15–17. American Ceramics Soc., Glass Div., Bedford, Pa. (C. S. Pearce, 4055 N. High St., Columbus 14, Ohio.)

19–22. Land and Water, Soil Conservation Soc. of America, 13th annual, Asheville, N.C. (H. W. Pritchard, 838 Fifth Ave., Des Moines 14, Iowa.)

19–24. American Soc. of Anesthesiologists, Pittsburgh, Pa. (J. E. Remlinger, 802 Ashland Ave., Wilmette, Ill.)

19–26. Allergology, 3rd intern. cong., Paris, France. (S. M. Feinberg, Medical School, Ward Memorial Building, 303 East Chicago Ave., Chicago, Ill.)

19–26. Medical Hydrology, 21st intern. cong., Madrid, Spain. (Dr. Francon, 55, rue des Mathurins, Paris 8e, France.)

20–21. Rubber and Plastics Instrumentation, natl. symp., Akron, Ohio. (D. R. Davis, General Tire and Rubber Co., Central Research Lab., Akron 9.)

20–22. American Oil Chemists' Soc., fall, Chicago, Ill. (Mrs. L. R. Hawkins, 35 E. Wacker Drive, Chicago 1.)

20–23. American Acad. of Pediatrics, Chicago, Ill. (E. H. Christopherson, 1801 Hinman Ave., Evanston, Ill.)

20–23. American Psychiatric Assoc., Kansas City, Mo. (1700 18 St., NW, Washington 6.)

21. American Soc. of Safety Engineers, annual, Chicago, Ill. (J. B. Johnson, 425 N. Michigan Ave., Chicago 11.)

22–24. American Assoc. of Petroleum Geologists, southwestern regional, Mineral Wells, Tex. (R. H. Dott, Box 979, Tulsa 1, Okla.)

22–24. Aviation Medicine, 4th annual symp., Santa Monica, Calif. (T. H. Sternberg, UCLA Medical Center, Los Angeles 24, Calif.)

22–26. American Soc. for the Study of Arteriosclerosis, annual, San Francisco, Calif. (O. J. Pollak, P.O. Box 228, Dover, Del.)

23–25. National Soc. of Professional Engineers, San Francisco, Calif. (K. E. Trombley, NSPE, 2029 K St., NE, Washington 6.)

23–25. Rocket Technology and Astronautics, intern., Essen, Germany. (Deutsche Gesellschaft fuer Raketentechnik und Raunfahrt, e.v., Neunsteinerstrasse 19, Stuttgart, Zuffenhausen.)

24–25. International Conference on the Insulin Treatment in Psychiatry, New York, N. Y. (M. Rinkel, 479 Commonwealth Ave., Boston 15, Mass.)

24–25. Taxonomic Consequences of Man's Activities, symp., Mexico, D. F. (H. C. Cutler, Missouri Botanical Garden, St. Louis.)

24–28. American Heart Assoc., San Francisco, Calif. (J. D. Brundage, 44 E. 23 St., New York 10.)

27–28. Child Research in Psychopharmacology, conf., Washington, D.C. (S. Fisher, Psychopharmacology Service Center, Natl. Inst. of Mental Health, Bethesda 14, Md.)

27–28. Plant Physiology, 9th annual research cong., Saskatoon, Saskatchewan, Canada. (D. T. Coupland, Plant Ecology College of Agriculture, Univ. of Saskatchewan, Saskatoon.)

27–29. Radio, Institute of Radio Engineers, fall meeting, Rochester, N.Y. (V. M. Graham, EIA, 11 W. 42 St., N.Y.)

(See issue of 15 August for comprehensive list)

Letters

Machines and the Brain

In the last few years there has been an epidemic of published statements, articles, and books which take for their subject the relationships of machines to brains. Many of these theses have been loosely constructed and have been filled with gross oversimplifications, vague approximations, and unjustified assumptions. Certainly some comparisons and contrasts can be made between known machines and human brains, but the paucity of knowledge of the latter mechanism has given rise to numerous ill-advised speculations. My general concern here is to attempt to attack some of this foggy thinking and, in particular, to respond to the article "Machines and the brain" by F. H. George, published in the 30 May 1958 issue of Science [**127**, 1269 (1958)].

It is asserted in that article that cybernetics has seriously proposed that the brain is a complex two-valued switching device. A more accurate observation is that the switchboard theory of nervous conduction was disappearing at just about the time that cybernetics first came on the scene. Wiener (*1*) himself suggested the possibility of a complex nondigital neural mechanism in addition to the well-known all-or-none transmission. Since then there has been ample evidence for synaptic and humoral mechanisms which form continuously variable, long time-constant systems. These systems, at least as complex as those using two-valued axonal transmission, mediate the performance of the binary systems. If one must speculate about the nature of the cerebral processes it may be reasonable to say that the flow of information between parts of the brain is essentially "digital" while the logical operations themselves are "analog." The futility of even hoping for adequate mathematical descriptions of the brain is made painfully clear when von Neumann observes (*2*) that the nervous system operation must differ considerably from what we consciously and explicitly consider to be mathematics.

George has equated simulated nerve nets to biological nervous tissue, implying that the flow of binary signals through explicit logical networks can form a reasonable basis for understanding brain function. One of the most embarrassing pieces of clinical evidence which such notions must explain is that very considerable portions of human brain can be removed without apparently destroying memory or function. In the ablation of a complete cortical hemisphere some five billions of the elements can be removed without much observable effect.

George has the discomfiting habit of implying much while actually saying little and therefore leaving the reader with more flavor than sustenance. The section entitled "Development of nets" illustrates this when the time comes to develop the nets and one finds that the author "leaves the matter for the time," never to return. He agrees throughout this section that certain things should be said, then neatly side-steps with comments that these matters have been discussed elsewhere; unfortunately, references to the "elsewheres" are not given.

George's "summary" of our knowledge of the human visual system is of dubious value. Even a cursory summary of that vast body of information (*3*), written for an interdisciplinary audience, could easily occupy a small volume. The "summary" which is given is a curious mixture of gross anatomy, psychophysics, and speculation. There is a section of this summary which is truly incredible. It is:

> Perhaps the most interesting feature of the visual pathways is the effect of summation resulting from the fact that information is being passed through a restriction. (Something very similar is seen in the auditory pathways.)
>
> The passing of information through a restriction is something that is characteristic of the central nervous system and makes temporal summation a necessity. It should be mentioned straightway that there is no difficulty in showing how this can be done in logical network terms.

To this I can only observe that the words and sentences are quite clear; it is only the meaning which is baffling.

One of the great puzzles to neurophysiologists, neuroanatomists, and psychologists alike concerns the origin and function of the alpha rhythm. There is not even a good set of speculations extant for explaining this brain-wave phenomenon, yet George confidently describes it as a scanning system to offset blurring due to aftereffects of retinal stimulation.

The impression is given that, after all, the cerebral cortex is a simple structure, only it contains so many variables that its description must be made in probabilistic terms. The author asserts that intercortical and subcortical connections "can be approximated by quite simple mathematical functions." This is extremely misleading in view of the fact that a *single* neuron may have hundreds of dendritic connections and as many, or more, synaptic processes. Even if we completely understood this "simple" unit and its time-varying parametric rules (which must include charge distribution, ionic concentration, membrane and humoral conditions), an adequate description for it would very likely be far from simple. To further construct a mathematical model for a modest volume of these elements (a single cubic centimeter contains roughly ten million of them) is an even more complicated task. We must then include statements about temporal and spatial summation, relative and absolute refractoriness, inhibition, and delay.

Most of those who have done "nerve-net" modelling have been careful to state that their constructs are extremely gross oversimplifications. In his "Probabilistic Logics" (*4*), von Neumann cautions that identifying the real physical or biological world with models constructed to explain it is indeed dangerous and that even plausible explanations should be taken with a very large grain of salt.

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References and Notes

1. N. Wiener, *Cybernetics* (Wiley, New York, 1948), p. 142.
2. J. von Neumann, *The Computer and the Brain* (Yale Univ. Press, New Haven, Conn., 1958), pp. 80–82.
3. See, for example, S. L. Polyak's 600-page volume *The Retina* or the same author's *The Vertebrate Visual System,* containing 1390 pages, 300 of which are devoted to bibliography.
4. J. von Neumann, "Probabilistic Logics," included in *Automata Studies* (Princeton Univ. Press, Princeton, N.J., 1956).

I have been kindly allowed to reply to the criticisms made by L. D. Harmon about my article "Machines and the brain," published in *Science*, 30 May 1958.

It is perhaps most appropriate to start with some admissions. Insofar as I may have created the impression that the cerebral cortex is simple, that machines could easily be built to simulate it in detail, or that everything is now cut-and-dried in digital terms, then I have certainly been guilty of misleading my readers. It may be easy enough in this sort of subject to create a false impression, and many writers have certainly made exaggerated claims.

That my writing and thinking may sometimes be foggy, as Harmon suggests, is a fact of which I am all too well aware. More particularly, I agree with the late John von Neumann's warning about *identifying* models with physical or biological systems. Von Neumann had a large influence on my own thinking—especially during a year spent at Princeton (1953–54)—and I would certainly never encourage identification of model and system modelled; nor, indeed, did I ever suggest such a foolish procedure.

I will now turn briefly to the task of justifying myself.

I would certainly claim that the methods of *finite automata* are of the greatest use in building models of the human brain. This is not to imply that the human brain can be wholly modelled as a digital system (Turing guessed that it was part digital and part analog, and with this most of us would agree). If, however, we were able to mirror many aspects of the brain in such digital terms, it would then be relatively easy to replace digital by analog parts. The procedure is essentially an *effective* one. It also has (and for this reason of its *effectiveness*) a clarifying effect on the concepts we use in neurophysiology.

The example Harmon quotes of brain destruction is an interesting one. This work is associated, primarily, with the name of Lashley. He certainly found that an alarming number of cortical areas could be destroyed, at least in rats, and there is much evidence from frontal lobotomy and other operations in human beings that show the same sort of thing. But why, one wonders, does Harmon imply that this is a special difficulty? The work of D. O. Hebb has already suggested a method for dealing with these results, and von Neumann's principle of multiplexing could certainly be used to account for them. Clearly, any model that claims to be sufficient for brain-model purposes will not depend on precise element-by-element efficiency. Since a large number of elements must be kept in reserve for the mediation of "new ideas," destruction of elements may well have the effect of destroying "creative" capacity.

Unfortunately, limitations of space forbid that I should treat the remainder of Harmon's remarks in detail. But I will summarize further comments in the form of a brief list. (i) Restriction and temporal summation are characteristics of central nervous tissue (see J. T. Culbertson); Harmon's example of my prose is not so convincing, although I would now write it rather differently. (ii) The switchboard theory of the nervous system is by no means out of fashion, and research is increasingly being done on it, perhaps especially in Britain. (iii) I believe it is possible that the cerebral cortex is indeed constructed on relatively simple principles, although, like the digital computer, it gains its great complexity from the enormous number of its elements. (iv) Although, unfortunately, no work has yet been published on the logical interpretation of the alpha rhythm, a great deal of work has been done in this field in Britain (*1*) and will soon, it is hoped, be published.

There is a great deal more to be said on this vast topic of brain models, and I would take this opportunity to emphasize the enormity of the problem of modelling the brain. I believe that the functional aspects are indeed more easily approachable than the anatomical. Nevertheless, there seems to be some reason for optimism about the future of the very powerful methods of cybernetics.

I would also like to take the opportunity to give a few of the many references (*2*) for the work referred to here and previously. I do not repeat the references (all of great importance) in Harmon's note.

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References

1. D. J. Stewart, "A notation for logical networks" and other unpublished notes.
2. J. T. Culbertson, *Consciousness and Behavior* (Brown, Dubuque, Iowa, 1950); F. H. George, "Logical networks and behavior," *Bull. Math. Biophys.* 18, 337 (1956); ———, "Logical networks and probability," *Bull. Math. Biophys.* 19, 187 (1957); D. O. Hebb, *The Organization of Behavior* (Wiley, New York, 1949); S. C. Kleene, "Representation of Events in Nerve Nets and Finite Automata," *Rand Research Mem. No. RM*[illegible] W. S. McCulloch and W. Pitts, "A logical calculus of the ideas imminent in nervous activity," *Bull. Math. Biophys.* 5, 115 (1943); A. M. Turing, "On computable numbers," *Proc. London Math. Soc.* (*1936–37*), ser. 2, vol. 42, pp. 230–65; A. M. Uttley, "The classification of signals in the nervous system," *Electroencephalog. and Clin. Neurophysiol.* 6, 479 (1954); ———, "The Conditional Probability of Signals in the Nervous System," *Rand Research Mem. No. 1109*.