these analyses indicate a highly significant decrease in the number of GABAergic axon terminals at sites of seizure foci.

The decreased number of GAD-positive terminals in our immunocytochemical preparations of epileptic monkey cortex could be explained by an alumina gel-produced loss of antigenicity of GAD molecules. However, this is unlikely since most of the alumina is located in macrophages with slight amounts in astrocytes (*16*). Furthermore, not all of the GAD-positive terminals are lost at seizure foci, and the staining of the remaining terminals indicates that the antigenicity of GAD is not affected by the alumina gel. In addition, a differential loss of antigenicity caused by differences in the diffusion of alumina from the application site is unlikely since, in monkeys with subarachnoid injections, the deep cortical layers display similar decreases of GAD-positive terminals to those observed in the superficial layers directly subjacent to the alumina gel. Therefore, the loss of immunocytochemically detectable GAD from cortical axon terminals indicates an actual loss of GAD molecules and this could be due either to a severe impairment of GAD synthesis, or to the degeneration of GABAergic somata or their axon terminals, or both.

Although previous biochemical data have indicated decreased GAD activities at seizure foci (*5*), our results extend this finding and show a numerical decrease of GAD-containing axon terminals. Whether these terminals actually degenerate or merely lose immunocytochemically detectable GAD is not known. However, a degeneration of GAD-containing terminals is suggested by the results of ultrastructural studies that show a decreased number of presumed inhibitory, symmetric synaptic junctions with somata and dendritic shafts of cortical neurons at seizure foci (*17*). In either event, a functional loss of GABAergic cortical neurons would occur. Our experimental preparations indicate that the magnitude of this loss is significant and could be expected to reduce the inhibitory synaptic control of pyramidal neurons, thus leading to a hypersensitivity of these cells to normal excitatory synaptic inputs (*4, 5*). The reason for this loss of GABAergic terminals at seizure foci is unknown, but it is possible that aspinous stellate neurons may be highly susceptible to alterations induced by alumina treatments. Further support for a GABAergic involvement in epilepsy is derived from pharmacological studies that show that certain convulsant and anticonvulsant drugs act at GABAergic synapses in the central nervous system (*18*). Thus, our results in combination with those of the other studies cited in this report support a hypothesis that a loss of functional GABAergic neurons leads to focal epilepsy.

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Interpreting "Imitative" Responses in Early Infancy

Meltzoff and Moore (*1*) have reported that 12- to 21-day-old infants can imitate specific gestures, including sequential finger movement, tongue protrusion, lip protrusion, and mouth opening. Their report invites close scrutiny because it attributes to the neonate cognitive capacities far beyond those which appear reasonable on the basis of current knowledge. There are serious defects in this research beyond the possibility of experimenter bias acknowledged by the authors with respect to experiment 1 and controlled for in experiment 2. Questionable scoring procedures were used. In experiment 1, infants were credited with imitation even when the scorers were not fully confident about it. In experiment 2, the scorer could have drawn on differences in the length of time the gestures were demonstrated in scoring for imitation.

The most compelling argument against the conclusions of Meltzoff and Moore comes from an examination of their statistical analysis. The results of experiment 1 were analyzed by comparing the number of scores in each of the four categories across the four modeling conditions. But the distribution of the different infant gestures within each modeling condition can and should also be analyzed. This analysis is essential because the infants were scored not only in terms of the four modeling gestures but also in terms of three additional gestures. The distribution of the seven scoring categories for the four modeling gestures is

Table 1. Distribution of responses across the four gestures shown to the infants. The figures in the first four rows are from figure 2 of (*1*). The remaining figures were obtained from A. N. Meltzoff. The italicized entries indicate matching responses. Abbreviations: LP, lip protrusion; MO, mouth opening; TP, tongue protrusion; and SFM, sequential finger movement.

Scored responses	Gestures shown to infants			
	LP	MO	TP	SFM
Lip protrusion	*27*	17	15	19
Mouth opening	11	*24*	17	19
Tongue protrusion	21	20	*30*	26
Sequential finger movements	14	13	16	*27*
Hand opening	22	24	28	24
Finger protrusion	18	19	10	8
Passive hand	18	16	18	13

shown in Table 1. It can be seen in the table that responses by the infants matching the model were more likely than some responses but not more likely than other responses. For instance, when sequential finger movement was modeled, the incidence of sequential finger movement responses (27) was clearly higher than the incidence of finger protrusion responses (8), but it was virtually the same as the incidence of tongue protrusions (26) and hand opening (24) responses. Whether one concludes that the infants did or did not imitate would then depend on an arbitrary selection of a comparison base. It can thus be concluded that when the data are properly analyzed, little evidence remains that neonates can imitate specific movements.

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2. I am grateful to Dr. A. N. Meltzoff for providing part of the information in Table 1. The helpful comments of E. Anisfeld and A. C. Goldstein are gratefully acknowledged.

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In their report on the imitation of facial and manual gestures by human neonates, Meltzoff and Moore (*1*) hypothesized an active cognitive process involving cognitive representation and motor matching. Methodological problems, including the selection of target neonatal behaviors, the scoring and definition of "imitative" behavior plus a current absence of convergent evidence for the cognitive sophistication of newborns, allow the questioning of this conclusion.

In experiment 1, four discrete but not unrelated behaviors were modeled: lip protrusion, mouth opening, tongue protusion, and sequential finger movement. These behaviors all participate in early feeding and clinging reflex systems. Six judges (for whom no rater reliability was presented) scored videotapes of neonatal reactions to each modeled behavior, ranking the four possible behaviors according to the likelihood that they had occurred. For analysis, the top two ranked behaviors were each scored "yes" and the bottom two "no." This is problematic for a number of reasons. First, a "yes" judgment for imitation could occur when the actual imitative act was not the one a judge thought had occurred but rather the one thought to be second most likely. No information was given regarding the degree of "error" in judgments of imitation (judgments that the behavior matching the model was second most likely to have occurred). Moreover, collapsing these ranks set the chance or guessing probability that a given behavior would be scored as imitative equal to .50. Even through statistical analyses revealed significant differences between the frequency of "yes" judgments for the behavior matching the modeled one and that for the other behaviors, there is no indication that the frequency of "yes" judgments for the matching behavior significantly exceeded the guessing probability. A final problem of this scoring system is that it blurred the distinction between the behavior categories: If different oral behaviors (for example, lip pursing and tongue protrusion) were often confused so that raters ranked them 1 or 2 with equal frequency (both of which would produce a "yes' categorization), arguments that a broad spectrum of distinct behaviors had been imitated would not be tenable. Such reasoning is important to support any conclusion that neonatal matching behavior was guided by a general cognitive process.

Further problems relate to the single nonoral behavior included in experiment 1, sequential finger movements. This specific label implies that manual actions commonly labeled differently, such as grasping, were not appropriately descriptive of the act. It is important for this to be the case, since one ground for rejection of a releasing-mechanism interpretation would be that the behavior in question is not one that has already been shown to occur in reflex fashion or to participate in fixed-action patterns. Lacking is a discriminant analysis of the degree to which typical infant grasping behavior was identified by the raters as an example of "imitation" or whether the unique serial ordering of finger movements was present. This sort of discriminant validity seems unlikely to be achieved when rater variability was such that the two top and bottom probability rankings for possible responses had to be combined.

Experiment 2 is less methodologically flawed than experiment 1, but it adds little to the conclusion that complex representation processes are involved in early matching behavior. Only two behaviors were studied (tongue protrusion and mouth opening), both were oral, and thus were drawn from a repertoire of socially elicited neonatal feeding responses.

Although the several theoretical explanations proposed by Meltzoff and Moore for the apparent matching behavior of infants are still viable, their conclusion that complex cognitive processes guided imitative responding was premature in the face of methodological limitations and a general absence, at the moment, of evidence indicating that neonates are capable of the requisite representational processes. At least as sensible is a conclusion that the neonatal behaviors studied were relatively fixed-action patterns that are isomorphic in form to the visual stimuli eliciting or releasing them.

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Meltzoff and Moore (*1*) have recently reported that infants between 12 and 21 days of age imitate tongue protrusion, lip protrusion, mouth opening, and sequential finger movements in response to an adult's performance of similar acts. Our research was stimulated by the prior work of Maratos (*2, 3*), who found that 1-month-old infants produced matching behavior in response to seeing an adult exhibit tongue protrusion. However, both Maratos and Meltzoff and Moore failed to include controls that would test an alternative interpretation—namely, that these responses could also be released by inanimate stimuli and did not represent selective imitation of the model. Our study explored this alternative hypothesis.

Twenty-four white full-term infants (14 boys and 10 girls) were seen at 6 weeks of age. A series of five stimulus events were presented in a counterbalanced order. The first, tongue protru-

Table 1. Mean (± standard error of the mean) number of tongue protrusions per minute in response to the five stimuli.

Stimuli	Boys (N = 14)	Girls (N = 10)	Means
Tongue	3.22 ± 0.54	6.24 ± 0.94	4.48 ± 0.58
Ball	2.55 ± 0.28	4.50 ± 0.42	3.36 ± 0.48
Pen	2.77 ± 0.80	5.23 ± 1.25	3.80 ± 0.72
Hand	1.80 ± 0.45	3.35 ± 0.90	2.45 ± 0.47
Ring	1.09 ± 0.44	2.54 ± 0.71	1.70 ± 0.41
Means	2.28	4.37	3.16

sion, was an adult sticking out her tongue in front of the infant, holding that position for 3 seconds, and then withdrawing the tongue. The hand movement consisted of the experimenter's opening and closing her raised right hand above the infant's hand. The inanimate stimulus events were a white ball with indentations resembling a golf ball (diameter, 4.5 cm) and a closed black felt-tip pen (length, 14.7 cm), which were moved toward the infant's mouth, held in position for 3 seconds, and withdrawn. These two events served as control stimuli for tongue protrusion. The fifth stimulus, an orange plastic ring (diameter, 8.8 cm) attached to a string, was dangled near one of the infant's hands and raised and lowered. This event served as the control stimulus for the hand movement. Each stimulus event was performed four times for a total of approximately 20 seconds and followed by a 10-second interval. Each sequence of four presentations was repeated three times with a longer interval of 15 seconds between the different stimuli. The stimuli were arranged to alternate between the mouth and hand regions so that carry-over effects would be minimized.

The infants were tested when they were in a state of alert inactivity (*4*). Infants were fed before the testing session in order to avoid excessive tongue protrusion that could be attributed to hunger. The infant was then seated in an infant seat with a small pillow at the top for extra support for the head. The session was videotaped with the camera directed at the infant. The experimenter stood 30 cm away and at a 45° angle to the right of the infant. The camera was situated at a 45° angle to the left of the infant at a distance of 1.6 m; it attracted very little attention from any of the infants. A very small light over the infant seat was operated by a foot pedal. The light was too dim to catch the infant's attention and served to signal change of stimulus on the videotape without indicating which stimulus was being shown. Although the experimenter was not visible on the videotape, the ball, pen, and ring were observed in a few instances (*5*).

Testing began with a 2-minute adaptation period in which the experimenter stood in front of the infant and spoke only to the extent necessary to prevent the infant from becoming alarmed. The experimenter did not smile. Infants were then shown the preset series of test items.

The responses were coded from the videotapes on an event recorder (Esterline Angus). Tongue protrusion was coded whenever the infant's tongue was visible on the screen. The principal variable reported here is any tongue protrusion lasting 0.5 second or longer on the record (*6*). Hand opening and closing was coded whenever the infant opened and closed at least three fingers of either hand. There were no discernible differences in the form of tongue protrusions or hand movements coded as responses to the five stimulus events. The mean correlations for interobserver reliability were .87 for frequency of tongue protrusions and .89 for frequency of hand opening and closing. A two-way repeated-measures analysis of variance was performed on each measure with sex as a between-subjects factor and stimulus as a within-subject factor. All analyses were performed on the rate of response per minute for each of the two measures.

The results support the hypothesis that the infants' tongue protrusion is a released response and not a selective imitation of the adult's act. The five stimulus events elicited differential rates of tongue protrusion [F (4, 88) = 7.63, $P < .001$] (Table 1). Although girls exhibited more tongue protrusions than boys to all stimuli [F (1, 22) = 9.02, $P < .01$], the pattern of the responses was the same for both sexes [stimulus-by-sex interaction, F (4, 88) < 1]. The effects of each of the five stimuli were compared according to the Newman-Keuls procedure (*7*). The pen was as effective a releaser of tongue protrusions as the tongue model, and there was no significant difference between tongue and ball. Tongue protrusion was not a result of general arousal, however, for the tongue model was significantly more effective at eliciting tongue protrusions than the hand ($P < .01$) and ring ($P < .01$). The pen also elicited significantly more tongue protrusions than the hand ($P = .05$) and ring ($P < .01$). However, the ball was not significantly more effective than the hand ($P > .10$).

Table 2. Mean (± standard error) number of hand movement responses per minute in response to the five stimuli at 6 and 14 weeks.

Stimuli	Age	
	6 weeks	14 weeks
Tongue	10.44 ± 1.59	15.39 ± 1.90
Ball	10.19 ± 1.93	13.37 ± 1.59
Pen	9.17 ± 1.75	14.24 ± 1.64
Hand	12.40 ± 2.19	18.95 ± 1.73
Ring	9.61 ± 1.65	16.69 ± 2.06
Means	10.36	15.73

In a second analysis we examined which stimulus elicited the greatest rate of tongue protrusions for each subject. Given that the tongue was one of three stimuli directed to the mouth, the null hypothesis would predict that 33 percent of the infants would respond maximally to the tongue. While the tongue was the best elicitor for 12 of 24 subjects, the pen and ball elicited the greatest number of tongue protrusions for 11 subjects [test of the goodness of fit, χ^2 (1) = 3.00, not significant (N.S.)]. Thus, the tongue was not a significantly more effective incentive than the pen and ball.

Hand opening and closing showed no selective response to any of the stimuli [F (4, 88) = 1.08, N.S.] and no sex difference [F (1, 22) = 1.90, N.S.], which suggests that hand movement may reflect general arousal at 6 weeks. Several infants rarely moved their hands at this early age. In a later session with the same infants at 14 weeks (*8*), the rate of hand opening and closing was greater than at 6 weeks [t (23) = 2.92, $P < .01$], and the infants responded differentially to the five stimuli [F (4, 88) = 3.36, $P < .05$]. However, hand and ring were equally effective in eliciting hand opening and closing (Table 2). This response was not the result of general arousal at 14 weeks since the pooled means for hand and ring were significantly greater than the pooled means for the tongue, ball, and pen [F (1, 22) = 8.24, $P < .01$]. There was no sex difference [F (1, 22) < 1].

The results of this study suggest the need to qualify the interpretation of prior research. Although matching behavior occurs at 6 weeks in response to seeing an adult display tongue protrusion, other inanimate stimuli are equally capable of eliciting the same response. A moving pen was as effective an incentive as a tongue model in releasing tongue protrusions. A ball was somewhat less effective; the shape of the stimulus may be important. Similarly, although matching behavior occurred to hand movement at 14 weeks, an inanimate stimulus elicited this response as effectively. Since these two behaviors can be released by events other than the ones modeled by an adult, there was no evidence to support the hypothesis that 1- to 2-month-old infants can selectively imitate a model.

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6. Rate of tongue protrusions lasting less than 0.5 second to the tongue, ball, pen, and hand were virtually identical, suggesting that this measure taps the level of arousal and is neither a released nor an imitative response. Both Meltzoff and Moore's second study (*1*) and Smillie and Coppotelli's study (*3*) were based on a measure similar to our principal variable; for example, Meltzoff and Moore scored tongue protrusions only when the tongue was thrust clearly beyond the lips.
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We will show that the criticisms of Anisfeld (*1*) and Masters (*2*) cannot be sustained and the results reported by Jacobson and Kagan (*3*) provide no support for the hypothesis that neonatal imitative reactions are mediated by releasing mechanisms.

Anisfeld had expressed concern that, in experiment 2 (*4*), "the scorer could have drawn on differences in the length of time the gestures were demonstrated in scoring for imitation." In fact, however, the scorer observed videotaped segments of the baseline and response periods, and these electronically timed periods were all precisely the same length (150 seconds). There were thus no time differences for the scorer to draw on.

Table 1. Outcome of Newman-Keuls procedure as applied to the overall means in table 1 of (*3*). The tongue, ball, and pen stimuli were presented near the infant's mouth. The hand and ring were presented near the infant's hand. Abbreviations: S, significant at or beyond the .05 level; N.S., nonsignificant, *P* > .05.

Stimuli	Tongue	Ball	Pen	Hand	Ring
Tongue		N.S.	N.S.	S	S
Ball			N.S.*	N.S.	S*
Pen				S	S
Hand					N.S.*
Ring					

*The significance of these three comparisons were not reported in (*3*). However, they can be determined (*14*) by comparing the magnitude of these mean differences with those for which Jacobson and Kagan provide significance levels.

Both Anisfeld and Masters suggested an alternative approach to analyzing the data from experiment 1. Although phrased differently, both arguments are mistaken for essentially the same reason. Anisfeld suggested that we should assess imitation of tongue protrusion, for example, by testing whether the number of tongue protrusion judgments exceeds those for the other categories of infant behavior. Similarly, Masters suggested that we test the number of tongue protrusion judgments against a .50 chance probability, since four infant behaviors were judged simultaneously and the top two judgments were collapsed to a "yes" and the bottom two to a "no." We cannot agree with either suggestion because each ignores both baseline and arousal differences among the various infant behaviors. For example, it is likely that the baseline level of tonguing exceeds other oral behaviors and that tonguing differentially increases relative to other behaviors when the infant is aroused by watching a human face. Thus, one cannot assume, as Anisfeld and Masters do, that the different categories of infant behavior are equiprobable during baseline and arousal conditions. Without this assumption, it becomes arbitrary to compare the frequencies of different infant behaviors directly to one another or to a .50 probability of occurrence.

The problem posed by different baseline and arousal frequencies is solved by analyzing the distribution of each measure separately across the different gestures demonstrated to the infant. For example, one should test the distribution of tongue protrusion scores across the mouth opening, lip protrusion, and sequential finger movement demonstrations. With this method, whatever the initial likelihood of a particular infant behavior, there is evidence for imitation if the frequency of this behavior varies as a function of the gestures demonstrated to the infant and it is greatest when this behavior is the one demonstrated. Such analyses were performed for our original report, and the results support the conclusion that the infants were imitating.

Masters questioned our finding of manual imitation in experiment 1 and suggested that judges sometimes may have mistaken a reflexive grasping response for sequential finger movements, thus leading to a false conclusion that sequential finger movement was imitated. This suggestion relies on two assumptions. The first is that the sight of a moving adult human hand elicits reflexive grasping in the human neonate. To our knowledge, this idea has not been previously advanced by any observer of infant behavior; tactile and proprioceptive stimulation are considered the elicitors of reflexive grasping in the neonate (*5*). The second assumption is that the judges could not discriminate a grasping response from sequential finger movement. But if the sequential finger movement demonstration had elicited grasping, then infant hand opening and closing (a major component of grasping) should also have been judged to peak for this demonstration. The frequency of hand opening and closing, however, was stable across the various gestures shown to the infants and did not peak for the sequential finger movement demonstration [table 1 in (*1*)].

Masters was concerned that, in experiment 2, the tongue protrusion and mouth opening gestures were components of "feeding responses" and therefore might be elicited by the sight of a social stimulus. Neonatal sucking, however, does not entail the full tongue extensions or wide-open mouths specified in our operational definitions of these scoring categories. In addition, the same experimenter sat in front of the infant for both the tongue protrusion and mouth opening demonstrations. Consequently, if global feeding responses were elicited by a social stimulus, there would be the same "feeding responses" in both instances. In fact, there were significantly more tongue protrusion responses to the tongue gesture than to the mouth opening gesture; conversely, there were more mouth opening responses to the mouth opening gesture than to the tongue protrusion gesture. Thus, the imitation effects we reported cannot be reduced to a (hypothesized) release of global feeding responses by the presence of a human face.

Jacobson and Kagan presented three stimuli in front of the infant's face (tongue protrusion, pen movement, and ball movement) and two near the infant's hand (hand opening and ring dangling). There were no differences in the rate of tongue protrusion among the three stimuli presented to the face. There were also no differences in the rate of hand opening between the two stimuli presented near the hand. Thus the essential findings of the study are ones of "no difference." Jacobson and Kagan interpret these data as showing that (i) a releasing mechanism mediates infant tongue protrusion, (ii) the three stimuli presented near the face are all sign stimuli that activate this mechanism, (iii) another releasing mechanism mediates infant hand opening (at 14 weeks old), and (iv) the stimuli presented near the hand are both sign stimuli that activate this mechanism. These conclusions are unwarranted.

The concept of sign stimuli can be meaningfully invoked only if one can identify a specific feature or set of features that define the class of objects that are to be labeled sign stimuli (*6*). It would be a misuse of the concept to argue that every object presented near the infant's face is a sign stimulus for infant tonguing and that every object presented near the hand is a sign stimulus for infant hand movements. In Jacobson and Kagan's study, the infants responded with equal rates of tonguing to the three stimuli presented to the face and with equal rates of hand opening to the two stimuli presented to the hands. There is no discrimination according to the features of the stimulus, and therefore no support for a releaser hypothesis (*7*).

Consider specifically the data for the rate of tonguing. Jacobson and Kagan suggest that the critical feature defining the releaser for tongue protrusion is the shape of the stimulus. They want to argue that narrow shapes moving toward the mouth (like a tongue or pen) elicit infant tongue protrusions, but that differently shaped objects like the ball do not. However, their data (table 1) show there is no significant difference in the rate of tonguing to the tongue, pen, and ball—the three differently shaped objects moved toward the infant's mouth. Thus although their last paragraph states that the ball is less effective than the tongue or pen, the data reveal that all three stimuli are equipotent in eliciting tonguing. These results do not show that shape is important and fail to identify anything that might be called a sign stimulus (*8*).

Jacobson and Kagan's interpretation of the rate of infant hand opening is similarly flawed by an overinterpretation of findings of no difference. The two stimuli presented near the infant's hand do not elicit differential rates of hand opening. Jacobson and Kagan interpret this as supporting the proposition that hand opening is a released response. What they need to show, but have not, is that certain objects presented near the infant's hand elicit a significantly higher rate of hand opening than others. There is thus no evidence that a sign stimulus has been isolated.

Jacobson and Kagan's only statistically significant effects come from comparing the stimuli presented near the face with those presented near the hand (table 1). Their effects are (i) a higher rate of tonguing to the stimuli near the face than to those near the hand, and (ii) a higher rate of hand opening to the stimuli near the hand than to those near the face for the 14-week-olds only. Releasing mechanisms do not provide the only plausible explanation for these results. For example, infants might have learned to produce more anticipatory sucking and tonguing movements to objects presented near their mouths than near their hands. Conversely, they might produce more preliminary reaching efforts (including hand opening) to stimuli presented near their hands than near their mouths (*9*). In any case, the locus of stimulus presentation accounts for all the significant effects. There is little reason to infer that the responses were governed by specific releasing mechanisms, and no evidence that the shape of the stimulus makes a difference.

Jacobson and Kagan's study contains four major methodological problems that are likely causes of the findings of no difference they obtained. (i) The camera and stimuli were situated at 45° angles to the left and right, respectively, of the infant. The infant's mouth may not have been visible on the videotape records at all times (the camera would be at a 90° angle from the infant's mouth when the infant faced the stimulus). It seems unlikely that one could obtain a valid scoring of infant tongue movements from such videotape records. (ii) The criterion for scoring a tongue protrusion was not sufficiently rigorous. Tongue protrusions were scored "whenever the infant's tongue was visible on the screen" for more than 0.5 second. Presumably, tongue protrusions could have been counted whenever the infant turned toward the camera and opened its mouth. (iii) The manner in which the stimuli were presented was not sufficiently controlled. The pen and ball were moved closer to the infant's mouth than was the adult tongue (*10*). Infants might make mouthing and tonguing movements to an object moved close to the mouth. For interpretable results, it is vital to control the distance of the stimuli from the infant, and the extent and type of movement. (iv) The hand and ring stimuli should be presented in front of the infant's eyes, not near the hands. Infants must see a gesture in order to imitate it.

These methodological flaws would greatly affect the data obtained. For example, the nonrigorous scoring criterion would tend to mask the signal (true tongue protrusions) with noise (visible tongues), and the poor camera angle would mean that only a subset of the infant's response would be photographed. In analyzing such data, it would seem prudent to make as few statistical assumptions as possible and therefore to rely on nonparametric rather than parametric statistics. Jacobson and Kagan introduced such nonparametric analyses to examine which stimulus elicited the maximum tongue protrusion response for each infant. The complete breakdown of the 24 infants according to the stimulus to which they responded maximally was as follows: to tongue protrusion, 12 infants; to the pen, 6 infants; to the ball, 5 infants; to the ring, 1 infant; and to the hand, 0 infants (*11*). If the infant's tongue protrusions were not differentially affected by the five stimuli, then one-fifth of the sample (4.8 infants) should have responded maximally to each of the five stimuli. In fact, one-half of the sample responded maximally to the tongue protrusion gesture (binomial test, $P < .001$). Jacobson and Kagan used a χ^2 test to compare the number of infants who responded maximally to the tongue with the number who responded maximally to the pen and ball combined [χ^2 (1) = 3.00]. A one-tailed rejection region could legitimately be used in this case to test the prediction that the tongue protrusion gesture will elicit more infant tongue protrusion than the control stimuli of pen and ball. This one-tailed test is significant ($P < .05$). Given the methodological flaws in Jacobson and Kagan's study, any interpretation of these findings would be highly tentative. We only wish to note that Jacobson and Kagan's own data undermine their argument and offer modest support for early imitation.

In our original report we introduced three different mechanisms that could potentially underlie early imitation (*4*). The debate here has focused on only one 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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2. J. C. Masters, *ibid.*, p. 215.
3. S. W. Jacobson and J. Kagan, *ibid.*, p. 215.
4. A. N. Meltzoff and M. K. Moore, *ibid.* **198**, 75 (1977).
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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4. A table of the results is available from the authors.
5. A. Cliff, P. Haggett, J. Ord, K. Bassett, R. Davies, *Elements of Spatial Structure: A Quantitative Approach* (Cambridge Univ. Press, New York, 1975).
6. P. Haggett, A. Cliff, A. Frey, *Locational Models* (Wiley, New York, 1977), p. 60.
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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