Letters

Psychoanalysis

In his review (20 May, p. 1078) of J. Allan Hobson's *The Dreaming Brain* (*1*), Robert Y. Moore states that "psychoanalysis, at least in the strict interpretation of that term, has largely disappeared from American psychiatry." That is startling news to this American psychiatrist.

If by "strict interpretation" Moore means only that most psychiatrists are not formally trained psychoanalysts, then that has always been true, so nothing has "disappeared." On the other hand, he is mistaken if he believes that psychoanalytic thought is not an important part, if not the most important part, of the psychodynamic training and practice of most psychiatrists. Concepts such as transference and resistance, repression, and the unconscious motivation of emotion and behavior are central to the daily work of most American psychiatrists I know, and they continue to be central to psychiatric education as well. Of course, psychiatric science has been profoundly expanded by the more recent advances in neurobiology. But a knowledge of, say, serotonergic mechanisms in the central nervous system only enhances, rather than supplants, the understanding of the mind gained through other approaches, including psychodynamic studies. And, certainly, the dispensing of a tablet to alter those serotonergic mechanisms is a far cry from an adequate treatment of a depressed human being.

As for psychoanalysis in its "strict interpretation," psychiatric residents, medical students, and trainees in other mental health disciplines in programs with which I am familiar are showing increasing interest in psychoanalytic thought and practice. Psychoanalytic institutes continue to attract bright and creative people for training in research and clinical work. Even academic psychology, after decades of antagonism, is developing a new interest in the unconscious mind, and many psychoanalytic findings are being substantiated or supported by the methodologies of "hard science."

Moore seems to look favorably on the proposition (more philosophical than scientific) that brain equals mind. He quotes with approval Hobson's comment that "a complete description of either (brain or mind) will be a complete description of the other (mind or brain)." My own familiarity with the literature does not convince me that we are very close to a complete description from either side of the duality. It is difficult to imagine that we will ever reach the day when the movements of molecules across cell boundaries will be all that needs to be said about human love, hate, envy, and joy. To my mind, that would be an incomplete description indeed.

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Adrenal Medulla Grafts in Animals

Several interesting issues were raised in Roger Lewin's Research News article "Cloud over Parkinson's therapy" (22 Apr., p. 390). I do not, however, agree with the statements suggesting that animal studies indicate that adrenal medulla transplantation gives poor results, that the grafted cells usually do not survive, and that most researchers have shifted their efforts to nerve cell transplantation. Survival of adrenal chromaffin cell grafts is very dependent on the technique used for transplantation. For example, transplantation to the parenchyma of the corpus striatum results in poor tissue survival (*1*). On the other hand, when adrenal medulla is transplanted to the walls of the lateral ventricle, or when these grafts are exposed to nerve growth factor, substantial numbers of chromaffin cells can survive (*2*).

Techniques for adrenal medulla transplantation that result in good graft survival also can produce significant behavioral effects. Although these behavioral effects are limited in degree, the efficacy of embryonic tissue grafts in animal models of Parkinson's disease is also limited. Most animal studies suggest that embryonic tissue transplantation would ultimately be the more effective procedure, but there are ethical issues and practical problems with this approach. Until these can be reconciled, efforts to maximize the efficacy of adrenal medulla transplants for the treatment of Parkinson's disease should not be abandoned.

The early enthusiasm over the benefits of the current adrenal medulla transplantation procedure for the treatment of Parkinson's disease was undoubtedly excessive. The present negativism may also be overstated. Animal studies, in fact, suggest that adrenal medulla transplantation will produce a partial effect and that they will neither entirely "succeed" nor "fail." Time and further investigation will reveal whether adrenal medulla transplantation produces sufficient clinical benefit to justify the associated risks. Meanwhile, no reasonable person should expect that this surgical procedure will result in a complete "cure" of Parkinson's disease.

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Federal Budget

Daniel E. Koshland, Jr.'s editorial "Setting priorities in science" (20 May, p. 965) demonstrates the lasting damage of the Reagan years. Koshland, Frank Press, Robert Rosenzweig, and others appear to have accepted as a starting point that "the discretionary part of the federal budget shrinks." Koshland recommends setting up new rules of combat for a shrinking pie.

The truth is, the "discretionary part" omits the defense budget—the largest part of that budget—and considers new revenues out of the question, regardless of source or need. The Gramm-Rudman-Hollings Act takes on a stature greater than any other law only because it is unchallenged by those who believe they can prevail under its rules.

It is time for the science community to talk about goals and opportunities—not budget cuts and project cancellations. The American people can be mobilized to support exciting new projects, and Congress can find funding for them. Perhaps the zero-sum game believers should get outside the Washington, D.C., Beltway for a few months and learn how to serve a growing population and expanding economy—which is not a zero-sum society.

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Crisis in Systematics

James H. Oliver, Jr.'s Policy Forum "Crisis in biosystematics of arthropods" (20 May, p. 967) would have the scientific community do the right deed for the wrong

reason. Systematics is not simply the identification of specimens, it is the reconstruction of evolutionary history; and it should be supported not just because it has practical applications (which it does) but because it is a fundamental scientific discipline.

Systematics is important because all living things are the product of history, and we can understand little about the diversity of organisms without knowledge of their history—the phylogenetic knowledge provided by systematics (*1*). One might just as well try to understand the current political conflicts in Central America or the Middle East without understanding the history of the people of those areas.

Oliver correctly deplores the placement of major systematics collections in "maintenance storage," and we further deplore the disposal of other collections (*2*) and the retreat of university biology departments into nonevolutionary disciplines. Many of the biological "laws" that such disciplines discover may be bounded by clade and place, and only systematic research—research on evolutionary history—can discover those bounds.

Other recent calls for the support of systematics (*3*) have also emphasized its practical and cataloging aspects. We agree strongly that systematics is insufficiently supported, but we fear that these arguments based exclusively on "usefulness" will backfire in the long run. Systematics will not attract the brightest students—the true innovators of theory and practice—if it is portrayed as an identification service. Systematics must command attention because of the intellectual challenge it represents, in and of itself, as the study of evolutionary history.

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1. For examples of the importance of phylogenetic knowledge to other disciplines, see J. A. Coddington, *Cladistics* **4**, 3 (1988); J. Felsenstein, *Am. Nat.* **124**, 1 (1985); W. L. Fink, *Paleobiology* **8**, 254 (1982); G. V. Lauder, *ibid.* 7, 430 (1981).
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4. We thank G. C. Mayer and E. E. Williams for their comments.

Mesoamerica, Not the New World

I was enjoying Roger Lewin's essay on agricultural origins (Research News, 20 May, p. 984) until I found myself misquoted. Lewin quotes me as seeing no evidence "anywhere in the New World" to suggest that population pressure was responsible for the beginnings of agriculture. What I said was that I saw no such evidence anywhere in *Mesoamerica* (a culture area stretching roughly from central Mexico to Honduras).

I would never have extended my comments to the whole New World, because I know that some areas, such as the Pacific Coast of Peru, had much higher population densities than Mexico. It is still too early to rule out population pressure in Peru.

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