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 These, in turn, are superseded by genes controlling metamorphosis to the adult frog.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.

DAVID J. MERRELL
Department of Zoology, University of Minnesota, Minneapolis 55455

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