count for swimming behavior differences, for thyroxine treated animals were also lighter than the controls although somewhat heavier than the cortisol group. In general, each rat was studied longitudinally and therefore prior swimming experience may have influenced the timetable of swimming development. Recent longitudinal as opposed to cross-sectional studies suggest however that this is not the case.

8. The front feet are held relatively immobile in parallel extension and are used only for climbing escape attempts or pawing at the glass or periodically to aid in turning. A preliminary test of the adult mouse and gerbil indicates that they also swim with front paws in inactive extension. However, adult rabbits and hamsters, like dogs and cats, use their front feet actively in contralateral extensor-flexor movements. Phylogeny of these species differences may relate to relative front limb specialization as it equips an animal to function effectively in its own ecological niche.
9. J. Altman and G. D. Das, *J. Comp. Neurol.* **126**, 337 (1966); J. Altman, *ibid.* **136**, 269 (1969).
10. D. P. Purpura, *World Neurol.* **3**, 275 (1962); J. T. Eayrs and B. Goodhead, *Acta Anat.* **93**, 385 (1957).
11. Control rats displayed long latency, monophasic, positive responses from the earliest time period analyzed. There is a progressive reduction of mean peak latency with age, and the waveform gradually changes to a biphasic configuration, attaining adult patterns when the animal is 18 days old. From 12 days onward, secondary slow waves appear after primary responses. In thyroxine treated rats a better configuration, shorter latency and biphasic responses, followed in most of the cases by slow waves, were obtained in all ages. These responses exhibited adult characteristics at 15 days of age, although even shorter latencies were still demonstrated at 18 and 120 days of age. In cortisol treated animals, evoked responses at 6 days of age were rarely seen. Consistent monophasic responses of longer latency than those of controls appeared at 9 days and progressively reduced their latency, displaying the same biphasic waveform and latency of the control by day 15.
12. H. E. Craigie, in *Neuroanatomy of the Rat*, W. Zeman and J. R. Maitland Innes, Eds. (Academic Press, New York, 1963), p. 11.
13. The structures controlling the integrated movements of swimming are unclear. Partial or total cerebellectomy, while completely disrupting coordinated antigravity movements may only slightly impair effective swimming in the adult dog. Labyrinthine structures appear to primarily coordinate swimming reflexes: R. S. Dow and G. Moruzzi, *The Physiology and Pathology of the Cerebellum* (Univ. of Minneapolis Press, Minneapolis, 1958), pp. 25, 27, 39, 101, and 273.
14. S. Levine, *Sci. Amer.* **202**, 80 (1960).

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Water: Nomenclature

Lippincott, Stromberg, Grant, and Cessac (*1*) published further experimental confirmation of the existence of orthowater and proposed that the species be renamed "polywater." Normally I regard nomenclature as a rather trivial scientific matter, but in the present instance considerable confusion could result if the proposed name is adopted. While it has a certain popular ring to it, the name "polywater" is equally applicable to any of a number of possible water species found in pure water, in solutions, and near interfaces, as well as to the species whose formation appears to be catalyzed by silica surfaces. The polymeric nature of liquid water, it should be noted, has been recognized since the 19th century (*2*). "Ordinary" water, therefore, can be accurately described as "polywater."

Alternatively I would like to propose the following system of nomenclature which represents an extension of the usage of Bernal and Fowler (*3*) and parallels the accepted usage for the solid phase:

In the bulk, pure liquid	
Water-i	The monomer
Water-ii	Small polymers $(H_2O)_n$ of $n = 2$ to 4
Water-iii	Large polymers of $n > 4$
	a. Randomly hydrogen-bonded
	b. Hydogen-bonded with at least non-ice-I-like near-neighbor order
Water-iv	Ice-I-like
Near solutes	
Water-v	Electrostricted water of hydration
Water-vi	Enforced water structures near ions (except water v)
Water-vii	Broken water structure near ions
Water-viii	"Icebergs" or clathrate structures near nonpolar solutes or nonpolar segments of macromolecules
Near interfaces	
Water-ix	Near neutral and nonpolar interfaces
Water-x	Near silica
Water-xi	Absorbed or chemically bound water

In the foregoing system "polywater" or orthowater is designated water-x.

In order to avoid the implication that these forms represent phases in the thermodynamic sense, in contrast to the case of the ices, lower rather than upper case Roman numerals have been used. The proposed scheme is a tentative working one, its categories may be replaced by more exact designations if and when the nature of the water species becomes more exactly identified. While systematic, the scheme is flexible —an important advantage for, in the light of subsequent studies, some of these species may be found to be nonexistent in the liquid (i, ii, and iv), some may be found to be synonymous (iii and iv; iii and vi; viii, ix, and x), and some may be further subdivided (xi); but the usefulness of the above proposed nomenclature should remain unimpaired.

Although not repeated in the above scheme, a given water species may occur in more than one of the three location categories: water-iv, for example, may be found in bulk solution, near solutes, and near interfaces; water-v, -vi, and -vii will surround charge sites on a surface as well as ions in solution; and according to Lippincott *et al.* water-x may exist in bulk solution as well as near silica surfaces.

The proposed system provides very brief, yet exact, descriptions of the various theories of water (the Bernal-Fowler theory becomes a water-iii_b–water-iv model; the Frank-Wen-Nemethy-Scheraga theory a water-i–water-iii_a model; the Pauling-Frank-Quist theory becomes a water-i–water-viii model; the Samoilov theory a water-i–water-iv model). It also describes complex situations, such as those obtaining in inorganic ion-exchangers (water-v–water-vi–water-vii–water-x–water-xi) and biomembranes (water-viii–water-ix–water-xi).

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References

1. E. R. Lippincott, R. R. Stromberg, W. H. Grant, G. L. Cessac, *Science* **164**, 1482 (1969).
2. See H. M. Chadwell, *Chem. Rev.* **4**, 375 (1927) for a review of early theories.
3. J. D. Bernal and R. M. Fowler, *J. Chem. Phys.* **1**, 515 (1933).

17 November 1969

Sex Ratios of Newborns and Schizophrenia

F. T. Melges (*1*), referring to my article (*2*), introduces new data from a previous report (*3*) which fail to show a relationship between the sex of newborns and mothers who develop postpartum schizophrenia. I have confirmed my findings and have, in collaboration with R. Levine, used an elaboration of my early speculations to predict successfully the sex of 44 of 47 infants, prediction based upon the history and course of the maternal mental illness (*4*).

In his report Melges utilizes the broad diagnostic criteria that I described, and in a personal communication states he ran "a separate analysis 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.

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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 hyoptheses 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*).

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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