SCIENCE

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AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

INDIAN ROCK ART

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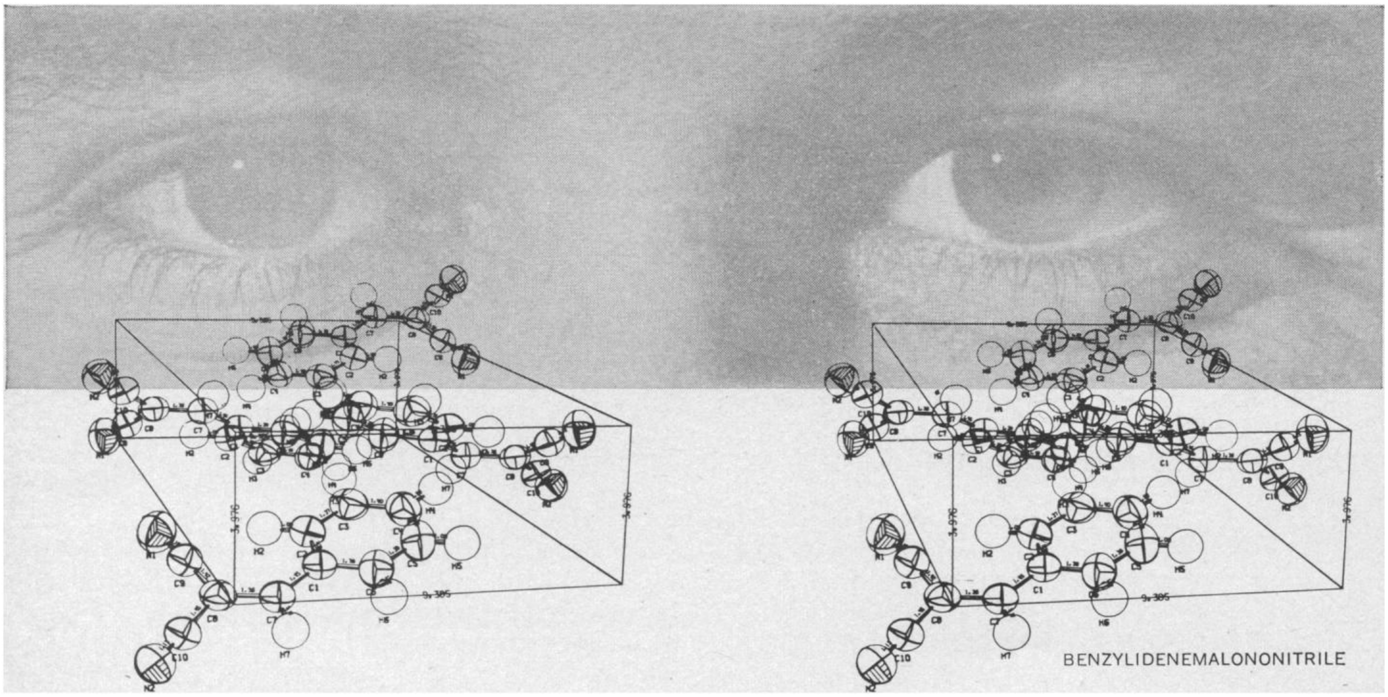

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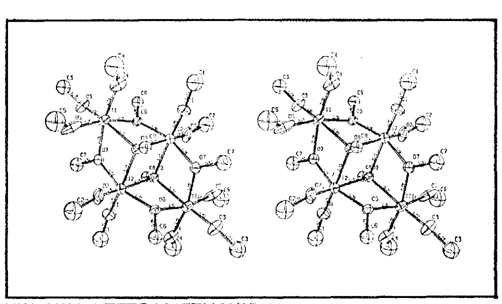

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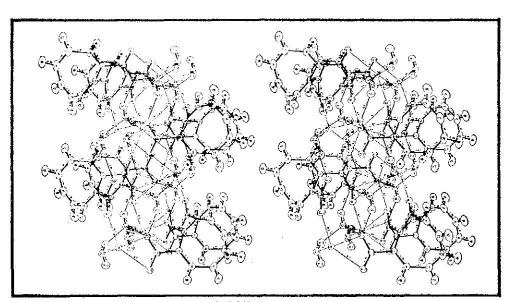

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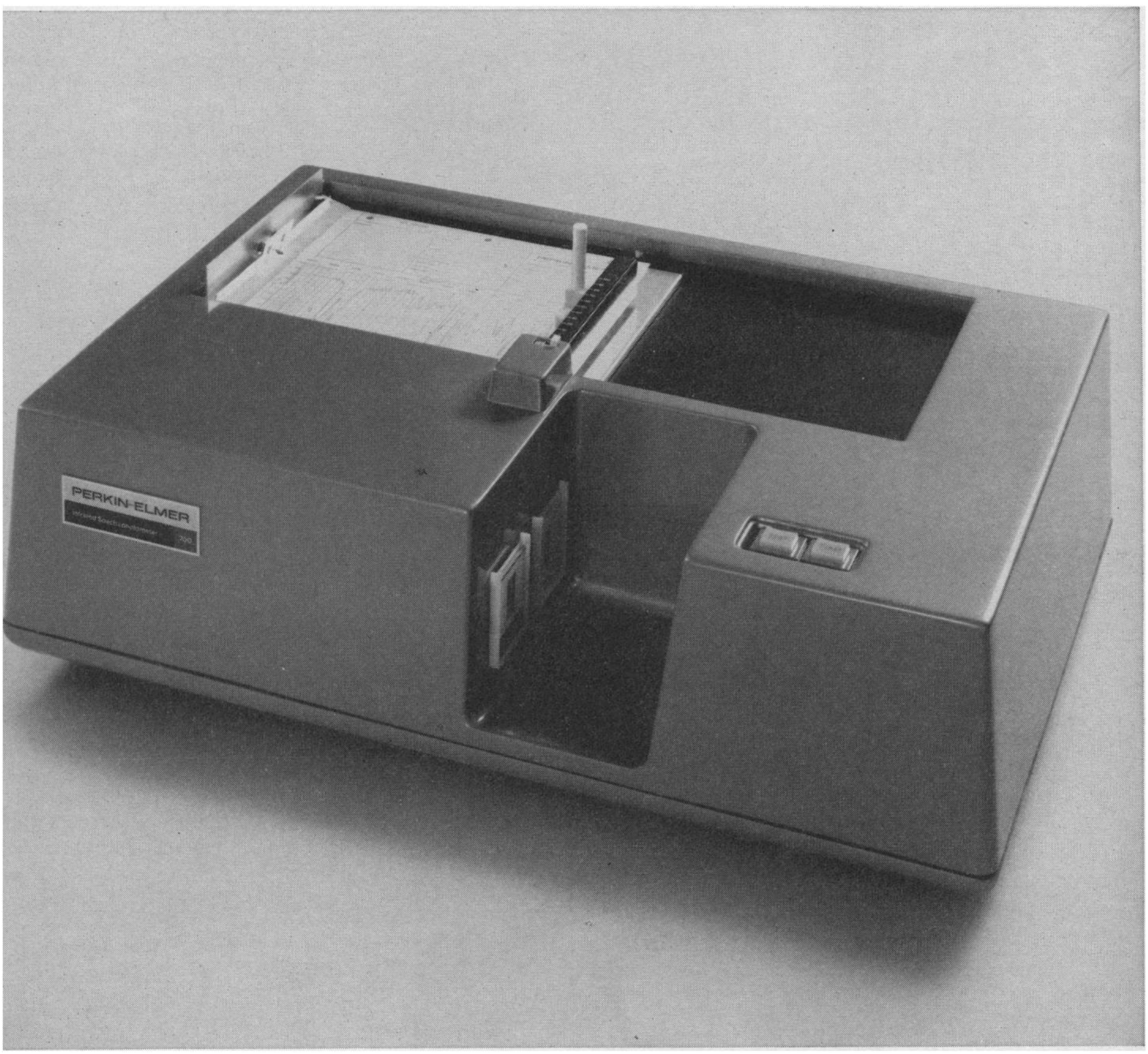

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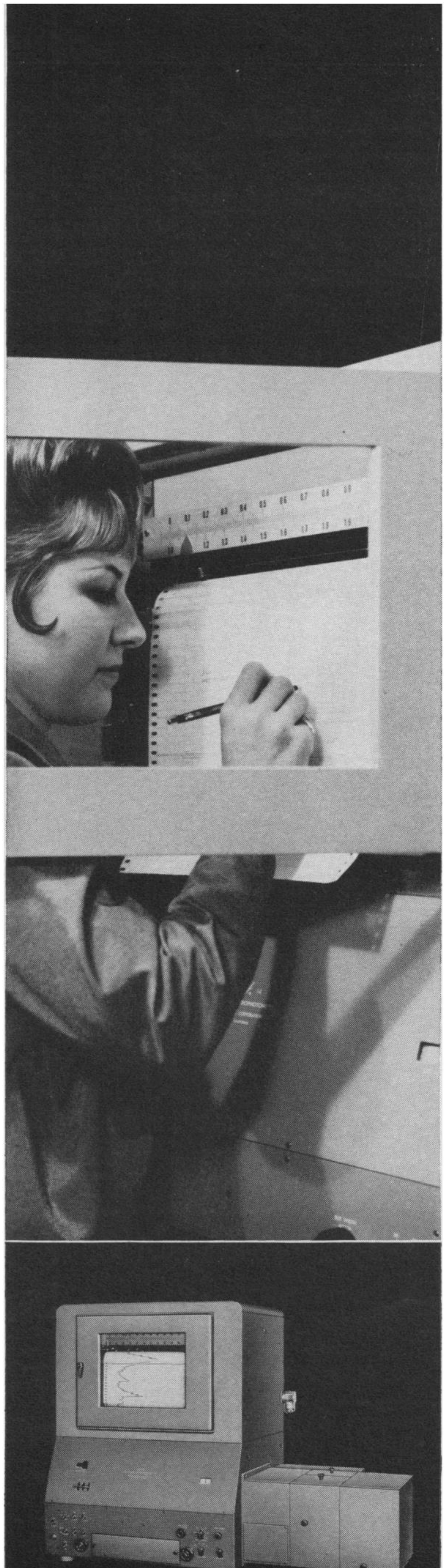

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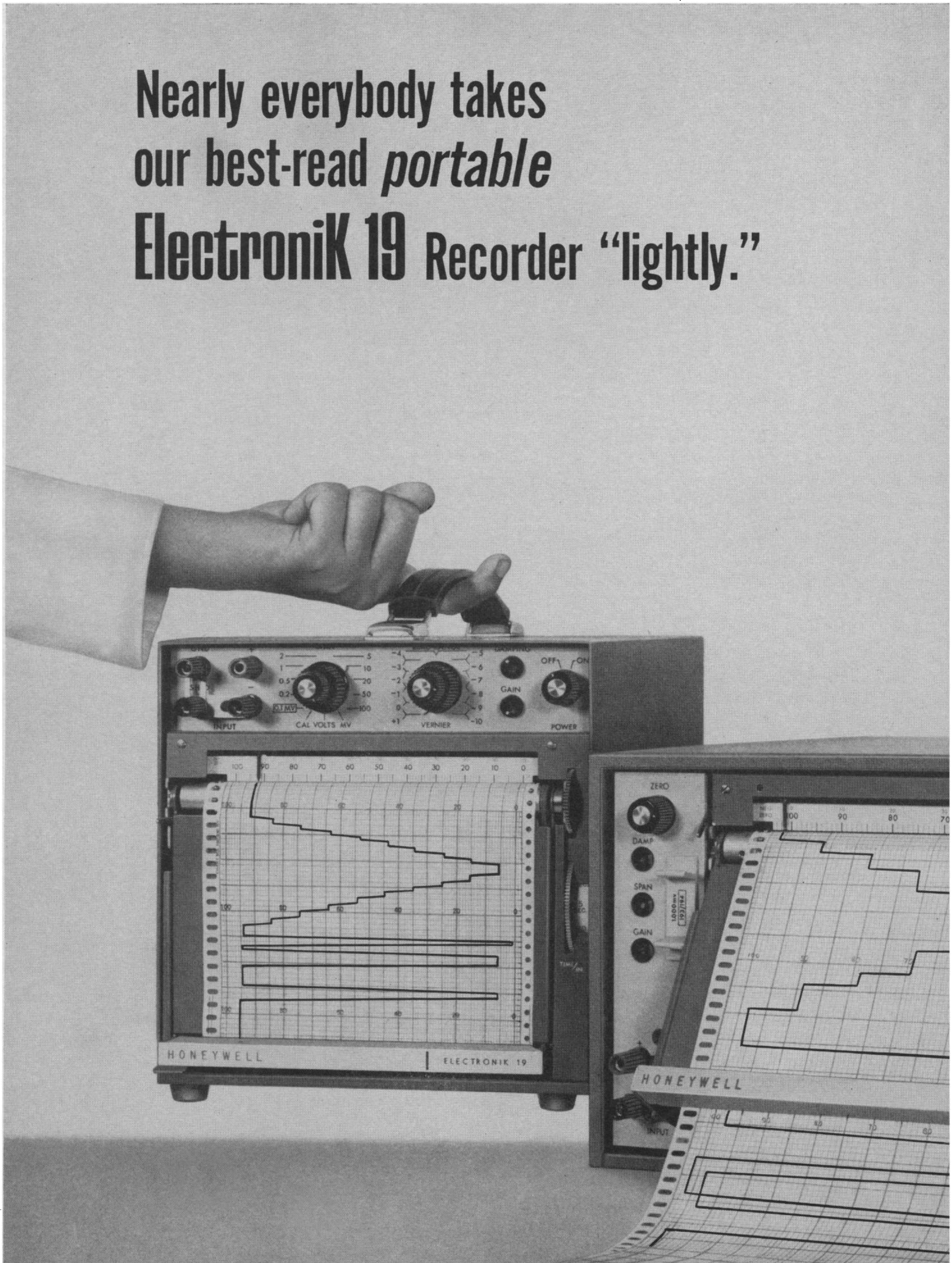
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teaching a subject develops a greater depth of knowledge than merely "taking courses." In addition, students who teach gain maturity and poise and should develop the ability to express themselves before a critical audience.

7) Many of our recent applications for faculty positions reveal that the individual has had *no* teaching experience. We should not be encouraging this same shortcoming in our better students.

8) Our teaching-assistant budget has been unable to provide funds for teaching assistance in advanced courses. Additional help would allow the faculty time for more meaningful teaching, for needed revisions, and for laboratory improvements.

9) Differentials in the dollar value of various types of support have led to a certain amount of discontent on the part of some of our students.

10) Departmental allocation of various types of support has proved difficult and complex.

Because of these concerns, we proposed to our administration that a Teaching Fellow Program be initiated and suggested the following procedures:

1) All graduate support funds (scholarships, fellowships, traineeships, NDEA's, NASA's, teaching assistantships, and various types of research assistantships) would be considered as one budget. This budget would determine our total support level. Individual stipends would be set at a level midway between the current experienced teaching assistantships and inexperienced teaching assistantships, less state and federal taxes. (The stipends should be tax-free as teaching would be made a degree requirement.)

2) Teaching and research assignments would be made equitably (research assistants would teach less) with everyone doing some teaching. Ideally, new graduate students would be assigned lighter teaching loads to speed their academic adjustment.

3) Annual raises for teaching fellows would be based on merit, *not* automatically awarded for "experience" as is done now. Another possibility would be to set aside a percentage of raise money to be administered automatically for "experience" while the rest would be awarded on "merit."

The proposed system, as outlined above, was tested "on paper," using our current budget, and it actually increased the number of graduate students we could support. In addition, the system proved easier to administer at the departmental level. Various federal and state regulations prohibit our proposal from being tested locally, but there is a possibility that such a system might be funded by a granting agency on an experimental basis with the state of Wisconsin providing the financial support currently provided for the teaching assistant budget. Such an experiment should improve both undergraduate and graduate education.

RICHARD A. PAULL
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The Draft: Tormenting Uncertainty

Abelson's conclusion that the draft is the "focal source" of discontent ("Student anxiety," 1 Dec., p. 1139), coincides with my own observations. Many of the students themselves do not fully realize the degree to which the draft disturbs them. I believe the draft is at the root of an anxiety and disturbance which spreads to all youth. Although a professional army at the enlisted as well as officer level seems to me to be the best solution, there are alternatives. At any rate, the present draft program is unsatisfactory and a menace to our social equilibrium now and for the future.

LOWELL H. HATTERY
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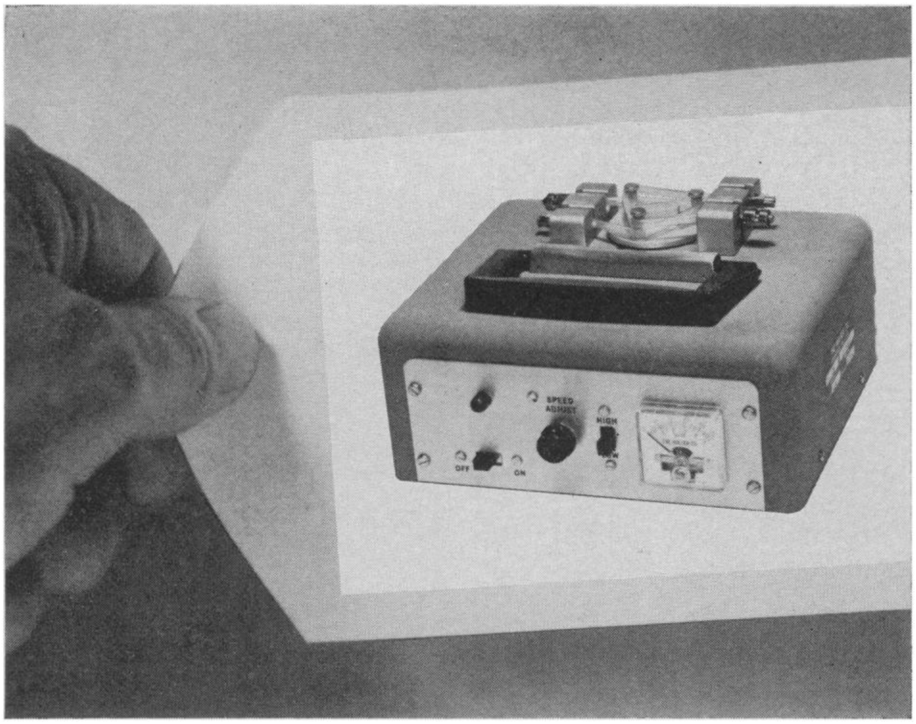
Unrest on our college campuses . . . may very well result from our present draft policy, which exempts young men from military service while they remain in college. The immorality of this policy, especially during a war which, we must assume, is of limited duration, and which therefore will result in some youths' permanent exemption from the rigors of warfare, is very likely having a more profoundly disturbing effect than any question of the validity of the war itself. It is not easy for men to live with the knowledge that their lives are being spared because they have been born into a social class that achieves college. It is easier to grow cynical about everything, than to face that reality, and the tragedy is that the deeper the sense of justice, the greater the urge to self-abasement. . . .

JAMES MUNVES
230 West 78 Street, New York 10024



. . . I am intrigued by the frequent omission in such discussions [of the draft policy] of one possible technique for eliminating this uncertainty. I refer to the simple process of enlistment, which is easily accomplished at a time to minimize interference with graduate school, career, or marriage, and offers in-service opportunities and choices not open to those drafted. Any student who finds himself living in "tormenting uncertainty" can expeditiously eliminate that uncertainty in this way, and failure to do so suggests that he at least prefers the uncertainty to this alternative.

JOHN L. PRATHER

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. . . I think it is an exaggeration to attribute "tormenting uncertainty" and all the ills supposedly resulting from it solely to the threat of the draft. Many students will live in a state of major uncertainty regardless of the nation's military needs, because this is a natural condition of the transition from youth in an educational milieu to adulthood in a practical world.

The draft is not quite as bad as you seem to make it. Perhaps the malaise of some students is due not so much to the uncertainty of the draft as to the fact that they might be compelled to perform a service for their country that could be uncomfortable or might interfere with their private plans. In other words, if the draft were 100 percent certain, in the form of universal service, I suspect that these young men would be no happier than they are now. What are they really against, uncertainty or service?

Also, there are many reasons for doubting that a lottery would be any less uncertain than current methods of selection. Under the 1967 draft law, students are guaranteed a deferment until they have obtained a bachelor's degree, and many will be assured of deferment through graduate school as well. This gives them a great deal of certainty. Then too, about 50 percent of all young men are certain of avoiding service altogether because of disqualification on physical or other grounds. For these, there is no uncertainty. . . . Proponents of a lottery seem to think that by a simple casting of lots the country can decide once and for all whether or not a particular young man's services will be required for the national defense. This is an extremely simplistic view of the nation's security and of its military needs, since these change markedly from year to year. But in the case of the prospective college student, the coin you are suggesting be tossed has on one side, "military service" and on the other, "college education." I wonder whether you really mean to advocate that college admittance be determined by the laws of chance.

JOHN D. ALDEN
Engineering Manpower Commission, Engineers Joint Council, 345 East 47 Street, New York 10017

An interesting solution to the short-range problem is presented in Wolfle's editorial "Selective service solution" (8 Dec., p. 1271). For the longer range, the academic and scientific communities generally endorse legislation embodying the "lottery" approach. I would like to suggest some guidelines for such legislation incorporating one important new concept:

1) A lottery by which the individual, as close to the time of high school graduation as possible, is placed into one of two groups: exempt or eligible.

2) Exempt individuals would be free of any potential responsibility unless major new military requirements arose and the Congress took further action as a result.

3) Eligible individuals would be subject to call during a period of 2 years only. During this period, until and unless called to duty, they would be eligible for benefits for attending school identical with those already provided to veterans.

This plan would have all the advantages of equity and maximum possible certainty which make any lottery plan attractive. In addition, it provides an option of socially useful compensation to those who must live with the uncertainty, and it provides a financial incentive for the government to optimize the size of the pool of eligibles.

Details, of course, are optional. The essential points are equity in the determination of callable individuals, maximum possible certainty for our young men, and compensation and inducement for continued schooling for those who must live with an uncertainty not of their making.

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Social Planning and Our Medical Schools

Medical schools are in danger of becoming service organizations instead of gatherings of gifted persons living in an atmosphere of freedom, independence, and emotional quiet conducive to good pedagogy and research. They are expected to (i) teach medical students, (ii) train paramedical students, (iii) do the major part of medical research, (iv) conduct large programs of postgraduate education, (v) service many governmental committees and voluntary health organizations, and (vi) practice medicine.

Under the terms of the Heart Disease, Cancer, and Stroke program recently established by Congress, the schools are now being asked to take responsibility for the health of the community. In addition, some commentators have called for leadership from the medical schools on such urgent social problems as the control of population and of environmental pollution, abolishment of poverty by "the best possible medical care," creation of psychological environments in which human beings can develop and prosper, cost and use of drugs and cosmetics, and the foods we eat.

In the face of such demands medical school faculties and interested laymen must define what kind of schools they want and what kind society needs. A practical institution for the training of practitioners and the delivery of "health care" to the community is one extreme; the other is an institution little concerned with its social responsibilities. I doubt that we want either. Nor do we want a mere average of the two—that is, an organization excellent in neither sense. Without going to either extreme, we can have schools that preserve an atmosphere of scholarship, critical and skeptical, and time enough to think, but only if we are careful to understand, and say, what we want in a medical school. To an important degree the future physician depends on the imprinting he receives during his medical school experience.

Defining the function of medical schools, however, does not solve the problem of delivery of medical care, although it may insure that practitioners will be better able to undertake it. The dean of Cornell University Medical School, John Deitrick, has made a wise suggestion: that community hospitals, after being upgraded, take over most of the responsibilities of health care now being foisted onto the medical schools. Some of these institutions have already grown in stature, and many now have structures similar to those of the medical schools and centers. They are efficiently performing teaching duties as well as providing specialty services and emergency-room care. Health education programs for the community are also possible.

Besides the great potential of the community hospitals, there is that of group practices and clinics in many cases entirely able to assume the responsibilities I have been discussing. Too often planners seem to be unaware of the fact that private practice is being powerfully supplemented by group practice and clinics. Training programs for interns and specialists of groups not affiliated with a university are at least as good as those at most medical schools. They provide a way, which is already at hand, for dealing with problems that seem to have grown so large in the imaginations of many as to provoke legislative and fiscal panic.

I am hoping that the "Ivory Tower" aspect of medical schools will not be destroyed and suggest that the necessary community, social, and medical functions can better be assumed by other existing agencies. I believe that it was a mistake to have saddled the medical schools with becoming the core of leadership for the Heart Disease, Cancer, and Stroke Regional Program. There is little to suggest that they are prepared for such responsibilities, nor should they be.

—IRVINE H. PAGE, *Cleveland Clinic*

that intensity might be represented by the number of fibers active. R. F. Schmidt pointed out that in some preparations significant responses could be evoked by a single afferent impulse. No simple ideas of "frequency modulation" could be generalized to the whole sensory system.

The relationship between signaling processes of this kind and "feature extraction" systems seems as yet unclear. Granted that, as H. B. Barlow indicated, features such as moving contours might be detected and signaled even at the retinal ganglion cell level (in rabbit), how is such information integrated with that from other types of retinal cell? Barlow suggested some reasons for associating the rabbit's directionally sensitive units with the control of reflex eye movements rather than with visual perception; but what of the "complex" and "hypercomplex" cortical cells found by Hubel and Wiesel in cat and monkey? Are their outputs, as Barlow suggested, the triggers of pattern recognition and clues to binocular depth perception; or do they supply only collateral cues to higher neuronal mechanisms, perhaps including those that facilitate or inhibit whole classes of recognitive process? There appeared to be little firm evidence on such questions, but it seems healthy that they were raised at a time when the connection of feature detectors with pattern recognition is often taken for granted.

Evidence was also sparse regarding the precise transformations borne by sensory signals at different processing stages. For example, sensitivity of single neurons to rate of change of frequency is enhanced in the higher levels of the auditory system, as reported by E. F. Evans and I. C. Whitfield; but does this indicate the function of these levels, or is it only a by-product? Again, Jung pointed out that in the visual system the level of spontaneous activity is progressively lower at each stage on the way to cortex, typically dropping from 40 per second in optic nerve to 20 per second in lateral geniculate to 5 per second in visual cortex. Information on "brightness" and "darkness" is separated in the retina and signaled to cortex in two antigonistic systems, and enhancement of contrast is present at all levels. But whether these facts reflect the principal function of the different stages is still unknown.

For the lateral geniculate W. A. Richards proposed the unsuspected function of size-transformation, partly on the basis of evidence that single geniculate units in unanesthetized monkeys change their firing rate with the approach or withdrawal of objects. A neural "zoom lens" at this level, it was suggested, might serve to maintain size and distance constancy. Here again, simultaneous microelectrode recording from many cells would be necessary to demonstrate such "remapping" directly.

Another hard question which produced few answers was whether temporal patterning of impulses, as distinct from mean firing rate, carried significant sensory information. W. M. Siebert had calculated that if all temporal information in the observed activity of the auditory nerve in response to a tone burst were exploited, human frequency discrimination could be orders of magnitude better than it is. In practice it seemed to be roughly what one would expect if our performance were based only on the overall statistical distribution of cochlear cell activity. A few exceptional cases are of course well known in binaural localization, for example, where interaural time differences of a few tens of microseconds can be detected. Again, in *Aplysia*, a train of pulse-pairs of constant frequency has been found to have different effects according to the interval separating each pair. By and large, however, evidence for rhythm coding of information in single fibers is as yet negligible.

Between the impulses in convergent fibers, small intervals of time can have large effects, but we have still no direct evidence that significant sensory information is represented by the relative temporal patterning of impulses in parallel channels. One reason for interest in the question is that if this principle were used in the nervous system, differential changes in the speed of propagation of convergent fibers could modify the response of the system to different input rhythms. This would offer an additional possible mechanism for the long-term retention of adaptive changes. A second advantage of rhythm codes, illustrated by the Morse code call system used in many hospitals, is the possibility they offer of specific selectivity without specific connections to the central office.

Two warnings emerged from discussion which may be worth reporting here. The first concerns the drastic effects of different anesthetics, even in liminal amounts, on the picture obtainable of cortical unit sensitivity to sensory stimuli. Techniques of im-

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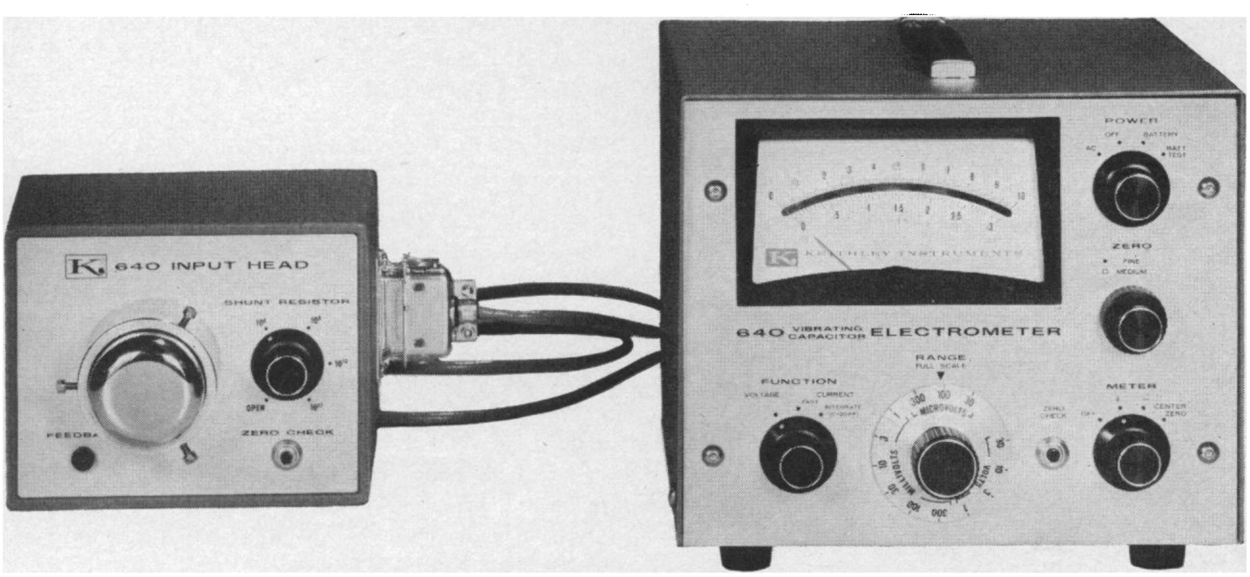

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planted electrodes enabling single units to be studied in unanesthetized animals promise to revolutionize many accepted ideas of sensory information processing. The other warning arose in connection with the use of computers to estimate the information content of sensory signals, when it was emphasized that what an observer can extract from a neural signal may bear very little relation to what the organism can do with it. Computer analyses only determine upper bounds, which can be ludicrously unrealistic as estimates of performance.

A third topic of great interest but of frustratingly small information-content was the central regulation of sensory information processing. Whitfield and Jung indicated that in most sensory pathways efferent fibers have been found which are presumed to lay open peripheral mechanisms to central control. What purpose might be served by such central regulation?

Automatic or voluntary gain control is one obvious possibility; but for this one would scarcely need the large number of efferent fibers observed in the auditory and visual pathways. Control of resolving power, differential control of sensitivity among members of a neuronal population, and even code-switching according to the current perceptual task, were among other suggestions. However, apart from the hint that receptive fields in unanesthetized animals are not fixed in shape, little relevant evidence was adduced.

A somewhat different class of efferent signal discussed by H.-L. Teuber and others is the corollary discharge presumed (though not yet unequivocally demonstrated) to run from motor to sensory centers, to prevent misinterpretation of the sensory effects of voluntary movement. If this hypothetical device were to operate by suppressing incoming signals (for example, from the eyes during voluntary eye-movements) this could employ some of the efferents in the optic pathway; but it was not clear that the suppression need take place at such a peripheral level, or even that it must take place at all. There seemed to be general acceptance for the view that only re-evaluation and not cancellation of sensory changes is required during voluntary movement.

It will be evident that from a theoretical standpoint sensory psychophysiology was felt to be in a rather precarious state. Old presuppositions are being upheaved, but their last traces have not disappeared (and perhaps rightly) from much of current experimentation. The need for clearer thought about the relation between physiological and psychological phenomena was urgently felt. It was generally agreed that with proper cross linkage the study of perception could supply useful clues to the design of fruitful physiological experiments, and vice versa. The role of the theory of information processing seems promising in this connection, since it belongs in a sense to both areas, and offers a kind of "interlingua" in terms of which data from one can be expressed so as to suggest or discipline ideas in the other.

On the other hand, the conceptual structures "ready-made" on the theoretical side today were felt to be woefully inadequate for all but the simplest of sensory situations. R. B. Livingston particularly emphasized the lack of any clear logical link between sensory experience and functional anatomy, suggesting that the "central gray core" of the central nervous system might be most relevant to the search for correlations. There was a general feeling that the simpler problems should be tackled first. As Mountcastle put it, with the strong concurrence of Eccles and Phillips, it is still from simple experiments, done well, that we shall learn what we most need to know in sensory physiology today.

Functional Organization of Movement

The shift of emphasis from static to dynamic system properties was amply illustrated in the third session (chaired by C. G. Phillips) where we were concerned with the preparation and control of ongoing action. What physiological signs may there be of the planning of voluntary action? How should we conceive of sensory-motor integration at a neural level? Have we enough circuit information on the cerebellum—or even on the spinal cord—to attempt a mathematical theory? These were typical of the questions that troubled us, tempered by Phillips' admonition that only problems with a reasonable prospect of solution were worth discussing.

One of the early triumphs of neural communication theory was the elucidation of the α-γ muscle control system as a follow-up servo. Now, it seems, there is some doubt whether the γ loop gain available is sufficient, and Phillips quoted evidence of Evarts and others that in higher mammals effective feedback loops extend up to cortex. Does

cortex then compute the mismatch between the action realized and the action intended, and use the result to initiate corrective force? If so, one could expect to record two kinds of cortical activity: one concerned with corrective action and the other reflecting the setting of the goal, as it were, before each action was initiated.

Data presented by E. V. Evarts supported this suggestion and aroused great interest. Electrodes recording from pyramidal cells of unanesthetized monkeys allowed cortical signals to be monitored during a highly motivated, learned movement. These signals were found to match closely the activity of the spinal motoneurons with which they were associated, varying according to the load against which the limb was working rather than merely specifying its position. Neighboring units were found to differ considerably and nonrandomly in their activity patterns, raising the question whether the motor cortex might not be compartmentalized in its organization in much the same way as the sensory cortex, so as to enable motor goals to be spelled out in fine detail. The fact that human subjects can quickly learn to control individual units selectively in their own motor nerves was cited as confirming the astonishing degree of detailed specificity present at least in the human motor-cortical population. The notion of reflex fields first introduced by Sherrington now seems to have a cortical counterpart, which may be considered to correspond to the idea of receptive fields on the sensory side. As was pointed out by Sherrington, such fields could change in size and shape. In discussion it was generally agreed that this area had reached a stage at which collaboration between control theorists and physiologists might be particularly fruitful in suggesting crucial experiments.

And so we turned to the cerebellum, most invitingly schematic of all vertebrate neural systems. Here, if anywhere, said Eccles, the computer engineer should come into his own. In the last few years, thanks largely to work in Eccles' own laboratory, our knowledge of cerebellar physiology has reached an unprecedented level of detail, backed by the anatomical work of Szentagothai and others. High hopes have been expressed that we now know enough for the mathematicians to be set to work on a realistic theory. We know which of the main components are excitatory and which are inhibitory.

Because of the astonishing regularity of cerebellar structure we can generalize about the directions in which excitatory and inhibitory signals run, as well as about their cells of origin and destination. Transmission times, synaptic time constants, thresholds—almost all key parameters are known. What then is lacking? Why did our discussion echo the feeling, expressed at other recent meetings on the cerebellum, that detailed theoretical modeling may still be premature?

The reason, I think, was twofold. First, at a neuronal level, a vital part of the functional circuit of the cerebellar system lies outside the well-mapped cerebellar cortex, in the deep nuclei and other regions less well known. Indeed, as Eccles pointed out, some of its most important integrative feedbacks come from the evolving movement itself. Moreover, although cortical connection patterns have been carefully established, the quantitative implications of multiple reciprocal innervation, by populations with differently shaped arborizations, involve too many still-undetermined quantities to be easily tabulated at this stage. In any case it is clear that the experimental and natural conditions are not strictly comparable.

Second, at a functional level, we lack any clear idea or even a sharp hypothesis, as to what the cerebellum is there to do. To be sure, the effects of cerebellar lesions, both clinical and experimental, give a general picture of its function as a modulator, integrator, and stabilizer of the components of voluntary action. But whether it stores learned patterns, or merely insures obedience to those stored elsewhere; whether it functions as a timing organ or merely as a smoothing device; what use it makes of the sensory information projected to it from all modalities—these and a host of other clues to a sensible theory are still missing or highly debatable. Our discussion, stimulated by a perceptive summary of the contrast between cerebellar and cerebral cortex by V. Braitenberg, left the theorists among us soberly encouraged that, in this area at least, there was plenty of scope for good ideas as well as good data.

The role of sensory signals in the regulation of movement, it was suggested by H. H. Kornhuber, offers as yet the most promising field for the application of systems analysis to neurophysiology. But there are serious snags, due to the adaptive nature of neural systems. The Nyquist analysis of the oculomotor system, for example, works well for periodic stimuli; but with unpredictable stimuli the system constants change in ways too complex for theoretical treatment. Only for lower forms such as insects and crabs has exact cybernetic analysis of sensorimotor coordinations been successful.

Qualitatively, however, some quite complex sensorimotor relationships have been clarified, particularly in the control of eye movements, and of limbs under cutaneous feedback. Evidence was mentioned that the cerebellum participates in the planning as well as the execution of saccadic eye movements; and Teuber reported the discovery by E. Bizzi at M.I.T. that the position of the eyes, rather than the force in the eye muscles, is represented in neural activity of the frontal eye field. Convergence of different sensory modalities on single motor cortical neurones is common, but how this sensory information is used is far from clear.

Intriguing clues which are emerging from studies of adaptation to sensory rearrangements were also reported by Teuber. Distortion of visuomotor coordination by prism spectacles, as in the work of Held and Hein, can be overcome more rapidly and completely if the corrective information is gained from the sensory results of active movements rather than from passive observation. The same movements imposed passively, in such a way as to generate no corrective information, of course produce no improvements. The suggestion was made that readaptation here requires, or is facilitated by, the presence of the hypothetical corollary discharge, but more direct evidence is still lacking.

Methodological Stocktaking

As may be imagined, a good deal of stocktaking went on in the concluding discussions under each of the foregoing heads; but as the last half-day was expressly devoted to methodological topics it will be convenient to report the substance of all these discussions together. Broadly speaking, we asked ourselves three questions:

(1) What types of investigation seem most promising as sources of bridge-building material between experiment and theory?

(2) What trends do we feel to be most profitable in theoretical model-making today?

(3) What implications are there, if any, for the training of research workers in this area?

Bridge-building material. Even if the best bridges between experiment and theory are those formed in one and the same head, different types of experimental study obviously vary in the suggestiveness, precision, or manageability of their material for the theorist. What the theoretical model maker wants most of all is the kind of data that enable him quickly to narrow down his range of options: to decide that a whole class of alternatives "won't do." It may easily happen that some of the most important studies for this purpose become relatively neglected, either because of the intrinsic physiological interest of some other types of experiment, or because of mere tryanny of fashion.

Thus a severe restriction on any neural theorist is that the system he postulates must have been capable of growing that way. Some genetic, embryological, and developmental data may be more crucial to the early stages of his theorizing than many measurements on mature brains.

Similarly, the fact that more primitive animals can employ simpler neural structures to do a given job may help considerably to identify the more essential features of a complex system like the cerebellum; and the abilities lacking in these simpler animals may offer clues to the functions of the elements they lack.

The effects of brain lesions, both experimental and clinical, have similar potentialities, though fraught with difficulties of interpretation, especially in clinical cases. There was particularly strong agreement with the views expressed by Teuber and Mountcastle as to the growing value of psychophysical studies for sensory physiology, both under normal conditions and those of stimulus deprivation or distortion.

The growth of small neural nets in tissue culture, and neural transplantation experiments, were mentioned as useful ways of isolating small enough systems to tackle mathematically. This was also a point in favor of studying animals such as *Aplysia* with ganglia having very few and easily identified neurons.

The theoretical importance of simultaneous multiunit records was frequently stressed, as a necessary complement to the single-unit studies favored by some participants. The brain, as Gerstein put it, is a parallel system, in which most of the information is carried by the ensemble of neurons rather than the single unit. We must find ways of studying the ensemble if progress is to be made. Stochastic multichannel stimulating and recording techniques were described by D. G. Lampard and S. L. Redman which may point to one way ahead here.

Sequential records from different sites are of limited value as a substitute; for (i) in an adaptive system it is not possible to assume that responses to the same stimulus recorded successively from different units are equivalent to simultaneous recordings; and (ii) to discover statistical dependencies between many units on the basis of sequential recordings is virtually impossible.

What we most lack is any means of identifying the significant elements of signal traffic in large cell masses—of passing from the single cell level to the level at which the slow-traveling waves of cortical activity are our data, for example. What would constitute a simple, well-done experiment at that level? Does the study of cortical surface potentials, or of evoked electroencephalogram potentials in human subjects who can report their perceptual correlates, offer a useful way in? Some of us felt that these techniques, now greatly sharpened by the advent of powerful computing methods and used in conjunction with single-unit studies, hold considerable promise. Others were not so sure. All were agreed on the special care needed in the design and interpretation of EEG experiments, with proper regard to individual differences in cortical anatomy, if the literature were not to be flooded with results of little generality.

Neuronal biochemistry, especially with the development of multibarrelled micropipette techniques, is opening up a whole new dimension for the neural theorist. The importance of axonal transport phenomena and other dynamic aspects of neuronal metabolism was emphasized by F. O. Schmitt and other participants. On the whole, however, it seemed too early to assess the relevance to the theory of neural information-processing. Current speculations as to the possible storage of information in neuronal RNA were not discussed.

Present trends in model-making. What then has happened to "neural net modeling" over the past 20 years? Cowan, in introducing the topic, felt that the most noticeable shift had been from Boolean formalisms postulating switch-like McCulloch-Pitts "formal

neurons" to powerful stochastic process techniques for the analysis and prediction of pulse-interval distributions and mass activity. He and others emphasized that different kinds of mathematical approaches are required for different purposes. To handle the problems of neuronal potential fields there would always be a need for biophysical models of the sort elegantly developed by Rall. To deal with local system properties differential equations were useful, as in the models of R. L. Beurle; to handle global properties the possibilities of multidimensional topology were being explored—with what value remained to be seen. Between the two extremes there is a gap at present unfilled.

H. C. Longuet-Higgins spoke in favor of trying to design automata to perform brain-like functions, in the hope that the unsuspected problems we would encounter might force us to discover solutions that the brain also may have had to employ. It was generally agreed that mere imitation of function could be a snare unless the solution had a reasonable chance of being unique; but it was suggested that information-flow-modeling on the principles of automata theory did offer a most appropriate conceptual framework for the design and interpretation of experiments on large-scale behavior. As G. C. Quarton put it, "loyalty to structure" and "loyalty to function" may call for quite different kinds of model-making. A. M. Uttley pointed out that by looking at a neural network simply as a conditional probability machine and imposing a very general criterion of informational efficiency, one could arrive at predictions leading to fruitful experiments.

The usefulness of model neuromimes was illustrated by a neat and remarkably successful simulation by G. Szekely of the spinal circuit controlling a salamander's leg movements; but the pros and cons of hardware models versus computer simulations versus mathematical analyses were not much discussed. Generally it was felt that the nature of the problem, rather than dogmatic prejudice, must be allowed to dictate the choice of method. It seemed clear that the kind of insight offered by each would have a different appeal to different temperaments.

This bears upon the whole question of what constitutes an explanatory theory—a question raised more than once, but not resolved. From observation of the contemporary scene, and of our 33 various reactions to theories proffered during the week, it seems woefully possible that the releaser of the "aha!" feeling for a mathematician may sometimes be quite different from that for an experimentalist. "Reduction to the familiar" for the theorist may mean the discovery that a barbarous physiological situation can be reduced to formal order by a familiar mathematical technique. For the experimentalist it may mean the recognition of a qualitative analogy between a new and a familiar situation, both equally intractable mathematically. Each may be tempted to reply "so what?" to the protestations of the other that he now "understands." Once again, we face Eccles' reminder that it is problem-centered rather than technique-centered investigation that makes good science.

What to teach. This perhaps brings us naturally to our concluding topic. How can we train research biologists to take advantage of what both theory and experiment have to offer in this area? Should every physiology student have courses in information theory, servo theory, automata theory, and high-powered mathematics, for example?

The general feeling among physiologists was that at the undergraduate stage at least there was too much essential biology to be learned for anything so ambitious to be added. Enough acquaintance with mathematics (including geometry) to give some idea of its scope and limitations; familiarity with the qualitative ideas of feedback theory and communication theory; and a working knowledge of statistics and computer programming as applied to experimentation: these seemed to be as much extra as we could expect a good physiology-degree course to include. The general principles of experimental design—for example, that an experiment gives most information when the probabilities of its various outcomes are equal—should also be covered, as a standard part of any science course.

Nor was there much support for the idea of hybrid undergraduate degrees in brain science. People should concentrate on mastering the facts of anatomy and of animal behavior by the age of 20, said Whitteridge; other things can follow in due course. Other participants urged that the more mathematical disciplines should be taught early, perhaps even in high school. It was thought best to accept the fact that only a proportion of students will be suited to both theoretical and experi-

mental thinking. As Barlow put it, one should strengthen people at their strong points rather than their weak points. They can then choose the specialist theoretical techniques (stochastic process theory, hierarchic automata theory, advanced computer techniques, servo system analysis were mentioned) to be acquired later according to their abilities and the research problems that come to interest them. Inasmuch as educational experiments of this sort are now being mounted on different lines in various countries, it was pointed out that we should have more concrete evidence to go upon in the near future.

Perhaps the greatest unanimity was on the human and personal aspects of a good interdisciplinary research training. To have a "feeling" for several disciplines cognate to one's own, so that one knows roughly the shape of questions that can be sensibly asked in case of need, may be far more important than to have a rusting armament in one of them which one seldom uses. But knowledge of this kind is best acquired through personal contact between specialists in each discipline who are prepared to give time to listen to one another on favorite topics. Similarly, it is through regular personal contact with the older hands who can pass on their enthusiasm in research to the next generation. Their job, as Teuber and Eccles particularly emphasized, is to exhibit attitudes as much as to teach facts and techniques: to stir imagination and curiosity and to convey that combination of critical power with humility that marks the true scientist. Perhaps personal contact is also the best way to convey two other qualities suggested as desirable—the ability to express thoughts with precision in ordinary language, and an ability to profit from the history and the philosophy of science.

Who then is sufficient for these things? Perhaps it is as well for our peace of mind that we disbanded without any formal resolutions, other than one of gratitude to the supporting bodies who made possible this unusual and rewarding inquiry.

The present report, compiled with the assistance of E. F. Evans, J. C. G. Nicholson, D. Regan, and J. P. Wilson, is of course a personal and highly condensed sample, but has benefited from the comments of all participants.

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Calendar of Events

National Meetings

January

25–27. **Mathematical** Assoc. of America, 51st annual, San Francisco, Calif. (H. M. Gehman, MAA, Executive Director, % SUNY at Buffalo, N.Y. 14214)

25–27. **Symmetry Principles** at High Energy, 4th conf., Coral Gables, Fla. (Conference on Symmetry Principles at High Energy, Center for Theoretical Studies, Univ. of Miami, Coral Gables)

27–1. American Group **Psychotherapy** Assoc., conf., Chicago, Ill. (M. Schiff, AGPA, Room 702, 1790 Broadway, New York 10019)

28. Fourth **Mössbauer** Symp., Chicago, Ill. (P. A. McNulty, New England Nuclear Corp., 575 Albany St., Boston, Mass. 02118)

28–2. Institute of **Electrical and Electronics Engineers**, winter power mtg., New York, N.Y. (J. W. Bean, American Electric Power, 2 Broadway, New York 10008)

28–2. **Testing and Materials,** winter mtg., Atlantic City, N.J. (T. A. Marshall, Jr., American Soc. for Testing and Materials, 1916 Race St., Philadelphia, Pa. 19103)

29–31. National Assoc. of **Private Psychiatric Hospitals,** 35th annual mtg., Miami Beach, Fla. (The Association, 353 Broad Ave., Leonia, N.J.)

29–31. Society of **Thoracic Surgeons,** annual mtg., New Orleans, La. (F. X. Byron, Executive Secretary, Society for Thoracic Surgeons, City of Hope Medical Center, 1500 E. Duarte Rd., Duarte, Calif. 91010)

29–1. American Assoc. of **Physics Teachers,** annual mtg., Chicago, Ill. (S. S. Ballard, Univ. of Florida, Gainesville 32603)

29–1. American **Meteorological** Soc., 48th annual, San Francisco, Calif. (K. C. Spengler, AMS, 45 Beacon St., Boston, Mass. 02108)

29–1. American **Physical** Soc., annual mtg., Chicago, Ill. (R. G. Sachs, Box 344, Argonne, Ill. 60440)

29–3. **Bio-Physical Techniques,** Hopatcong, N.J. (Center for Professional Advancement, P.O. Box 66, Hopatcong 07843)

February

1–2. American **Chemical** Soc., 3rd Middle Atlantic regional mtg., Philadelphia, Pa. (H. Reiff, Smith, Kline and French Labs., 15th and Spring Garden St., Philadelphia 19130)

1–3. **Solar Astronomy,** Tucson, Ariz. (N. Sheeley, Kitt Peak National Observatory, 950 North Cherry Ave., P.O. Box 4130, Tucson 85717)

3–7. American Acad. of **Allergy,** annual mtg., Boston, Mass. (J. O. Kelley, Executive Secretary, 756 N. Milwuakee St., Milwuakee, Wis. 53202)

3–9. National **Asphalt Pavement** Assoc., annual, Los Angeles, Calif. (The Association, 6715 Kenilworth Ave., Riverdale, Md.)

4–7. American **Crystallographic** Assoc., Tucson, Ariz. (W. L. Kehl, % Gulf R&D Co., P.O. Drawer 2038, Pittsburgh, Pa. 15230)

5–6. **Underwater Photo-Optical Instrumentation** Applications, San Diego, Calif., (Society of Photo-Optical Instrumentation Engineers, P.O. Box 288, Redondo Beach, Calif. 90277)

5–8. American Soc. of **Heating, Refrigerating and Air-Conditioning Engineers,** semiannual mtg., Columbus, Ohio. (J. I. Szabo, 345 E. 47 St., New York 10017)

5–9. **Applied Transducers** Workshop for Scientists and Engineers, Hopatcong, N.J. (Saul Gordon Associates Center for Professional Advancement, P.O. Box 66, Hopatcong 07843)

6–7. **Sanitary Engineering,** 10th conf., Urbana, Ill. (Div. of University Extension, Room 116e, Illini Hall, Champaign, Ill. 61820)

6–8. American College of **Radiology,** annual mtg., Chicago, Ill. (W. C. Stronach, American College of Radiology, 20 N. Wacker Dr., Chicago 60606)

6–9. **Reinforced Plastics,** 23rd annual div. conf., Washington, D.C. (L. P. Williams, Soc. of the Plastics Industry, 250 Park Ave., New York 10017)

7. **Medical Library** Group of Southern California, Los Angeles, Calif. (Medical Library Assoc., Inc., 919 N. Michigan Ave., Chicago, Ill.)

7–9. American Acad. of **Occupational Medicine,** Washington, D.C. (B. D. Dinman, Dept. of Industrial Health, Univ. of Michigan, Ann Arbor 48104)

7–9. American Assoc. of **Petroleum Geologists,** Southwestern Section, Wichita Falls, Tex. (E. A. Baldon, % Tom B. Medders, 414 City National Bldg., Wichita Falls 76301)

7–9. Methods of **Air Pollution and Industrial Hygiene** Studies, 9th conf., Pasadena, Calif. (P. K. Mueller, Air and Industrial Hygiene Laboratory, California State Dept. of Public Health, Berkeley 94704)

7–10. American **Educational Research** Assoc., Chicago, Ill. (The Association, 1126 16th St., NW, Washington, D.C.)

8–10. Pacific Northwest **Oceanographers,** 17th annual mtg., Seattle, Wash. (L. H. Larsen, Dept. of Oceanography, Univ. of Washington, Seattle 98105)

8–10. Society of **University Surgeons,** annual mtg., Lexington, Ky. (W. G. Austen, The Society, Massachusetts General Hospital, Boston)

10. **History of Science,** Columbus, Ohio. (R. L. Stuckey, Ohio Acad. of Science, 1735 Neil Ave., Columbus 43210)

12–14. **Aircraft Design** for 1980, Washington, D.C. (Meetings Manager, 1290 Sixth Ave., New York 10019)

13–15. **Aerospace and Electronic Systems,** Los Angeles, Calif. (R. M. Emberson, 345 E. 47 St., New York 10017)

13–15. **Military Aircraft Systems,** Washington, D.C. (Meetings Manager, 1290 Sixth Ave., New York 10019)

13–16. American **Public Power** Assoc., Sacramento, Calif. (A. Radin, General Manager, Suite 830, 919 18th St., NW, Washington, D.C. 20006)

14–16. **Offshore Exploration** Conf., New Orleans, La. (M. F. Oberacker, Manager,

Administration, OECON, P.O. Box 88, Palos Verdes Estates, Calif.)

15. **Commercial Application of Ultrasonics**, New York, N.Y. (J. N. Antonevich, Blackstone Corp., 1111 Allen St., Jamestown, N.Y. 14701)

16–17. **Neurobiological Aspects of Psychopathology**, New York, N.Y. (J. Zubin, 722 W. 168 St., New York 10032)

16. **Reliability Symp.**, 9th annual West Coast, Beverly Hills, Calif. (R. J. Green, TRW Systems, Building E2/6043, One Space Park, Redondo Beach, Calif. 90278)

16–18. **Repair and Regeneration**, symp., San Francisco, Calif. (Continuing Education in Health Sciences, University of California, San Francisco Medical Center, Parnassus and Third Ave., San Francisco 94122)

18–21. American Inst. of **Chemical Engineers**, 63rd natl. mtg., St. Louis, Mo. (L. L. Fellinger, Monsanto Co., 800 N. Lindbergh, St. Louis)

18–22. Society of **Economic Geologists**, New York, N.Y. (R. A. Laurence, P.O. Box 1549, Knoxville, Tenn. 37901)

18–22. Technical Assoc. of the **Pulp and Paper Industry**, 53rd annual mtg., New York, N.Y. (K. G. Chesley, TAPPI, 360 Lexington Ave., New York 10017)

19–20. **Engineering Project Investment Analysis**, Austin, Tex. (D. E. Griffith, Program Director, Taylor Hall 153, College of Engineering, Univ. of Texas, Austin 78712)

19–21. **Biophysical Soc.**, 12th annual mtg., Pittsburgh, Pa. (Local Arrangements Chairman, I. Bendet. Biophysics Dept., Univ. of Pittsburgh, Pittsburgh 15213)

19–23. **Transportation Engineering** Conf., San Diego, Calif. (W. H. Wisely, 345 E. 47 St., New York, N.Y. 10017)

19–25. American Soc. of **Civil Engineers**, San Diego, Calif. (W. H. Wisely, ASCE, 345 E. 47 St., New York 10017)

21–22. **Industrial Pharmacy**, Austin, Tex. (W. L. Guess, Dept. of Pharmacy, College of Pharmacy, Univ. of Texas, Austin 78712)

22–24. Central **Surgical** Assoc., Cleveland, Ohio. (B. L. Willman, 1402 S. Grand Blvd., St. Louis, Mo. 63104)

25. **Psychoanalysis**, 6th annual scientific conf., New York, N.Y. (A. Blatt, Chairman Program Committee, 7 W. 96 St., New York 10025)

25–29. American Inst. of **Mining, Metallurgical, and Petroleum Engineers**, 97th annual mtg., New York, N.Y. (C. Hopkins, 345 E. 47 St., New York 10017)

26–28. American **Physical Soc.**, Boston, Mass. (W. W. Havens, Jr., Columbia Univ., New York 10027)

26–28. Association of **Asphalt Paving Technologists**, Atlanta, Ga. (L. L. Kole, Box 619, Ann Arbor, Mich. 48107)

26–28. Association of **Iron and Steel Engineers**, Western mtg., San Francisco, Calif. (T. J. Ess, 1010 Empire Bldg., Pittsburgh, Pa. 15222)

26–1. American Assoc. of **Junior Colleges**, 48th annual conv., Boston, Mass. (The Association, 1315 16th St., NW, Washington, D.C. 20036)

27. National **Multiple Sclerosis** Soc., New York, N.Y. (S. Lawry, Executive Director, 257 Park Ave., South, New York 10010)

27–28. National **Dairy Engineering** Conf., 16th annual, East Lansing, Mich.