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Analysis of methyl esters labeled with carbon-14. The curves represent the output of a mass detector. The white vertical lines represent the times at which the fraction collector was actuated. The bar graph represents the radioactivity in each fraction in counts per minute, determined by a 10-minute count. (Ref.: Journal of Lipid Research 3, No. 1, 44, January, 1962)
enzyme kinetics, the molecular basis of vision, and photosynthesis), Thermodynamics and Transport Systems (thermodynamics, diffusion and active transport, and information theory), and Specialized Instrumentation (optical, spectroscopic, and isotopic instruments, and computers). Each chapter concludes with a short list of references, and each main section includes a set of discussion questions.

Ackerman’s style is concise and clear, and the necessary biological (and biochemical) concepts and terminology are explained and defined as they are introduced. The book is well produced and appropriately illustrated. It should be extremely useful as the textbook for a course in general biophysics and, to more advanced workers, as a source for independent reading or reference. To the physicist or the engineer, it offers a pleasant opportunity to acquaint himself with those biological or bio-chemical fields in which his own techniques have been employed with success.

Biophysics: Concepts and Mechanisms is intended for students of biology and medicine who are without a background in either calculus or physics, and both subjects are introduced in a somewhat abbreviated fashion. The topics treated here are quite similar to those treated by Ackerman, but their treatment is necessarily less detailed. A set of problems and a list of references are provided at the end of each chapter. The style is informal and at times even whimsical.

An unfortunate number of errors, both of fact and of typography, remain in the text—the following are a small sample: “Because they carry more energy than photons in the visible region, the photons in the ultraviolet region are less likely to be absorbed” (p. 92); “Punctures [in the lung], called air embolism ...” (p. 33); ... [the ion] is deflected there by the magnetic field, by an amount determined by the weight of the flying particle ...” (p. 119); “If waves are diverging, or being dissipated or scattered, the important general rule, called the 'inverse square law,' is obeyed” (p. 52). In other instances imprecision detracts from the presentation: “... in destroying the bacteria, escherichia coli and bacteria coli, in foods or in our water supply. Each of these is killed by about 14 x 10^-4 ergs per bacterium” (p. 93); “The heart is a pulse pump. It distends ... closes its inlet valves, and contracts, forcing the blood out through the aorta” (p. 35).

Although this book is written for a deserving audience and its subject matter is well chosen, the many errors make it impossible to recommend the book in its present form.

M. S. Blois

Biophysics Laboratory, Stanford University

Note

Water Maps

Water Atlas of the United States (Water Information Center, Port Washington, N.Y., 1962. 7 pp. + 40 plates. $6.95), by David W. Miller, James J. Geraghty, and Robert S. Collins, contains 40 well-prepared maps; all are on a uniform scale of 1:16,500,000 (260 miles per inch).

The maps contain data on physiographic provinces, average annual precipitation, areas of cloud seeding operations, mean annual evaporation, average temperature of groundwater, strontium concentration in streams, and the amount of water used for various purposes. Each map is accompanied by a few paragraphs of explanatory text.

Professional workers in the field will find nothing in the atlas that they do not already have in their libraries.

Nineteen maps are adapted from publications of the U.S. Geological Survey and eight from books sponsored by Resources for the Future. The layman can easily be misled by the apparent simplicity of the extremely small-scale maps which cannot represent accurately the complex areal patterns of the various factors, especially in the western United States. The brief descriptions are overly generalized and superficial.

The atlas does not live up to its advance billings as “a single authoritative reference book; nor does it provide “answers to almost every conceivable question on water.” It is clearly a commercial venture which falls far short of meeting the need for a detailed and comprehensive national water atlas.

Ray K. Linsley

Department of Civil Engineering, Stanford University

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Meetings

Autoregulation of Blood Flow

The intrinsic ability of an organ to maintain the rate of its own blood flow relatively constant, when the arterial driving pressure for flow is changed, has been a controversial area of research, and recently was the subject of a research workshop involving experimental demonstrations, laboratory discussions, and formalized discussions. During a 5-day period, 26–30 November 1962, 14 representatives from the majority of American laboratories working in this field convened to discuss their findings. The first two days were spent with William Waugh (University of Kentucky); the latter three days were spent with Francis Haddy and Lerner Hinshaw (University of Oklahoma).

Six of the visiting participants brought their own apparatus and demonstrated their experimental techniques and results in the host laboratories. The experiments included ten different preparations which involved the circulation of the dog kidney, brain, intestine, skeletal muscle, and foreleg. In most instances the spectator investigators extended the original experiment to elucidate certain points of particular interest to them, and in many respects this was the most beneficial part of the tour.

Some physiologists have not observed the phenomenon of autoregulation of blood flow; while those who have observed it have not agreed on a probable cause. It seems possible that the wide variability in experimental results is due to the variety of experimental techniques employed.

W. H. Waugh (Kentucky) studied autoregulation in the isolated kidney with blood perfused by a donor animal. Small-vein pressures were measured by a catheter passed retrograde into the venous system. With large changes in arterial pressure, there were only small changes in small-vein pressure. Dissection of the kidney showed the catheter tip had not passed beyond the renal calyces. Data from other experiments in which the catheter tip was found in arcuate or interlobular veins showed similar changes in small-vein pressure. Waugh also reviewed his previous work which suggests that an active myogenic vascular response to the level of transmural pressure is the cause of renal circula-
tory autoregulation. G. Grupp (Cincinnati), in addition to Waugh, also stressed the influence of vasoactive agents on autoregulation of renal flow. He reviewed his work (i) on the relatively constant rate of renal oxygen consumption and heat production with changes in renal blood flow and (ii) on the shift from aerobic to anaerobic renal metabolism with short-term vascular occlusion.

H. E. Schmid (Bowman-Gray), using an electromagnetic flowmeter, studied flow regulation with acute changes in arterial pressure in the in situ, non-cannulated kidney. He also reported the presence of autoregulation after kidney decapsulation. A blood-perfused, isolated kidney technique was described which showed that the autoregulatory resistance change can be localized to the specific end-arterial vasculature of arteries in which pressure changes occur (R. B. Harvey, Minnesota). Additional reviews on renal circulatory autoregulation were presented by A. R. Koch (Washington) and F. J. Haddy (Oklahoma). Koch presented an analysis of the effect of varying the tonicity of the arterial blood and of the effect of osmotic diuresis on renal vascular resistance; and Haddy found that elevated arterial CO2 tension did not abolish autoregulation in the kidney.

Experiments on the isolated kidney perfused from a heart-lung preparation showed changes in renal tissue pressure were largely responsible for the major resistance changes underlying renal autoregulation (L. B. Hinshaw, Oklahoma). Deep-venous pressure rose considerably with large elevations in arterial pressure, thus favoring the tissue-pressure concept. Identical deep-venous pressures were also measured by Waugh, who inserted a venous catheter of much smaller bore into the same preparation.

Recent studies of renal blood flow and glomerular filtration rate showed indirect evidence that the chief resistance changes underlying renal circulatory autoregulation are located in the preglomerular vasculature and that there is no significant redistribution of cortical and medullary blood flow with autoregulation (E. E. Selkurt).

In studies of cerebral blood flow by C. Rapela (Bowman-Gray) a blood pump was interposed in the arterial path and sometimes reduced or abolished cerebral circulatory autoregulation. However, hypercapnia is exceedingly effective in abolishing autoreg-

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R. M. Berne (Western Reserve) described the autoregulation of coronary blood flow and its absence in the fibrillating heart. He also suggested that such blood flow is controlled locally by myocardial oxygen tension, and this control appears mediated in part by the release of adenosine. R. D. Jones (St. Luke’s Hospital, Cleveland) and Berne demonstrated intense circulatory autoregulation in the isolated thigh muscles which is sometimes impaired by incorporation of an arterial blood pump.

Additional demonstrations included preparations of (i) the isolated gastrocnemius-plantaris muscular vascular bed (W. N. Staingby, Florida), (ii) the heart-lung-foreleg (Hinshaw), and (iii) the isolated intestinal loop (P. C. Johnson and K. M. Hanson, Indiana). The elevation of arterial pressure caused a large increase in blood flow followed by a slow return toward the control level in the isolated gastrocnemius-plantaris muscular bed. In this preparation the steady state vascular resistance decreased with arterial pressure elevation. However, other experiments have shown a direct relation between arterial pressure and vascular resistance. In Hinshaw’s heart-lung-foreleg preparation, and in Haddy’s dog foreleg preparation no autoregulation of foreleg blood flow was observed. The relative paucity of skeletal muscle vasculature in the dog foreleg, compared to foreleg skin and paw, was pointed out.

The last day of the conference was concerned with three main topics.

1) The group discussed criteria which should be applied to determine whether changes in tissue pressure are responsible for autoregulation. In an organ where the major resistance changes are due to generalized tissue-pressure changes, the greatest resistance changes should be found in those vessels most sensitive to collapse, such as the veins, while the pre-venous resistance will tend to remain constant. This type of autoregulation should be accompanied by sizable changes in interstitial pressure or lateral deep-vein pressure.

2) The group considered the expected behavior of a preparation ex-
hibiting myogenic autoregulation, a type which depends upon vascular transmural pressure. This response should exist only in vessels possessing active smooth muscle tone, or might be evoked in previously atonic vessels if they are sufficiently reactive to the stimulus, and should be abolished by any agent which paralyzes vascular smooth muscle. Such autoregulation may occur in the absence of a parallel change in tissue pressure, venous resistance, or organ weight. When venous pressure is elevated, total vascular resistance should increase except where capillary pressure is high (for example, in the kidney), or where tissue pressure increases substantially.

3) The group considered metabolic autoregulation. Generally, resistance is dependent on blood flow in a manner consistent with maintenance of an adequate nutrient supply. In comparing it to myogenic autoregulation it is similar in that it requires active smooth muscle tone and may be seen in the absence of parallel changes in tissue pressure, venous resistance, or organ weight, and dissimilar in that vascular resistance to blood flow should decrease with elevation of venous pressure.

Finally, the group discussed further experiments which should be performed to determine the nature of autoregulation demonstrated in the various organs.

The workshop was a most useful method to the participants in trying to resolve individual differences and in determining the areas most likely to be fruitful in this research field. It was generously supported by a grant (H-7124) to one of us (P.C.J.) from the National Heart Institute.

PAUL C. JOHNSON
Department of Physiology,
Indiana University, Indianapolis
WILLIAM H. WAUGH
Department of Medicine,
University of Kentucky, Lexington
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13–18. Condensation Nuclei, 5th intern. symp., Clermond-Ferrand and Toulouse, France. (H. Dessens, Laboratoire de Physique du Globe, Faculté des Sciences, Univ. de Toulouse, Toulouse)
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(See 29 March issue for comprehensive list)

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in the direction of giving a closer scrutiny" implies that the goal is not yet reached. The point of diminishing returns, where the advantages of having funds available for medical research is outweighed by the time consumed in securing and administering them, may be close.

An aspect of the reports quoted by Congressman Fountain that has received inadequate recognition from scientists is the eloquent statements by the leaders of the National Institutes of Health in support of the liberal policies that they have been following. Clearly, NIH cannot support this point of view indefinitely against the desires of Congress, on whom they are, after all, dependent for funds. Scientists outside the government must also help in convincing Congress and the people that there are at least two sides to this question. The issue has come up initially with respect to support of the health sciences, but it may not stop there.

**BRIAN MACMAHON**

*Department of Epidemiology, School of Public Health, Harvard University*

**Creativity and the Indigent Student**

It is distressing to see... [you] give support to the archaic idea that a hungry student is a superior student [Science 139, 79 (11 Jan. 1963)]. Some of the penetrating minds of the past and present may have been starved during their formative period, but to assign a cause and effect relationship is absurd. The same reasoning would suggest that we decrease by 50 percent the pay of all present scientists so that they will be twice as creative, thereby eliminating the need for a crash program.

Freed from financial pressure the "man of moderate endowment may show flashes of genius." Why dilute his academic struggles with monetary adversity?

**WILTON H. BUNCH**

*Crow Indian Hospital, Crow Agency, Montana*

... Not all can be Enrico Fermi, but any reasonably competent Ph.D. can add to the sum of knowledge from which the Enrico Fermis draw. If recent Ph.D. theses are pedestrian, is it the fault of the Ph.D. candidate or of the professor and system under whom the work is done?

Furthermore, poverty at the graduate school level is not an automatic virtue. Probably lack of financial assistance has hindered more scholars, potential and actual, than reasonably adequate stipends could possibly do.

**GUY W. MCKEE**

127 Orchard Road,
State College, Pennsylvania

... [the] report [of the President's Science Advisory Committee entitled "Meeting Manpower Needs in Science and Technology"] does not imply "that scientists, like nuts and bolts, are interchangeable and can be mass produced." It does imply that graduate schools will assert their traditional selectivity and accept only those students who are capable of quality academic performance; that science majors are not continuing their education because of financial difficulties; and that they can complete their programs earlier and do more creative work when devoting full time to educational pursuits than when working at odd jobs like cleaning pigeon cages.

The implementation of this document may not produce enough scientists—only because it doesn't start early enough!... To really increase the number of graduate students we must identify and encourage gifted youngsters in the secondary school—probably even more effectively in the elementary school. There are many studies to substantiate the fact that interest in science is "killed" or "kindled" early.

**GLADYS S. KLEINMAN**

*Rutgers University, New Brunswick, New Jersey*

... I have noticed that a relationship exists between the amount of expensive laboratory equipment and the ingenuity with which problems are solved and techniques developed. A laboratory in the early stages of growth, and short of money for equipment, develops a high proportion of new information through improvisation. As the physical plant takes on more elaborate equipment, experimental design more often is set up around the instrumentation than around the problem to be solved.

**HUGH H. HOTSON**

*Maritime Corporation, Seattle, Washington*

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Presented of Papers

All scientists (and indeed nonscientists too) are aware that talks at professional meetings should be concise, lucid, and held to the allotted time. All are equally aware of how short we fall of this goal. Too often talks are rambling, confused, slow in getting underway, and then rushed and garbled as the speaker runs out of time. All this could be avoided if it were required that each speaker present the chairman of his session with a magnetic tape recording of his talk for playback over the hall's public address system. The author would sit on the platform, signal for slides at the right time, and be prepared to field questions at the end. He would have adjusted his talk to the proper length at home (or else the chairman could reject it) and he would have had to listen to himself, the salutary effect of which would be incalculable.

M. A. Van Dilla
Los Alamos Scientific Laboratory, University of California, Los Alamos, New Mexico

Missing Links in Computer Intelligence

The paper by Ulric Neisser on “The imitation of man by machine” [Science 139, 193 (18 Jan. 1963)] describes three characteristics of human thought which are absent from machine programs. I would like to add a fourth characteristic which is, perhaps, the most important one. This is the property of “consciousness,” the ability to be aware of the stimuli coming to us from our sense organs, and of the thoughts circulating in our own nerv-
ous systems—and to be conscious of the fact that we are conscious.

While Neisser touches on this matter in his discussion, I believe that the property of “consciousness” is worth emphasizing.

Conceivably a computer could be programmed to give emotionlike responses, or to operate with a multiplicity of motivations. The central question here is whether a computer could be built which would be aware of its emotions, motivations, and the world around it. While this would appear to be an inconceivable feat (some would say impossible), the fact remains that our own human consciousness is, somehow, the end result of physical interactions taking place within the mechanisms of the body cells. There is no a priori reason for assuming that these operations cannot be performed by man-made circuits (although the complexity may make it impractical).

Present-day computers think unconsciously and compulsively. In the jargon of psychoanalysis, they consist entirely of superego, and are devoid of ego or id. The question of humanlike machines, translated into these terms, becomes: Can we build a computer with an ego?

Milton A. Rothman
James Forrestal Research Center, Princeton, New Jersey

Neisser seems to believe that popular misconceptions about “thinking” machines (they are not capable of “thinking”—even in quotes) are due to a misunderstanding of the nature of human thought. Indubitably! But it is equally due to a misunderstanding of the very nature of machines and machine operation. Even the most sophisticated computing machine cannot do anything it is not programmed to do, although much of the program is now “built in” into the machine and does not have to be spelled out in detail by the programmer.

If a comma was omitted in the program, then what was intended to mean two small adjacent numbers is not two small adjacent numbers but one large number, and the machine reacts accordingly. Every machine is literal without any sense of discrimination, common sense, or humor. It is the obedient servant of man—like the disastrously obedient slaves in fairy tales of the past—and in its very obedience lies the danger, for men do not always wish as wisely and well as an omnipotent master.
Most important in Neisser's article is his brief allusion to the use of computing machines to "make social decisions." Again, the very thought of using machines to "answer" questions of human and moral values, or taste, betrays a—widespread, unfortunately—lack of understanding of the very nature of computing machines, of mathematics, and of logic. These machines are eminently suitable to implement the solution of problems in mathematics and logic for the simple reason that they are built in accordance with such laws. Every circuit is the hardware manifestation of a Boolean-algebra equation.

Therefore a machine can solve any problem that can be expressed as a mathematical equation, which means—at least, in theory (in practice, we sometimes lack proper understanding of the problem or mathematical skill to formulate it)—any problem for which a purely rational solution is possible. Social decisions must never be made on purely rational grounds. They are primarily questions of human and moral values and, let us hope, good taste.

Alice Mary Hilton
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While Neisser avoids explicit statement of the extreme position that differences between human thought and the functional properties of modern computers are insurmountable, he clearly implies support of that position by his subtitle.

Neisser's arguments do not support a mystical or irrational view of the differences between men and machines. Instead of demonstrating any inviolable distinction between the two, he has pointed the way toward making machines more nearly "human" and hence more useful to humans.

The chief differences, Neisser states, arise from the developmental (and, one might add, even the phylogenetic, historical, and ontogenetic) origins of man. Human thinking is inseparable from other human activities and processes. It "takes place in, and contributes to, a cumulative process of growth and development... . The cumulation of learning is interwoven at every point with inborn maturational sequences." A machine will continue indefinitely to pursue any goal programmed into it (this is perhaps its most inhuman feature); whereas the motivations which govern human thought are complex, subtle, and
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changing. The computer acquires and retains information in a systematic and controllable fashion; whereas the human thinker "lives willy-nilly in an accumulating context of experiences which he cannot limit even if he would."

The real question is whether or not these differences are so fundamental as to rule out forever the possibility of our building some day a machine that can make rational and useful social decisions. . . .

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Ulric Neisser's article brings to mind a remark I was privileged to hear from J. von Neumann during an informal talk on computers given at the Institute for Advanced Study at Princeton, in 1948. A woman in the audience started raising the canonical question, "But, of course, a mere machine can't really think, can it?" For a while he tried to put it off with a good-natured gesture, but she persisted. So he turned to his tormentor and said: "Look here. You insist that there is something a machine cannot do. If you will tell me precisely what it is that a machine cannot do, then I can always make a machine which will do just that."

The full import of this remark may have been lost on the person to whom it was directed, but to others in the audience it answered, in a sudden flash of understanding, many half-formulated questions. There is no limitation at all inherent in the machine; the only limitations on making "machines which think" are our own limitations in not knowing exactly what "thinking" consists of.

Von Neumann's remark applies equally well to all of the alleged differences pointed out by Neisser. I suggest that his arguments, far from establishing any "deep difference between the thinking of men and machines," describes only the present state of ignorance of psychologists concerning what growth, emotion, motivation, creativity, and so forth really are.

This does not mean, as Neisser implies, that it would be desirable to incorporate all these features into machines of the future. For most applications of machines, this would amount to a deliberately built-in unreliability. I could hardly disagree more strongly with the implications of the remark, "If machines really thought as men do, there would be no more reason to fear them than to fear men." It is just the fact that machines do not get confused by emotional factors, do not pursue hidden motives opposed to ours, do not get bored with a lengthy problem, that makes them far safer agents than men for carrying out certain tasks. What we have most to fear in the world today is not machines which lack these "human" features, but men who, unfortunately, have them.

E. T. Jaynes
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My paper was not concerned with the inherent limitations, if any, of machines. I attempted to describe the differences between existing or contemplated computer programs on the one hand, and human thinking on the other. It is true that human thought processes are not well understood, but this seems irrelevant to the accuracy of my description. Jaynes' opinion that emotion and growth are deplorable sources of unreliable seems equally irrelevant.

I would like to comment directly on the remark attributed to von Neumann. It is not necessarily true that a program can be written to carry out any well-specified task. The following counter-examples are due to Oliver G. Selfridge:

The speaker may be asked to make a machine to defeat Botvinnik at chess, or to select the painting (from 100 in a contest) which will be awarded first prize by the judges. He will be unable to make such machines at present, and equally unable to give formal proof that he can ever succeed in doing so. (We do have promising leads for the first of these problems, but success cannot be guaranteed.)

If it be replied that these tasks are not specified "precisely," one may enquire what further precision is required. It will probably appear that the underlying idea of a precise definition is rather like a computer program. In that case the assertion reduces to "If you will tell me how to program a task, I can always do so."

Even the last statement may not be right. It is possible that some tasks, including the simulation of human thought, are so complex that the specifi-
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Self-Stimulation Experiments

Your publication of papers by Margules and Olds [Science 135, 374 (1962)] and by Hoebel and Teitelbaum [Science 135, 375 (1962)] leads me to propose the following physiological explanation of the association which they describe between mechanisms for self-stimulation and for feeding in the lateral hypothalamus. It is my opinion that in a self-stimulation experiment the negative feedback loops of normal feeding mechanisms are replaced by an artificially constructed loop having a positive sign.

Under natural conditions, an activation of the lateral hypothalamus induces or facilitates feeding behavior. Included in the many possible varieties of such behavior is bar pressing—one of the responses which lateral hypothalamic activity will induce for feeding. Ordinarily such behavior induced by the lateral hypothalamus leads to ingestion of food, and this leads in turn to a number of physiological changes which inhibit further intake of food and suppress the activity of the lateral hypothalamus. But when, as in a self-stimulation experiment, the bar pressing leads not to food ingestion but to electrical stimulation of the lateral hypothalamus, then that part of the brain can only become still more active. Consequently the animal is that much more likely to press the bar again, and every further press enhances the chances of more presses. Induced to press the bar in the first place by a naturally occurring activation of its lateral hypothalamus, the animal receives for its press only a recurrent stimulation into the region which originated the bar pressing.

This distinction between a normal, negative feedback loop and an artificial, positive loop avoids the paradox mentioned by Olds [Physiol. Rev. 42, cation would take a full lifetime to write, and the resulting program 1000 years to de-bug. There is no way of knowing in advance; we must find out by experimenting.

Dogmatic assertions of the omnipotence of computers tend to stir up a multiplicity of, often unpleasant, reactions in the reader. They do not have the supposedly compensating advantage of being true.

ULRIC NEISSER

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554 (1962)] in these words: "In any event it is clear that stimulation of the same lateral area has two usually dis-associated effects . . . the effects of the primary drive itself . . . [and] the effect of the primary reward related to that drive. . . . Therefore the possibility that the electric stimulus constitutes a simple internal surrogate for either is unlikely" (pp. 593–94). It seems more probable that self-stimulation of the lateral hypothalamus is a surrogate for natural stimulation of the lateral hypothalamus—nothing else.

If my interpretation for the relationship between feeding and self-stimulation is correct, then one can predict that any region of the brain where self-stimulation is observed must function as a component of a similar physiological system, in which the animal can be taught to use bar pressing as a part of some normal behavioral sequence.

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While I find myself in sympathy with Brobeck's view, I find it difficult to agree fully for two reasons:

1) At the beginning of a self-stimulation experiment, bar pressing has never been previously associated with or been instrumental in feeding behavior. It is a random response like ear twitching or tail movement, and it should be kept in mind that any random response may be used to trigger the electric stimulus. The chosen response will quickly rise in frequency, gradually excluding other responses from the immediate repertory, until the chosen response predominates and occurs at a maximum possible rate. It is difficult to understand why this response should be chosen for repetition just because of its temporarily contiguous relationship to the subsequent increment in lateral hypothalamic activity. The increment should make all food-related or other possible responses more likely, but I do not find in Brobeck's explanation any reason why the response emitted just prior to artificial stimulation of the hunger drive should be marked for immediate repetition. We think of a hungry animal trying the habitual responses in an effort to get food, and if these fail, trying others. If some item of the new repertory were followed by a sudden rise in hunger or in any internal activity generator, would that response be repeated? If so, why?

2) If the size of the supra-threshold

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**James Olds**

Department of Psychology,
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Electric field in a self-stimulation test is approximately 1-mm sphere, it seems unlikely that the sphere in such a complex structure as the hypothalamus in a small animal like the rat is homogeneous in regard to function. One millimeter is the cross-sectional diameter of the whole medial forebrain bundle which might so far as we know mediate the whole gamut of emotional control. Thus, on anatomical considerations alone it seems that an electric stimulus here must be having more than one effect.

Two possible explanations occur to me; different from Brobeck's, but equally plausible.

1) The electric stimulus might simply activate two different mechanisms, one yielding eating behavior, the other yielding behavioral reinforcement. The mechanisms might be grouped in anatomical proximity in the lateral hypothalamus so that both could be brought under control of a common deficit-sensor (such as the hypothetical glucose receptor). In such a case, a deficit in nutrients would have two consequences, (i) generating activity directly in the eating behavior system, and (ii) lowering thresholds in a "taste" system so that stimulation by food would "taste better"; that is, it would have more power as a positive reinforcement over antecedent operant behavior patterns. If such an anatomical proximity existed, electric stimulation, acting as something of a bludgeon, would have two effects, evoking directly the eating behavior system and the positive reinforcement mechanism of the "taste" system.

2) The electric stimulus has the same effect as food in the mouth which causes eating and repetition of antecedent behavior. These views are related both to one another and to Brobeck's view, but they emphasize the distinction between drive-caused behavior and reward-caused selection of a particular behavior for repetition. In drive-caused behavior an antecedent condition heightens the activity level of the organism facilitating all behaviors, thereby causing an increase in the diversity of behavior. In reward-caused behavior a stimulus subsequent to a random response causes that particular response to be repeated at the expense of all other random responses, thereby diminishing the diversity of behavior.