tive; 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.

SANDRA W. JACOBSON*
JEROME KAGAN
Department of Psychology and Social Relations, Harvard University, Cambridge, Massachusetts 02138

References and Notes

1. A. N. Meltzoff and M. K. Moore, *Science* **198**, 75 (1977).
2. O. Maratos, thesis, University of Geneva (1973). Others reporting matching behavior in early infancy include R. Zazzo [*Enfance* **10**, 135 (1957)] and J. Gardner and H. Gardner [*Child Dev.* **41**, 1209 (1970)]. In addition, Smillie and Coppotelli (*3*) found that 6-week-old infants exhibit matching behavior in response to watching tongue protrusion and mouth opening modeled for them.
3. D. Smillie and H. Coppotelli, unpublished manuscript.
4. P. Wolff, *Psychol. Issues* **5**, 1 (1966). All subjects were seen between 8 and 10 a.m. In most instances they were brought to the laboratory while asleep and about to wake up.
5. The initial expectation of S.W.J., who scored the tapes, was that the matching behavior represented selective imitation. Hence if any bias was introduced by the occasional appearance of the inanimate stimuli on the screen, it should have worked against the results in this report. Additionally, the coder was never able to determine which of the modeled behaviors was being displayed.
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.
7. B. J. Winer, *Statistical Principles in Experimental Design* (McGraw-Hill, New York, ed. 2, 1971), pp. 191–196.
8. The research reported here was part of a larger longitudinal study. Tongue protrusion responses after 6 weeks are not presented here because they were affected by an experimental manipulation. Hand movement was not affected by the experimental intervention.
9. This research is part of S.W.J.'s thesis [Harvard University (1977)]. Supported by grants from the National Institute of Child Health and Human Development (HD 10094) (to J. K.) and the Department of Psychology and Social Relations, Harvard University. We thank the parents and infants for their participation; D. Smillie for his suggestions; and J. Jacobson, P. deVilliers, P. Zelazo, and M. Sullivan for their assistance.
* Present address: Department of Psychology, MacKenzie Hall, Wayne State University, Detroit, Mich. 48202.

19 December 1977; revised 2 May 1978

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.

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.

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.

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 [$\chi^2(1) = 3.00$]. 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.

ANDREW N. MELTZOFF
M. KEITH MOORE
Child Development and Mental Retardation Center, University of Washington, Seattle 98195

References and Notes

1. M. Anisfeld, *Science* **205**, 214 (1979).
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.

5 January 1979; revised 4 May 1979

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 logA on logD does not provide a proper test of his theory, however. LogA and logD 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 logA and logP are independent random variables, then a regression of logA on logD 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 logA is generally twice that of logP and nothing more.

The proper test is to regress logA on logP,

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

Stephan's theory predicts that this relation will also be negative, since if the slope relating logA to logD, 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, logA and logP − logA).

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 logA and logD. But a negative relation between logA and logP 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 logA and logP, 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.

DANIEL R. VINING, JR.
CHUNG-HSIN YANG, SHI-TAO YEH
Regional Science Department, University of Pennsylvania, Philadelphia 19174

References and Notes

1. G. Stephan, *Science* **196**, 523 (1977).
2. *Britannica World Atlas* (Encyclopaedia Britannica, Chicago, 1966).
3. G. Stephan, *Am. Sociol. Rev.* **37**, 365 (1972).
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.

1 February 1978; revised 18 July 1978

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-

 0036-8075/79/0713-0219$00.50/0

Interpreting "imitative" responses in early infancy

AN Meltzoff and MK Moore

Science **205** (4402), 217-219.
DOI: 10.1126/science.451596