with ours, those in the nucleus accumbens are about ten times higher than the values we obtain. This therefore stresses the need for standardized dissection of the human brain or an anatomical description of a region to accompany neurochemical data. Such differences in dissection procedure and other possible technical factors (*3*) may explain our failure to confirm the increase in NE content of ventral septum in schizophrenia reported by Farley *et al.* (*1*). Since three of their four cases had committed suicide their results may not be typical of the larger psychotic population that we have sampled. We concur with the suggestion of Farley *et al.* that it will be important to examine a larger series of cases and agree about the difficulties of interpreting changes in catecholamine content in the brains of patients with schizophrenia who have received long-term treatment with neuroleptic drugs.

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3. Our results are expressed per gram of protein, since we find variable water content in paraventricular regions in frozen brain tissue.

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The report by Farley *et al.* (*1*) on the norepinephrine patterns in four brains from patients they diagnosed as paranoid schizophrenic is clinically an anachronism. Diagnosis was made by Bleuler's criteria, which are really no criteria at all as they have no reported reliability or validity. It is also unlikely that of four "schizophrenics" three committed suicide, a sequela more suggestive of affective disease. Farley *et al.* could have used one of several sets of modern research criteria (*2*), each supported by reliability and validity data. Their failure to do so has left us with a non sequitur of "hard data."

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We fully agree with Taylor (*1*) that criteria other than Bleuler's could have been used in classifying our psychotic patients. We doubt, however, that this would have influenced the significance of our biochemical study (*2*). The fact that in all of our four patients the direction as well as pattern of limbic norepinephrine (NE) changes was the same strongly argues in favor of diagnostic homogeneity of the patient material studied. This is all that a valid clinical classification, be it Bleulerian or otherwise, can possibly be expected to provide—considering the arbitrary nature of all presently available diagnostic criteria for schizophrenia. In respect to Taylor's remark on suicide and schizophrenia, we fully disagree. First, it is textbook knowledge that in schizophrenia suicide is not uncommon, with "more schizophrenics than manic-depressives commit[ting] suicide" (*3*). Second, and more to the point of studies such as ours, postmortem brain studies in a per se nonlethal illness such as schizophrenia heavily depend on suicide material that is autopsied in the medical examiner's office and therefore more readily available (and in a better condition for postmortem studies) than cases dying of natural causes in chronic psychiatric institutions.

We could not agree more with Bird *et al.* (*4*) regarding the need for a standardized dissection procedure of human brain as the basis for meaningful comparisons of biochemical data obtained in different laboratories. As they correctly observe, their and our areas of nucleus accumbens almost certainly represent two anatomically different entities; this also applies to the area of ventral septum, which, in our dissection, is identified by its characteristic topographic relation to what we define as nucleus accumbens. In view of these crucial differences regarding anatomical definitions, the failure of Bird *et al.* to find significant changes in NE levels in these two regions, as defined in their dissections, has no bearing on our positive results (*2*). Although our sample size of schizophrenics was small, it was diagnostically homogeneous (as discussed above). Most important, the measured differences, especially in the bed nucleus of the stria terminalis and the ventral septum, were large and statistically highly significant. Although using large numbers of cases may make small deviations from normal statistically significant [as the relatively small increase of dopamine in nucleus accumbens in schizophrenics shown by Bird *et al.* in their table 1 (*4*)] the pathophysiological significance of a given (biochemical) alteration is not determined by sample size but by the degree of the observed change. We enviously congratulate Bird *et al.* on the large number of schizophrenic cases that they had been able to collect and analyze. Since the hospital diagnosis of schizophrenia was sufficient for inclusion in their study, their case material, in sharp contrast to our material, probably represented a highly interesting collection of several subtypes of schizophrenic illness. It would not be surprising if by appropriate subgrouping of their case material, a different and more interesting biochemical picture emerged than that shown in their table 1. In this respect, it may be relevant that Kleinman *et al.* (*5*) reported above-normal NE levels in the nucleus accumbens of two paranoid schizophrenics; similarly, Carlsson (*6*) observed highly significant above-normal mesencephalic NE levels in three cases of paranoid schizophrenia. Neither group found any NE abnormalities in nonparanoid schizophrenics or other psychotics.

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