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Thickening thin data: the maternal role in developing communication and language[1]

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1 History

To many students of child speech . . . it seems that the linguistic data available to the child are so thin that we can only account for his knowledge by assuming that it is, in substantial degree, innate. It is possible, however, that the surface data seem as thin as they do because they are imagined in too static a form, as a set of still photos, unconnected model sentences. It may be as difficult to derive a grammar from unconnected sentences as it would be to derive the invariance of quantity and number from the simple look of liquids in containers and objects in space. The changes produced by pouring back and forth, by gathering together and spreading apart are the data that most strongly suggest the conservation of quantity and number. The changes produced in sentences as they move between persons in discourse may be the richest data for the discovery of grammar. (Brown 1968: 287–8)

In the decade since Roger Brown's Presidential Address to Division 8 of the American Psychological Association, these words have had remarkably little effect upon the way language development has been studied. The handful of investigators who have made parent–child discourse their subject of study have restricted themselves almost exclusively to analysis of mothers' utterances out of context (e.g. Snow 1972; Friedlander et al. 1972; Phillips 1973; Holzman 1974; Newport 1977; and unpublished studies reviewed by Slobin 1975) or limited types of contingency in verbal interaction (Cazden 1965; Brown et al. 1968; Brown & Hanlon 1970).

Recently a number of authors have discussed mother–child communication more generally, with language coming to be seen as essentially a subset of communicative skills, the development of language depending upon the prior development of communication itself (Bruner 1975b; Halliday 1975; Harris 1975; Lieven 1978, Moerk 1975; Riegel 1975). Our current work falls within this new

tradition. In each of the papers just listed the evidence is extremely thin, and this chapter summarizes some recent attempts to thicken it.

What interests us about language is not grammar, but those features which linguistic skills share with communicative performance in general. Those features are also found in mother–infant discourse over the first year of life. While not explaining the development of language, this fact provides a new source of data by which our hypotheses may be enriched.

The *language-learning capacities* of our species have obviously been provided by evolution. *How* these capacities evolved is a question I cannot deal with; but *what* the capacities are, what it is with which we are endowed that makes language development possible, is what I take to be the question motivating a search for the precursors of language in infancy.

To find nothing in early communication but protolanguage and precursors of language would be virtually devoid of explanatory value. It is self-evident that everything one observes in adult language has an origin, and if one defines a phenomenon sufficiently broadly and vaguely one can always see some manifestation of it or analogy to it, in infancy or perhaps in the womb. Taken by itself this tells us nothing about the process by which the early form comes to be the later form. In fact it need not be the case that the formal similarity has any psychological, developmental reality at all.

Hence what should be looked for is neither linguistic *genius* in the human infant nor linguistic *training* in the human environment. Rather it is developmental *process*. Pre-speech communication, which is a matter of changing patterns of interaction over the whole course of the first year, is of interest because, among other things, it enables the child to *learn* language. Instead of looking at the complexity of human language and shaking our heads at how the child can ever master it, and rather than assuming that he is in some absurd sense born knowing it already, I would rather acknowledge that natural languages are just those, among the infinity of languages we might imagine, which happen to be very easy to learn given the developmental processes with which evolution has supplied us. While it is conceivable that the nature of language and the characteristics of specific languages exert some effects upon mother–infant interaction, I would emphasize the opposite point. It is almost a corollary of Darwinism: only those natural languages could have survived which

happened to lend themselves to acquisition by one- to three-year-olds.

That this is an overlooked but potentially fruitful way of looking at language development may be illustrated with respect to the issue of 'critical period'. No satisfactory answer has been found to the question: what is there about the physical structure of the brain which makes it brilliant at acquiring language between the ages of one and ten, but rather stupid thereafter? Suppose instead we ask (among other questions): what is there about the behavior of the child in relation to other people which makes a certain kind of rule-learning possible in the early years and more difficult given his later modes of interacting?

Further, I would assume that developmental processes inhere in the social systems of which the infant is only a part, rather than in the infant himself. What it means to say that *homo sapiens* evolved as a linguistic creature is that processes of skill transmission evolved, largely depending upon interaction with the mother, which are fundamentally unlike anything occurring in other species.

Although the actual materials upon which evolutionary processes can work are those of the two organisms mother and child, as separate entities (reproductive physiology, neonatal reflexes, sensory and motor pathways etc.), the explanation for evolutionary changes at the level of each partner's behavior can be found only in the fit between the two – and more precisely, in the development of the individuals through interaction with one another. What is true of the newborn whale, kangaroo and penguin and of the newly hatched duckling is as true of the human. It is neither the baby whale's propensities for swimming nor his mother's propensities for pushing him to the surface which guarantees his survival and his subsequent exposure to the necessary and sufficient experiences for whaleness. It is instead the perfect design of each partner's behavior for the other's.

2 Theses

This chapter will present some findings which I think go beyond the formal correspondence between certain pre-speech communicative behavior of infants and properties of older children's or adults' language. Let me summarize my four claims with respect to these observations.

2.1

Each of the observations is merely suggestive. At the present state of our knowledge it is quite possible none of them has any relevance to the problem of how language develops. For example, they could be aspects of a general process of nonverbal communication, and language proper could be a separate system which begins to develop later, only later becoming coordinated with nonverbal communication. (This is, of course, not our contemporary view; but it is far from having been disproved.)

2.2

The observations, by myself and others, have a good deal of consistency despite their being drawn from a wide domain of ages, response modalities, situations and cultures. They suggest a set of basic processes by which infant and mother in the first year of life prepare and equip themselves for developing a broad range of skills in the second and third. Specifically, without rejecting either of the two broad approaches which have dominated theories of child language – that parents somehow teach language and that children are predisposed to learn it – I would add a third. The one- and two-year-old child has been prepared, by his prior interaction with primary caretakers, both to learn linguistic rules and to elicit instructive material under optimal circumstances for learning.

2.3

These notions can be formulated as testable hypotheses; that is, the idea that the essential skills for learning language develop systematically in mother–infant dyadic behavior is, while not yet proved, a provable or potentially disprovable theory.

2.4

The crucial evidence needed to test such hypotheses will come from studies of *individual differences* in mother–infant interaction and in early language development. There is little more to be learned in this particular area from studies of one, three, or half a dozen children. Formal analogy has its place, but to explain development we must find functional continuity.

3 The evidence

The Columbus Project takes its name from the community hospital in Chicago at which our fifty subjects delivered. We followed these subjects through five observations of mother–infant interaction (one in the hospital and four at home), ending when the infants were six months old; a series of return visits will begin at twenty-two months. Four standard, structured interaction situations form the core of the project, our principal method being microanalysis of sequences and temporal patterns. We are fortunate in that each of these situations is either identical or very similar to those studied by a growing number of other investigators around the world (see for examples Schaffer 1977a).

It is by now a commonplace to say that the avenues of interaction between mothers and infants are two-way streets: each partner adapts to the other over time. However, an important qualification of that theme emerges from the studies I will discuss. The kinds of effect infants have upon their mothers are very different from the kinds of effect which go the other way. The roles of the two partners in the dyad are different.

3.1 Feeding

The first of our observed situations is natural feeding. Kaye & Brazelton (1971) found that while mothers fit their 'jiggling' mainly into pauses between their infants' bursts of sucking and believe that this speeds up the onset of the next burst, in fact the jiggling (of the bottle or baby) tends to have the opposite effect, delaying the next burst. With our present sample we have found that the *cessation* of jiggling elicits a greater than chance probability of onset of sucking within about two seconds (Kaye 1977). This led us to predict that [redacted]

The dialogue-like turn-taking which we and others have seen in very early feeding sessions is a matter of the mother's fitting her behavior into the infant's natural rhythms. I have argued (Kaye 1977) that neurological immaturity is the infant's great asset, providing him with biological regularity through which his mother can quickly learn to anticipate his behavior. If it is true that the burst–pause pattern evolved because of its utility in establishing social exchanges between mother and infant, this still does not mean it has the quality of a social exchange from the infant's [illegible]

an infant gets from his mother (many runs are both temporal and content). Stern argues that this sort of variations-on-a-theme stimulation is optimal for holding the infant's attention. The work of Tronick et al. (1975) illustrates vividly what happens when mothers (with great difficulty) refrain from their normal modes of interaction, and approach their infants with a still face. Infants as young as ten weeks briefly smile and run through their repertoire of facial expressions, limb movements etc., attempting to initiate normal interaction. This failing, they avert their gazes entirely.

The opportunity to view videotapes of some fifty different pairs in face-to-face play impresses one with the consistency of individual mothers' behavior over a twenty-week period. However, the individual consistency appears less in specific responses such as facial exaggeration, tickling, playing with the baby's limbs etc. than in general strategies for interpretation of the task. How a mother conceptualizes the situation we have created for her – what agenda she seems to have for her infant – may vary among mothers, between mothers and fathers, and between cultural/racial groups. J. Callaghan and A. Fogel, under the direction of D.G. Freedman, videotaped twenty Hopi and sixteen Navajo mothers in the face-to-face situation. Freedman and I, together with our students, have been comparing these with the Columbus Project mothers by a variety of micro and macro techniques. One thing we can all agree upon is that our impressions of the three samples vary greatly with the methods by which we examine them. A global impression of a whole session can elude microanalysis, and on the other hand trends in the trees can fail to be validated by a look at the forest. One reason this may be the case is that one needs functional categories in place of or in addition to response categories. Coding 'attempts to provide attractive stimulus' or 'attempts to orient infant's body and head', for example, may prove more meaningful (and even more reliable) than coding the mother's hand movements objectively.

This aspect of our project, even within our Chicago sample, is still at the stage of coding and exploratory analysis. My own current notion is, however, that the two-way communication we see developing over the sessions at six, thirteen and twenty-six weeks involves considerably more subtlety on the mother's part than on the baby's. Let us consider the infant first. He periodically becomes more aroused and less aroused, through self-regulatory processes as well as in response to maternal stimulation (Thomas & Martin 1976). The infant

also initiates activity: at six weeks this takes the form primarily of indexing his internal states, but by three months his role of social games takes the form of true signals, and by six months there is much independent exploration.

The mother adjusts her behavior to these developmental changes, but she also makes constant micro-adjustments to changes in her infant *within* each session. She seems to be processing information continuously in three different dimensions.

The first dimension is that of timing. There are moments when one can be sure of a maternal response, even though one cannot predict what it will be. The mother seems to be looking for slots into which she can insert her behavior – and this of course reminds us of jiggling and the burst–pause pattern in sucking. As Fogel (1976) points out, however, the mother's intention is not just to fit into the pattern but to alter it, to prolong the 'on' phase of her infant's attention and activity cycles and to generate in him a response to her. Since she has to act in the relatively short space of time when she can capture and maintain his attention, it is not surprising that in all these studies the best predictor of *when* a mother will respond is the moment when her infant's gaze shifts back to her (Brazelton et al. 1974; Stern 1974b; Fogel 1977).

A second dimension is the infant's arousal. Maternal responses can be classified along a continuum from 'turning on' to 'turning off' the infant. Mothers serve as buffers to keep their infants at moderate levels of arousal, neither too high nor too low. They do this partly for their own convenience and pleasure; but they also do it, I believe, for the same reason that an animal trainer maintains his animals at a moderate level of hunger. Performance and learning depend upon the infant's state, and mothers devote a great deal of energy and vigilance to the maintenance of an optimal state. Whether this is instinctive or a conscious purpose need not concern us here: I present it as a descriptive

so. When he stops performing his part in this 'dialogue' she may continue hers for a while, like a medic attempting heart resuscitation. It is mainly when the infant offers nothing for a long time (5—10 seconds) that she goes back to her bag of tricks. Sometimes she will try to initiate a game whose previous history is unknown to us. (Fogel is currently gathering data on the introduction and history of social

with respect to one or more dimensions. It may even be that the mother who is usually out of phase with her infant's own cycles and who fails to leave space for his responses is the same one who tries to achieve an inappropriately high level of excitement and the same one who pushes her own agenda while missing his initiatives.

If this sort of sensitivity versus insensitivity is indeed a consistent discriminator among mothers, does it have any consequences? After all, all of the infants will eventually learn to function in society and to speak English. In our sample the grammatical features of the language acquired by all of the children will be virtually indistinguishable. Still, some will acquire it faster than others. Furthermore the way language is *used* will vary across different dyads, and this in turn may have consequences for socialization and for the kinds of knowledge about the world which the children acquire in their preschool years.

An important observation is that each mother and infant develop a set of games of their own. If these conventional games are the foundation of later communication and language-learning skills, how is it that there are not certain games they all have to learn? Even if we say the games all share certain structural features (and they do), so that it does not matter precisely which ones a mother and infant play, we have still to account for the assiduousness with which each pair practices and perfects its own games, and introduces variations upon familiar themes. Instead of merely sampling the pool of potential games, a dyad goes into depth with particular ones. There is no particular response to a particular stimulus which has to be learned by all members of the species. Instead, what seems to be important is the *process* of conventionalization, the mutual topic-comment, the modularization (Bruner 1973) of dyadic routines of *some* kind, the learning to anticipate when and how a partner's behavior will change.

3.3 Detour reaching

When the infants in our project were six months old we observed them in our third structured situation, a teaching task. We noticed (Kaye 1970, 1976) that when mothers were asked to help their infants retrieve a toy from behind a plexiglass barrier their behavior could be reduced to three basic strategies. Of interest in addition to a mother's predominant instructional strategy was the question of her timing. We have found that the determinant of *when* she responds is the infant's gaze – just as in the face-to-face play studies, except that

here it is gaze aversion *from* the task which elicits maternal intervention. As in the face-to-face studies, *what* a mother does at these junctures can vary enormously. We did find one factor affecting the choice of strategy: the infant's behavior on a pretest. 'Showing' or demonstration of the reaching path seemed to be the most common strategy tried to some extent by nearly all mothers. 'Shoving' or manipulation of the baby's arm and hand around the edge of the barrier was a typical strategy when the infant had been active and frustrated on the pretest, while simplification of the task for a gradual 'shaping' strategy was tried when infants had shown no signs of being close to solution on the pretest, and relatively little interest in the task. (All these results were previously found in a sample from Cambridge, Massachusetts on whom we had no longitudinal data.)

In short, mothers use task analysis combined with baby analysis. The detour reaching situation – and, we believe, instructional interaction in general – is responded to by the mother as if her task were to adapt the infant's agenda to her own. She tries to meet him where he is; she accommodates quickly to his behavior changes, but always her behavior has a direction with respect to his. And success is a question of balance between the two agendas.

I have tried to show that this characteristic of instruction begins in feeding as well as in play. We believe that, as in the case of the jiggling behavior described above, mothers learn to be better and better at the balancing act, though it also matters what kind of infant they have and what their experience is together. These hypotheses require analysis of individual differences in a longitudinal study with a fairly sizeable sample. A great deal could be learned, in addition, if one could cross mothers and infants temporarily as one would do routinely with laboratory animals.

In looking for commonalities across studies of mother–infant interaction, one can focus on response modalities or on functional characteristics (just as one can when looking for consistency in individual dyads). It is interesting that it should be the infant's gaze shifting that determines so much of the timing of the mother's behavior. This too may have origins in early feeding: too late in our work mothers' comments began to suggest that their infants' eye openness might explain why the mothers chose to jiggle in some pauses and not in others. Robson (1967) pointed out the dramatic changes produced in mothers' perceptions of their infants at about one month, when eye contact first becomes established. Collis & Schaffer (1975) have found that

when infants are in seats parallel to their mothers, their direction of gaze is a strong predictor of where the mothers will look and even where they will point. This last finding is an important one. It is not the case that infants look where their mothers tell them to look; yet mothers behave as if it were the case. They fit their own behavior into the infant's so that the infant's subsequent behavior will seem to be a contingent response. This is true in feeding, in face-to-face play and in instruction. Gradually, the infant does come to fit his behavior into his mother's dialogue. We still have very little understanding of how mothers' false beliefs about their infants' behavior come true.

Note that behavior of a particular organ, e.g. the infant's eye and head movements, can serve to develop far more general types of behavior, signaling and responding, which are not limited to any modality. This is exactly how we should expect evolution to have worked. The adaptive consequences may be structural, system properties of the species; but the means of transmission must begin with physical material, with the concrete behavior of sensory and motor organs.

The mother, of course, responds in an unlocalized fashion from the first. She may jiggle the infant, breast or bottle, stroke his cheek, tweak his foot or call his name. The infant only gradually comes to be free of his own fixed responses. He develops from a stage in which his eye movements are merely interpreted as an index (our evidence is that contrary to mothers' impressions he sucks better when they are closed); to a stage when they really are an index and the mother's behavior with respect to them matters (early face-to-face play); to a stage when they are interpreted as a signal (later face-to-face play); to a stage when they really are a signal.

3.4 Imitation

This brings us to imitation, the fourth task in which we have observed all of our subjects. This observation, too, is made at six months, but I am the mother. We developed a procedure for eliciting imitation based on our own idealizations of face-to-face play and of the 'showing' strategy in maternal instruction (Kaye 1971). I alternate my turn with the infant's turn, letting him control my behavior in the following manner. When he looks me in the eye, I make a series of five 'goldfish' movements with my lips; every time his eyes return to mine, I repeat the 'burst' exactly. This proceeds for as many trials (typically 20–40) as he initiates, without any contingent response to his imi-

tative attempts. The first remarkable result of this method is that virtually all infants pick up the rule of the game, the fact that they can elicit my mouthing at will, within two trials. This knowledge is indicated by their sequence of acts: making their own imitative attempts, then becoming still, glancing at my eyes quickly as though flicking a switch, then looking immediately to my lips in *anticipation* of my response. The second remarkable result is that roughly two-thirds of the six-month-old infants achieve at least one *burst* of mouth movements (median trial number: 10), and one-third give bursts on three or more trials (Kaye & Marcus 1978). This is particularly surprising in view of the fact that the infant cannot see his mouth, and thus gets no feedback as to the correctness of his imitation.

We began analyzing each infant's sequence of imitative movements over trials in the hope of finding a systematic order through which all would move. For example, one immediately sees behavior of the following kinds, all of which can be shown to occur at a higher rate during trials than in the preceding two-minute baseline period: mouth movements, both opening and closing, from the baby's resting position; open–close movements of the hands; tongue movements; and rhythmic bursts of limb movement. We thought there might be some developmental order in these approximations of the model, an 'order of acquisition' (Brown & Hanlon 1970) within a single session. We found evidence for system within each infant's protocol – for example, they often put *mouth* movements together with limb *bursts* on the same trial before moving ahead to *mouth bursts* – but there are many paths to success. If one allows the infant to control the model's timing and his own arousal, his hypothesis-testing and skill-polishing resources by this age are already highly potent.

Our explanation for the infant's ability to imitate mouth movements without feedback from me is that he has already engaged in mouthing games with his mother and father, who have imitated his behavior in much the same way as he now imitates mine. This is an hypothesis which we can test using our longitudinal sample. At one level we must look for predictors of imitative success in the general smoothness and richness of face-to-face play at earlier sessions. At another level, however, in this case one must also predict specifically from mouthing games in the earlier sessions. Thus our ability to test this particular hypothesis depends upon the adequacy with which three videotape sessions sample the games in which mothers and infants engage daily.

4 Discussion

The unanswered question in the imitation study is precisely the same as in the other studies described here: how does the infant come to be able to make the kinds of contingent response to adults, which adults have been making to him over the preceding months? What am I to conclude when an eighteen-month-old, trying to put a raisin in my closed mouth, opens her own mouth exactly as mothers do when feeding their babies? Or when twenty-four-month-olds in a dissertation by Poppei (1976) use showing, shoving and shaping to teach eighteen-month-olds, with whom they have interacted for only 15 minutes in an unfamiliar playroom, how to operate a cookie dispenser?

The infant begins life with the capacity to elicit certain instructive kinds of behavior from adults. Somehow he gradually takes upon himself some of the aspects of the adults' role in interaction: imitation, adjustment of timing etc. This in turn gives him even finer control over adults' behavior, so that he gains further information and more and more models of motor skills, of communication, eventually of language. By the time his representational and phonemic systems are ready to begin learning language, he is already able to make his intentions understood most of the time, to orient himself in order to read and interpret others' responses, to elicit repetitions and variations. We are back to Brown's (1968) insights.

What we can add, on the basis of the recent studies of mother–infant interaction, is that there is a fundamental asymmetry of mother and child roles. In this paper I have discussed five asymmetries: (1) the mother's superior flexibility with respect to her own timing and anticipation of the infant's fairly regular cycling; (2) her agendas for the infant, always pulling him forward developmentally along a kind of intuitive curriculum; (3) her ability to monitor and code his changes of expression rapidly, switching gears and seizing opportunities for imitation; (4) her flexibility in substituting alternative means towards ends, as compared with the infant's relative fixedness in certain response modalities; and (5) her creativity in introducing variations in her own repetitions and in her part of the dyadic game. The last may be the most important, encompassing the other four. I have suggested elsewhere (Kaye 1977) that mothers violate conventions which are working well, just to test their own hypotheses that the apparent contingencies are real ones. The effect is to introduce disequilibrium for both partners, providing the raw materials for further negotiation

of conventions. Thus the process of differentiating dyadic schemata is at least as important as the schemata themselves.

Our imitation paradigm is a kind of tissue culture for testing the infant's capacities at a given age with the 'mother' held constant. It is far too simple to be a model of how real mothers ever behave. Some time ago L. Minnerly and I conducted an experiment in which mothers were asked to read illustrated sentences to their two-and-a-half-year-olds, alternating their own reading with pauses in which the children could respond. We are still analyzing transcripts of these sessions, which were taped daily over the course of a week. Pertinent to the studies discussed above is the fact that every mother adapted our instructions to her own style. Timing, motivation and balance of agendas seem to characterize different mothers' responses to the task, and what can be considered 'optimal' at any time in each of these dimensions may be directly related to what the child has already achieved in his language development.

We are therefore including the sentence-imitation procedure along with spontaneous speech samples in our Columbus Project visits in the third year. Will the mothers who had difficulty with timing, arousal and the balancing of agendas in the first half-year of life still have difficulties of the same kind? Will there be a chance for dyads to succeed in their language interactions, even if they were relatively unsuccessful in infancy? Or will patterns already be established, with some dyads firmly on an upward spiral and others doomed downward? Perhaps most important, will the child's own abilities to imitate, to elicit imitations, expansions, and extensions, to make himself understood and to generalize linguistic rules depend in any way upon the nature of his prior communicative experience?

The reader must regard this paper as an early observation, and engage in longitudinal analysis of our theoretical development. This will require patience over a period of years, for our conceptualizations of the processes of communication between infants and adults necessarily develop more slowly than the processes themselves. It is half a century since G.H. Mead wrote:

> Gestures, if carried back to the matrix from which they spring, are always found to inhere in or involve a larger social act of which they are phases. In dealing with communication we have first to recognize its earliest origins in the unconscious conversation of gestures. Conscious communication — conscious conversation of gestures — arises when gestures become signs, that is, when they come to carry for the individuals making them and the individuals responding to them, definite

meanings or significations in terms of the subsequent behavior of the individuals making them; so that, by serving as prior indications, to the individuals responding to them, of the subsequent behavior of the individuals making them, they make possible the mutual adjustment of the various individual components of the social act to one another, and also, by calling forth in the individuals making them the same responses implicitly that they call forth explicitly in the individuals to whom they are made, they render possible the rise of self-consciousness in connection with this mutual adjustment. (G.H. Mead 1934: 69)

Note

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