of those patients" to ensure the diagnoses of schizophrenia. However, he did not utilize the more detailed criteria described by Schneider and Fish (*5*), which is the only guarantee of selecting a similar sample. Melges also expressed the view, not shared by me, that research in "schizophrenia per se" should not be undertaken as the diagnostic category is too broad and inclusive. The use of Schneider and Fish's criteria increase the homogenicity of any sample of schizophrenics.

Melges and I agree that the problem of chronicity is pertinent to the difference in our samples. Practically all the women he studied were having an acute, short-lived psychotic episode, whereas my sample included only chronically ill individuals (hospitalized more than 3 years following the postpartum psychoses). Protheroe (*6*) reports the long-term outcome to be poor in process schizophrenia developing in the postpartum period.

These differences in diagnostic criteria and in chronicity of illness may have resulted in Melges' selecting a patient sample not comparable to my own, that is, reactive (psychological) instead of process (organic) schizophrenics. Whether a variable offspring sex ratio could result from such a sample difference must await further investigation.

MICHAEL A. TAYLOR
9309 Murillo Avenue,
Oakland, California 94605

References

1. F. T. Melges, *Science* **166**, 1026 (1969).
2. M. A. Taylor, *ibid.* **164**, 723 (1969); *ibid.* **165**, 380 (1969).
3. F. T. Melges, *Psychosom. Med.* **30**, 95 (1968).
4. M. A. Taylor and R. Levine, *Biol. Psychiat.* **1**, 97 (1969); R. Levine and M. A. Taylor, unpublished; M. A. Taylor and R. Levine, *Biol. Psychiat.*, in press.
5. K. Schneider, *Clinical Psychopathology* (Grune and Stratton, London, 1959); F. J. Fish, *Schizophrenia* (Wright, Bristol, England, 1962).
6. C. Protheroe, *Brit. J. Psychiat.* **115**, 9 (1969).

19 January 1970

Regarding Periodic Phenomena

The opening statement in the report of Morley and Stohlman (*1*) illuminates the usually unstated but prevailing assumption about the operation of physiological systems—to wit, that most of the system variables come to a constant state when disturbances cease. Their report discusses the periodic nature of red cell concentration that they measured in dogs. To introduce their work, they state: "Many body parameters are known to be actively controlled in such a way as to oppose disturbances and result in a more or less steady state. A commonly assumed and expressed corollary to this concept of active regulation is that a perfectly steady state results when no external disturbances are acting. However, clear exceptions to this corollary exist." The authors go on to suggest that oscillating steady states are rare and refer to a few instances known to them as introduction to their own work.

For many years I have been actively developing a contrasting thesis, namely, that living systems, and in fact all systems, can operate in no other way but through epochs of periodic (cycling) "steady states" and aperiodic switch states. Cyclic theories of systems are as old as man's written thought, and investigators such as Huntington, van der Pol, and the many who study circadian rhythms in biosystems have all actively pursued the importance of particular periodic phenomena. Nevertheless, I know of few besides Richter and me who have tried systematically to extract a variety of cyclic data from the biological system and to put forth hyopotheses about the underlying causes of these individually distinctive cycles. Such investigations are essential, because there is a regrettable paucity of information about sustained, unperturbed normal operation of living organisms. So far, there has been virtually no systematic biospectroscopy.

The sharp issue that lies ahead is not merely the question of whether few or many systems are known to vary up and down, but rather of how regulation in the biological system is achieved dynamically. The common view is that biosystems react to wipe out the causes of disturbances. My view is that active, nonequilibrium (but not far removed from equilibrium) thermodynamic processes are involved in a large spectrum of autonomous oscillators in the living system and that the regulated average state emerges from adjustments in the parameters determining the operating points of these oscillators. This difference in viewpoint is fundamental and has in it the germ of a revolution in biology (*2*).

A. S. IBERALL
General Technical Services, Inc.,
Upper Darby, Pennsylvania 19082

References

1. A. Morley and F. Stohlman, Jr., *Science* **165**, 1025 (1969).
2. A. Iberall and W. McCulloch, *J. Basic Eng. (Trans. ASME)* **91**, 290 (1969).

2 February 1970

Orientation by Pigeons

In his report (*1*) entitled "Orientation by pigeons: Is the sun necessary?" Keeton presents, at length, data that he feels will make necessary a major reformulation of a principal hypothesis. However, after critically examining Keeton's data and conclusions, we feel he has given insufficient consideration to evidence obtained by other investigators and provided only a modest addition to their work. We would like to make the following comments.

Keeton reports a total of 21 scores from two one-sample experiments in which normal birds were released (his Figs. 1 and 11). One might add 31 scores of control birds from the two-sample experiments with clock-shifted birds. Both vanishing and homing data were no different from data obtained under sunny skies. Similar releases under overcast have previously been published by Hichcock, Kramer, Matthews, Schmidt-Koenig, and Wallraff providing several times the number of scores now published by Keeton. Randomization or deflection of initial orientation was observed about as often as apparently undisturbed orientation [for reviews see (*2*)]. Keeton does not present enough scores to change this balance either way. As discussed in the literature, randomized or deflected initial orientation and poor homing are commonly observed in sunny conditions (for example, Keeton's Fig. 9.) Thus it is very difficult to demonstrate the effect of overcast in one-sample experiments, particularly with only a few experiments with small sample sizes. The major problem remains to define overcast in some rigorous physical way, that is, assurance of continuous invisibility of the sun for pigeons over the entire area covered by a homing flight.

Keeton defines skies as "overcast" when clock-shifted birds were not deflected in two-sample experiments. This sky condition cannot be extrapolated from one time and location to other times or days or locations (for one-sample experiments) without some physical definition. Although Keeton criticizes Matthews for using this unsatisfactory procedure, he uses it himself. Again, Keeton is not the first to report releases of shifted birds under overcast. He reports three such releases, one of which was not entirely under overcast. In one of the other two releases, no birds homed the same day. This leaves only 11 control and 10 experimental homing scores for an assessment

of homing under overcast, and 30 experimental and 31 control vanishing scores for an assessment of initial orientation under overcast. These data serve as the principal basis for Keeton's arguments. Schmidt-Koenig (*3*) published data of two full and two fractional releases under overcast with 33 experimental and 32 control vanishing scores, with results not distinguishable from releases under sunny skies. Thus, Keeton's data are not sufficient to settle this question and not sufficient to reject certain hypotheses.

Keeton does accept that the sun is used as a compass when available and thus confirms the concept of map and compass. It is hard to understand why this concept—incomplete as it may be—must now be reformulated or radically amended, even if the birds do not need the sun. That they do not need the sun under certain conditions has unequivocally been demonstrated before by nocturnal homing (*4*). The theoretical possibility of one-step navigation systems not requiring a compass at all has also previously been entertained in the literature (*2*, *5*). Finally, the statistical examination of Matthews' (*6*) overcast releases now advanced by Keeton has also been done and published previously (*7*).

KLAUS SCHMIDT-KOENIG
I. Zoologisches Institut der Universitat, 34 Gottingen, West Germany
DENNIS L. MCDONALD
Department of Zoology, Duke University, Durham, North Carolina 27706

References and Notes

1. W. T. Keeton, *Science* **165**, 922 (1969).
2. K. Schmidt-Koenig, in *Advances in the Study of Behavior*, D. S. Lehrman, R. A. Hinde, E. Shaw, Eds. (Academic Press, New York, 1965), vol. 1, p. 217; H. G. Wallraff, *J. Ornithol.* **107**, 326 (1966).
3. K. Schmidt-Koenig, *Z. Tierpsychol.* **15**, 301 (1958).
4. U. V. St. Paul, *J. Ornithol.* **103**, 337 (1962).
5. J. S. Barlow, *J. Theoret. Biol.* **6**, 76 (1963).
6. G. V. T. Matthews, *J. Exp. Biol.* **30**, 243 (1953).
7. H. G. Wallraff, in *Proceedings of the 14th International Ornithological Congress, Oxford, 1966*, D. W. Snow, Ed. (Blackwell Scientific, Oxford, England, 1967), p. 331.

20 November 1969

I shall try to respond to Schmidt-Koenig and McDonald's major points in the order in which they mention them.

1) I actually reported (*1*) on only 12 (not 21) vanishing bearings of "normal" birds (my Fig. 1); the birds in Fig. 11 had been in the light-control rooms prior to release and were thus comparable to the control birds of Figs. 4, 6, and 10. If we add to the 12 birds of Fig. 1 the 40 nearly normal control birds of Figs. 4, 6, 10, and 11, we see a total of 52 vanishing bearings from five releases with no indication of disorientation under overcast, nor is disorientation exhibited by the 30 clock-shifted birds in three releases under overcast. Furthermore, in the more than 30 other releases (more than 300 bearings) under overcast that my colleagues and I have conducted at distances of 32 to 160 km [mentioned in (*1*) but not reported in detail], we have seen disorientation in fewer than 10 percent of the releases in which normal birds with some previous homing experience were used. That orientation under heavy overcast is not due to the birds' seeing the sun through breaks in the cloud layer is unequivocally demonstrated for the first time by our clock-shift experiments. I claim, therefore, that my results differ substantially from those of the authors that Schmidt-Koenig and McDonald cite. However, the fact that these authors saw orientation under overcast in some of their releases tends to support my contention that the sun is not essential for orientation by pigeons.

2) I said we have repeatedly found that the bearings of clock-shifted birds are not deflected when conditions are such that we cannot locate the sun. It would seem, then, that a reasonable working definition, and one that can "be extrapolated from one time and location to other times or days or locations," would be that overcast means the cloud cover is so thick that a man cannot determine the sun's location by normal observation.

3) Contrary to Schmidt-Koenig and McDonald's statement, I did not criticize Matthews' lack of a physical definition of overcast. I did object to his comparison of releases that utilized birds of different ages and experience, and that were conducted in different years.

4) Schmidt-Koenig and McDonald say that one of my three releases of shifted birds was "not entirely under overcast." Presumably they mean the release of 24 July, of which I said that "a short period of sun . . . developed near the loft while the birds were en route." I mentioned this for the same reason they do, that is, that it makes homing success from this release unreliable for comparing control and shifted birds. It does not, however, affect vanishing bearings, and it is on these that the principal thrust of my conclusions rests.

5) Concerning his own releases of clock-shifted birds under overcast (*2*), Schmidt-Koenig has said elsewhere (*3*) that they were "only a few accidental tests, too few for a firm conclusion."

6) Demonstration that the sun compass is not necessary seems to me to require at least a reformulation of Kramer's concept since, as I indicated, the map component "must be capable of providing, on its own, sufficient information for true navigation." This is not the sort of map component that Kramer discussed. Indeed, there is no evidence that the map-and-compass model can profitably be applied to orientation under overcast. I do not claim, however, that this orientation is a one-step process.

7) I agree with Schmidt-Koenig and McDonald that nocturnal homing by pigeons long ago suggested that the sun compass is not necessary. Indeed, in the original draft of my paper I had a paragraph discussing the published results of night releases as well as my own studies of nocturnal homing, in which I have had pigeons home from distances up to 49 km. Unfortunately, this discussion was omitted from the published paper because of requests to shorten it. However, I consider my releases under overcast at Petersberg to be more conclusive, since the published nocturnal tests did not adequately control for possible use of landmarks, as Matthews (*4*) has recently pointed out.

8) I regretfully admit my failure to mention Wallraff's (*5*) reanalysis of Matthews' (*6*) data.

WILLIAM T. KEETON
Section of Neurobiology and Behavior, Cornell University, Ithaca, New York 14850

References

1. W. T. Keeton, *Science* **165**, 922 (1969).
2. K. Schmidt-Koenig, *Z. Tierpsychol.* **15**, 301 (1958).
3. ———, in *Advances in the Study of Behavior*, D. S. Lehrman, R. A. Hinde, E. Shaw, Eds. (Academic Press, New York, 1965), vol. 1, p. 217 and especially p. 253.
4. G. V. T. Matthews, *Bird Navigation* (Cambridge Univ. Press, London, ed. 2, 1968).
5. H. G. Wallraff, *J. Ornithol.* **107**, 326 (1966).
6. G. V. T. Matthews, *J. Exp. Biol.* **30**, 243 (1953).

17 February 1970