other stimulated structures (Table 1).

After achievement of criterion performance to lateral geniculate (LG) stimulation, peripheral versus LG conflict was carried out in three cats. Occasional instances were obtained in which electrical stimulation of the LG at the higher frequency successfully contradicted the flicker cue at the lower frequency, while LG stimuli at the lower frequency seldom prevailed over higher frequency flicker signals. Lateral geniculate stimulation was uniformly ineffective to control behavior in auditory-LG conflict.

Our findings of high levels of differentiated stimulus generalization and rapid transfer to RF stimulation provide strong support for the contention that discriminations such as these are mediated by the average temporal patterns of firing in extensive neuronal ensembles rather than by discharges in particular synaptic pathways representing a specific experience. These RF stimuli cannot conceivably reproduce a unique and intricate topology of synaptic discharges corresponding to those normally excited by particular peripheral signals. Undoubtedly, gross electrical stimuli merely impose a corresponding temporal pattern upon masses of cells. The stability of performance when the fine structure of RF stimuli was altered, as well as the stimulus generalization obtained so readily when other brain regions were stimulated, provides further proof that these discriminations do not depend upon activation of specific synapses or pathways. These results cannot be attributed to nonspecific factors because they require correct discrimination between two different patterns of stimulation applied to the same site.

Lateral geniculate stimulation successfully contradicted visual cues only when the rate of central stimulation was more rapid than the flicker. Lateral geniculate stimuli completely failed to contradict auditory cues at either rate. Visual cues were hardly ever found successful in contradicting auditory cues. These results suggest that LG stimulation simulates visual sensation. The ability of RF stimuli to preempt control of behavior whether in conflict with visual or auditory cues shows that RF input does not merely simulate the sensations caused by ordinary sensory events, but seems to provide unique access to the brain mechanism which interprets sensory events of whatever modality. These findings suggest that the organized firing of anatomically extensive neuronal ensembles accomplished by patterned RF stimulation simulates the activation of specific memories.

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7. Some animals learned first to discriminate between the two auditory cues, A_1 and A_2, and then received transfer of training to the two visual cues, V_1 and V_2. Other animals first learned the visual and then the auditory discrimination. The two stimulus repetition rates were 5 versus 1.8 hertz in three cats and 4 versus 2 hertz in the other three. For three cats, the differential responses consisted of pressing either the left- or the right-side pedal on a work panel to avoid foot shock (conditioned avoidance response). For the other three cats, the same differential response was motivated by food reinforcement. Food-motivated animals received food only in the training situation, except on weekends. Sessions were usually 1 day apart and consisted of 40 to 200 trials.
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9. All brain stimuli were trains of 30 biphasic constant-current pulses 200 μsec wide at a frequency of 200 pulses per second, repeated at rates corresponding to the discriminated flicker and click signals. Stimulus intensity was selected by ascertaining the current threshold to elicit motor responses or to interfere with ongoing behavior, and then reducing to approximately one-half that value for training. Brain stimuli were specified by Nuclear-Chicago stimulators (model 7150) photically coupled to the brain by Tektronix stimulus isolation units (model 2620). Phase locking of peripheral and central stimulation and system timing was accomplished by a control unit of our own design. Electrophysiological responses were amplified with a Grass model 7 polygraph, stored on a 14-channel model 1000 Mnemotron tape transport, and analyzed with a PDP-12 computer.
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Origin of Martian Channels: Clathrates and Water

The similarity of many large martian channels to terrestrial ones has led to speculation that at some time in the past there was sufficient water on the surface of Mars to erode the observed channels (*1*). Some of these channels are so large, however, that flow rates many times that of the Amazon River are suggested. The fact that these channels frequently begin in "chaotic terrain" and lack tributaries implies an underground source capable of supplying the large flows.

The most obvious underground sources would seem to be either the rapid melting of an ice permafrost or the release of liquid water trapped beneath such a permafrost. Milton (*2*), noting that there is a considerable heat problem associated with the rapid melting of a permafrost, proposes that the liquid water might come from the depressurization of CO_2 hydrate, which could exist at depths where the pressure exceeded 10 bars and the temperature exceeded 0°C. This explanation ignores the obvious alternative that *liquid* water could already exist under those conditions. In fact, with the molecular ratio of H_2O/CO_2 of 15/1, cited by Milton, only about one-third of the available water can be tied up in the clathrate compound. Thus, a far larger volume of water would already be in liquid form and could be tapped simply by breaking the overlying permafrost without the decomposition of the clathrate (*3*).

Although a clathrate, rather than an ice, permafrost eliminates the need for an external heat source, it still requires a local heat source and thus the heat problem remains. In fact, since 150 cal is required to release 1 g of water from the clathrate compared to 80 cal required to release 1 g of water from ice, the heat problem is aggravated by the introduction of clathrate. What dispels the problem is not that the water is in the clathrate but that the water is already *liquid* at a temperature above its freezing point and no latent heat need be supplied at all!

Even if the only available water were bound up in the clathrate, in order to maximize the amount of water released the ratio of the clathrate to the rock matrix would have to be carefully adjusted at all points beneath the surface to just cool the surrounding rock to 0°C as the clathrate is exhausted. If a higher fraction of clathrate were present, once 0°C is reached, the necessary latent heat of decomposition would be obtained by the freezing of some of the liquid water just released, thus removing its contribution to the expected flow. In fact, even when excess water is already available in liquid form, clathrate decomposition would reduce the amount released because part of it would freeze.

Thus, regardless of the H_2O/CO_2 ratio, the presence of water in a CO_2 hydrate is detrimental to its release from an underground reservoir. The water must already be in liquid form if the release is to be rapid. As we have discussed (*3*), water trapped under a permafrost layer could be released by any meteorite impact or tectonic activity which ruptures the permafrost, without the necessity of heat transfer. Flow of water on the surface would not necessarily require that the martian atmospheric pressure be greater than the triple point of water, since at lower pressures a relatively thin skin of ice would form and prevent the water from boiling away (*3*).

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Polynesian Voyaging

At the end of the report (*1*) on the "Probable Fijian origin of quartzose temper sands in prehistoric pottery from Tonga and the Marquesas," Dickinson and Shutler conclude with the following point: "The implication for Polynesian origins is a continuing infusion of Melanesians into Polynesia from Fiji (and perhaps elsewhere). . . ." While I would not disagree with the generality of their statement, in the particular case in question I offer an alternative viewpoint based on work in Western Samoa.

In Western Samoa we have shown that if at an early stage in the sequence the inhabitants possessed pottery and then abandoned it, a few sherds from the early levels tend through subsequent disturbances to be incorporated in later deposits (*2*). The same phenomenon, for example, led some archeologists to initially misinterpret the duration of ceramics in the Tongan sequence by as much as 1000 to 1700 years (*3*). It has also led to a similar misinterpretation of the Marquesan evidence, where pottery has been dated to much later periods than is warranted (*2*). In my view, the handful of Marquesan sherds, most of them of local origin, plus a few imports, derive largely from secondary excavation contexts or, as at location M, from the surface. They are an indication that excavations in Marquesan sites have not yet sampled or, in one possible case at location M, have inadequately sampled those basal deposits with abundant pottery which lie at the early end of the Marquesan sequence and date before A.D. 300.

The alternative viewpoint I offer then is this: The few imported sherds from Fiji found in the Marquesas represent pots brought by the founding population, in contrast to those in Tonga, which are correctly interpreted as evidence of two-way contact at the earlier end of its prehistoric sequence, just as the pottery observed by the 19th-century explorers in Tonga attests to such contact at the late end of that sequence (*3*). This is understandable, for Tonga, Samoa, Futuna, Ellice, and other islands of western Polynesia are, together with Fiji, part of an historically well attested area of two-way voyaging in Polynesia (*4*). If the founding population of the Marquesas came from western Polynesia, as is generally agreed, the geological evidence that some of the pottery they brought with them was imported from Fiji is a further indication that this pattern of voyaging goes back to some point before A.D. 300. That should occasion no surprise, as it was probably from Fiji around 1100 to 1200 B.C. that founding populations moved into and rapidly colonized the adjacent western Polynesian island groups of Tonga, Samoa, and Futuna. Thereafter, there is evidence for imported adzes in contexts predating 500 B.C. in Tonga, indicating contact with Samoa or some other island of western Polynesia across the andesite line (*5*). In addition, Davidson and Shutler review other evidence from Samoa that supports a continuation of contact with Tonga and Fiji during the last 1000 years (*6*). On this basis I conclude that we are now fairly close to demonstrating archeologically that two-way voyaging in one of the two areas of Polynesia where it was known historically has been maintained since the time of settlement nearly 3000 years ago. In one case it led to the carrying of items exchanged in that contact to a new homeland in the Marquesas before A.D. 300.

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