emission stimulated by an irradiating electron beam. By suitable acceleration and energy-filtering we hope to form images of the atoms comprising the sample. In this way it should be possible in principle to distinguish between the various atoms of the sample (for example, carbon, nitrogen, and oxygen) and the substrate (for example, beryllium), since the Auger electron emission energies are a strong function of the atomic number.

6) In order to achieve high resolution by imaging Auger-emitted electrons we propose the design of an objective lens with low aberration and a high numerical aperture. In principle, such a lens can be built by interposing suitably shaped conducting, thin foils, thereby avoiding the limitations contained in "Scherzer's theorem" (*5*). (This scheme requires that the emitted electrons be first accelerated to some 50 kv before encountering the foils.) Recent experiments in this laboratory on evaporated thin foils and computer ray tracing indicate that this approach is feasible.

We would also like to propose that the term "molecular microscopy" be reserved for those instruments and devices in which neutral atoms and molecules are the probing particles. Light microscopes and electron microscopes are named for the probing radiation, and it seems reasonable to do so with the molecular microscope. We feel strongly about the adoption of this convention since we have been exploring in the last 2 years the possibilities of studying high spatial resolution and, under various conditions, the emissions of neutral atoms and molecules (evaporation and scattering) from surfaces (*6*). Our first instrument, in which water molecules were used, is now beginning to yield data.

We agree with Breedlove and Trammell on the importance of neutral atoms as probing particles, and we believe that the development of such techniques will be of particular importance in surface studies involving the weak forces between molecules and in delicate microanalysis appropriate to chemical and biological problems.

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3. We have exposed ^{3}H-labeled DNA on a copper substrate held at 4.2°K to 20 coulomb/cm^2 of charge from 800-volt electrons (current density, 0.5 ma/cm^2). Radioactivity measurements before and after electron irradiation show that about 55 percent of the tritium label remains when the specimen is irradiated at 4.2°K, whereas only 8 percent of the tritium label remains if the specimen is irradiated at room temperature (all other experimental conditions remaining the same). In these experiments it was necessary to make all radioactivity measurements at room temperature. Hence, it is possible that some tritium label was lost while the specimen was being warmed to room temperature from 4.2°K. We are endeavoring to carry out a series of more quantitative experiments on electron damage to biological molecules as a function of temperature and radiation dosage in which both the electron irradiation and the radioactivity assay will be made at the same temperature.
4. W A. Pryor, *Sci. Amer.* **223**, 70 (August 1970); ——— and V. Tonellato, *J. Phys. Chem.* **73**, 850 (1969).
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7. We thank Dr. H. C. Box for discussions concerning his recent work and K. Thomson for his able assistance in the electron damage experiments.
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Autoshaping

Gamzu and Williams have reported the "classical conditioning of a complex skeletal response," by use of the technique of autoshaping (*1*). If a light is repeatedly projected on a standard response key a few seconds before a food dispenser operates, a hungry pigeon will begin to peck the key, presumably as it will peck other stimuli—for example, seed pods—which are related to food in less arbitrary ways. The process seems more accurately described as the classical conditioning of a stimulus which elicits a response of phylogenic origin.

An experiment performed at Indiana University in 1946 had other features that may be of interest. The upper half of one end of a pigeon box of standard size consisted of a translucent plastic plate. A food dispenser was located near the floor at the right. A spot of light about 6 mm in diameter was projected on the plate at the usual height of a pigeon key. The spot appeared at the right edge of the plate and moved to the left, covering the length of the plate in about 4 seconds. When it reached the left edge, the food dispenser operated.

The pigeon began to peck the spot, as in autoshaping, but it pecked as if it were driving the spot across the plate. When the plate was lightly greased, a print lifted from the surface showed the contacts made by the beak in a single transit. Prints showed slashes, often 2.5 cm or more long. It was observed that they were all made when the beak moved from right to left.

In a later stage of the experiment, the slashes were less specifically directed. As the spot approached the left-hand edge, sickle-shaped curves were described, sweeping around and down toward the dispenser. Adventitious contingencies involving the operation of the dispenser may have been responsible for this shift in topography.

It seems clear that a feature of the environment can be converted into a stimulus that elicits responses characteristic of the phylogenic endowment of the species. The observations reported by the Brelands were of that nature. The effect is quite different from operant conditioning, even though both processes generate responses having similar topographies.

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Reference

1. E. Gamzu and D. R. Williams, *Science* **171**, 923 (1971).

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Skinner's note reports an early encounter with autoshaping, together with an ingenious method of recording unexpected topographical elaborations of the response. In his experiment as in ours, the significant finding was the development of orderly, externally directed skeletal behavior beyond that specified by operant reinforcement contingencies. Not only does such behavior belie the "law of least effort," but, more importantly, it underscores the need to include factors other than response-reinforcer contingencies in the analysis of learned behavior. It is clear that phylogenic considerations must be taken into account when considering how experience integrates behavior with an environment—even a phylogenically novel one.

The experimental analysis of operant behavior has in general focused on situations where the nature of the response, as well as interactions among stimulus, response, and reinforcer, are presumed to be arbitrary—that is, dependent only on experimentally controlled relationships. The application of such results to situations involving nonarbitrary relationships and highly organized response systems must be made with care. Thus, while it is possible that adventitious contingencies of reinforcement were responsible for some aspects of the systematic elaboration of the pecking response described by Skinner, the possibility must also be considered that each component of the behavior represents an organized response pattern released into the situation by the stimulus configuration and an associative process. A procedure previously reported by Williams and Williams (*1*), which prevents the operation of direct or adventitious reinforcement contingencies, might prove helpful in analyzing the origin and development of the behavior Skinner describes. Such a procedure is currently being used in our laboratory to study the origin of topographical variants of the key-pecking response; the procedure has also proven useful for analyzing the interaction of different sources of control in the situation we reported (*2*).

It seems appropriate to acknowledge here that our work, although it adds new considerations to the analysis of operant behavior, depends as heavily on methods and concepts originally developed by Skinner as it does on naturalistic methods and ethological analysis.

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Scanpaths and Pattern Recognition

In their report (*1*) Noton and Stark present evidence that in 39 out of 60 instances four subjects made essentially the same initial sequence of eye movements (scanpaths) the second time they viewed a low-visibility picture as they did the first time. This result is not surprising. What is surprising is the authors' suggestion, based on these findings, that scanpaths tell us something about how subjects remember and recognize patterns. Noton and Stark acknowledge that under normal conditions recognition does not require eye movement. They then propose an internal attention mechanism in which the subject processes successive features of the pattern in the same sequence as that of the motor scanpath. On this precarious peg they hang their theoretical argument.

There are a number of problems associated with this line of reasoning. If a nonsense figure, never before seen by the subject, is exposed tachistoscopically at an exposure time too short to allow for eye movements, or if it subtends a visual angle of less than 2° (eye movements unnecessary), the subject will recognize the pattern exposed again under the same conditions, or under new conditions in which eye movements are permitted or even necessary. In this example there has been no opportunity for eye movement during the learning phase. How then can pattern recognition be due to the internal representation of the "memorized sequence of behavior" (*1*)? What sequence of behavior?

We recognize objects in various orientations and under a multitude of conditions. No one can seriously believe that if all subjects were forced, during recognition, to scan figure 1 (*1*) by a completely different path, even starting from the final scan and working backward, the picture would not be easily recognizable. The fact is that distinctive features are "normally" analyzed by the central nervous system without repetition of a fixed sequence.

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Our experiments were specifically and carefully designed to force the occurrence of scanning eye movements so as to externalize part of the visual pattern recognition process and thus to make it available for objective measurement and scientific study. A major assumption, one that we pointed out in our report (*1*), is necessary when we extrapolate from eye-movement scanpaths to hypothecated serialized attention shifts for the further application of our results to the normal viewing of bright small pictures or to pictures presented tachistoscopically. We stated: "Normally this processing is largely internal and beyond investigation" (*1*). In Noton's earlier "theory" paper (*2*) and in our full experimental paper (*3*), we discussed the serialized attention shifts; the fact is that these are not completely deterministic, requiring a feature-network modification of the feature-ring theory, and, in the case of tachistoscopic presentations, requiring short-term memory as well.

We are surprised that Spitz does not recognize the value of our experimental discovery of the scanpath in a field where so few hard data exist. The "precarious peg" that Spitz mentions (*4*) is not our extrapolation but the willingness of psychologists to theorize about processes in the central nervous system concerning which no experimental evidence exists. An example from his technical comment is characteristic—"The fact is that distinctive features are 'normally' analyzed by the central nervous system without repetition of a fixed sequence" (*4*).

The serial feature-ring theory based upon analogies from computer science preceded and predicted the experimental results (*2*). The scanpath is a clear objective finding presented in both the learning and recognition phases of viewing under experimental conditions similar to ours. Finally, the scanpath plays an important role in the strategy of eye-movement control (*5*) and will have to be taken into consideration when psychological theories of visual pattern recognition are further elaborated.

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