On the Directionality of Medial Forebrain Bundle Fibers Mediating Self-Stimulation

German and Holloway (*1*) propose an inventive method for determining the direction of conduction of medial forebrain bundle (MFB) fibers. The fibers that mediate self-stimulation have been shown by Ungerleider and Coons (*2*) to project from bilateral stimulation sites to a common synaptic pool. Unfortunately, the data of German and Holloway do not effectively support their further conclusion that the fibers conduct to a predominately posterior convergence site.

First, the authors are quite arbitrary in their grouping of data for statistical analysis. In their discussion of methods they state: "Half of the animals were tested with one set of C-T intervals (*3*) (0.1, 0.5, 0.8, 1.5, 5.0, and 20.0 msec), and half were tested with a different set (0.1, 1.0, 3.0, 6.0, and 20.0 or 25.0 msec)." At some point, however, the authors decided to omit some of these intervals (0.5, 0.8, and 3.0 msec) and combine others. Their only explanation is, ". . . since the self-stimulation performance was quite similar under the two sets of C-T intervals, all six rats were combined for group analysis for the C-T intervals of 0.1 msec, 1.0 or 1.5 msec, 5 or 6 msec, 20 or 25 msec, and for the C-C condition." It is not clear why 1.0 and 1.5 msec were combined rather than 1.0 and 0.8 msec. German and Holloway's conclusion that the fibers conduct in a posterior direction is based solely on comparisons with this particular data point, the grouping of 1.0 and 1.5 msec.

Even so, we doubt the statistical reliability of their results. They perform multiple *t*-tests to test significance. Using repeated tests increases the chance of obtaining a significant result by chance. Significance is reported but in only one of the critical comparisons with adjacent points—that is, the 1.0- or 1.5-msec Ca-Tp interval (anterior C pulse followed by posterior T pulse) compared to the 0.1-msec Ca-Tp interval—and then only with $P < .06$. Thus, even after rearranging the data, use of liberal procedures (uncorrected multiple *t*-tests) fails to achieve significance at the conventional minimum level ($P < .05$).

We believe that what marginal results were obtained were due to aberrant data points. In Fig. 1, we have replotted their summary data [figure 1 in (*1*)] to make this more comprehensible. Theoretically, one would expect to see a relatively smooth symmetrical drop-off from some chosen peak. The curves do appear to slope downward from the point 1.0, 1.5 Ca-Tp. However, the one neighboring point for which significance is marginally claimed is unexplainably low. Although this point, 0.1 Ca-Tp, is nearest to the chosen peak, it is numerically smaller than the four other data points that have C-T intervals within 10 msec of the chosen peak. The large numerical difference between two neighboring points suggests the magnitude of the unreported variability. Replotting the data as a single curve also shows that their supplemental statistics, the average rank order correlations, are based on highly selected post hoc trend models which also depend upon the abnormally low point, 0.1 Ca-Tp.

Even if there is an effect to be found it is unlikely that the authors would have seen it, since their rats were pressing near their maximal rates in all C-T conditions. Bar-pressing rates were limited to an absolute maximum of 100 min^{-1} by the 0.6-second duration of the pulse train per press. In our experience, this would make for a practical maximum of roughly 85 min^{-1} for exceptional rats. Since both electrodes had to yield bar-pressing rates greater than 30 min^{-1} for the animal to be included in the study, it may be assumed that ceiling rates for individual animals were distributed throughout the range of 30 to 85 min^{-1}. German and Holloway reported mean rates of slightly more than 60 min^{-1} in the highest condition and slightly less than 50 min^{-1} in the lower condition. This range is small in comparison to that of Ungerleider and Coons, who found drop-offs from about 40 min^{-1} to about 10 min^{-1} at comparable C-T intervals (*2*). We suspect that possible differences were obscured by a mean ceiling rate near 65 min^{-1}.

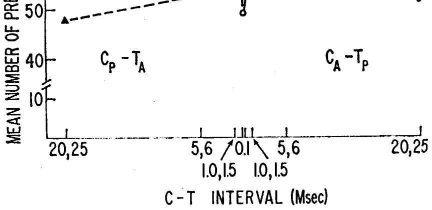

Fig. 1. Summary data replotted from figure 1 of German and Holloway (*1*). The data are presented as one continuous curve on linear coordinates. Each point on the abcissa represents a grouping of different C-T intervals, as presented by German and Holloway.

One technical point: It would be helpful if authors of two-electrode experiments would discuss the possibility of electrical interaction between the electrodes during stimulation. If current flows from the stimulating electrode to the inactive electrode, changes in behavior could be due to double stimulation of one site, which would result in refractory period complications.

In conclusion, convergence of MFB pathways for self-stimulation may be at the posterior end of the tract, but we believe that it remains to be demonstrated.

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References and Notes

1. D. C. German and F. A. Holloway, *Science* **179**, 1345 (1973).
2. L. G. Ungerleider and E. E. Coons, *ibid.* **169**, 785 (1970).
3. The C-T interval is the time between each of the two paired pulses, the first pulse of each pair called the conditioning or C pulse, and the second called the test, or T pulse. The C-C interval is the time between successive C pulses in the train.

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The methodological criticism raised by Yeomans and Koopmans (*1*) is not well founded. For example, the grouping of data points for statistical analysis was not arbitrary, but included points that were numerically close or identical for the two groups of animals tested at different C-T intervals. In the case of grouping C-T intervals of 1.0 and 1.5 msec rather than 1.0 and 0.8 msec, this was done because the anterior-posterior electrode offset was 3.0 mm, and we felt that the longer C-T interval combination would more adequately reflect the neural conduction time between the two electrodes. Since the statistical results were unchanged with either combination, this grouping of data points did not seem misleading, although it would have been helpful to explain our reasons in the original report.

The fact that $P < .06$ for the *t*-test comparing performance between C-T intervals of 1.0 and 1.5 msec with 0.1

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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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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