LETTERS

In Defense of Frogs

I can not resist putting in a few words in defense of frogs. The article "Evolution in DNA: Changes in gene regulation" by Gina Bari Kolata (Research News, 8 Aug. p. 446) includes the statement, "However, Wilson and his associates report that frogs, which are anatomically simple organisms, exhibit fewer chromosomal changes than mammals, which, of course, are more complex."

Frogs and mammals are chordates, vertebrates, and tetrapods, with all that this implies, and examination and dissection will reveal considerable homology in the anatomy of the adults of both groups. The frog embryo develops into an aquatic free-swimming tadpole, a stage that lasts about 3 months in *Rana pipiens* before metamorphosis to the adult form occurs. At metamorphosis, the aquatic, gill-breathing, herbivorous tadpole changes into a terrestrial, lung-breathing, carnivorous tetrapod. Metamorphosis involves considerably more than the resorption of the tail and the development of legs and lungs. For example, nitrogen becomes excreted primarily as urea rather than ammonia, the visual pigment changes from porphyropsin to rhodopsin, and the hemoglobin changes to a type showing a decreased affinity for oxygen and the Bohr effect. Thus, the tadpole has structural and biochemical adaptations also observed in freshwater fishes; the adult frog, structural and biochemical adaptations typical of terrestrial tetrapods. The mammals, on the other hand, have no free-living stage comparable to that of the tadpole.

The point is, of course, that in addition to the genes programming for early differentiation of the embryo, there must also be genes controlling the development of the tadpole. These, in turn, are superseded by genes controlling metamorphosis to the adult frog. With regard to the life cycle, it can easily be argued that frogs are anatomically more complex than mammals. In addition, adult *Rana pipiens* in Minnesota spend almost half the year overwintering in lakes and streams and only half the year as terrestrial animals. Again the life history suggests that the necessary genetic controls may be more complex in frogs than in mammals.

A final comment on another statement in Kolata's article: "Sibling species, unlike human beings and chimpanzees, are virtually identical morphologically." If I were a fruit fly working on primate taxonomy, I might consider humans and chimpanzees to be sibling species. Apart from minor differences in size, brain size, hairiness, and opposability of the great toe, they are, after all, quite similar.

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Social Policy and Mental Illness

Arnhoff's article "Social consequences of policy toward mental illness" (27 June, p. 1277) is valuable because it focuses on costs not usually considered in individual patient care. Specifically, these are the consequences for the family of maintaining the person in the community. Unfortunately, this notion is embedded in a context which may be misleading to the less informed and casual reader. The following comments are offered as a corrective.

Arnhoff proposes that the stability of rates of psychoses through time and culture, in conjunction with concordance studies, should disallow the continuum hypothesis of behavioral dysfunction and minimize the value of early intervention programs that are indiscriminately applied.

First, Arnhoff states that rates of psychiatric hospitalization in *Western* (emphasis added) societies have remained stable. The *Western* should not be taken lightly. Torrey (*1*) recently reviewed the question of the universality of schizophrenia—the largest category of the major psychiatric disturbances—and found that schizophrenia is observed in all cultures exposed to Western industrialization and technology. He is more cautious in assigning a universal prevalence to schizophrenia, not because it is impossible, but because there are insufficient data to warrant that conclusion.

Arnhoff also offers in evidence the stability of armed forces hospitalization rates for psychoses from World War I to World War II. Yet, in Coleman's review (*2*) he reports 39 percent of World War II medical discharges were psychiatric; 27 percent of the Korean War medical discharges were psychiatric; and, in the case of the Vietnamese war, the number of psychiatric discharges was insignificant. While these data are not restricted to the major psychiatric disturbances, they demonstrate how changes in screening, combat preparation, and rest and recreation practices affect reported rates of disturbance.

The confidence that may be placed in rate data is limited by the reliability of diagnosis. Zubin (*3*), in his review of the literature concerned with this problem, reports a great diversity in diagnostic agreement as well as diagnostic persistence through time, both for schizophrenia and for the affective psychoses. Reviews of cultural differences in the application of the psychiatric nomenclature display still more divergencies (*4*). For example, British clinicians produce greater consensus, but list fewer symptoms than do American workers and seemingly have a preference for the affective disorders. In brief, since known problems exist in psychiatric diagnosis, we must exercise care and skepticism in discussions of prevalence rates.

With regard to the continuum hypothesis, some investigators are persuaded that the clinical entity conception of schizophrenia is not as valuable as one which focuses on the psychological dimensions of response latencies, perceptual phenomena, and psychophysiological activity, which may be disrupted on occasion in all people (*5*).

Finally, Arnhoff is correct that most workers accept the involvement of heredity in the development of severe psychiatric disorder, particularly in the schizophrenias and the manic-depressive psychoses (*6*). Yet, the acceptance of this view does not necessarily exclude either the continuum hypothesis or early intervention procedures. A polygenic view which is compatible with the diathesis-stress model (*7*) would allow many different combinations of a set of genes interacting with many different combinations of environmental experiences, and would produce a continuum of life outcomes from severe and chronic disorganization of the reality processes to a fruitful, creative, and fulfilling existence. Early intervention treatments are aimed at the development of social resources and environmental changes that support healthy behavior. This is precisely the difference in emphasis between "mental illness" and "mental health," which Arnhoff dismisses as a euphemism.

Arnhoff informs us that the interaction between institutional quality and patient illness has gone unrecognized in the formulation of current policy. He reports that private mental hospitals are frequently able to provide quality care and avoid the institutionalization fallout. He suggests instead that the logical fallacy "if bad hospitals are bad for patients, then all hospitals are bad for patients" has prompted the reduced involvement of hospitals in the care of patients. However, access to quality care is largely determined by economic resources. The urban lower classes produce more schizophrenics (*8*), and they are the people most likely to receive bad treatment during hospitalization. Any description of the deleterious consequences of hospitalization without consideration of the socioeconomic context of the patient is an

unfaithful portrayal of both current psychiatric practice and the social forces that induce change.

Arnhoff reports a contradiction in which patients are treated in the setting which presumably instigated their disability. The major problem for any psychological treatment is generalization—whether it be from the institutional setting to the noninstitutional, or from the consulting room to the living room. Behavior change must be produced in the nonasylum situation, by the reduction of debilitating symptoms, the increase in coping skills, or both. Sufficient data have been accumulated to show that immediate environmental contingencies are powerful determinants of behavior, even among patients with chronic psychoses (*9*). Difficulties encountered with the transition from an institutional to a noninstitutional setting may be lessened by carrying out treatment interventions in the community whenever possible. Arnhoff expresses disappointment that halfway houses and sheltered workshops are shunted aside in the public clamor. These programs are carried out in the community.

Arnhoff's point that benefits to the patient alone can no longer be the criterion for care when there is a possibility of untoward effects on others is valid. Yet, the consequences of a psychiatrically disturbed parent in the family are not clearly defined, nor are they uniform (*10*). Arnhoff concurs with this when he says that ". . . mentally ill parents can and do provide care which ranges from essentially sound to the other extreme of terrible neglect and trauma."

Arnhoff's implicit leap to the speculation of eugenic solutions is particularly disturbing. Diagnostic unreliability, effective schizophrenic parenting, invulnerable children, and socioeconomic, status-linked, risk rates should prompt other approaches before we counsel people on mate-selection, prohibit sexual actitivy, or, at the extreme, engage in sterilization procedures. An affirmative response to genetic engineering, on the basis of the available data, places the burden for psychiatric disability upon the patient and his family. Caplan and Nelson (*11*) identify the implications of person-versus situation-centered problem definitions in the development of social policy planning. Person-centered explanations, in which disabilities are said to originate in the individual, deflect attention from the social system's failure to cope with the problem and instead place emphasis upon person-change rather than system-change solutions. As Caplan and Nelson indicate, the issue is not whether explanatory variables are located in the person, or that constitutional differences among people are real and active determinants of behavior, but rather it is to underscore the fact that important human consequences are attached to problem definitions. With regard to the alarming specter of the poised "thousands of schizophrenics . . . now in the community with biological capability of reproduction," the Supreme Court has ruled that patients may no longer be confined without treatment. Justice Stewart has informed us that ". . . the state (may not) fence in the harmless mentally ill solely to save its citizens from exposure to those whose ways are different . . ." (*12*).

"Indiscriminate community treatment" should be rejected as should any indiscriminate treatment. The costs of psychiatric disability upon the family and the larger community should be reflected in the evaluation of treatment effectiveness. Ultimately, the need for monitoring of service is constant, and we must also remain cautious in offering assistance to others, because the consequences for freedom and choice are great.

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11. N. Caplan and C. D. Nelson, *Am. Psychol.* **28**, 199 (1973).
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Arnhoff omits mention of a psychiatric treatment modality, now in use as one ingredient of a comprehensive approach to the major psychoses, particularly schizophrenic disorders in young people, which may well remedy some of the systematic inconsistencies and shortcomings in current mental illness policy which he details. Psychotherapy with the family, during hospital treatment of the identified patient and continuing into the transitional period following discharge, or carried out simultaneously with effective outpatient treatment, appears to fulfill two important desiderata stated by Arnhoff: (i) "the need to abandon the individual patient model in favor of a more extensive, complicated (and costly) systems model"; and (ii) dealing with the "conceptual inconsistency of treating the patient in the very environment that is seen as the cause of his problems." Whatever one's etiological views, treatment of the family in coordination with full-scale treatment of the patient provides a means to remedy both the accrued adaptive deficiencies of the patient *and* the failures and weaknesses of the family system that have facilitated the overt illness, or have developed in step with the unfolding disorder. As with any psychiatric treatment technique, the suitability of various available family approaches for various types of malfunctioning of the family in its supportive, socializing, and humanizing tasks needs to be defined; it appears that this may occur more along the lines of family typology than of patient diagnosis.

That this rapidly expanding item in the psychiatric armamentarium, with a substantial clinical and theoretical literature and rationale, should have been neglected by Arnhoff is perhaps understandable. Much of the impetus for the use of family study and treatment has come from private psychiatric hospitals (*1*), which as he states have often provided the models for what can be accomplished in care and treatment in institutions, and has come from there with good reason, in that the numbers, levels of training, and experience and professional stimulation of the staff of these hospitals can be kept at a high (and costly) pitch. When public policy encourages similar investment in the care and treatment of the large numbers of psychotic patients whose plight Arnhoff describes, a genuinely full spectrum of services may emerge, starting with the patient and his immediate environment, the family.

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Gardner's note is quite useful in pointing to family therapy for the major psychoses as a recognition of assistance beyond the patient per se. Its usefulness is further enhanced by the comments in the last paragraph detailing the public policy aspects and implications.

As for Barocas's commendable heuristic concerns, I will attempt to briefly respond to each. Initially, and then later, he refers to my comments on the "stability" of psychosis rates. However, what I did state (p. 1277) was, ". . . that there was no evidence

to support the *belief that psychosis was increasing* [italics added] in modern society, and no systematic data have yet appeared to alter . . . [this] conclusion." The subtlety of this distinction could be missed by the casual reader. The italicizing of *Western* by Barocas so it "should not be taken lightly" adds nought to the original, since throughout the discussion, I repeatedly mentioned Western society to specifically indicate that no attempt was being made to discuss or imply the biology of the species. Barocas agrees that insufficient data exist to make such comparisons, and I fail to see how I can discuss or make comparisons to nonexistent data.

After readily accepting Torrey's very useful discussion of schizophrenia across races, cultures, and nations (factors which are literally unmanageable sources of unreliability), Barocas then raises the issue of the unreliability of psychiatric diagnoses—a notorious and tedious, but valid, issue that has plagued all of us doing research and study in this area these past 25 years. Yet these are the data we have, so either we cautiously use them or we accept the "no data" beliefs and conjectures of those who state that psychosis is on the increase.

Barocas's comments and additions regarding rates of armed forces psychosis deal with psychiatric discharges and psychiatric disturbances in general, not psychosis *rates*. We are well aware of the changes in practice in triage, treatment, and so forth, but I discussed *rates* of psychosis according to available data. Considering the extensive literature on psychiatric epidemiology, it is difficult to understand Barocas's utilization of summary reports and an introductory textbook on abnormal psychology for an evaluative reference base. While he mentions British versus American diagnostic differences, he glosses over the greater reliability of the British psychiatrists. The pioneering work of Zubin and others in the joint United States–United Kingdom project mentioned by Barocas demonstrated what can be accomplished in this regard with assiduous training and study and reflects a frustrating neglect to date by American psychiatrists.

As for the comments regarding factors influencing access to treatment, these were beyond the scope of the article. Even cursory familiarity, however, with the extensive psychiatric and sociologic literature on health-care utilization and delivery indicates acceptance of the role, not only of economic resources, but also of social class, geography, transportation, population density, religious beliefs, cultural values, tolerance of deviancy, and so forth, and so forth. But I was merely discussing hospitalization effects per se, not why and how they get there to begin with.

As for the observation that halfway houses and sheltered workshops are in the community, Barocas does not note my discussion of a continuum of treatment and the "indiscriminate" all-or-none utilization of community treatment—key points in the article—until his final paragraphs.

Regarding early intervention programs that support "healthy behavior," this value-laden concept, along with "positive mental health," suffice it to say, has not produced definitions which are even reasonably reliable, valid, or measurably precise. No systematic data have appeared to support claims of primary prevention, and precious little controlled data have been found to show prevention of much of anything. Yet, such program developments and acceptance are precisely the sort of value-laden, politically initiated, global efforts the article deals with and which continue to divert resources, funds, and manpower from the care and treatment of major mental illness. As to my dismissal of "mental health" as a euphemism, again, this nowhere occurs in the article. Let reader Barocas consult my article and the cited references to understand the historical context in which the term arose, and whence its usage spread, as it was stated by me.

As for an "implicit leap to the speculation of eugenic solutions . . . ," I neither inferred nor suggested all the evils that seem to disturb Barocas. A careful reading of my article would show that my interest lies in *less* rather than *more* ill-conceived social meddling. A paragraph, however, which might have been more helpful to Barocas, regarding the need for birth control advice, counseling, and so forth for young psychotics, was deleted in the editing process.

The recent Supreme Court decisions mentioned by Barocas are laudatory for their protection of civil liberties, but have absolutely no relevance to what I stated, again unless Barocas believes not discussing these decisions implies advocacy of involuntary commitment, indentured servitude, and so forth. His point does reinforce the article's theme of the shabby treatment, or nontreatment, in the publicly supported hospitals.

Essentially, my article detailed what might be facetiously titled: "There's no such thing as a free lunch"—an idea which to many is abhorrent, since it directs thought to unpleasantries which ultimately are paid for. While conclusions other than those the writer intended may always be drawn, an author always hopes that the reader is able to keep clear what is author-stated and reader-interpreted.

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