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* President, American Crystallographic Association, 1977.

Probability of the Pittsburgh Deaths

In attempting to decide whether the three deaths following swine flu vaccination in Pittsburgh were coincidental, Mitchell Gail (Letters, 11 Mar., p. 934) infers on the basis of a simple probabilistic calculation that "the chance that some clinic would experience three or more deaths on some day during the first week of the inoculation program is appreciable and could easily be as high as 10 percent, even if the vaccine is perfectly safe."

Gail's calculation is perfectly correct as far as it goes, but what needs to be stressed is that the result is extremely sensitive to the assumptions one makes and to the numbers one substitutes in the formulas. Furthermore, he has ignored three significant features of the Pittsburgh incident, all clearly indicated in the very first paragraph of Philip M. Boffey's article (News and Comment, 5 Nov. 1976, p. 590): the deaths were sudden, the three individuals were all inoculated within an hour of each other, and all died within 6 hours of being inoculated.

To make the discussion clearer, let us consider a 10-hour working day of a clinic divided into time intervals of duration τ (for example, 1 hour) and denote by $n(\tau)$ the number of patients of the appropriate age group who visit the clinic during one of the designated τ intervals. Let α be the mortality rate (per person per day) of the age group in question, so that $\alpha n(\tau)$ is the expected number of deaths during the day of those (in the appropriate age group) who came to be inoculated during the specified time interval of duration τ. The probability that the number of deaths will be less than three is

$$\{1 + \alpha n(\tau) + \tfrac{1}{2}[\alpha n(\tau)]^2\}e^{-\alpha n(\tau)}$$

which is approximately [for small $\alpha n(\tau)$]

$$1 - [\alpha n(\tau)]^3/6$$

and the probability that three or more deaths will be recorded in one of 1000 clinics on one day in a week is

$$1 - \{1 - [\alpha n(\tau)]^3/6\}^{700\times 10/\tau}$$

Assuming with Gail that 1000 people visit a clinic every day we can set

$$n(\tau) = 100\tau$$

and the desired probability comes out (approximately) to be

$$\tfrac{7}{6}\,\alpha^3 10^9 \tau^2$$

Gail sets $\alpha = 10^{-4}$ and $\tau = 10$, that is, a day is taken to be the basic unit, which yields 11.6 percent. This is larger than the 10.3 percent that Gail gets because the approximation used above is a little too crude in the numerical range he considers.

Since the Pittsburgh inoculations preceding the three deaths were given within an hour, a good case can be made for taking $\tau = 1$, thus decreasing the probability by a factor of 100. Also, Gail's α is an overall death rate which includes lingering causes of death such as cancer. If one focuses on the fact that the deaths in question were sudden, a decrease of α by a factor of 2 seems not unreasonable. Moreover, since the deaths occurred within 6 hours of the visits to the clinic, it seems justifiable to take α to be the death rate per 6 hours, thereby decreasing it by an additional factor of 4, which would reduce the probability by another factor of 512. Hence, we conclude that the chance that the three Pittsburgh deaths occurred by coincidence is about 1 in 500,000, rather than 1 in 10 as Gail concludes.

We are, of course, well aware that by taking $\tau = 1$ we can be accused of using selectively a posteriori information (that is, that the three who died have all been inoculated within an hour). However, by the same token, taking $\tau = 10$ constitutes selective disregard of a given piece of information.

It all boils down to a definition of coincidence, and our calculation shows that the result depends very sensitively on the definition. Our calculation can be looked upon as a simple statistical test of the hypothesis that the batch of vaccine used during the crucial hour was faulty. This justifies the use of hourly intervals as units. All in all we think that, since a reasonable definition could yield a very low estimate of the probability of an accidental coincidence, it would have been only prudent to investigate very carefully the Pittsburgh deaths.

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Labeling Theory

Jane M. Murphy's article, "Psychiatric labeling in cross-cultural perspective," (12 Mar. 1976, p. 1019) severely criticizes the "sociological" or "relativistic" approach to mental disorders. Although the article contains some valid points, the author has ignored crucial evidence which supports the "sociological" approach. Contrary to Murphy's claims, a certain percentage of American patients who have "problems in living" are labeled psychotic. These individuals would not be reliably diagnosed as psychotic in transcultural comparisons. The treatments (for example, hospitalization) which follow from their being labeled psychotic may at times be damaging. The following evidence tends to support these contentions.

1) It should be emphasized that the author's central thesis is correct: the major psychoses (schizophrenia and manic-depressive psychosis) appear to be found universally (*1*). Murphy, however, claims that "sanity appears to be distinguishable from insanity by cues that are very similar to those used in the Western world." This statement implies a universality of signs and symptoms and a reliability of diagnoses which do not exist. Recent cross-national studies of psychiatric diagnosis have demonstrated that the recognition and diagnosis of symp-

toms is not nearly so reliable (*2–4*). In a comparison of British and American psychiatrists, Kendell *et. al.* (*2*) found that three types of patients emerged. First, the two groups of psychiatrists agreed on the major diagnosis (but not on the subtype) of those patients who exhibited classic "textbook" symptoms. Second, a majority of the two groups agreed on the major diagnosis of patients who showed a mixed picture of affective and schizophrenic symptoms. Third, several patients considered nonpsychotic by the British were considered psychotic by a majority of the Americans. Kendell *et al.* concluded from this last group that the diagnosis "schizophrenia" is now used so freely in America (particularly on the East Coast) that it has become virtually synonymous with functional mental disorder. Katz *et al.* (*4*) also found that American psychiatrists tended to diagnose as schizophrenic patients whom the British diagnosed as neurotic or "personality disorder." The authors of these two studies emphasize that these differences were not merely semantic, that is, were not instances of perceiving the same symptoms but calling them by different names. Psychiatrists actually *perceived* different symptoms in the behavior of the patients.

2) Kendell *et al.* (*2*) describe the type of patient who caused the greatest disagreement as not manifesting any obvious psychotic symptoms, but showing a general inability to form lasting relationships or cope with the everyday problems of life. More than one-third of the American group diagnosed these people as schizophrenic while virtually none of the British did. Advocates of the "sociological" approach to mental disorders argue that in America many people with "problems in living" are diagnosed as mentally ill and processed in psychiatric facilities because they present a problem for society (*5*). The results cited above suggest that for a certain percentage of American patients this is true. The empirical question remains, of course, as to what percentage of American "psychotics" would not be diagnosed transculturally as psychotic and would thus be treated differently.

3) Murphy argues that societal reactions to the various forms of mental illness are universal. There is something reassuring in this claim for it implies that, if a reaction is universal, it is somehow inevitable and "right." With this argument, Murphy tries to refute proponents of the sociological approach to mental disorders. These authors argue that, because psychiatric labels are somewhat arbitrary and because they imply certain treatments, these labels and the treatments which ensue can be used to control troublesome individuals. Moreover, those labels and treatments may be extremely damaging to the patient's welfare. Such a case, of course, is dramatically depicted in Ken Kesey's novel, *One Flew Over the Cuckoo's Nest*.

Actually, the empirical evidence seems to support this view. As was argued above, psychiatric labels *are* somewhat arbitrary. It is also true that certain labels imply certain treatments. A person diagnosed as psychotic is much more likely than a nonpsychotic to be (i) involuntarily committed, (ii) held not legally responsible, (iii) forced to take medication, (iv) treated with shock, or (v) hospitalized for extended periods. Countless studies have shown that prolonged confinement in mental hospitals tends to produce chronicity, and several studies suggest that a major function of mental hospitals is to house the indigent, rootless, and helpless who have nowhere else to go (*6–10*). Indeed, the results of such studies finally prompted a movement to shift most patients to outpatient treatment, but readmission rates are high, and the problem of chronic institutional syndrome remains a major one (*7, 9*). Thus, the evidence examined above tends to support, in part, tenets of the sociological approach to mental disorders. The questions remain as to what percentage of American patients would not be reliably diagnosed as psychotic transculturally and whether these patients should even be treated in psychiatric facilities. These questions are subject to empirical investigation.

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In the societies that she studied, Murphy found a perfect correlation between crazy behavior and the application of a "crazy" label with accompanying cures and controls that were similar in different societies. This correlation does not disconfirm all theories that claim that the labeling and subsequent treatment of a person helps to produce and sustain crazy behavior. Murphy dwells exclusively on versions of labeling theory that assume extreme cultural variation. These theories predict cultures that don't show the correlation, and hence are not supported by her data. However, although cultural relativity is associated historically with labeling theories, it is not logically required in a labeling theory.

Consider the following two general hypotheses for nonrelativistic labeling theories. First, in every society unspecified interactions between innate characteristics and experiences produce some crazy behavior in people who haven't been labeled crazy. Second, these behaviors are accounted for and dealt with by people in ways that are remarkably similar in different societies. The similarity is produced by universal characteristics of practical reasoning about the causes of behavior and of social interaction and control. A complete labeling theory would specify the universal elements in detail and their interaction with unique features of a culture and with historical processes, such as the growth of science and technology. I find these two hypotheses alone interesting and of practical importance, whether or not labeling causes crazy behavior. But for present purposes add a third hypothesis that the account that people give of crazy behavior and their subsequent treatment of the person who acts crazy produces a lot more crazy behavior and powerfully sustains it. Any theory that contains these three hypotheses predicts Murphy's data. In the light of recent work on cross-cultural universals in language and cognition, the second hypothesis seems more reasonable than relativistic alternatives, which were a bias inherited from early anthropology.

Murphy's data is one more blow to

extreme cultural relativism. It suggests, not that labeling theorists should abandon their position, but rather that they should work out testable hypotheses that don't include cultural relativity as a background assumption.

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Stillings comments on the aspect of labeling which suggests that how people account for psychotic behavior and what they do about it "produces a lot more crazy behavior and powerfully sustains it." I infer that he means "a lot more crazy behavior" in people who are already disturbed, "more" than would be the case if surrounding people did not have such attitudes and take such actions concerning insanity. Townsend, on the other hand, does not contend that the wider use of the diagnosis of schizophrenia by American psychiatrists than by British psychiatrists produces more schizophrenic behavior in American patients. Rather, he suggests that the wider use may involve misapplication of a label and that the actions which result from misapplications may damage those who are falsely labeled. Stillings calls for a new kind of labeling theory, while Townsend's comment is a warning that does not, I believe, contradict my conclusions in the way that he indicates it does.

Stillings suggests that the data I presented on the seeming universality of insanity could be accounted for by a nonrelativistic labeling theory based on the universality of "practical reasoning" about the causes and control of insanity. Thus, he implies that what is universal is not insanity, but social attitudes which shape behavior so that it conforms to a universal stereotype of insanity. The main conclusion I drew in my article is that the causes of insanity appear to be ubiquitous in human groups. This certainly does not rule out etiology based on social experiences. Since the holding of attitudes and the giving of explanations are unquestionably ubiquitous features of the human condition, societal reactions are clearly one place to begin in hypothesizing how social circumstances may cause psychotic behavior.

Stripped of relativism, labeling theory would mainly have to be investigated in experimentally designed conditions, for there appear to be few "experiments of nature" which can be employed for comparative analysis. If the attitudes which produce psychotic behavior are based on practical reasoning, they should be amenable to change and modification. An educational program for producing change would be vast and slow, and the fact that these attitudes may also be buttressed by strong emotions might mean that they would be resistant to change. The magnitude of the effort does not, of course, mean that it might not ultimately be worthwhile but, if there are more readily available means of solving or reducing a human problem, it is imperative from an ethical point of view to explore them.

Labeling theorists have tended to cast their research in either-or terms regarding nature-nurture and to present the sociological approach in opposition to those approaches which consider schizophrenia as a disease which may someday be brought under control by prevention and which in the meantime may be open to certain kinds of amelioration through medication and other forms of treatment. Given the state of evidence at the present time regarding genetic, biochemical, and social correlates of psychosis, a more promising approach than labeling research may reside in strategies which focus on the interaction of factors.

Townsend agrees with me that the pattern of mental illness we call schizophrenia is found in most if not all cultural groups. It should be noted, however, that he attributes to me statements about the universality of manic-depressive psychosis which I do not make. I indicated that the definitions of insanity in the groups I studied resemble schizophrenia. The pattern of alternating elation and depression which is characteristic of manic-depressive psychosis was not described to me; nor was it isolated as a pattern in a similar study carried out by Edgerton among four tribal groups in East Africa (*1*). Edgerton, like myself, draws a parallel only to schizophrenia. This does not mean that manic-depressive psychosis does not exist in these groups. It does suggest that the lay public in these non-Western groups describe patterns which might reflect the manic phase of such a process but they do not focus on alterations in behavior and mood in one person over time. This is clearly a topic which needs more research.

The main point made by Townsend is that I ignored evidence on the lack of transcultural agreement about the definition of psychosis, evidence which he interprets as showing that in America, especially, people are damaged by being put into hospitals. I have no data comparing hospitals to communities as environments, but the information I offered can be interpreted as a caveat that communities are not necessarily better. Also, it does not seem appropriate to suggest that *One Flew Over the Cuckoo's Nest*, a work of fiction, gives reliable information on the adverse effects of hospitals in America.

The studies Townsend describes on differences between British and American diagnostic practices grew out of a series of epidemiological comparisons; a pertinent analysis is that by Kramer (*2*), who showed that in 1956 and 1957 the first admission rates for schizophrenia were higher in the United States than in England and Wales, while the reverse was true for the affective psychoses. The first admission rates for all diagnoses were, however, comparable (125.8 per 100,000 in Britain and 126.5 per 100,000 in the United States). This reflects differences in diagnoses, but it does not support Townsend's view that the broader definition of schizophrenia was a vehicle by which more Americans were treated in hospitals than elsewhere. Further, in the United States 75 percent of all patient care episodes were in inpatient services in 1955, while by 1971 this had decreased to 43 percent, with comparable changes regarding outpatient care (*3*). Also, the first admission rate for schizophrenia to state and county mental hospitals fell from 21 percent of all first admissions in 1962 to 13.9 percent by 1972 (*4*). These trends and comparisons do not bear out the picture Townsend gives of what is going on in the United States.

The Kendell, Cooper, and Gurland studies on British and American differences contributed to an ongoing multinational effort sponsored by the World Health Organization to standardize psychiatric diagnoses. These efforts are based on recognition of national, regional, and historical differences in the nomenclature employed and the training provided within the field of psychiatry. However, one can question whether variations of these "within culture" types invalidate the concept of insanity and whether they suggest that the definition of insanity is culturally arbitrary.

I emphasized that the Eskimos and Yorubas with whom I worked conveyed the idea of a pattern or a model of behaviors as representing their view of insanity and that among those identified as insane some individuals fit the pattern in several dimensions, but none was called insane on the basis of one component only. This is similar to what goes on in innumerable classification and diagnostic procedures; that is, judgments are made regarding how well or how poorly the empirical

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LETTERS

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phenomena fit the mental image of a pattern. It is the pattern in its overall outline which seems to me to be the same in different cultural groups, including our own and other industrial societies. This is also the conclusion drawn in the multinational studies which stemmed in part from the British-American comparisons (5). Given the fact, however, that a pattern is involved, it is open to variable interpretation following "schools of thought." It seems to me that the influence of such "schools of thought" explains historical and regional variations better and is a more logical interpretation of the American and British differences than a basic cultural difference in the definition of insanity. Further, the borderlines and the gray areas of a pattern are undoubtedly troublesome. They invite the "reading into" which reflects the emphasis of one's training in this or that school of thought. This is partly why borderline cases are differentially perceived, as Townsend indicates. It is especially regarding gray area phenomena that effort to standardize and refine diagnostic criteria is needed if the adverse consequences which Townsend points to are to be avoided.

Last, Townsend suggests that I encourage complacency about these issues and that I imply that what is universal is reassuringly "right." In the published version of my article I followed what seemed to me the wise counsel of an editor and removed a final statement of what I consider to be the implications of the research I reported. They were, however, opposite to what Townsend supposes them to be. The main implication I see is that psychosis is an exceedingly serious affliction in all the many places around the world where it is found; it deserves the soundest possible research from as many points of view as can reasonably be seen to lead to humane treatments and ultimately, it is to be hoped, to control and prevention.

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