cumbersome. For the present our approach seems suitable as an alternate to more conventional time series measures (such as cross-spectra and coherences) for studying relations between EEG and brain information processing.

ENOCH CALLAWAY
PETER R. HARRIS
Langley Porter Neuropsychiatric Institute, University of California, San Francisco 94143

References and Notes

1. L. K. Kaczmarek and W. R. Adey (*Brain Res.*, in press) demonstrate that weak electric fields can change calcium and γ-aminobutyric acid efflux from the brain.
2. C. E. Shannon and W. Weaver, *The Mathematical Theory of Communication* (Univ. of Illinois Press, Urbana, 1949); G. A. Miller, *Am. Psychol.* **8**, 3 (1953); W. L. Hays, *Statistics for Psychologists* (Holt, Rinehart & Winston, New York, 1963), p. 610. Information transmission is considered to be equivalent to uncertainty reduction. If P_i is the probability of an event in the ith of N categories, uncertainty (H) is

$$H = -\sum_{i=1}^{N} P_i \log_2 P_i$$

For a given EEG channel, $N = 4$, and P_i is estimated from the relative frequency of EEG samples classified into each of the four categories. If uncertainty in channels x and y are H_x and H_y, and that in the contingency matrix is $H_{x,y}$, the coefficient of information transmission is

$$100\ (H_x + H_y - H_{x,y})/$$
$$(\text{minimum } H_x, H_y)$$

In the present example, H_x and H_y were always quite close to 2, so the reader can easily convert to approximate information transmission in bits.
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On the "Square" Model of Maya Territorial Organization

Marcus has offered some models of Maya hierarchial place arrangement and claims that locational analysis, which she "borrowed from cultural geography," confirms her epigraphic analysis (*1*, p. 911).

Of particular interest to the cultural geographer are the maps supposed to illustrate this confirmation. Her figure 4 represents a regional capital (Palenque). Around the regional capital, she says, "developed the familiar hexagonal lattices of secondary centers predicted by the Central-Place Theory " (*1*, p. 911). To me it looks more like a square.

In her figure 5, "Calakmul, one of the four regional capitals . . . surrounded by six secondary centers with almost equidistant spacing" (*1*, p. 913), it is of interest that the angles shown are more easily explained as multiples of 9° than as derivatives of 60°. In her figure 6, "Hexagonal lattices in the vicinity of Tikal . . ." (*1*, p. 914), I notice that the same is true. (Uaxactun-Palmar = 117°, Palmar–San Clemente = 108°, San Clemente–Dos Aguadas = 54°, Dos Aguadas–Uaxactum = 81°, El Gallo–Nakum = 54°.)

In her choice of apical places, Marcus suggests, but does not specify, epigraphic reasons. Of the six places, two ("?" and "?") cannot have convincing epigraphic evidence of relationship to Tikal, and of the remainder, only one—Nakum—would seem to qualify. However, a marriage is not usually regarded as evidence of subservience, but of equality.

I have offered some hypotheses regarding Maya territorial organization (*2*). I might summarize them in the following sentence: The Maya landscape does not represent the blind working-out of economic factors, but is a single, coherent, and highly planned arrangement of places, relationships, and areas.

It seems that attempts to analyze landscapes into predetermined polygonal patterns are stymied when the people subjected to this scheme have a strong, nonhexagonal geometric plan of their own. This, I believe, is the case with the lowland Maya.

If we cannot "tile" the Maya "plane" with hexagons, we can at least "till the plains" of Yucatec research and look at sites which have names other than "?". Perhaps square research has more to offer than hexagonal after all.

MICHAEL A. ROMANOV
Department of Geography, University of Oregon, Eugene 97403

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Marcus's demonstration of the territorial organization of the Lowland Classic Maya (*1*) shows the striking regularity of distribution of major ceremonial centers in the areas around Tikal and Calakmul, based on the survey data of Bullard (*2*) and Ruppert and Denison (*3*) and continuing the rather late application of locational theory in the field of Maya studies which began in 1972 (*4*). The trends and regularities noted in her article are, however, present over a far larger area of the Southern Maya Lowlands (or Central Area) in the Late Classic than she gives evidence for. There is evidence to support not only her contention, "that there was, between A.D. 600 and 900, an overall organization of the entire Maya lowlands" (*1*, p. 911), but also mine, "that the overall distribution of major centres is in fact a function of the whole network, with local factors determining only the actual siting—in other words, that strategic location dominates tactical siting rather than otherwise" (*5*).

I have discussed (*5*) the overall distribution of Late Classic major ceremonial centers in the Central Area, showing by the use of locational analysis (principally distance from the nearest neighbor) that fairly uniform lattices of different sizes existed all over the rain-forest zone, with the distances between sites and the sizes of their "regions of control" decreasing steadily toward a "core" in northeast Petén, Guatemala, where Bullard's survey was carried out. This model depended on coeval and coequal functioning of major centers in the Late Classic, but the possibility of a hierarchy of such centers was canvassed and regretfully excluded because of the unreliability of most of the available data on site size, "since the nature of the inequality and its permanence are factors at present only hinted at in the few recent large-scale site plans and recent epigraphic work" (*5*).

I have noted Bullard's "restatement in practical and archaeological terms of Christaller's theory of central places" and have suggested that "the recent work of Hodder [*6*] on the 'ceremonial centers' of Roman Britain, the walled towns, shows that the model can and should be applied to the Classic Maya situation, although the level of data availability and reliability is much lower."

This casually flung gauntlet Marcus has now taken up. The notion of Copán,

Palenque, Tikal, and Calakmul as the capitals of the four quarters of the Maya world in the early Late Classic is a persuasive particularization of Barthel's ideas, although one might perhaps equally see Tikal as "capital of the center" and one of the large sites in Belize, such as Nohmul or Caracol, as "capital of the east." Her later series of proposed "capitals," with Seibal and Motul de San José replacing Palenque and Copán (based on the inscription on Seibal Stela 10), is less convincing. The two additions are so much closer to each other and to Tikal (Motul de San José is 50 km from Tikal and about the same from Seibal, the latter being 104 km from Tikal; Palenque is 274 km from Tikal, and Copán 296 km) that some local dynastic or political event seems a more likely explanation for the listing of emblem glyphs than a fundamental realignment of the territorial world-view of the entire Central Area, in spite of the appearance of the first alien elements at Seibal in the iconography of Stelae 10 and 11.

According to Marcus's hypothesis, a hexagonal lattice of hierarchically nested sites should have developed around each of the four "capitals" (with, presumably, the settlement pattern also drastically changing itself in response to the change of status of Copán, Palenque, Seibal, and Motul de San José). However, she has only illustrated this at all effectively for Tikal and Calakmul, and even there only the spacing of the major centers is certain, the lower levels of the hierarchy being filled by surmise rather than firm information. It would seem that until further field survey of minor centers and residential clusters is carried out the situation cannot be resolved any further than Flannery (*4*) and I (*5*) have already done.

The cosmological view does not explain the difference in mean nearest-neighbor distances between the Tikal and Calakmul groups of sites, which can be seen simply as part of a general pattern of ceremonial center distribution involving much of the Maya Central Area. In noting the progressively tighter packing of sites toward the "core" area of northeastern Petén, I sought an explanation by analogy with the modern city, on the grounds that "a ceremonial centre is, after all, a service centre for a population in much the same way as a modern shopping–civic center–church complex is" (*5*); in the city, the packing of service centers toward the middle is a function of increasing population density. I suggest that the apparent paradox of many of the largest, oldest, and monumentally richest of Classic Maya sites being most closely packed and with the smallest surrounding regions of control was the result of a socially circumscribed and numerically increasing population engaged in frenetic ceremonial activity and alliance. This view fitted the available data on site size, number and frequency of monument erection, expansion of settled area, and decline of nutritional status, and it had a persuasive ethnographic parallel in Chagnon's work on the Yanomamö, which has been applied to the Maya situation by Carneiro (*7*). I saw in this development the beginning of Maya "superstates," with Tikal as an obvious candidate, a development that was terminated by the collapse of Classic civilization for probably these same demographic and economic reasons.

In sum, then, Marcus's cosmological theory is an attractive one, and certainly my evidence from the analysis of the locations of 83 major centers suggests that some form of overall explanation for the distribution of sites must be sought; but knowledge of the true, or even likely, hierarchical relationships between even major centers is still appallingly deficient. An increase in the quality of our basic data is needed: until then, our beautiful theoretical houses will be built upon foundations of factual sand.

NORMAN HAMMOND
Centre of Latin American Studies, History Faculty, Cambridge University, Cambridge, England

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20 September 1973

I am happy to reply to Romanov's and Hammond's comments, especially as some of their questions have been raised by other colleagues. The answers to several of these questions are to be found in my article (*1*), but perhaps I did not state them clearly enough.

I do not feel that the "angles" between secondary centers—9°, 60°, 108°, and so on—are the most crucial variable. The critical point is that secondary centers occur at *equal distances* from a primary center; the "geometric figure" they form will depend on whether there are 4, 6, or 8 of them. In fact, it is probably more accurate to think of them as lying somewhere along a circle drawn at a set distance from the primary center; locational geographers have used hexagons only because they can be "packed together" in ways that circles cannot. In the case of Palenque [figure 4 in (*1*)], which worries Romanov, I purposely drew no lines connecting the secondary centers because survey there is so incomplete that no one knows how many other sites are missing. I added Palenque merely to show that its secondary centers seem to be equidistant from the capital (not necessarily from each other).

Romanov suggests that I have not specified the epigraphic reasons for making certain sites "apical." I stated two of the criteria: (i) Stela A at Copán lists the four capitals [figure 3 in (*1*)] and (ii) secondary dependencies use their capital's emblem glyph, but not vice versa (*1*, p. 913). The latter criterion also answers one of Hammond's questions: Data on the hierarchical arrangement of centers are not as "appallingly deficient" as he suggests, since secondary centers mention capitals, tertiary centers mention secondary centers, and so on down the hierarchy.

Stela A at Copán lists four specific capitals—*only* four. Thus Tikal cannot, as Hammond suggests, be the "capital of the center"; on Stela A, Tikal is assigned to the west. Nor can Nohmul or Caracol be "the capital of the east"; that direction is assigned to Copán. These four sites were not selected by me: the Maya themselves carved their emblem glyphs in a clause.

Romanov is bothered by my data on the marriage alliances linking secondary centers to their capitals, claiming that marriage is usually a sign of equality rather than subservience. That may be true in the post-Renaissance West, but the reverse is true in non-Western

chiefdoms and archaic states. A convenient example is in Leach's work on the Kachin of highland Burma, where the giver of the bride becomes *mayu* (higher status) and the recipient *dama* (lower status), and marriage alliances seal a contract of dependence (*2*, p. 151):

> We have seen that a crucial element in the structure of *gumsa* society is that when an individual marries out of his or her own social class it is normally the man who marries up and the woman who marries down. . . . Nevertheless, despite what happens in practice, Kachin formal theory is that bride price is adjusted to the standing of the *bride*. It is a theory which permits a powerful chief to pick and choose among potential suitors for his daughters and to use their marriage as direct instruments of political alliance.

There are two levels on which I discussed Maya settlement patterns. The highest level—a cosmological plane which divides the world into four quarters, each with a capital—affects only the capitals. A lower level—which has to do with the equidistant spacing of secondary centers from their capitals—presumably results from the service functions of primary and secondary centers. Both Romanov and Hammond raise questions that hint at a confusion of those two levels. Surely the quadripartite organization related to the capitals does not imply "the blind working-out of economic factors," as Romanov suggests; economic factors presumably operated at the secondary and tertiary level. Similarly, Hammond should not *expect* cosmology to "explain the difference in mean nearest-neighbor distances between the Tikal and Calakmul groups of sites"; that is not the level on which the cosmological model applies. Rather, I suggested that differences in population density were more likely responsible (*1*, p. 913): "Assuming that major centers were designed to serve populations of roughly the same size, this suggests that the population of the Calakmul area may have been half that of the Tikal area."

If population differences are involved (and I lean more toward them than toward the "frenetic ceremonial activity" suggested by Hammond), then different intersite distances imply different population distributions. This would also explain another phenomenon that bothers Hammond: the short distances between capitals in A.D. 849.

In A.D. 731 [figure 3 in (*1*)] the four lowland Maya capitals [Copán, Tikal, Calakmul (?), and Palenque] were widely spaced. By A.D. 849, Copán and Palenque had ceased to put up monuments and had lost much of their political importance and population. Indeed, the whole lowland Maya area was losing population, and many other sites were in the process of being abandoned. When Stela 10 at Seibal was carved in A.D. 849, a list of four Maya capitals [figure 3, line 4, in (*1*)] no longer included Copán or Palenque. They had been replaced by Seibal and Motul de San José (?), respectively, while Tikal and Calakmul (?) continued in the same positions they had occupied in A.D. 731. The shorter distances between capitals probably reflect the abandonment of large areas of the Petén and the concentration of a shrinking population into a much smaller area rather than a "fundamental realignment of world view."

A number of archeologists share Hammond's view that "the largest, oldest, and monumentally richest of Classic Maya sites" are somehow crowded into a "core" area around Tikal. This certainly is not true. In fact, Copán, Calakmul, Yaxchilán, and a number of sites far from the "core" have more carved monuments (stelae, altars, stairways, lintels, and so forth) than Tikal, Uaxactún, or any of their nearest neighbors. Moreover, the "core" theory that Hammond tries to resuscitate is doubly misleading, for it implies that social and political development might somehow be more advanced in Tikal's quadrant. As far as anyone knows, all four quadrants had the same organization.

To Hammond's and Romanov's questions I would like to add one more that I have been frequently asked: How contemporary were all the sites in the hexagonal lattices I illustrated? I am sorry if I failed to make this clear in the original article: the sites were chosen because all of them that were erecting monuments had Long Count dates during the same 50-year segment of the Late Classic period.

Finally, I would like to underscore Hammond's observation that the application of locational theory in Maya studies is "rather late." Although Christaller's work (*3*) had been available since 1933 and Lösch's (*4*) since 1938, neither was really brought to the attention of archeologists until the 1960's, when geographer P. Haggett, in 1965 (*5*), and archeologist D. L. Clarke, in 1968 (*6*), made the techniques widely known. Archeologists at the universities of Chicago (*7*), Arizona, and Michigan turned to the new methods. At Michigan, H. T. Wright and G. A. Johnson applied them to the Near East, while K. V. Flannery and R. E. Blanton applied them to Mesoamerica. Johnson's work culminated in a paper delivered in England in 1970 and published in 1972 (*8*). In 1971, Flannery suggested that I search for epigraphic data on the hierarchical ordering of sites in the hexagonal lattices, data he published in 1972 (*9*). I decided that the best approach was through the use of emblem glyphs (*1*). I am delighted to read that Romanov's dissertation and Hammond's paper in press both deal with the location of Maya centers. Surely this burgeoning interest in the rules that underlie settlement patterns in archaic states will provide some of the archeological breakthroughs of the 1970's.

JOYCE MARCUS
University of Michigan Museum of Anthropology, Ann Arbor, Michigan 48104

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