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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... HOWARD D. ZUCKER
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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 schizophrenia...
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 taking. 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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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.

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. Insurers 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 physicanly 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 then 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-
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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References


. . . 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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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 psycho-dynamic 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 pseudo-patients 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
and physiological parameters, psychiatric diagnosis may be inaccurate"—regardless, I would add, of how intuitively convinced the diagnostician is of his own accuracy.

The impression given by the letters, as by textbooks in psychiatry and abnormal psychology, is that psychiatric diagnoses are sturdy and highly reliable. The fact is that the unreliability of psychiatric diagnoses as they are commonly made has been known for a long time, so long that it is remarkable that the impression of sturdiness could have been sustained in the face of such an overwhelmingly contrary literature.

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 affiliated with university medical centers. The private hospital, while not directly affiliated, was closely associated. Moreover, it was not only the admitting officers who diagnosed. These diagnoses were confirmed by treating physicians and psychologists, in case presentation conferences and in discharge conferences.

Is it possible, as Burr suggests, that "more went on in the admitting offices than is reported" in the article? Of course it is. No one can do research in this area without being extraordinarily sensitive to the issues of experimenter bias (7) and demand characteristics (8) that are continuously present in 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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1. See U. Neisser, Cognitive Psychology (Apple- ton-Century-Crofts, New York, 1967); R. Ta-
guir and L. Petruzzo, Eds., Person Perception and Interpersonal Behavior (Stanford Univ. Press, Stanford, Calif., 1958); R. Brown, So-
2. E. Langer and R. Abelson, "A patient by any
other name ... : Clinician group difference in labeling bias" (unpublished manuscript, Yale University).
7. R. Rosenthal, Experimenter Effects in Behav-

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 con-
cern 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 "rev-
olutionary" theories, data, and meth-
ods 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 contro-
versial data. All communications will be handled in a professional manner.

GERALD DOUGLAS
CONNIE HEIMDAHL
Department of Sociology,
University of California,
Santa Barbara 93106

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 fa-
cilities, and effective contraception—none of which requires heavy industry...
on anything like the present scale. Man-
kind's ultimate survival requires no
source of inexhaustible energy other
than the sunlight that makes the crops
grow.

The argument for "inexhaustible
energy," with its inexhaustible dangers
to present and future generations, is
really an argument that the habits of
material consumption of the "affluent
society" must be maintained at all costs.
No doubt many will find it difficult to
imagine life without a car in every
garage and an electric toothbrush in every
bathroom. But life can go on without
them.  

EDWARD JAHN
Service Integration for
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Suite 450. Richmond 23219

Hydrogen and Power

Near the end of his discussion of a
"hydrogen economy," Bockris (23 June
1972, p. 1323) lists certain difficulties
that would be faced in the establish-
ment of such an economy. His concern
about the public's fear of hydrogen is
really no problem at all. The public
already accepts a natural gas distribu-
tion system that spreads throughout the
United States and the streets of most
cities. Occasionally explosions and fires
have been caused by gas leakage from
this system, but, on the whole, the
system operates both efficiently and
safely.

A primary problem in Bockris' pro-
posals could be the reluctance of the
public to give up the convenience of the
electrical distribution system that cur-
cently gives them essentially anything
they want at the flip of a switch. Al-
though a considerable amount of lit-
erature has appeared on the subject
of fuel cells, they are not yet a part
of the economy of the United States
and no one really knows what prob-
lems would arise if we attempted to
use fuel cells in large numbers to
supply the power needs of individual
homes.

Whether the difficulty Bockris lists
as "conservatism" is really a problem
depends on his definition of conserva-
tism. If he is thinking of the economic
inertia that is built into existing sys-
tems, then I agree. The power com-
panies of the United States have billions
of dollars invested in electrical distri-
bution systems, and they continue to
build more as the demand for electric
power increases. Since the supply of
natural gas is decreasing, some experi-
mentation by the gas companies appears
to be the most practical way to deter-
mine whether the system he proposes
could actually be made feasible on a
commercial basis.

C. SHARP COOK
Department of Physics,
University of Texas at El Paso,
El Paso 79968

Having easily available electrical
power is a primary aspect of the hydro-
gen economy. Indeed, one of the few
ways whereby we can easily have abun-
dant electricity in the future is by using
hydrogen as the medium for trans-
porting energy from great distances
(from large atomic reactors on floating
platforms, or solar energy farms in re-

te locations). The hydrogen could
be reconverted most efficiently by fuel
cells, or it could be used to run internal
combustion engines and local power
houses.

The fuel-cell era of 1958-1968 ended
when NASA developed fuel cells which
it considered satisfactory for auxiliary
power in space. There has been little
progress since, especially in the con-
version of hydrocarbons, because no
substitute for noble metals has been
found (1). It has also been difficult to
find a cheap catalyst for fuel cells; one
prospect is nickel.

The commercial feasibility of a hy-
drogen economy depends largely on
the cost of hydrogen at the point where
the power is used; this is the cost of
energy at the source, plus $0.001 per
kilowatt hour per 1609 kilometers over
which hydrogen is transported. Thus,
an advantage of the hydrogen economy
is that the original source could be sev-
eral thousand kilometers distant, a use-
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comparisons made in the past between
new and conventional sources will have
to be revised. In thinking of a hydrogen
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of fossil fuels at least 15 years from
now. The indication is that the cost of
energy from conventional sources will
then be several times the present cost.

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Hugh B. Donahoe, 50; associate chairman, chemistry department, St. Louis University; 11 December.

Denise Dooley, 81; professor emeritus of chemistry, College of Notre Dame of Maryland; 23 December.

Irvine C. Gardner, 83; former division chief, National Bureau of Standards, U.S. Department of Commerce; 29 December.

Samuel Koenig, 73; former chairman of sociology and anthropology, Brooklyn College, City University of New York; 29 December.

Gordon C. Lee, 56; dean, College of Education, Texas Tech University; 26 November.

Albert C. Lichte, 81; former professor of mechanical engineering, Yale University; 13 December.

Maxwell J. Littman, 58; associate professor of microbiology, Mount Sinai School of Medicine; 25 December.

Cecil C. Lynch, 68; professor emeritus of chemistry, University of Delaware; 27 December.

Nathan L. Michener, 74; former professor of pharmaceutical chemistry, Butler University; 22 December.

James L. Moore, 64; associate professor of ophthalmology, Johns Hopkins University; 16 December.

Elizabeth K. Moyer, 60; associate professor of medicine, Boston University; 28 December.

William B. Parsons, 84; retired professor of clinical surgery, College of Physicians and Surgeons, Columbia University; 2 January.

Mindel C. Sheps, 59; professor of biostatistics, School of Public Health, University of North Carolina, Chapel Hill; 15 January.

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