wards are not marked by a great deal of bizarre behavior; behavioral abnormalities often decrease immediately on hospitalization or soon after. Subjective symptoms also are often relieved by hospitalization. Absence of symptoms while in a special environment does not necessarily mean absence of illness; a patient admitted for bleeding peptic ulcer may "feel fine" and not be bleeding 24 hours later—he has his ulcer still. . . .

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. . . Staff failure to change diagnoses and treatment modalities is, in my experience, most related not to their inability to distinguish changes in patient presentation from one day to the next but rather to investments made in the already established diagnosis and treatment plans. And staff avoidance of patient contact is probably related, at least in part, to the difficulties imposed by the nature of psychiatric illness itself. When I worked in internal medicine, I found the medical patients who stopped me in the ward corridors to be no less "demanding" than I later found psychiatric ones to be. However, their demands could more easily be met by me within my physicianly role: I could usually inform them of their tentative or final diagnoses, of my current thinking about their illnesses, of the investigative procedure yet to be done, and of their prognoses. They frequently complained of symptoms (say, pain, or palpitations, or esophageal burning) which were quickly treatable with medications. Psychiatric illnesses requiring hospitalization are, as a rule, less well understood than most medical illnesses, and are often more chronic and socially debilitating. Most psychiatrists find it harder to answer the inevitable corridor questions raised by psychiatric patients: "What do I have?" "When will I be able to leave?" "Should I get a divorce?" "When will I stop feeling this way?" "When will this medication begin to work?" It seems to me, however, that facing patients, and trying to answer these questions as honestly as possible, difficult as they are, is far more important than avoiding them and the patients who ask them.

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. . Rosenhan quite correctly states: "Staff shortages are pervasive, staff time at a premium. Something has to give and that something is contact." Nobody can disagree with this statement. It is almost the rule that the more severe the psychiatric condition the lower the staff-patient ratio and the less effective the treatment. A recent study at McGill again confirmed that tragic fact and suggested a restructuring of psychiatric services to establish optimal maintenance of treatment and rehabilitation (*1*). Space does not permit discussion of the needed reorganization of psychiatric services throughout the United States and Canada. No reorganization is altogether possible if, in response to some fashionable doctrines, the reality of psychological disorders is denied altogether. The problem is not that psychiatric hospitals are "insane" places, but that many of them are inadequate. This makes it, of course, impossible to provide adequate diagnosis and care to psychiatric patients.

Rosenhan says that "while financial stresses are realities, too much can be made of them." This statement is ab-

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solutely incorrect. No medical or psychiatric care can be properly administered without proper financing; this applies, as has been disclosed on many occasions, not only to psychiatric patients, but also to the mentally retarded, to the disabled, to geriatric patients, and to many others suffering from chronic conditions. But let us simply state the facts about expenditure for psychiatric patients as compared to expenditures for other patients: In 1970, the average daily expenditure in public mental hospitals was $14.89 per patient per day, with one state spending as little as $5.80 per day. Veterans Administration hospitals spent $30 per patient per day and private psychiatric hospitals about $48. These amounts usually include the expenditure for medical and psychiatric treatment. During the same period, general hospitals spent about $80 per patient per day (*2*). This does not include doctors' fees. . . . These financial facts alone account for staff shortages, poorly trained and insufficient, even though well-meaning, professional personnel, and the type of attendants described by Rosenhan. . . .

The fact that any nomenclature which categorizes or applies to mental functioning eventually becomes a derogatory term or label is well-known—"idiot," "moron," and "imbecile" were coined as scientific terms but eventually became a form of invective and had to be replaced by "mentally retarded." Yet no scientific approach to physical or mental illness is possible without a nomenclature, unless one is willing to give up a scientific approach to illnesses altogether. Psychiatric nomenclature is not uniform and includes etiologic, psychodynamic, and even sociological aspects of mental and behavioral disorders. Yet the attack on psychiatric nomenclature as some kind of pernicious "labeling" comes very close to a denial that any mental disorders characterized by objectively ascertainable symptoms, behaviors, and tests altogether exist. In the not so distant past, "tuberculosis" and "syphilis" were words shunned by polite society. Fortunately this did not deter physicians and researchers from diagnosing and treating these conditions. If, as Rosenhan advocates, we avoided psychiatric diagnoses, labeled by him as labels, and replaced them by focusing "on specific problems and behaviors," then the verbal designations of these problems and behaviors would become labels, too. . . .

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. . . Rosenhan calls for more research. But the social psychology of mental institutions has been a major research field for 20 years. What Rosenhan describes we have known in exquisite theoretical and practical detail for over a decade. But he ignores the psychology of institutional change. Knowledge, per se, does not produce change. Goffman's classic research on total institutions was conducted at a famous hospital. When I interviewed staff at that hospital some 10 years after Goffman, most knew of his book (*1*), most did not know it was a study of their hospital, and noth-

ing had changed in the hospital in the 10 years.

Rosenhan suggests that if we do not send people to insane places our impressions of them are likely to be less distorted. Here he assumes that psychiatric hospital units are inevitably bound to a gross distortion process. That is an assumption that can be empirically tested. There is abundant evidence to indicate that organizational change is possible to redress the distortions Rosenhan describes (*2*). His alternative is to retain disturbed persons in their communities for treatment, where a "nonpejorative environment" can be provided. His support for community treatment programs is certainly consonant with theoretical and therapeutic concepts of the day, but his rationale is not supported by evidence that the community is nonpejorative. The disturbed or deviant person is labeled as such before he comes to the psychiatric hospital, not after (*3*). . . . Cumming (*4*) has done a brilliant social role analysis of the total human services system in a community which demonstrates the same phenomena Rosenhan describes in the hospital. . . .

The Rosenhan article implies that the problems lie solely with the mental health professions and psychiatric institutions. But institutions and professional practice exist in reciprocal relation to public attitudes and public demands. Inhumane institutional practices in part reflect the demands and expectations of the society. The rejection and dehumanizing of the psychiatric patient within the institution can be seen as a projection and acting out of the community rejection and dehumanization of the labeled deviant (*5*). . . . The conclusions and recommendations that Rosenhan proposes miss the central issues and end up as scapegoating observations rather than as catalytic clarification.

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These letters deserve to be read with care. The issues they raise are important ones, and the feelings they convey need to be appreciated if one is to get a sense of where the crossroads in psychiatry are today. However much I wish it were otherwise, the authors and I simply do not agree on the major counts.

"On being sane in insane places" described two largely separate issues: diagnostic practices in psychiatric hospitals, and the experience of patienthood. Most of the comments are addressed to the first issue, and it is well that we review briefly the part of the study that is germane to that issue:

Eight people gained admission to 12 psychiatric facilities by simulating a single symptom, hallucinations. The contents of these hallucinations were of a kind that had never been reported in the literature. Beyond that simulation and some concomitant nervous

ness "no further alterations of person, history, or circumstances were made. The significant events of the pseudopatients' life histories were presented as they had actually occurred. Relationships with parents and siblings, with spouse and children, with people at work and in school, . . . were described as they were or had been" (p. 251). Eleven of the pseudopatients were diagnosed manic-depressive. All were discharged "in remission." None was found sane.

The theoretical predicates for the research derive from the large literature on the effects of contexts on perception (*1*). Perception is clearly an active process. While we tend personally to believe that we can always disembed a figure from its ground, the fact of the matter is that the meaning and value that are attributed to a figure are in some part contributed by the ground. A book found on your desk is perceived to be more valuable than one in your wastebasket. A hand in the air has different meaning according to whether you are sitting in a classroom, making a right turn in your car, or marching in a German parade during the 1940's.

While we may think that in examining a patient we have disembedded him from the context in which he is found, that assumption is open to reasonable question and is in fact the basis for our study. A recent experiment by Langer and Abelson (*2*) may make the effects of context more clearly germane to the present case. They videotaped an interview in which discussions were focused on a client's job histories and difficulties, then asked well-trained psychodynamic psychologists and psychiatrists to rate the degree of adjustment of the client, telling half the raters that they were observing a job interview and the other half that it was a psychiatric interview. Those who thought they were watching a job interview rated the patient as much better adjusted.

With respect to diagnosis, the issue that is implicated in the study has apparently been widely misunderstood. The issue is not that the pseudopatient lied. Of course he did. Nor is it that the psychiatrist believed him. Of course he must believe him. Neither is it whether the pseudopatient should have been admitted to the psychiatric hospital in the first place. If there was a bed, admitting the pseudopatient was the only humane thing to do.

The issue is the diagnostic leap that was made between the single presenting symptom, hallucinations, and the diag-

nosis, schizophrenia (or, in one case, manic-depressive psychosis). That is the heart of the matter. Had the pseudopatients been diagnosed "hallucinating," there would have been no further need to examine the diagnostic issue. The diagnosis of hallucinations implies only that: no more. The presence of hallucinations does not itself define the presence of "schizophrenia." And schizophrenia may or may not include hallucinations.

Lest the matter reduce to one scientist's word against another, let us look to the standard for diagnosis in psychiatry, the *Diagnostic and Statistical Manual* (DSM-II) of the American Psychiatric Association:

> 295. Schizophrenia. This large category includes a group of disorders manifested by characteristic disturbances of thinking, mood, and behavior. Disturbances in thinking are marked by alterations of concept formation which may lead to misinterpretation of reality and sometimes to delusions and hallucinations, which frequently appear psychologically self-protective. Corollary mood changes include ambivalence, constricted and inappropriate emotional responsiveness and loss of empathy with others. Behavior may be withdrawn, regressive, and bizarre. . . .
>
> 295.3. Schizophrenia, paranoid type. . . . characterized primarily by the presence of persecutory or grandiose delusions, often associated with hallucinations. Excessive religiosity is sometimes seen. The patient's attitude is frequently hostile and aggressive, and his behavior tends to be consistent with his delusions. . . .

But, you will say, "hallucinations" is not a diagnosis at all but merely a description. Indeed that is so, and as we shall soon see, those descriptions are all that may be warranted by the current state of knowledge.

The matter of psychiatric diagnosis is qualitatively quite different from what it is in general medicine. Diagnostic reliability in medicine is not perfect, but it has much more going for it than psychiatric diagnosis. Consider Blair's example of bleeding ulcers. It is the case, as Blair points out, that the bleeding can abate but the peptic ulcers remain. The presence of that ulcer, however, is verifiable independently of the bleeding. It is precisely because we can check urine, perform blood tests, palpate, examine reflexes, look inside, and more, that we are on considerably better ground in medical diagnoses than we are in psychiatry. To my knowledge, schizophrenia is not independently verifiable beyond what a patient says and does. Fleischman put it well: "Given our current ignorance of biochemical

As early as 1938, Boisen (*3*) pointed to the role of local convention in psychiatric diagnoses. He found that some 76 percent of patients were diagnosed hebephrenic schizophrenic in one Illinois hospital while only 11 percent were so diagnosed in another. Ash (*4*) found that three psychiatrists seeing the same male patients could agree on the diagnosis in only 20 percent of the cases. Agreement rose to 34 to 43 percent when two psychiatrists were diagnosing.

Examining the matter from a different angle, Zigler and Phillips (*5*) investigated the frequency of 35 common presenting symptoms among 793 hospitalized psychiatric patients. All 35 symptoms were found among both neurotics and schizophrenics, 34 were found among character disorders, and 30 in manic-depressives. With such overlap, how reliable or valid can diagnostic categories be? True, the investigators found many small relationships between specific symptoms and diagnostic categories, but the very triviality of those relationships itself underscores the magnitude of the problem of validity. The literature on the reliability of psychiatric diagnoses is large and spans more than three decades. Several extensive summaries are available (*6*).

It is tempting to disregard the data from our experiment and the earlier evidence and simply say that it is not diagnosis that has failed, only diagnosticians. We were ourselves tempted to that judgment during the early part of the study, before we had seen the spectrum of hospitals and diagnosticians that were included in the study. Let their characteristics speak for themselves. Eight of the 11 public hospitals conducted approved psychiatric residence programs, as did the private hospital. Three of the public hospitals were psychological research, and especially in efforts that involve participating and observing simultaneously. But some things can be said to establish the potential limits of these distortions. First, when a pseudopatient was found to have departed grossly from the formal protocol, he was eliminated from the sample (as indicated in note 6 of the article). "Subtle" departures from protocol during the admissions interview, and subsequently, would of course not be reflected in the pseudopatients' notes —that is precisely the nature of experimenter bias. The possibility, therefore, cannot be dismissed. Note, however, that visitors' observations on the pseudopatients after they were admitted uniformly disclosed no departures from role in the direction of craziness. Moreover, the ease with which psychiatric diagnoses were questioned when a pseudopatient was expected (in the "challenge" experiment) raises some questions whether the "experimenter bias" was perhaps as much in the mind of the diagnostician as it might have been in the behavior of the pseudopatient.

On another matter, a writer questions whether the pseudopatients, several of whom had no previous experience in psychiatric hospitals, were capable of making the "complex" interactional observations reported in the article. In fact, those data were obtained in four hospitals by experienced researchers who had worked in similar settings. But that need not have been the case. Counting when and how often staff come and go from the "cage" hardly requires experimental know-how. Nor does asking standardized questions at fixed intervals and observing subsequent behaviors tax the skills of even a beginning researcher. These are quite simple matters, in fact.

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As to the reality of psychological suffering, I made my views clear at the outset of the paper, in the third paragraph: ". . . Nor does raising such questions deny the existence of the personal anguish that is often associated with 'mental illness.' Anxiety and depression exist. Psychological suffering exists. But normality and abnormality, sanity and insanity, and the diagnoses that flow from them may be less substantive than many believe them to be." Let me be perfectly clear about this: To say that psychological suffering is a myth is to engage in massive denial. But to imply, as Wiedeman does, that psychological labeling does not itself create suffering is to similarly engage in denial.

Some assert that the appearance of this paper can only hurt psychiatry. The possibility that it might help psychiatry, that sensitization to issues in diagnoses and treatment might lead to beneficial change, seems not to arise. For the record, let me make clear that the theory that underlies this effort, and the report itself, do not support the vilification of psychiatric care. Psychiatry may be less knowledgeable than it believes itself to be but that is hardly surprising when one considers the magnitude of the problems which it must address. In the closing paragraph, I wrote, "It could be a mistake, and a very unfortunate one, to consider that what happened to us derived from malice or stupidity on the part of the staff. Quite the contrary, our overwhelming impression of them was of people who really cared, who were committed, and who were uncommonly intelligent. Where they failed, as they sometimes did painfully, it would be more accurate to attribute those failures to the environment in which they, too, found themselves. . . . Their perceptions and behavior were controlled by the situation. . . . In a more benign environment . . . their behaviors and judgment might have been more benign and effective."

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Communication among Scientists

The article by Griffith and Mullins, "Coherent social groups in scientific change" (15 Sept. 1972, p. 959) is complementary to a research project we are currently conducting. Communication among scientists should be of vital concern to all of us, as scientific endeavor is inextricably intertwined with the flow of information through the scientific community. We are specifically interested in the process by which "revolutionary" theories, data, and methods become known to researchers, and the factors influencing the impact of these new perspectives and information on the people concerned.

We are seeking communications from scientists who feel that they have been denied a platform for the expression of new ideas, or of valid but controversial data. All communications will be handled in a professional manner.

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Inexhaustible Energy

In his letter to *Science* of 2 March (p. 856) Alvin M. Weinberg asserts that "an inexhaustible energy source is a necessary—not a sufficient—condition for mankind's ultimate survival." This confuses the needs of mankind with the needs of an industrial system that is devoted to the endless increase of color television sets and aluminum beer cans.

Mankind managed to survive numerous millennia before the advent of energy-hungry industry. The continued survival (in reasonable comfort) of the human race entails conditions such as adequate food, sanitation, medical facilities, and effective contraception—none of which requires heavy industry