msec (condition Ca-Tp) is not a major criticism. There is nothing magic about the .05 level of statistical significance. The failure to attain the "conventional minimum level" was caused by the variance in performance of one rat, which inflated the overall variance for the 0.1-msec C-T interval in condition Ca-Tp. Were it not for this animal, the mean level of performance would have been even lower, and the overall variance would have been similar to that for the other C-T intervals. Thus, the low level of performance for the 0.1-msec C-T interval under condition Ca-Tp does not seem to reflect an "aberrant data point."

Theoretically, one would expect to see a relatively smooth symmetrical drop-off from some chosen peak [figure 1 in (*1*)] only if one assumes that the stimulated fibers mediating intracranial self-stimulation (ICSS) are all of the same axonal diameter and therefore have identical conduction velocities. Because higher current was required to meet the ICSS criteria with the anterior preoptic area electrodes than with the posterior medial forebrain bundle electrodes, it is conceivable that a wider range of fiber diameters was activated anteriorly. Thus, the rate of change of postsynaptic effects produced by the anterior and posterior electrical stimuli would differ, and a smooth symmetrical drop-off from the point of maximal heterosynaptic facilitation would not necessarily be predicted.

The rats did not seem to be responding at a ceiling level. The number of lever presses per minute was the dependent variable, and the rat could press the lever during a stimulus train. Thus, the rats were not limited to a maximum of 100 responses per minute and indeed pressed in excess of 100 times per minute on several trials. Although the range in ICSS responding was relatively small, it is difficult to compare this range with that of Ungerleider and Coons (*2*) because they used higher current, longer C-C intervals, longer stimulus train durations, and a different dependent variable.

If one assumes the validity of the Ungerleider and Coons data (*2*) in suggesting that ICSS impulses converge from opposite sides of the brain onto a common neuronal pool, our data suggest that a convergence site exists caudal to the stimulation electrodes. Our data do not refer to all ICSS loci or to the direction (antidromic or orthodromic) of action potential conduction, nor do they rule out the possibility of convergence at some rostral site. Until the critical neural substrates for ICSS are discovered [reviewed in (*3*)] the interpretation of much ICSS experimentation will remain open to question.

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References

1. J. S. Yeomans and H. W. Koopmans, *Science* **183**, 102 (1974).
2. L. G. Ungerleider and E. E. Coons, *ibid.* **169**, 785 (1970).
3. D. C. German and D. M. Bowden, in preparation.

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Acanthaster: Tests of the Time Course of Coral Destruction

Glynn (*1*) has expressed the opinion that it would "be imprudent to continue supporting the extermination of *Acanthaster*, except in those areas where it can be demonstrated to constitute a real threat to the continued existence of reefs," and with this we wholly concur (*2*). However, he does not give reasons why. Paine (*3*) has hypothesized that *Acanthaster* may be important in maintaining the diversity of tropical reefs in much the same manner as other starfish, such as *Pisaster*, in temperate water communities. Branham *et al.* (*4*) determined that *Acanthaster* preferentially feed upon the faster growing of an essentially two-species coral assemblage in Hawaii and suggested that its predation prevented that species from fully monopolizing available space. Furthermore, Porter (*5*) considered his data to show that *Acanthaster* predation on Panama reefs definitely helps maintain coral species diversity there. Thus, the imprudence of continued control measures is not simply a matter of opinion but has a scientifically valid basis. In fact, it remains to be demonstrated that predation by *Acanthaster* at any population size constitutes a threat to the continued existence of any reef, despite the ravages this starfish has wrought on the Great Barrier Reef and on Guam.

On the basis of quantitative field data, Glynn has constructed a "hypothetical time course of coral destruction by *Acanthaster* at varying population densities." The figure presented [figure 2 in (*1*)] provides the first opportunity to actually estimate the carrying capacity of a given reef for an *Acanthaster* population and infers what the fate of the reef might be above and below this level. To verify Glynn's time course of events, one need only return to Uva Island where his work was carried out, to see if living coral cover has increased by the predicted 21.4 percent per annum minus *Acanthaster* destruction. However, since the amount of coral destroyed by *Acanthaster*, at the present population size, is small relative to the total coral coverage, an experimental approach might be more fruitful. One could simply remove the starfish, and, provided other factors remained constant, two things should become evident in succeeding years. First, coral cover obviously should have increased at a more rapid rate. Second, and more important, the predominant coral *Pocillopora*, which according to Glynn constitutes 85 percent (numerically) of *Acanthaster*'s food locally, should have increased disproportionately. The second result would lend support to Paine's (*3*) hypothesis. Alternatively, one could collect starfish in adjacent regions and add them to the population on the Uva reef in numbers sufficient to exceed the predicted carrying capacity. If the *Acanthaster* added were sufficient to overwhelm the reef's carrying capacity, thereby placing the starfish under food limitation, the population should migrate in search of other reef areas, as proposed in the food limitation hypothesis (*6*).

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References

1. P. W. Glynn, *Science* **180**, 504 (1973).
→ W. A. Newman, *ibid.* **167**, 1274 (19 → T. F. Dana, *ibid.* **169**, 894 (1970); J. M. Branham, *BioScience* **23**, 219 (1973).
3. R. T. Paine, *Am. Nat.* **103**, 91 (1969); see also T. F. Goreau, J. C. Lang, E. A. Graham, P. D. Goreau, *Bull. Mar. Sci.* **22**, 113 (1972).
4. J. M. Branham, S. A. Reed, J. H. Bailey, J. Caperon, *Science* **172**, 1155 (1971).
5. J. W. Porter *Am. Nat.* **106**, 487 (1972).
6. T. F. Dana, W. A. Newman, E. W. Fager, *Pac. Sci.* **26**, 355 (1972).

2 October 1973