place on the agenda of the forthcoming UNESCO meeting in Paris 4–13 September, where the major objective will be to seek a scientific basis for the rational use and conservation of the resources of the biosphere.

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Contracting Policies of Artificial Heart Program

In his letter "Bioengineering contracts slight universities" (28 June), Angelakos cites the example of some contracts awarded by the Artificial Heart Program of the National Heart Institute in his discussion of the desirability of developing bioengineering groups within the university framework. I would like to present some facts that bear on his assessment of the problem.

1) Angelakos mentions grants and contracts of the Artificial Heart Program; this program awards contracts only. Another part of the National Heart Institute awards grants, a number of which are to academic institutions in support of work in the artificial heart area. It is worth noting that academic institutions may pursue both the grant and contract route or may elect to eschew contracts; industrial organizations are not eligible to receive NIH grants, so their only route is the contract one. It is possible that this sort of self-selection may play a role in the interest in and distribution of contracts.

2) Angelakos mentions the possibility that one factor in the apparent lack of participation in such contracts by academic institutions may be the limited dissemination of appropriate information among universities. The mailing list of the Artificial Heart Program has on it the name of every medical school in the country, many of the engineering schools, departments, and institutes, as well as a large number of individuals at these academic institutions.

3) All proposals for contracts from industrial and academic organizations are subjected to exactly the same competitive review: In the first of the three-stage review every artificial heart contract proposal goes through before contract awards are made, a scientific and technical evaluation is made by an *ad hoc* group of knowledgeable experts, all of whom are from universities or government, and none of whom is ever from industry; this would imply, I believe, no anti-university bias in our review process.

4) It is not true that "less than 20 percent of the funds [in $3 million of contracts] went to university laboratories." Many of the contracts and funds awarded to industrial organizations involve the participation, either through subcontracts or consultations, of academic institutions. The list that Angelakos based his comments on did not contain such information, since its purpose was merely to list the identities of the principal contractors.

5) Of three contracts awarded for evaluation of physiologic effects of cardiac assist devices, it is true, as Angelakos points out, only one went to a university laboratory; in one of the others, however, a cardiovascular surgeon from a nearby eminent medical school plays a major role; in the other, a number of persons from the medical staff of the Artificial Heart Program Office have been participating very closely in the studies. What Angelakos did not mention, because it was not included in the list he based his reactions on, is that these three contracts are in addition to two others that were awarded 1 year earlier and are still in effect; one is to a medical school and the other to an industrial organization that has a very close working relationship with one of the leading academic hospitals in the country. Furthermore, all five of these contracts are operating according to protocols and plans developed by the Artificial Heart Program staff in consultation with a task force composed of academic, medical, and engineering people.

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Behavior: Questions of Influences

Perhaps a polemic book deserves a polemic review. At least the combination is not an unusual one: Witness E. W. Hansen's comments on Zing-Yang Kuo's controversial book *The Dynamics of Behavior Development* (5 April, p. 58).

Most of us would agree that the positions attributed to Kuo should be roundly criticized. Doubtless Kuo himself would concur. It is an unhappy fact that the major criticisms in the review were directed at positions that were not expressed, or implied, by the author.

In fairness to Kuo, it should be observed that his assumption that behavior development is a continuous process does not require that, as Hansen puts it, "everything that occurs at a given point in time is equally important in influencing everything that is to occur in the future." On the contrary, significant portions of the volume are concerned with factors that make for a loss of plasticity of behavior during ontogeny. Notwithstanding the assertions in the review, Kuo shows a clear appreciation of the selectivity and specificity of environmental effects upon behavior. The fact that Kuo reports several experiments on the specificity of early experience for later behavior scarcely indicates a "cop-out" on this issue.

Kuo does not claim, as Hansen says he does, that it will be possible "to control the evolution of the organism in the future 'independent of somatic changes . . . .' " Apparently this statement was taken from Kuo's discussion on page 203 of the issues involved in the evolution of behavior, not of the organism. For Kuo, there is a difference. Briefly, he argues that certain species-atypical behavior patterns (diet preferences, habitat selection, agonistic responses, and the like) can be established by controlling the environmental context in which the young animal is reared. And, "As long as the general nature of the new environmental context remains relatively unchanged, despite inevitable variations, we may expect that the newly induced behavior patterns, or behavioral neo-phenotypes of the group as a whole would be carried on from generation to generation" (p. 201). This is hardly a "mystical" process, especially in the light of Kuo's own experimental demonstrations of how contextual factors and the behaviors of the maternal animal play significant roles in the shaping of the responses of the young (see, for example, pp. 66–82).

Does the volume offer anything new? Remarkably, the review overlooked the fact that the book contains the reports of more than a score of previously unpublished experiments on the development of such diverse activities as appetite preferences, locomotion and flight patterns, fighting, sexual behaviors, vocalization, and song acquisition. Though the work covers a 40-year span, it is neither prosaic nor a "rehash." Included in this collection, for instance, is one of the few demonstrations of the

contextual relativity of social behavior of dogs that has appeared. Although I would have liked to have seen more of the studies reported in greater detail, sufficient information is given to permit attempts to extend the work (*1*).

Clearly the volume is not free of defects: Kuo is polemical, sometimes to a fault; he fails to relate his ideas to recent movements in ethological and learning theory; there is a lack of precision in his use of some concepts, making them vulnerable to misinterpretation. Nevertheless, it is a major statement of position from a distinguished comparative psychologist on issues that are still very much alive. To this reader at least, Kuo's insights (and research) continue to be provocative and stimulating.

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References and Notes

1. Findings generally consistent with Kuo's results have already appeared. See, for example, G. M. Burghardt and E. H. Hess, *Science* **151**, 108 (1966); R. B. Cairns, *J. Comp. Physiol. Psychol.* **62**, 298 (1966); R. B. Cairns and J. Werboff, *Science* **158**, 1070 (1967); V. H. Denenberg, G. A. Hudgens, M. X. Zarrow, *ibid.* **143**, 380 (1964); H. Goot, *Anim. Behav.* **10**, 232 (1962).

A careful reading of Kuo's book will indicate that my review dealt directly with the major positions adopted by the author. My basic objections to the book are not to the positions themselves but to the extreme form in which Kuo would have us adopt these positions. The framework provided by Kuo is a framework within which the science of behavior cannot proceed, and this was the theme of my review.

In response to Cairns's first specific comment, my reference to a "cop-out" was a reference to Kuo's handling of criticisms of the relationship between movements before hatching and later behavior. This example indicated to me that Kuo carries to an extreme the position that behavioral development is a continuous process. I think that if we carry things to the extreme that this example suggests, hypotheses concerning specific relationships (for example, the relationship between embryonic actions of the chick and later specific behavior patterns) are exchanged for vague and certainly much less meaningful statements such as the one made by Kuo on page 114.

Just as the prenatal visual responses and leg movements are historical antecedents of postnatal food-getting behavior, the prenatal movements of the beak, the head,

and, in fact, the whole visceral system are part and parcel of innumerable gradient patterns of postnatal behavior such as "courtship," "threat," "preening," "running," "attacking," and innumerable other patterns of social behavior.

Cairns's second specific criticism of my review concerns Kuo's treatment of behavior and evolution. Certainly, environmental contributions to the evolutionary process cannot be denied. It should, however, be kept in mind that the very plasticity with which Kuo would have us deal has not appeared independent of genetic changes, nor can we ignore the adaptive value of changes of behavioral patterns and their selective value on the genetic composition of surviving generations. If Kuo thus couples an evolution of behavior independent of somatic changes with a freedom from "the rather dubious twin concepts of 'natural selection' and 'survival value' of behavior for the species . . .," I think calling this view of evolution mystical is justified.

The remainder of Cairns's comments are matters of individual taste, and not subject to the same kind of discussion as the aforementioned material. I do agree, however, that the experiments included are not reported in great detail, and this is a factor which interferes with proper evaluation.

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Declaration of Independence

In writing about the Institute of Defense Analyses (17 May, p. 744) Greenberg says ". . . RAND was created by and works almost exclusively for the Air Force. . . ." In fact, the RAND Corporation was formed in 1948 as a California nonprofit corporation by its private organizers who selected its own board of trustees. Air Force Project RAND was initiated in 1946 and the contract was transferred from the Douglas Aircraft Company to the RAND Corporation in 1948. While Project RAND was its nearly exclusive concern for some years, RAND's contracts with other federal agencies, private foundations, and the City of New York (and its program of research supported with its own funds) now account for about 35 percent of its total effort.

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