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## Toward the Origin of Dialogue

*Kenneth Kaye*

Clov: What is there to keep me here?

Hamm: The dialogue.

Samuel Beckett, *Endgame*

### Introduction

There is an obvious similarity between the burst-pause pattern in sucking during the first month of human life and later burst-pause cycles of activity. These are found in visual attention to objects (alternating with gaze aversion), face-to-face interaction (cycles of arousal and passivity), trials in skill acquisition, turns in instructional interaction, and language. Is there a developmental course, from burst-pause or on-off cycles in the individual to turn-taking and dialogue in social systems?

Orienting to the breast, sucking and swallowing are precocious in comparison with other skills—developing from reflexes to organized intentional schemata in a matter of days rather than months after birth. This precocity explains the long history (e.g. Marquis, 1931) and recent proliferation of experimental studies of classical conditioning and reinforcement in the newborn, using sucking as the critical response (cf. Kaye, 1967; Kessen *et al.*, 1970; Millar, 1974).

Investigators differ from one another in the size of the lenses through

which (literally or figuratively) they view the phenomena of sucking. Experimental studies invariably focus upon the infant alone, usually his mouth alone; while students of socialization and attachment regard the mother-infant dyad as their subject, and anthropologists may concern themselves with the nuclear or even the extended family as it involves itself in the care and feeding of new members. Clearly there is an inverse relation between the width of one's view and the sharpness of focus as well as the experimental control one can manage. Nonetheless, the different types of investigation have much to offer one another in the way of cross-validating theoretical interpretations and refining hypotheses. Microanalysis is to no avail without the accompaniment of macroanalysis.

I presume that the mechanisms of early learning which evolved for our species and which are available to the newborn infant are just those which serve the needs of early development and survival in the social systems into which human infants are born. A good example is the infant's pre-adapted capacity to learn phonemic discriminations and to segment perceptual continua categorically (e.g. Eimas *et al.*, 1971). We may also presume that some social systems evolved as a consequence of the human infant. And in the case of an individual family, the systems which develop partly for the goal of producing a child now develop further as a consequence of the infant's birth. Patterns of child-rearing, cultural expectations for newborn infants, and differentiated child-care roles which human societies have created—diverse as they may appear to be—could only survive if they allowed infants' innate programmes to interact with the environment so as to acquire conceptual, social, and linguistic skills in an orderly and productive way. Thus the "environment" of the infant is more than just a given reality with which he has to deal; and his innate equipment is more than just a given reality to which his parents have to accommodate themselves. The environment and the newborn infant are in a sense made for each other, and our job is to understand the whole system as a set of mutual causes and consequences.

Our own work falls in the category of naturalistic observations of the dyad or "nursing couple" (Middlemore, 1941), with an emphasis upon what we believe to be species-universal patterns of behaviour and development. We are using a zoom lens, narrowing in for the details while trying to retain an awareness of the wide-angle picture.

#### BURST-PAUSE PATTERNS

Virtually the entire literature on burst-pause patterns is based upon artificial feeding or non-nutritive sucking under laboratory conditions. When the infant\* sucks on a pacifier or blind nipple, his pauses in sucking tend to be at least as long, or longer than, his bursts of sucks. The bursts consist of four to ten sucks at a rate usually somewhat less than one per second, never faster than two per second. Thus the bursts may last between three and about 15 seconds, with the pauses in pacifier sucking having about the same range. The pattern is least variable over time if the infant is asleep. There are obvious differences in the parameters (both central tendency and variability) of the burst-pause pattern between one session and another, but whether there are stable individual differences in infants across sessions is still problematic. Kron *et al.* (1968) reported individual consistencies in sucking rate and pressure, but not in bursts or pauses. Studies of individual differences in sucking rate (e.g. Balint, 1948) are unhelpful because the rate is typically averaged over a session without regard to the bursts (Kaye, 1967).

When the nipple delivers milk or water at a normal level of flow, the burst:pause ratio increases. There are fewer bursts of only three to five sucks, but the upper limit remains (typically) about nine or ten sucks. The pauses are almost all shorter (in seconds) than the bursts. When the flow is very rapid, as with a full breast or a nipple whose hole is too large, the pauses may disappear entirely and one sees bursts as long as one or two minutes. This fact, and the difference between nutrient delivery systems in one laboratory and the next, apparently led to the erroneous equation of the burst-pause pattern with "non-nutritive" sucking. Actually, the infant always sucks in bursts and pauses; rapid milk flow may tend to prolong the bursts and shorten or eliminate some pauses; while, on the other hand, very short bursts can be and are used by mothers as a clue that something is occluding the nipple. (Kaye, 1972; and additional subsequent observations.)

When milk delivery is made contingent upon some component of the infant's sucking, he appropriately modifies the length of his bursts (Bosack, 1967; Hillman and Bruner, 1972). When some external reinforcer (other than delivered fluid) has been made contingent upon

\* This paper deals exclusively with the first few weeks of life; unless specified otherwise, by "infant" we mean "newborn infant".

his sucking, the pause durations have been modified (Brassell, 1971). Speaking very generally we might infer that the bursts are modified by nutrient delivery (Kaye, 1972) while the pauses depend upon other factors. Unfortunately, bursts cannot so easily be separated from pauses since the onset of one is the point of termination of the other. Is it correct to say, for example, that the infant shortens his bursts when there is no milk? Or should we say that he pauses more often? One is really dealing with the organization of events in time, in this case sucks, and when we treat the pause or even the somewhat arbitrarily defined burst as an event in itself we are doing so only as a matter of convenience.

Yet bursts and pauses *seem* like events to an observer. Although the suck is obviously the appropriate unit of behaviour for recording, the fact remains that the designations "he is sucking" (burst) and "he is waiting" (pause) feel right subjectively. Thus the question arises, does the infant's mother perceive his sucking as being organized in this way? Does she behave any differently during bursts vs. pauses? If so, does this possibly have any consequences for their later interaction?

Several years ago we reported data from ten infants in Cambridge, Mass. and Cambridge, England, observed three to five times during the first month of life (Kaye and Brazelton, 1971). The fact that their mothers tended to jiggle them (or their bottles) during pauses in sucking, and expressed a belief that this "wakes them up" or elicits a resumption of sucking, led us to test whether jiggling indeed had such an effect. Contrary to the mothers' belief, the midwives' and nursery nurses' advice, and the bulk of the literature (e.g. Middlemore, 1941), we found that the pauses were longer if mothers jiggled than if they did nothing. Furthermore, it was not a matter of their having jiggled in the especially long pauses; the mean latency from onset of pause to jiggle was nearly always less than the mean duration of pauses without jiggles. Our principal hypothesis in the present study was that we would replicate the Kaye and Brazelton findings: that jiggling would increase rather than decrease the pause, postpone rather than hasten the next burst.

No physiological function suggests itself for the pauses in sucking, at least from the baby's point of view. He swallows and breathes concurrently with his sucking. He pauses no longer on the second breast than on the first, nor do we typically see any increase in the pause durations over the course of a session; thus fatigue seems to be ruled out. It is

possible that the pauses evolved to allow the flow of more milk, but their temporal parameters are longer than would be suggested by the physiology of the mother's breast (Ardran *et al.*, 1958). Accordingly, we speculated that a possible adaptive function of pauses might be their one most striking effect: to elicit a response from the mother. This hypothesis demanded that we study neonatal feeding from the point of view of interactive behaviour and learning in a natural social context.

#### TURN-TAKING.

Two distinct themes in recent research gave further impetus to our analysis of early feeding. One was the interest in exchange of speaking turns, signalled by an orderly set of partly conventionalized, partly universal rules among adult speakers (Duncan, 1972). Certain specific types of signal, notably eye-contact, are shown to be important in the studies of adults by Duncan, by Kendon (1967) and Argyle (1972), and in studies of mother-infant dyads (Jaffe *et al.*, 1973; Robson, 1967; Schaffer *et al.*, this volume). This naturally leads us to ask whether the rules for exchange of turns may be learned in early infancy, and if so how; or whether on the contrary they may characterize communication in the human species at any age, irrespective of learning.

A second theme has to do with the development of language acquisition proper. A large number of investigators have begun to focus upon the interactive contexts in which linguistic relations are presumably learned. One (and only one) of the necessary precursors of language development is the alternation-of-comments-upon-a-common-topic discussed by Bruner (1975), deLaguna (1927), and Macnamara (1972) among others. The crucial proviso is that a learner of a language must be able to alternate his own attempts at expressing propositions, semantic relations, etc., with the attempts of a model. Such alternation may serve a number of functions, including some which provide information to the child and some which provide information to his mother. For the sake of this paper, it will be sufficient to point out that turn-taking is more than just a characteristic of language, whether learned or unlearned; it is a necessity for the *acquisition* of language. A child will be able to extract little information from adult utterances, or from the mismatch between his own utterances and those of adults, unless there is some high probability he and they are talking about the same thing. This is where gaze direction becomes especially important.

The evidence that mothers tend to look where their infants are looking (Collis and Schaffer, 1975) suggests that the infant may have at least one means of eliciting utterances about an object in which he is interested, long before he can produce even a poor verbal reference to that object.

Similar arguments can be made about the acquisition of skills such as reaching for a toy behind a detour (Kaye, 1976). The infant's gaze movements elicit appropriate object-directed interventions by adults. In all such tasks, there needs to be temporal proximity between the trials of the learner and the model. Timing is crucial in the effect of any feedback—reinforcing, comparative or corrective—upon learning. Thus dialogue ought to be acquired or built into the system first, so that the specifics of language, object manipulation, social ritual, or whatever can then be learned efficiently. Put this way, we can think of dialogue as a necessary context for language acquisition as well as other kinds of learning, and we would be inclined to pursue any phenomenon in early infancy which bears a resemblance to dialogue.

## Method

The subjects for this study were 30 healthy full-term infants delivered by vaginal route in Columbus Hospital, Chicago, and their mothers. The mothers included 15 primiparae and 15 multiparae, 12 breast- and 18 bottle-feeders; all were Caucasian, English-speaking, U.S.-born women. There were 18 boys and 12 girls. The feeding sessions for this analysis were the first 40 sessions which we happened to have coded, cleaned up, and stored in a computer disk, out of an eventual 100 sessions with 50 subjects in a longitudinal study.

Half the sessions took place on the second full day of the infants' lives, between 36 and 60 hours after delivery. All of these sessions were in the mothers' rooms in the hospital, sitting up in bed, between 5.00 and 5.30 p.m. The mothers had just eaten dinner; the infants had been examined, at about 4.30, using the Brazelton Neonatal Assessment procedure (Brazelton, 1973). None of the male infants had yet been circumcised. All were discharged from the hospital within two days after this session.

The other 20 sessions took place at age 12–18 days, in the subjects'

homes. There were some morning, some afternoon, and a few evening feedings at this age. Some fathers, siblings, and an occasional neighbour were present. The Brazelton examination was usually administered before, but in some cases not until after the feeding. However, all observations took place at a time and place in which the mother said she would have been feeding her infant, even if the observers had not been present. (Some mothers know when this time will be, several days in advance; others do not know even when the next feeding will be. This is true of both breast- and bottle-feeders, and does not correspond very well to mothers' claims as to whether they feed by "schedule" or "demand". Both the mother who can and the one who cannot accurately tell us in advance what time the baby will be fed on a subsequent day, appear to regard this as a matter of course.)

For the present preliminary analysis of our data, we have a cross-sectional and a longitudinal sample. Only 10 of our subjects are represented in the two-day as well as the two-week sessions. (This explains how the 40 sessions happen to come from 30 subjects.) The other subjects, on whom we cannot do a longitudinal analysis here, are mainly cases in which the computer-readable transcripts for the other session were not yet ready. When we refer to the "longitudinal sample" in this paper, we shall mean the 10 subjects observed twice. When we refer to the "cross-sectional sample" we shall mean the other 20 subjects, plus half the longitudinals randomly assigned to the two-day group (their two-week sessions being ignored), and the other half assigned to the two-week group (their two-day sessions being ignored). This gives us a total of 30 independent sessions for cross-sectional analysis, 15 at each age.

There were two observers at each feeding, one looking at the mother and one at the infant. Each observer held a digital keyboard attached to a portable cassette recorder (Datamyte, Electro General Corp., Hopkins, Minnesota). Depressing any combination of the numerical keys caused those digits to be recorded, and depression of the "Time" key caused the elapsed time to be recorded, to a precision of 0.2 seconds. Since their two clocks were synchronized (by simultaneously coding the beginning and end of each session), the coders were able to make independent observations of mother and infant, preserving the sequence of events in the dyad.

Observer 1 coded (1) the mother's placement of the nipple in her infant's mouth (ONBT), and its removal (OFBT); (2) her direction of

gaze, at (LOOK) or away from (AWAY) the baby's head; (3) the beginning (JIGG) and end (STOP) of bouts of jiggling, defined as any displacements of the baby's body or cheek, the breast, or the bottle which would have the effect of moving the nipple with respect to his mouth; and (4) the beginning (STRK) and end (STOP) of bouts of stroking, defined as all forms of touching which could be sensible to the infant yet not affect the relative positions of mouth and nipple. Among the events which we ignored, the most salient ones were (1) the mother's vocalizations to the baby, observers, and other people; (2) the type of jiggling, whether moving the bottle in and out or from side to side, pressing on part of the breast, tickling the baby's cheek, or jiggling his body; and (3) rocking her own body together with the baby's, normally in a rocking chair.

Observer 2 coded the baby's taking of the nipple (ONBT), and any rejection of it (OFBT); and each individual suck (SUCK). The sucks were coded at the point of maximum closure (expression), with no regard for swallowing and no information, obviously, about suction. Of behaviour which the observer unfortunately had to ignore, the most promising would have been whether the infant's eyes were open or closed. (It was not possible to crowd the room with a third observer.)

To avoid having to depress two keys for every suck, we used the "Time" key alone. This difference between the tasks of the two observers had the effect of giving Observer 2 a shorter response time. Furthermore, the regularity of the sucking rate within bursts—along with the fact that she was coding the downward, or last visible phase of the suck—enabled her to code each suck just about simultaneously with its occurrence. In fact, a 16mm, 24 frames/s film of one of our sessions indicated that her finger depressed the key only  $5 \pm 1$  frames after the event. Observer 1, however, was  $35 \pm 10$  frames late—due to the fact that he was coding less predictable events. We therefore subtracted 1.2 seconds from all of the coded events for each mother, before mixing the two codings into a single transcript. This gives us our "best estimate" of the actual time of occurrence of events; however, the variability in response times when one is coding live sessions makes it imperative to remember that any inter-event latency A-B of less than about one second could easily have been a sequence B-A, or a simultaneous occurrence of B and A. (Reliability tests on video-tapes of newborn feedings indicate a "confidence interval" or mean deviation of 1.4 seconds for recordings of maternal categories and 0.9 seconds for onset of bursts and pauses in sucking.)

As for the simple reliability of coding vs. failing to code an event, our comparison of the coding of the filmed mother with the live coding made while we were filming was only 0.85. However, this does not reflect the true reliability of coding. Comparison of *two* codings made from the film or video-tapes yields reliability coefficients greater than 0.90, and we believe this would also be true of two live coders who were somehow made deaf and blind to one another's presence. (The Data-Byte makes a slight click, apparently ignored by our subjects but sufficiently audible to us (along with the information from peripheral vision) so that we never regard codings of the same events at the same time as being independent; therefore, at the moment we do not have data on the reliability of live codings.) One can see better live, of course, than from a film—but it is possible that ambiguous events are lost or disambiguated by the camera angle. This would have the effect of giving codings from a film a higher reliability coefficient. Yet the live coder, who can change his position and use extra cues outside the camera's vision, is surely the more valid one so long as events are not happening too rapidly to be coded live.

A third type of reliability which concerned us was of the specific measures to be derived. Between two codings of the same video-tape by different observers, for example, the duration of jiggling (JIGG-to-STOP) correlated 0.92. Correlation of individual observations between observers *within* a session does not give a good estimate of the reliability of derived measures (such as the median jiggle durations analysed later in this paper) *across* subjects. However, the latter can be assumed to be at least as high or higher than the former, if between-subject variability in the durations themselves is at least as great as within-subject variability. Thus a reliability test of this type, the only practical one under the circumstances, is fairly conservative.

Table I shows a sample computer transcript of one of our sessions. To save paper we do not print these transcripts with constant time scale. Instead we print a line every time a new event is coded. As a visual aid we print AWAY continually until it is ended by LOOK; JIGG or STRK (stroking) continually until ended by STOP. The events labelled SUCK, however, are discrete events. BRST and PAUS will be explained in the following section.

Our preferred method of analysis is to eschew time-sampling. When possible we live with the complexity of real time, segmenting our sessions into units which are behaviourally defined (such as JIGG-to-STOP

Table I

Two-minute excerpt from a transcript

Time (sec)	MOBT	MOJG	MOLK	BABY	BTPS	Time (sec)	MOBT	MOJG	MOLK	BABY	BTPS
274.6	ONBT			SUCK	BRST	339.0	ONBT				
275.6				SUCK		340.8				SUCK	
276.6				SUCK		344.8				SUCK	
278.6					PAUS	348.8					BRST
286.8				SUCK		349.6				SUCK	
287.6				SUCK		350.4				SUCK	
293.8				SUCK		351.4				SUCK	
294.4				SUCK		352.6				SUCK	
298.9	JIGG					353.4				SUCK	
301.0	JIGG			SUCK	BRST	354.2				SUCK	
301.6	JIGG			SUCK		356.2					PAUS
302.6	JIGG			SUCK		357.8				SUCK	BRST
302.8	STOP					358.8				SUCK	
304.6					PAUS	359.4				SUCK	
307.8	JIGG					361.0				SUCK	
308.2	JIGG			SUCK	BRST	363.0					PAUS
308.4	JIGG			SUCK		365.4				SUCK	
309.2	STOP					366.0				SUCK	
309.4				SUCK		374.8	JIGG				
311.4					PAUS	375.3	STOP				
313.6	JIGG					380.6				SUCK	
314.0	JIGG			SUCK		381.6				SUCK	
314.6	JIGG			SUCK		383.5	JIGG				
314.8	STOP					385.2	JIGG			SUCK	BRST
320.2	JIGG					385.8	STOP				
320.4	JIGG			SUCK	BRST	386.0				SUCK	
321.0	STOP					386.8				SUCK	
321.2				SUCK		388.8					LAST
322.2				SUCK		389.0	JIGG				
323.4				SUCK		390.4	JIGG			SUCK	
325.4					PAUS	391.2	JIGG			SUCK	
326.8		AWAY				396.0	STOP				
328.7		LOOK				397.4				SUCK	
329.2				SUCK	HFBS	398.0				SUCK	
329.8				SUCK		402.2	OFBT				
331.2				SUCK							
331.4	OFBT										

and PAUS-to-BRST) and asking questions about their temporal organization.

## Results

A monograph in preparation will take the High Road to understanding our full corpus of data: analysing first the organization of sucking at each age; then the intrinsic organization of the mothers' jiggling at each age; then the relation between jiggling and sucking and its possible development over the first two weeks; then (by comparison) the relation between stroking, jiggling, and sucking; and finally the possible role of the mother's gaze direction in affecting her interaction with the infant. Fortunately for the reader of this paper, economy of time and space demand that we take the Low Road. Thus in addition to limiting ourselves to the 40 sessions explained above, we shall also ignore the mothers' stroking and looking behaviour. Since these subjects were involved in an intensive longitudinal study including neonatal assessment and video-tapes of interaction at home through the first six months of life, a large number of additional variables have yet to be studied. We found no differences by sex or by feeding method in any of the measures to be discussed here.

### THE BURST-PAUSE PATTERN

The precise number and durations of the bursts and pauses in any sucking session depends upon the criterion one uses. Figure 1 shows the distribution of all suck-to-suck intervals in the first 400 seconds of a single session. This bottle-feeding at two weeks was chosen because it happens to be both our longest session and the one with the least amount of maternal activity. The pattern, however, is the same in all our sessions. One sees a bimodal distribution with most intervals less than 2.0 seconds, and substantial variance among the intervals longer than 2.0. As is the case in this session, we almost never see an interval of exactly 2.0 seconds between sucks, and therefore this was our criterion value; intervals greater than 2.0 were regarded as pauses. Furthermore, we required at least three sucks, or two consecutive intervals of 2.0

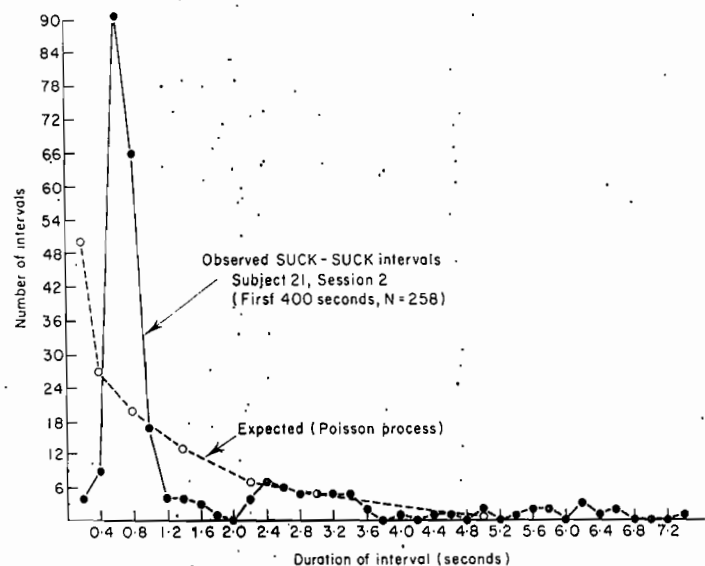


Fig. 1. suck-to-suck intervals found in the first half of a long bottle-feeding session, at two weeks. Expected values are based on an exponential distribution where  $N = 258$  and  $\lambda = 0.70$ .

seconds or less, as criterion for a burst. Others have arrived at these same criteria (Sameroff, 1967; Kaye, 1967).

Thus the event BRST was written at the first suck of each burst, and PAUS 2.0 seconds after the last suck of each burst as shown in Table I. A suck-to-suck interval of 3.6 seconds would become a PAUS-to-BRST of 1.6 seconds, and so on. If OFBT (off-breast or off-bottle) came after a PAUS, we changed that PAUS to LAST. If OFBT came after a BRST and before the PAUS, we changed the BRST to HFBS (half-burst) and deleted the PAUS. For analysis we exclude the time after LAST or HFBS (i.e. while the nipple was being withdrawn), and also exclude the time between ONBT and the first BRST.

Pauses, the PAUS-to-BRST intervals, may contain one or more isolated sucks. However, in the full 15-minute session of Fig. 1, only 11 of the 82 pauses contained isolated sucks. This is representative of the typical pattern. More troublesome is the fact that a rigid criterion such as 2.0 seconds inevitably omits some very short pauses, and introduces some

which are not "really" pauses or which would not be perceived by the mother as pauses. What we defined as (for example) bursts of 6.0 and 5.0 seconds, separated by a 1.0-second pause, might "really" have been a 12-second burst. However, these errors, like the variability in observer response time, will work against any significant findings. The only systematic distortion to be kept in mind is that the shorter the criterion interval for pauses, the shorter both the median bursts and the median pauses will appear to be.

Figure 1 shows the deviation of suck-to-suck intervals from an exponential distribution. Many intervals cluster around 0.8 seconds, the median for this session; the intervals greater than 2.0 seconds are essentially a random distribution except that there are more very long ones, which of course are pauses.

Figure 2 is a plot of the BRST-to-BRST and PAUS-to-PAUS intervals from the same session as Fig. 1. These events also deviate from the exponential distribution; the onset of a burst or pause is obviously not a Poisson process. However, these intervals are not distributed bimodally like the suck-to-suck intervals in Fig. 1. There is periodicity, or uniformity in the intervals.

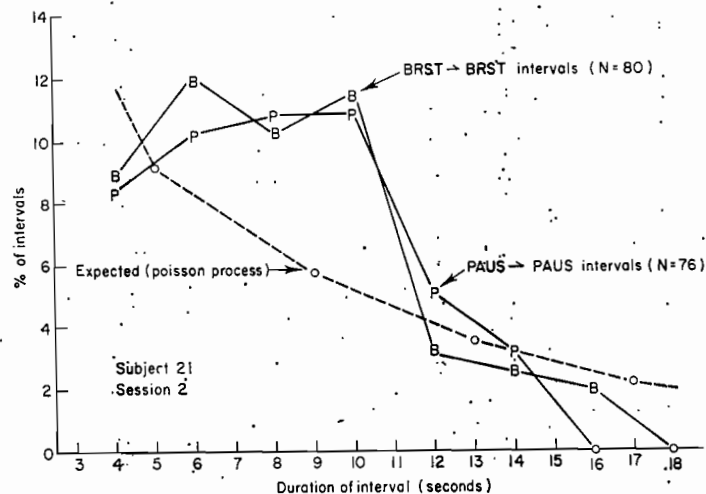


Fig. 2. BRST-to-BRST and PAUS-to-PAUS intervals.  $N = 82$ ,  $\lambda = 0.012$  for both curves.

There is no reason *a priori* for the BRST-to-BRST curve in Fig. 2 to fit so closely the PAUS-to-PAUS curve. If the pauses were rests we might expect them to be correlated positively with the lengths of the preceding bursts, making the BRST-to-BRST intervals less uniform than the PAUS-to-PAUS. If on the other hand the natural function of the pauses were to let milk collect in the nipple, then at least in breast-feeding we might expect the pauses to be positively correlated with the following bursts. This would make the PAUS-to-PAUS intervals less uniform. Alternatively, the bursts and pauses might be hypothesized to have an inverse relation: if the pause somehow compensated for the duration of the preceding burst, then BRST-to-BRST would be much more uniform than PAUS-to-PAUS. Such hypotheses, however, find no support in our data. The correlation is zero between pauses and their preceding bursts ( $N = 1147$ , across all sessions) and zero between pauses and their following bursts.

Is there any stability in the individual infant's burst-pause pattern? Certain parameters of sucking, such as pressure and per minute efficiency, have been shown to distinguish individual infants over several feedings through laboratory apparatus (designed to monitor pressure, rate, etc.) (Balint, 1948; Kron *et al.*, 1968). But these investigators have failed to show any stability in the burst-pause pattern. Nor is there evidence that the individual consistency in the infant's response to such apparatus is matched by a corresponding consistency in his feeding under normal (interaction) conditions. We as yet have no substantial evidence supporting individual consistency in our dyads. There was a reduction in pause length by the time our longitudinal infants were two weeks old, as shown in Table II. Despite this general reduction there seems to be some significant correlation in individual dyads' median pause lengths. If supported by our data yet to be analysed, this would be consistent with a view of pauses as relating to the mother-infant interaction rather than the flow of milk (which differs greatly between hospital at two days and home at two weeks).

#### JIGGLING

Sixty-two per cent of the jiggles in all of our sessions came during pauses, despite the fact that just under 45% of the elapsed time was pause time. Figure 3 compares the probability of one or more jiggles

Table II  
Comparison of two-day and two-week sessions

	Median burst length (N)		Median pause length		Proportion of session, jiggling		Median jrc-stop duration	
	Two-day	Two-week	Two-day	Two-week	Two-day	Two-week	Two-day	Two-week
Cross-sectional (Mean of 15 Ss at each age)	8.2 (26)	7.1 (27)	4.7	5.7	0.150	0.123	2.25	1.69
t-test	N.S.	N.S.	N.S.	N.S.	N.S.	N.S.	N.S.	N.S.
Longitudinal Ss:	Two-day	Two-week	Two-day	Two-week	Two-day	Two-week	Two-day	Two-week
03	7.6 (34)	4.3 (9)	13.5	17.0	0.278	0.072	3.50	0.75
04	3.9 (26)	6.4 (31)	8.5	3.7	0.411	0.056	3.50	1.00
09	8.8 (7)	7.5 (34)	6.0	2.3	0.112	0.025	3.00	0.50
16	5.9 (10)	18.8 (12)	18.3	2.3	0.213	0.175	4.25	1.50
21	7.8 (9)	5.1 (82)	2.6	2.5	0.106	0.038	1.00	1.50
25	5.6 (13)	4.7 (26)	6.6	4.5	0.142	0.097	1.88	1.00
28	5.7 (13)	<sup>a</sup> (0)	11.3	<sup>a</sup> (0)	0.136	0.199 <sup>b</sup>	(1.23)	<sup>a</sup>
31	5.4 (56)	6.2 (47)	1.7	1.6	0.194	0.114	4.50	0.75
32	5.4 (11)	7.2 (11)	6.8	4.4	0.162	0.060	2.25	0.75
36	5.7 (11)	6.9 (40)	2.3	2.0	0.228	0.128	3.13	2.00
(mean)	6.18	7.46	7.76	4.48	0.198	0.096	3.00	1.08
Rank order correlation	$r_s = 0.04$	$r_s = 0.53, p = 0.05$	$r_s = 0.24$	$r_s = -0.21$				
Binomial sign test	2-tailed N.S.	2-tailed $p = 0.039$	1-tailed $p = 0.010$	1-tailed $p = 0.019$				
t-test (Wilcoxon):	N.S.	N.S.	$t = 2.75, p < 0.05$	$t = 4.23, p < 0.001$				

<sup>a</sup> One subject is omitted because his session at two weeks contained never more than one burst per ONBR-to-ORBR period, thus no official pauses (the bottle was removed during each of four pauses).

<sup>b</sup> Jiggling occurred only during excluded parts of the session (see text).



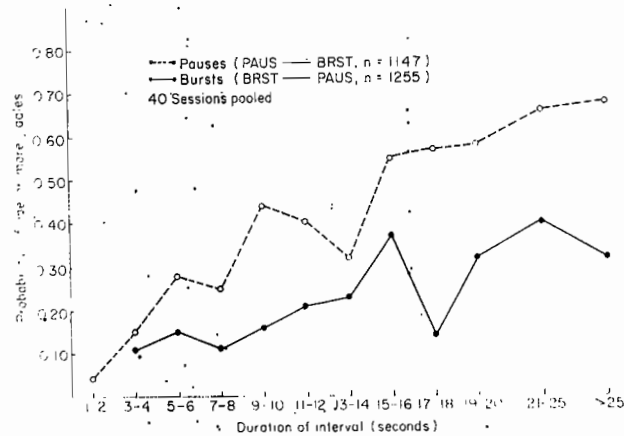


Fig. 3. Probability that at least one jiggle will fall into an interval, depending upon the length of the interval; bursts compared with pauses.

falling into a pause of any length, with their probability of falling into a burst of the same length.

As reported by Kaye and Brazelton (1971), the pauses containing jiggles were significantly longer than those without. In the earlier study, we compared the *logs* of pause durations (raw durations do not meet the assumption of homogeneous variance) by a *t*-test for each subject at each session, finding a preponderance of significant differences all in the same direction. With much more data now we are dissatisfied with central tendencies like the mean of logs or the median, preferring instead to look at the whole distribution. Figure 4 compares pauses with one or more jiggles, and pauses with none, over all sessions. The data on each group—two-day breast, two-day bottle, etc.—look the same.

One might expect pauses with jiggles to be longer just because there is a greater chance of any random event falling into an interval, the longer the interval. But we can show in a simple way that this is not the explanation. The median duration of the 896 pauses without jiggles was 2.5 seconds (mean = 4.4). The median duration of pauses with jiggling was 10.2 seconds (mean = 15.0). Even the portion of these pauses that followed the first jiggle had a median of 5.5 seconds (mean = 9.7), significantly longer than the pauses without jiggles ( $t = 6.1$ ,

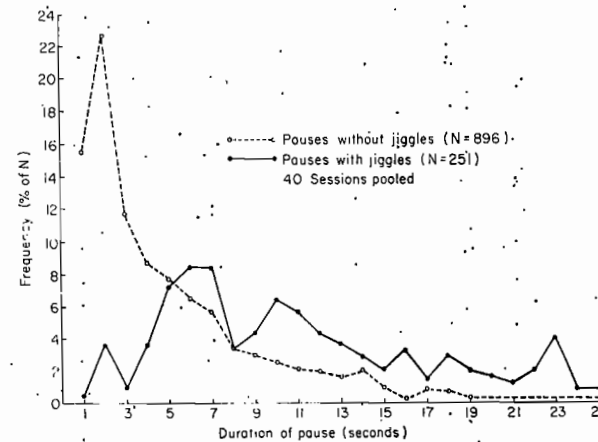


Fig. 4. Duration of pauses with and without jiggling.

$p < 0.001$ ). I interpret this as sufficient evidence that jiggling *lengthens* the pause.

#### STOPPING

It is mildly interesting that mothers' jiggling is contingent upon the pauses in their infants' sucking, and that they are wrong in believing the infants' BRST to be contingent on the jiggling. However, in the data presented so far we do not have an interaction. We have simply the behaviour of one person organized biologically, and the behaviour of another person fitting into the organization somewhat. It is a one-way direction of effect, and furthermore it is simply a response—nothing we would call learning.

If, however, the behaviour of mother and infant were to be thought of as turn-taking, then we should not consider JIGG as the mother's turn. JIGG, the onset of jiggling, is only the beginning of her turn which has a duration JIGG-to-STOP just like BRST-to-PAUS. The event STOP can be thought of as comparable to a speaker's yielding the floor. Is a BRST any more likely to occur immediately after a STOP than at other times? The answer is yes.

If an event such as BRST is *not* contingent in any way upon another

event such as JIGG, then the latencies JIGG-to-BRST should be distributed as a Poisson process, fitting the exponential distribution as they do in fact in Fig. 5. These latencies are from the last JIGG in a pause to the

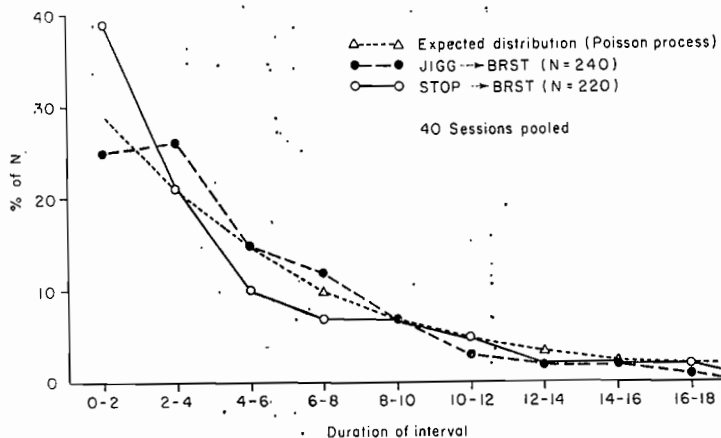


Fig. 5. Latencies to BRST from last JIGG in a pause; and from last STOP.

next BRST, regardless of what intervenes. The sequence may be JIGG-STOP-BRST, or it may be JIGG-BRST, followed only later by a STOP. Similarly, the last STOP in a pause can precede the last JIGG: the sequence can be JIGG-STOP-JIGG-BRST. *A priori*, the STOP-to-BRST could be longer or shorter than the JIGG-to-BRST times. However, the issue is not which is longer or shorter on the average, but how each of them compares with a random or Poisson process. The STOP-to-BRST latencies in Fig. 5 do not coincide with the expected values. There are many more short ones than expected, and many more than there are of the short JIGG-to-BRST latencies.

The statistical significance of the STOP-to-BRST curve's deviation from an exponential distribution can be tested by a statistic  $\Sigma|x-\bar{x}|/2\Sigma x$  whose expected value is equal to  $1/e$  and which is normally distributed with calculable variance (Cox and Lewis, 1966). For four sub-samples of our data—the longitudinal and cross-sectional groups at each of two ages—this statistic is always between  $1z$  and  $3z$  above the expected value. The graphs all have the form of Fig. 5, with visibly more of the very short waiting times (0-2.0 seconds) between STOP and BRST than

we could expect by chance. Thus we can conclude that a mother is more likely to elicit a BRST within two or three seconds when she stops jiggling than when she starts jiggling.

#### THE MOTHER'S TURN

The preceding sentence expresses a conclusion from our data in terms that cannot help but raise the further question of reinforcement. If indeed mothers begin by thinking of (or unconsciously relying upon) the jiggle as an elicitor of sucking bursts, and if indeed the burst is more often contingent, especially in the critical range of 1-2 seconds, upon the STOP than upon the JIGG, should we not predict a change in the duration of jiggling? Instead of jiggling and watching for the next burst, mothers might learn to jiggle and stop. This would be reflected in a number of changes in the many types of sequence found in our data. But if it were a fundamental and robust phenomenon, it ought to be reflected in the simple durations of jiggling, throughout the session. We can hypothesize that jiggling in general becomes shorter in duration over the first two weeks. Table II presents the data. These values are not to be regarded as precise. In fact, there were many latencies recorded as 0.0, when the observer saw that the jiggling had ended by the time he had entered JIGG. The data reflect more of these at two weeks. For the cross-sectional sample a t-test comparing median JIGG-to-STOP durations fell short of significance at 1.28 ( $p = 0.213$ ). For the longitudinal sample a matched t-test showed a highly significant change ( $t = 4.23$ ,  $p < 0.001$ ). The two-day latencies of 2.25 or 3.0 seconds may seem quite short, but the medians themselves range from 1.0 to 4.5; at two weeks the range is 0.5 to 2.0.

The difference was evident in the cross-sectional comparison, but individual variation prevented its reaching significance. This fact itself is interesting, suggesting the possibility of stable individual differences in jiggling duration which we were able to test in our longitudinal sample. Using the median JIGG-to-STOP duration we found a correlation of only  $-0.35$  between two days and two weeks. But the STOP-to-BRST latencies at two days predicted the JIGG-to-STOP latencies at two weeks. The correlation of  $0.53$  (N.S.) is merely suggestive; since it is based on a small N. But the amount of variance accounted for is surprising, and we look forward most of all to replicating this particular finding with our larger corpus of data. It would suggest not just that mothers learn

shorter bursts of jiggling, but that those mothers change the most whose infants reinforced them the most.

Lest we seem to suggest that all of the effects in early feedings are those of the infant upon his mother, attention should be directed back to the other side of the story. Pauses were shorter at two weeks than they had been at two days. And there were more of the short STOP-TO-BRST latencies at two weeks. These exceeded the frequency of short JIGG-TO-BRST latencies by 25% at two days and by 50% at two weeks. The rule "you end your turn and I'll start mine" was being learned by the infants as well as by the mothers. It is not a matter of a new rule suddenly emerging. The learning in this case seems to consist in heightening the frequency of certain interactive sequences which occur naturally with some probability and then are selected by the dyad-system.

## Discussion

### MICROANALYSIS

In contrast to the complexity of our analysis (much of which still remains to be done) the results appear to be rather straightforward. Mothers begin by interpreting their infants' pauses as signs of flagging and as occasions for stimulation. Gradually, apparently under the influence of a tendency on the infant's part to respond to the *end* of jiggling by a resumption of sucking, the mothers change their response during pauses from "jiggle" to "jiggle-stop". By two weeks both the duration of jiggling and the duration of pauses are shorter.

This suggests a degree of symmetry between the behaviour of both partners. When her baby pauses the mother might jiggle, or she might not. When his mother stops jiggling the infant might suck, or he might not. While he is in the midst of a burst she is unlikely to jiggle—though the likelihood increases as the bursts get very long. While she is in the midst of jiggling he is less likely to resume sucking—though the likelihood goes up if she continues too long.

There is also, however, some important asymmetry. The smooth alternation of turns comes about, when it does come about, by the mother's accommodating her turns to the temporal organization imposed by the infant. Her turns get shorter and so (therefore) do his

pauses. His bursts do not depend upon her intervention at all. If she does nothing, he will organize his sucking in essentially the same manner. There is merely a slight tendency to delay the onset of a burst, all other things being equal, until after her jiggling stops; or to hasten the onset of some of the bursts so that they come right after she stops.

We do not suggest that the infant is in any way conscious of the effect his behaviour may have, or that he intends to reinforce his mother. Nonetheless, because jiggling tends to prolong the pauses, and jiggle-stop tends to elicit a resumption of sucking, the potential reinforcement is there. "Tends to elicit" does not mean every time, of course; but partial reinforcement is the best kind. It seems to be only a matter of the number of very short-latency "responses" of BRST following a STOP. One way of reading Fig. 5 is that there are about one-third more STOP-TO-BRST latencies under two seconds than we should expect by chance. This may well be sufficient to shape a tendency on the mother's part to STOP jiggling, and thus to reduce the duration of JIGG-TO-STOP over the course of many feeding sessions.

Two sessions, approximately the seventh and the seventieth times the mother feeds her infant, certainly do not make a learning curve. We are currently making video-tapes of a few mothers, every other day during this period, to see if we can see the changes in jiggling and in the burst-pause pattern over a gradual period and despite each day's new disruptions. (One of the striking things about the first two weeks is that, on balance, there is very little "routine". The homes of our subjects are certainly very different from a well-controlled animal laboratory, and if consistent trends survive the exigencies of each new day and the various sources of unsystematic error, they must be robust trends indeed.)

The fact that STOP does increase the probability of BRST—or, put another way, that it sometimes has no effect and sometimes is followed by an immediate BRST, is not difficult to understand when we consider the nature of neonatal reflexes. This response seems to lie somewhere between a reflex like the Moro which is externally-stimulated but still depends upon the infant's state and activity, and a primary circular reaction like vocalization. By the second month an adult can substitute his own "ahhh" for the infant's, filling the pause, and elicit a repetition of the sound from the infant. This is reminiscent of the burst-jiggle-burst sequence. Since sucking is different in form from jiggling, neither we nor the mother perceive her infant's behaviour as imitation. Yet her belief

that the bursts are contingent upon jiggling is about as well founded as her later belief that his "ahhh" is an imitation.

#### MAGROANALYSIS

It is necessary now to present a view of the broader context in which our observations have been made. My interpretation draws upon some additional observations in the course of this study as well as the theoretical issues and biases my students and I have been developing.

The newborn infant and his mother have very different agendas. The infant responds to hunger as to pain, cold, or distention of his bowel: he is equipped with certain reflexes, and he gradually becomes able to anticipate what the sequence of events will be as those reflexes occur. His time-frame expands slowly from seconds to fractions of a minute. Even as we see intentional breast-seeking replace rooting, anticipatory tonguing replace crying, we still see an organism which is concerned only with getting control over whatever is immediately necessary to obtain the goal when the time comes. He is learning, but it is a matter of being conditioned by very immediate results in his environment. The infant is responsive to, and affected by his experience of, the transition points from hunger to feeding, from cold to warm, etc.

With the mother the situation is very different. While we may casually use the term reinforcement, the fact remains that we can point to no intrinsic reward for her in the infant's bursts. If this event has meaning for her it is only because she is concerned with a larger picture. More than any other topic in these early weeks of her baby's life, the mother talks about his cycles (Sander, 1962). "When will he sleep?; how long will he sleep?; when will he be hungry again?; what is it that probably explains his crying at this time of day?; how much milk will he take at this feeding?; what does his behaviour now tell about what he is going to do or want next?" She needs to see regularity in his behaviour if for no other reason than to feel she understands him. His way of controlling the uncertainty in the world is to develop appropriate techniques for responding to whatever unpleasantness may arise. Her way is to develop a theory of the infant, and one (we believe) which is really a theory of the infant-in-time or time-in-the infant.

Yet it is not enough for her to be able to anticipate the infant's cycles. The mother (if you will permit some speculation) wants to see herself as a *participant* in the organization of his day. Without disrupting

his cycles she wants to make them contingent upon her own schedule, or at least to see herself as having made him accommodate himself to her. Some of our mothers fantasize accommodations by their infants: reporting strict schedules when they are clearly on demand, claiming credit for altering the infant's sleeping or attentional behaviour when they have actually accommodated their own techniques by trial and error. Other mothers express frustration at not yet regaining any sense of control over their lives.

I think a similar process is occurring within the narrower time-frame of the feeding session. The mother is not satisfied with perceiving a burst-pause pattern innately wired into her infant. In fact, it is surprising how few mothers express any awareness of this regular cycle. Their awareness is largely subconscious, and it takes the form of seeing regularity in the interaction rather than in the infant. "Don't go to sleep, now" (jiggle, jiggle). The jiggling behaviour itself is less important than the function it serves, which sometimes takes other forms such as calling the infant's name, clicking the tongue, or perhaps stroking. It is a strange sort of researcher who tries to report what he thinks his subjects are trying to say in their behaviour. But as Emerson said, "Words are also actions and actions are a kind of words." During early feedings, in the first month before mutual smiling and other games begin, the mothers' actions express to me: "I'm not just sitting here holding this bottle. I'm *feeding* my baby."

The infant, let me reassure you, does not inspire me to paraphrase. Yet he is the one with the remarkably organized behaviour. We must assume that a variety of adaptive functions affected the evolution of his behaviour. One set of functions have to do with obtaining food. But his behaviour affects his social environment as well, and these effects have in turn had evolutionary consequences. When we think of the infant's differentiation of reflexes into skills, etc., as accommodation to an environment, we need also to be aware of how the environment is differentiating itself, at least partly as an accommodation to the infant. The reflexes with which he is born are appropriate ones given the nutritive social environment into which he is born, which may include sets of "reflexive" built-in responses of its own. However, the organization of the infant's behaviour will itself impose and elicit an organization in the environment.

It is true that the human newborn is "autistic" (Mähler *et al.*, 1975) and "subcortical" but it is a mistake to regard that fact strictly as a

matter of "helplessness". Surely the immaturity in which our species delivers its young is no mere accident of premature parturition. The newborn's immaturity at birth may be his most important asset—for it guarantees a degree of salient regularity, rhythmicity, and predictability to his behaviour which will not again be possible once higher cortical processes take over. The human mother apparently makes use of the fact that she can predict the temporal structure of her infant's behaviour. She uses it to build, through mutual differentiation of responses, a basic pattern of interaction which will *not* depend upon biological clocks but upon mutual monitoring and feedback.

It may be that a species which is going to rely so heavily upon communication and co-operation, and in which such a vast amount of knowledge and basic skill is going to be passed on through interaction and discourse, needs to deliver its young while they still have the capacity to teach their parents how to interact with them. The biological clocks of the newborn are indeed remarkable as is the co-ordination of sucking at birth; but they are not a matter of precocity. Whatever their mechanisms, these clocks are characteristic of things that have been going on in the fetus's nervous system during most of his gestation. They represent the very opposite of precocity. The fact that the most neurologically advanced organism is born in a neurologically primitive state needs explaining. Only by looking at the psychological development of the dyad can we begin to explain it.

I should not like to limit the relevance of our findings to turn-taking alone. Although it has intrinsic importance for linguistic dialogue and for instruction, turn-taking is only one aspect of the enormously rich exchange that goes on between infants and other people. True, the infant and his mother learn to take turns and to read one another's signals about the beginnings and ends of turns. But at a more general level they are learning how to anticipate one another's behaviour, which is just as important in simultaneous activities, such as smiling, as it is in the alternation of turns. Similarly, as their interaction continues in subsequent feedings and other situations, they learn to read intentions; to interpret affect; and perhaps most important to read the signs of how the other partner is interpreting their own behaviour (Winnicott, 1967).

Previous authors have used the word "dialogue" to describe early mother-infant interaction, often with respect to breast-feeding (e.g. Resta, 1955). The literature is old and diverse, and I cannot do justice to it here. However, the notion has been much more general than

simply "turn-taking". For Spitz (1963a, b, 1964) the exchanges between mother and infant gave the infant a sense of being responded to, of having his action completed in a context, and thus were seen as crucial in establishing his well-being and sense of identity:

"... my proposition is that the mutual exchanges between mother and baby consist in a give and take of action and reaction between the two partners, which requires from each of them both active and passive responses. These responses form series and chains, the single links of which consist in what I call 'action cycles', each completed in itself and at the same time anticipating the next link. I designated these seriated response exchanges as the 'precursor of dialogue', as a primal dialogue."

Spitz, 1964, p. 774

Both kinds of dialogue (simultaneous and alternating) provide for joint engagement in a task and the opportunity for mutual imitation—which are necessary if language and conceptual development are to proceed. In addition, the various forms of dialogue share two other important features. These features tie together the several tasks—feeding, face-to-face play, stranger-interaction, bathing, and teaching—in the longitudinal project from which the present data are drawn. One feature is the strengthening of a cathexis between the two partners. It is a bond consisting of affect plus rules, and we consider its main significance to lie in its transfer outside the mother-child dyad. Each partner develops skills and expectations which he will at least try to apply with this same partner in other situations, and also with other partners. Thus we conceive of the child's development of a *self*, as well as the mother's continued development of her own self-concepts, as a matter of transfer of learning from the dyad-role to other roles in which each of them will become engaged. For the infant these will include interaction with other adults, peer play, and eventually schooling. For the mother they include experience with her subsequent infants, but also nurturant, tutorial, and accommodative roles with her husband, her parents, etc.

A second feature of both simultaneous and alternating dialogue is the provision of a stable context within which one partner or the other can introduce variation (Stern, Fogel, this volume).

Within the interactive situations we are studying microanalytically I expect to find recurrence of the general phenomenon discovered in early feeding: first a phase in which the mother tries to fit in to her infant's more or less autonomous patterns of behaviour, then a phase

in which they achieve a mutual contingency or "game". What happens subsequently is well-known, at least for later face-to-face interaction and peekaboo-type games. The contingent sequence becomes a goal in itself for both partners, and they attempt to initiate it and prolong it (Brazelton *et al.*, 1974; Bruner and Sherwood, 1974). Finally, there is the phase in which one or the other partner violates the rule (Stern, 1975). Probably a major function of this violation (and part of its intrinsic humour) is the verification and mutual confirmation of the fact that the rule really was what he or she thought it was. But an effect of violation is to disrupt the ongoing sequence, and sometimes to *disconfirm* the other partner's expectancies. So the stage is set again for phase one, finding regularity, and phase two, establishing mutual contingencies, each time at a slightly more sophisticated level. If this theory is valid, then with each elevation of the dialogue there is acquisition of skills which are potentially transferable beyond the dyad.

Are phases three and four found in sucking as well, or do they depend upon more cognitive apparatus and a higher level of communication than the infant has attained in the first month? We do not yet know. Our notion is that the advanced phase of interaction with respect to sucking serves as a very primitive phase with respect to mutual smiling and other facial communication. Much the same processes are recapitulated in the development of one domain of interaction after another. Thus the communicative successes or failures of a dyad multiply as time passes.

## Conclusions

1. When feeding under natural conditions in the first few weeks of life, infants suck in bursts of fairly regular duration separated by pauses of fairly regular duration. There is no correlation between the duration of a pause and that of the burst immediately preceding or immediately following it.

2. Findings in this and other studies are consistent with—but do not firmly establish—the idea that duration of bursts depends more upon milk flow while duration of pauses depends upon other factors including interaction with the mother.

3. Mothers tend to jiggle their infants, stroke the mouth area, retract

the nipple, or jiggle the bottle more during pauses than during bursts of sucking.

4. Contrary to the mothers' (and nurses') beliefs jiggle reduces the probability of an immediate burst of sucking.

5. The cessation of jiggle slightly increases the probability of an immediate burst of sucking.

6. Between two days and two weeks post partum, mothers reduce their duration of jiggle so that there are far more short jiggles, and the behaviour basically becomes "jiggle and stop" rather than "jiggle until he starts sucking again".

7. This phenomenon is the earliest example of infants and mothers learning to take and give turns.

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