of these three possibilities—the sign-releasing mechanism—with no conclusive result. As both Masters and Anisfeld correctly noted, our hypothesis that neonates can detect intermodal matches implies a higher level of perceptual-cognitive organization than current theories suggest. Recently, Meltzoff and Borton (*12*) obtained evidence corroborating this hypothesis in an experiment using a nonimitative, intermodal matching task. Four-week-old infants looked longer at a shape matching one they had orally explored than at a nonmatching shape, thus confirming that neonates can indeed detect certain intermodal (tactual-visual) matches. Such converging experiments, using both imitative and nonimitative tasks, will afford strong tests of our position. We emphasize, however, that future research on neonatal imitation must fulfill the three methodological requirements we detailed (*13*) if it is to address the phenomenon we reported and elucidate the underlying mechanisms.

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5. A. Peiper, *Cerebral Function in Infancy and Childhood* (Consultants Bureau, New York, 1963); T. E. Twitchell, *Neuropsychologia* **3**, 247 (1965).
6. N. Tinbergen, *A Study of Instinct* (Oxford Univ. Press, New York, 1951).
7. Even isolating specific effective feature would not conclusively demonstrate the existence of a sign stimulus that was releasing a response. A "released response" also implies certain characteristics about the form and organization of the response pattern [(*6*); K. Lorenz and N. Tinbergen, *Z. Tierpsychol.* **2**, 1 (1938)].
8. Jacobson and Kagan discussed the data as if the infant's rate of tonguing to the tongue and pen are similar to each other and different from that to the ball (thus, that shape is important). However, a comparison of the mean rate of tonguing to these stimuli contradicts this grouping of the data. If any trends are to be discerned (the differences are not significant) they are that the infant's tonguing response to the two inanimate stimuli (pen and ball) should be grouped together as both being inferior to the response to the adult tongue model. The data show [table 1 in (*3*)] that the largest difference among the means is between the tongue and the ball (1.12), the next largest is between the tongue and the pen (0.68), and the *smallest* difference is between the pen and the ball (0.44).
9. J. Piaget, *Origins of Intelligence* (Norton, New York, 1952); B. L. White, P. Castle, R. Held, *Child Dev.* **35**, 349 (1964); J. S. Bruner and B. Koslowski, *Perception* **1**, 3 (1972); T. G. R. Bower, *Development in Infancy* (Freeman, San Francisco, 1974).
10. The experimenter presented her face at a fixed distance from the infant's eyes and far enough away that she was never visible on the videotape recording of the infant. In contrast, the pen and ball were moved directly toward the infant's mouth, coming close enough that they were sometimes visible on the videotape.
11. S. Jacobson, thesis, Harvard University (1977).
12. A. N. Meltzoff and R. W. Borton, paper presented at the biennial meeting of the Society for Research in Child Development, San Francisco, 15 to 18 March 1979.
13. We argued that studies must (i) distinguish true imitation from a global arousal response, (ii) ensure that the imitative reactions were not due to shaping by the experimenter or the parents, and (iii) use blind scoring techniques.
14. B. J. Winer, *Statistical Principles in Experimental Design* (McGraw-Hill, New York, ed. 2, 1971), p. 528.

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Political Subdivision and Population Density

Stephan (*1*) observes that in most countries there is a negative relation between the area of a territorial subdivison and the density of its population, which is well approximated by the formula $\log A = a - 2/3 (\log D)$. He derives this relation from the general premise that nations subdivide themselves into territorial units so as to minimize the total time expended by their populations in visiting and operating the administrative subcenters. A regression of $\log A$ on $\log D$ does not provide a proper test of his theory, however. $\log A$ and $\log D$ would have a negative relation even if administrative boundaries were drawn completely independently of and without regard to the distribution of population. For example, if $\log A$ and $\log P$ are independent random variables, then a regression of $\log A$ on $\log D$ will have an expected slope of $-\mathrm{Var}(\log A)/[\mathrm{Var}(\log A) + \mathrm{Var}(\log P)]$, since $D = P/A$. The observed clustering of slopes around $-2/3$, therefore, could simply mean that the variance of $\log A$ is generally twice that of $\log P$ and nothing more.

The proper test is to regress $\log A$ on $\log P$,

$$\log A = a' + b' \log P \qquad (1)$$

Stephan's theory predicts that this relation will also be negative, since if the slope relating $\log A$ to $\log D$, b, is between 0 and -1, as in theory it is, then b' will also be negative. In particular, if $b = -2/3$, then $b' = -2$. This test, unlike Stephan's, is not open to the objection that it merely confirms an artifactual relation between two variables (in his case, $\log A$ and $\log P - \log A$).

To confirm this negative relation between subdivision area and population, we collected data from the source (*2*) used in Stephan's original empirical work (*3*). Sixty-five nations, those having at least ten primary political subdivisions, were chosen for study. Regressions were fitted to each country's data to determine the coefficient b' in Eq. 1. The hypothesis that b' is negative was tested by means of the standard two-tailed t-test with $N - 2$ degrees of freedom (*4*). In accordance with Stephan's findings, 62 out of 65 nations showed negative relations between $\log A$ and $\log D$. But a negative relation between $\log A$ and $\log P$ was found in only 20 of the 61 nations where it would be predicted (that is, where b is between 0 and -1), and in just 12 of these 20 is this negative relation significant at the 10 percent level. In fact, of the 41 nations with positive relations between $\log A$ and $\log P$, in 23 that relation is significant at the 10 percent level.

These results indicate that the partitioning of a nation's space is probably more random than purposive. Given a random partitioning of a space over which a population is randomly distributed (*5*), we should expect a positive relation between area and population, since the larger partitions will, on the average, contain the larger populations. Our data (*4*) give some support to this expectation.

The fallacy of Stephan's statistical work cannot, however, detract from the strong visual impression given by density maps that closely settled areas tend to be subdivided more than sparsely settled areas (*3*, *6*). To confirm this relation statistically will require the measurement of density independently of area. The mean distance between inhabitants in an area is one such variable but unfortunately is difficult to measure.

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7. Partial support for this study was provided by NSF grant SOC 76-04821.

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Vining objects to my analysis because it "merely confirms an artifactual relation between two variables." In discussing correlations between two variables of the form y and x/y, Snedecor (*1*) states:

> Having observed some unwarranted interpretations of such correlations, Karl Pearson dubbed them "spurious," and this rather de-

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rogatory title has led people to distrust them. Of course, it is the interpretation that may be spurious. The correlations are on the same footing as any others.

A little further analysis of the data employed by both Vining and me reveals, for the aggregate, that the variance is .62 for log$A$ and log$D$ and .41 for log$P$, with a covariance of .21 between log$A$ and log$P$. Consider Vining's interpretations above in the light of these results.

As Vining suggests, the observed clustering of slopes around $-2/3$ could simply mean that the variance of log$A$ is generally twice that of log$P$ and nothing more; empirically, however, the ratio is 3 to 2, not 2 to 1. The expression for the expected slope between log$A$ and log$D$ would be that given by Vining if the covariance between log$A$ and log$P$ were zero; empirically, however, it is not zero. The slope relating log$A$ and log$P$ would be -2 if the $-2/3$ slope between log$A$ and log$D$ represented a case of perfect correlation; empirically, however, the correlation between log$A$ and log$D$ is far from perfect ($r^2 = .42$), and it is thus not surprising that the slope between log$A$ and log$P$ is in fact .5, not -2 as "expected" according to Vining's argument.

A mathematical relation is not the logical equivalent of a statistical relation, since the latter anticipates the possibility of error in either or both of the related variables. For this reason the regression coefficient b_{xy} does not even imply its own inverse coefficient b_{yx} except under perfect correlation. Simple regression analysis assumes some error in the dependent variable, none in the independent variable. If we assume error in both log$A$ and log$P$, my test was appropriate since the error term would be minimized or nonexistent in my independent variable (log$D$ = log$P$ − log$A$). Vining's analysis is not appropriate by this argument.

Vining says that adequate tests of the theory will require measurement of density independent of area. In previously published work (*2*) I report such tests relating unit areas to regional densities measured independent of unit areas. In work currently under review (*3*) relations between unit areas and regional densities are reported with results virtually identical to those published earlier (*4*) using unit densities (conclusion: unit densities provide reasonably good estimates of regional densities). And in another work under review (*5*) area is related to population potential, a variable measured independently of unit or regional density, with results mathematically and statistically consistent with the earlier findings.

Vining says that the process of territorial division is more random than purposive. I have never argued that it was purposive, only that it results in time minimization. This is not surprising; most random processes in nature have least-energy outcomes. I have not yet developed an adequate model of the random process. I was working on one which contained assumptions very much like some of those mentioned by Vining (I described it to him in a letter in September of 1977), but I later abandoned it because of the problems noted in my second paragraph above. For what it is worth, I believe a proper theory could be developed from the theory of breakage (*6*) with increasing settlement intensity and improved transportation acting as positive and negative forces.

For the record, I want to note two earlier derivations (*7*) which can easily be shown to be similar to my own derivation. They were brought to my attention by Jay Callan of Rutgers University.

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Crystal Structure of [Leu⁵]Enkephalin

[Leu⁵]Enkephalin has been reported to grow as thin, small crystals from aqueous methanol solution in space group *C*2, cell constants $a = 31.871$ Å, $b = 8.535$ Å, $c = 12.467$ Å, $\beta = 96.53°$, $Z = 4$ (*1*). We (T.L.B., L.H., I.J.T., R.A.P., and B.A.M.) have now grown large crystals whose diffraction patterns show clearly extra rows of weak spots which cannot be indexed on this cell. The new photographs indicate a cell with constants (at 4°C) $a = 31.937$ Å, $b = 17.084$ Å, $c = 24.861$ Å, $\beta = 95.54°$, and $Z = 16$. These new indices, $h'k'\ell'$ are related to the indices of the smaller cell $hk\ell$ by $h' = h$; $k' = 2k$; $\ell' = 2\ell$; and show systematic absences $k' + \ell' = 2n$, indicating space group *A*2. Strong reflections have indices $h' + k'/2 = 2n$, and these correspond to the reflections indexed on the smaller *C*2 cell. Other reflections are considerably weaker but in retrospect can be observed after long exposure in the diffraction pattern of the small crystals used in the earlier analysis.

The backbone conformation and intramolecular hydrogen bonding scheme of the previous structural analysis, interpreted in the light of these new findings, can therefore be described as an average of four nearly identical molecules. These new results also make it possible to explain the disorder of the tyrosine side chain as two crystallographically independent conformations. The details of the intermolecular hydrogen bonding and of the side-chain orientations await a full refinement using the complete set of x-ray data.

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