in mass and temperature, with a resultant wide distribution of cooling times, onsets of life, and technological advances on the part of the intelligent beings that have evolved. This is to say that many thousands of years may separate the evolution of planets X and Y to the point of intragalactic communication. It follows that with this sort of "arrival spectrum" a given planet cannot afford to send signals throughout the centuries toward planets in a less advanced stage of development.

It may well be that in the older, cooler planets—located, in general, in the outer fringes of the galaxy—intelligent beings "arrived" technologically many thousands of years ago; in that case, the logical thing for them to do would be to wait for developing societies (like that of Earth) to signal *them,* once the new civilizations have "arrived."

In short, the rule for initiating intragalactic communication could well be: Let the innermost (relative to the center of the galaxy), and supposedly youngest, planet, send signals toward the outermost.

Thus it may well be up to Earth to make itself known by beaming signals of high intensity in narrow bands toward the outer stars in the galaxy. The time and cost involved is the price man must pay for being a curious and sociable being!

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Hello Out There (Project Ozma)

Twinkle, twinkle, little star,
  Out in space so very far.
If you're as bright as we think you are,
  Beam us a signal, little star.

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Olfactory Discrimination

Michelsen recently reported in *Science* [**130**, 630 (1959)] some interesting work from the Harvard Psychological Laboratory on olfactory discrimination in the pigeon. The general findings as summarized in the abstract were as follows: "A discrimination, based on olfactory stimuli, was established in two pigeons by an operant conditioning procedure. Results from control sessions demonstrate that the discrimination can be attributed only to the presence or absence of olfactory stimuli." I was pleased to learn of these findings since they made more tenable the hypothesis

advanced in a previous publication [A. D. Calvin, C. M. Williams, N. Westmoreland, *Am. J. Physiol.* **188**, 255 (1957)] that olfactory cues could be a supplemental aid in homing.

Michelsen has shown a great deal of ingenuity, working in a very difficult area; however, there are some aspects of the investigation as presented in *Science* which should be brought to the reader's attention.

One apparent weakness in the experimental design is in the control conditions. According to the article, during the control sessions the training schedule was so devised as to destroy the discrimination, and thus make the control sessions meaningless. In a personal communication Michelsen informed me that this implication was due to a clerical error and that actually the key numbers given in the paragraph on control conditions should be altered in such a fashion as to make the control sessions adequate in terms of the previous experimental procedure.

The second disturbing aspect of the study is not so easily remedied. It relates to the way the "olfactory" discrimination was established. Training was begun with sec-butyl acetate, which as Michelsen noted, is a trigeminal nerve irritant. After a discrimination was established, isooctane was substituted—this is an odorant with "minimal irritating effects"—and the discrimination did not break down. Aside from speculation as to what possible physiological changes might have taken place in the organism during the training sessions with sec-butyl acetate, it is apparent that while the study did demonstrate that a discrimination based on a trigeminal nerve irritant such as sec-butyl acetate could be maintained when a "minimally irritating" substance such as isooctane is substituted, this does not answer the more basic question as to whether an "ordinary" bird (one not given discrimination training with a trigeminal nerve irritant) could learn to make an olfactory discrimination.

It should be pointed out that Michelsen did not claim that his data answered this question, but no mention of this restriction is made, and an affirmative answer is certainly implied in the summarizing statement quoted above. In his letter to me, Michelsen acknowledged that he had no data to answer the question of whether an olfactory discrimination could be established in a pigeon without the preliminary use of a trigeminal nerve irritant. Work had been planned to answer this question, but due to technical difficulties and time problems, it was not carried out.

Michelsen in our correspondence noted that some operative work on the subjects had been done which had not

been reported in the *Science* article. He cut the olfactory nerves of the pigeons and found that the olfactory discrimination disappeared. In addition, tests on one bird were rerun, with sec-butyl acetate as the stimulus, and in this case the discrimination immediately returned to its postoperative level. A number of different implications can be drawn from this finding, but space precludes a discussion of them here. It seems to me, however, that the results point to the likelihood that under the conditions of Michelsen's tests an olfactory discrimination could *not* be established with an "ordinary" bird that had not previously been exposed to a trigeminal nerve irritant. Of course, this question can only be resolved by a direct experimental attack on the problem.

As Michelsen has stressed, much of the earlier work in this area was very poorly conducted, and it is difficult to draw valid conclusions from these studies. To sum up the present state of affairs, there has not been a single laboratory experiment which conclusively demonstrates the existence of precise olfactory discrimination in "ordinary" birds. This obviously does not mean that birds cannot make such a discrimination, and I personally believe they can, but we must have additional laboratory findings before we reject the null hypothesis. Perhaps Michelsen's study will provide the necessary spur for such investigations.

ALLEN CALVIN
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I would like to thank Calvin for pointing out the serious error that appeared in my earlier paper on olfactory discrimination in the pigeon. However, the remainder of the comment suggests no valid reason for altering the conclusion that pigeons are capable of olfactory discrimination.

In describing the control procedure (p. 631), the article states, "seven pecks by bird No. 264 on key No. 2 produced the food reward. For bird No. 263, both saturators were filled with isooctane. When air passed through the saturator that formerly contained distilled water, seven pecks on key No. 1 produced the food reward." Due to a clerical error, the key numbers in this paragraph were incorrectly placed. The paragraph should read: "seven pecks by bird No. 264 on key *No. 1* produced the food reward. For bird No. 263, both saturators in the delivery system were filled with isooctane. When air was passed through the saturator that formerly contained distilled water, seven pecks on key *No. 2* produced the food reward." With this correction, the order of events in the study becomes clear. The birds demonstrated

behavior that was presumably due to the presence or absence of olfactory stimuli. The control procedure provided evidence that the birds were responding differentially *only* to the presence or absence of an odor.

Calvin's only objection to the study is that the birds learned to make a discrimination based on a trigeminal nerve stimulant prior to learning to discriminate an olfactory stimulus. The use of this procedure, Calvin believes, has made these birds different somehow from "ordinary" birds. Two possible implications stem from this belief. The first is that birds are never exposed to vapors that stimulate the trigeminal nerve in their normal environment. This seems a most unreasonable assumption. The second possibility is that the use of sec-butyl acetate, by some unknown mechanism, created in both of these birds the ability to smell. This hypothesis has far-reaching consequences and should be subjected immediately to experimental verification.

Many studies have ended with evidence that birds are not able to smell. One difficulty with many of these studies is that no evidence was presented that the experimental techniques and odor-control systems were adequate for demonstrating a discrimination based on a vaporous substance, even if the birds under investigation could smell.

In the study under examination I first demonstrated that the apparatus and procedure used were adequate for establishing a discrimination based on a vaporous substance. This vapor, sec-butyl acetate, stimulates the trigeminal nerves and therefore could be used without the assumption that any function of the olfactory nerves was involved. After establishing the procedure as a valid one for studying the problem at hand, I began the second part of the experiment, using isooctane. At this point in the experiment it would have been justifiable to state that the pigeons could not smell isooctane, if negative results had been observed. If the initial training had not been carried out, such a conclusion would have been unwarranted.

I have made some additional studies with these two pigeons since the article in question was written (W. J. Michelsen, in preparation). After the conclusion of the last part of the study reported in *Science*, the olfactory nerves of both birds were severed. Neither bird was able to discriminate isooctane postoperatively. Sec-butyl acetate was reintroduced as the stimulus with one bird postoperatively, and this discrimination was readily learned. Postmortem examination of the brains of these birds revealed that the trigeminal

nerves of both birds were intact and that the olfactory nerves had been severed (S. J. Cobb, personal communication). This study, as was the case with the one reported earlier, presents evidence that can lead us to no conclusion other than that pigeons are capable of smelling isooctane.

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Airglow Cells

In the interest of historical accuracy I feel obliged to make a brief comment in connection with the letter by David M. Gates [*Science* **131**, 266 (29 Jan. 1960)] in which he criticized certain aspects of the article by R. C. Staley on high-altitude observation techniques [*Science* **130**, 845 (2 Oct. 1959)]. Gates writes: "F. E. Roach . . . was responsible for the exciting discovery that the diffuse light from the upper atmosphere appears in large patches in the night sky, called 'airglow cells' . . .," and he attributes this discovery to an article by Roach, Tandberg-Hanssen, and Megill, which was published in 1958.

In reality, the existence of large luminous patches in the night sky was known at least 20 years earlier. In the director's report of the Yerkes and McDonald Observations for 1938–39 [*Publ. Am. Astron. Soc.* **9**, 306 (1939)], I wrote: "Elvey found on several nights large luminous areas in the sky, usually near the horizon and often in the south, southwest and southeast." As far as I know, C. T. Elvey never claimed to have "discovered" this phenomenon, but he observed the luminous patches intensively at McDonald Observatory, both by means of photoelectric photometers and by means of a large nebular spectrograph. He made numerous trips to various neighboring locations in order to secure simultaneous observations at pairs of stations, separated by some tens of miles, and he frequently discussed with me the apparently irregular motions of the patches across the sky. He published some of his results in the *Astrophysical Journal* [**97**, 65 (1943)].

None of this detracts in any way from the importance of Roach's more recent work. He, too, was a highly valued member of the Yerkes-McDonald staff, and he was associated with Elvey in some of the earlier stages of work on the night sky at McDonald Observatory.

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