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10 The Moral Philosophy of Microanalysis

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The microanalysis of videotape, film, or digital event recordings of naturally occurring behavior can be more than a method of research. It can appear to be a political act—that is, taking sides in a controversy of broad ideological proportions.

Microanalysis involves (1) the exploration of relationships among events *within* sessions, as opposed to counting how many times predefined categories occur and comparing those counts across sessions or across experimental conditions; and (2) taking the passage of time into account, either in terms of sequential units or in continuous real time. Although some of us are by temperament more inclined to this sort of work than are others, no one would claim that it can stand alone or that it obviates the necessity for "macro" kinds of case studies on the one hand, or for experimental design on the other.

Before we had videotape and computers, when coding, tabulating, and counting were costly and time consuming, investigators were under more pressure to plan their analyses in advance and to limit themselves to testing prior hypotheses. Our work today is more exciting and more revealing. However, the value of microanalysis is limited by the lack of a clear set of principles governing its use. We must begin to do something no one has yet done: to devise a set of procedures for using these tools without deceiving ourselves or our readers about the amount of fishing—casting, reeling in, casting again—that we have done.

The topic of this book is a field of study in which the need for methodological principles is particularly acute. Of all the areas of psychological research, none requires more subjectivity than the study of affect; and in the lifespan there is no period that raises more complex issues about the subjectivity of the observer than does infancy (Kaye, 1982).

To a certain extent, the problem is a basic paradox in the nature of coding. Science is simplification; the reduction of chaos to order; recasting the unknown in terms of the known. The process of coding, our particular type of simplification, is *not-seeing*. The challenging part of learning to be a good coder is learning to *not-see* most of what is happening and to see only certain categories of things. One has to ignore the differences within categories, trusting one's intuition that the gross category is important. We do this first between the videotape and the coding sheets or the keyboard; then we do it again between the computer disk and the printout, and again between the printout and the published article.

In fact, by positioning the camera and zooming in on only a portion of the scene, we commit ourselves to not-seeing most of what is in the room. This process is merely continued as we define each coding category. "Smiling" commits us to not-seeing a dozen variations in smiles that our eyes and minds are perfectly capable of seeing, indeed beg to see; but we learn to shut them out. We commit ourselves to a guess about a class of smiles we think might be equivalent, at one level, ignoring the ways in which every smile is different. (This, in fact, is exactly what the parent has to do in coding the infant's expressions and what the infant has to do in coding the world.)

The paradoxical aspect of all this selectivity is that its goal is to discover relationships we did not know about before. We want to see as much as possible: If we have to narrow our vision, we want to be able to open it up again, refocus, and try narrowing it in a different way. One of the things I hope to do in this chapter is to share two techniques that I have found to be helpful in this process. However, I want to do that in the context of a first attempt at some principles to guide the use of all such techniques in microanalytic research.

SIX PRINCIPLES FOR MICROANALYSIS

The first principle is that *methodology is only the handmaiden of hypothesis.* The servant must not be allowed to become master. There is no best way to do research in general. On the contrary, when hypotheses are stated more and more specifically, they lead inevitably to the design of procedures for their own confirmation or disconfirmation.

The second principle is that we must try to *hypothesize a process.* It is not enough for the microanalyst to have an idea that certain events are somewhat "related" or even that one is contingent upon another. We need to hypothesize a mechanism, or several alternative mechanisms, by which the occurrence of one type of event might affect the occurrence of the other.

In other words, the exploratory goals of microanalysis do not permit us to by-pass the necessary theoretical work, a priori. Just as an oil exploration crew knows what it is looking for and has reasons for sinking test wells in particular areas, the explorer for behavioral contingencies had better have reasons, not just intuitions.

As I prepared to list the different kinds of relations one might hypothesize between two categories of events A and B, I realized with horror that I could list as many as 1152 different hypotheses, based upon the $4 \times 4 \times 3 \times 4 \times 3 \times 2$ classification scheme shown in Table 10.1. Furthermore, that scheme is far from complete. It merely indicates the most basic alternative kinds of things we might mean when we say of some domain of interaction, "Behavior A has an effect upon behavior B." Even without embellishing the classification, the number of possibilities is much larger than 1152 because polynomial combinations of the different kinds of effects are possible for any A and B. I refer to some of the distinctions in Table 10.1 when illustrating, later, some microanalyses from our own work.

TABLE 10.1
Classification of Microanalytic Hypotheses

Effect of:	1. Discrete *occurrence* of event A.
	2. Continual, ongoing *state* of A ("behavioral context")
	3. *Parameter* of the A series, as a *continuous* variable
	4. *Parameter* of the A series, as a *threshold*
Effect on:	1. Discrete *occurrence* of event B
	2. *Parameter* of B (e.g., rate, stochastic probability of occurrence as a function of time since A, etc.)
	3. *Structure* of series B (periodicity, clustering, etc.)
	4. *Resetting* clock on which B depends
Mechanism:	1. *Physical* (e.g., A is a push; B is a fall)
	2. *Neurological* (e.g., orientation to a sound)
	3. *Conventional* (a learned signal-response contingency)
Closure:	1. *Direct* (A is the actual cause of B)
	2. *Mediated* (A affects Q, which affects B)
	3. *Correlated* (A and B are both affected by Q)
	4. *Subset* (A is only a subset of the class of events that affect B, or A only affects a subset of B)
Rule:	1. *Obligatory response*, optional signal
	2. *Obligatory signal*, optional response
	3. Obligatory *response and* obligatory *signal*
Roles:	1. *Symmetrical* (person 1's A affects person 2's B as 2's A affects 1's B)
	2. *Asymmetrical* (different effects of person 1 upon person 2 than of 2 upon 1)

The third principle is a heuristic: *Begin with departures from randomness.* It is a good idea to think about behavior B as a series of events in time, and to begin by asking whether they occur randomly—that is, as a Poisson process—and if not, how they differ from a random series of occurrences. A Poisson process means that the probability of a *B* occurring at any point in time is independent of when the last *B* occurred. The discrete events *B* are, in short, independent of each other, which means that they must also be independent of any nonrandom events. A Poisson process has zero autocorrelation (see Gottman, Chapter 12, this volume); the frequency distribution of intervals between *B*'s is exponential, positively skewed; and the log-survivorship function (plotting on the *Y* axis the number of intervals longer than each duration plotted on the *X* axis) is a straight line with negative slope. The latter two results are mathematically equivalent, and are always true when the autocorrelation is zero at all time lags (though the converse need not be the case).

If the *B* series is random, then it is either unrelated to any behavior *A*, or it is closely related (a direct consequence) while *A* itself is a Poisson process.

If the *B* series deviates from a Poisson process, the first question is how; the second question is whether *A* is one of the things responsible for that departure from randomness. These questions take us back to the stage of hypothesizing a process (principle 2) and devising a method appropriate to that hypothesis (principle 1). Chapter 12 of this volume deals with one way of approaching the question, how much of *B*'s nonrandom structure is accounted for by the structure of *A*? In a time series, the observation at each point is a continuous variable, such as the daily temperature or the price of wheat. A point process can be translated into a continuous variable rate, either by averaging over a moving window large enough to include several events, or by taking the reciprocal of each interval from one occurrence to the next. Time-series analysis is a powerful way of allocating the relative roles of intrinsic structure versus interactive effects, but by translating the analysis into a regression problem it can take us away from the specific alternative forms the interactive effects may take.

Fig. 10.1 illustrates the test of an hypothesis directly involving the issue of randomness versus clustering. *B*, in this case, is a heterogeneous category of infants' facial expressive behaviors while being held face to face in their mothers' laps. Such expressions had been found to cluster into "runs" or "turns," and we wondered under what conditions they did so. If they were indeed intentionally communicative, the clusters of expressions should occur when the babies were attending to their mothers' faces. If they were a matter of conversational turn taking, the infants' facial expressions should cluster together when the mothers were relatively quiet, during the pauses in their own expressive behavior to the infants. The first of these hypotheses was only slightly confirmed for the 6-week-old infants, more clearly confirmed for the 13-week and 26-week infants: The log-survivorship function changed from a straight line to a sagging one, with a break at about the

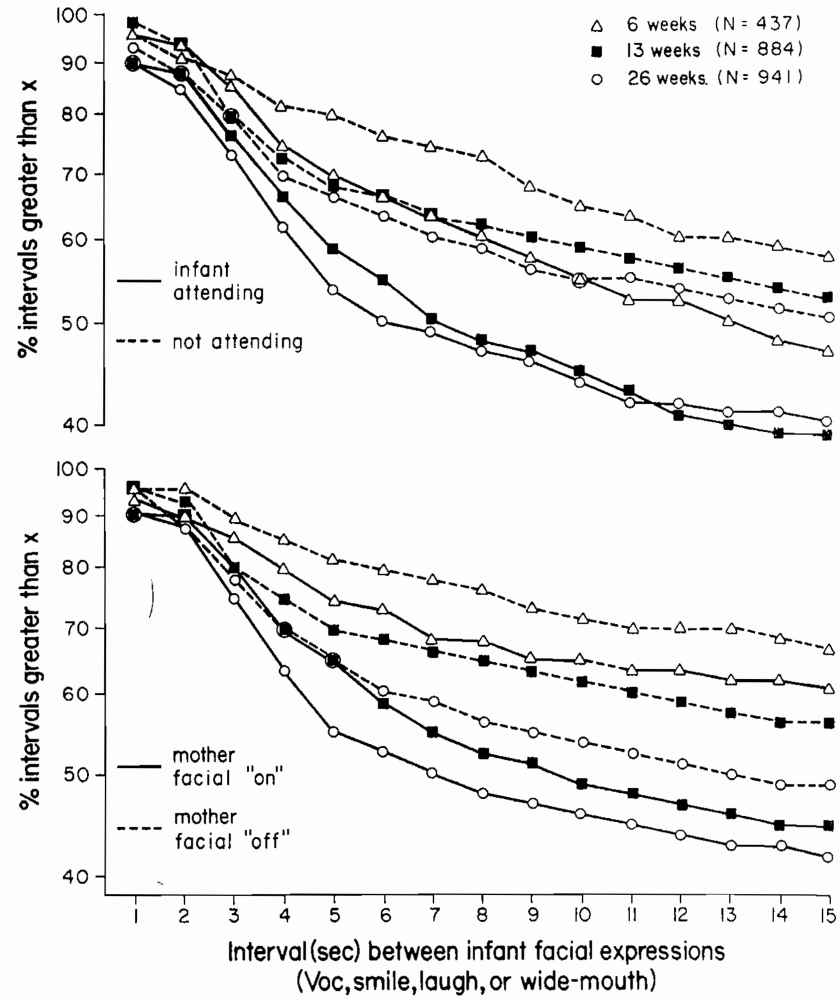

FIG. 10.1 Log-survivorship functions showing the organization of infants' facial expressions in face-to-face sessions with their mothers, deviating from a Poisson process. *N* of mothers and babies = 47. (Data from Kaye & Fogel, 1980.)

point of the 5-second intervals. This means that there were more expressions within 5 seconds of each other than would have been found by chance, given the average rate with which such expressions occurred. The difference between the two conditions, attending to the mother versus not attending, was significant at 13 and 26 weeks. (In this case, to capture the interaction of the three independent variables, we used a repeated-measures analysis of variance, with the proportion of intervals less than 5 seconds as the dependent variable.)

The second hypothesis was disconfirmed. In fact, the opposite was the case: Infant facial expressions clustered together while mothers were making faces at them, not while mothers were pausing. Again, the effect increased with age.

Now let us proceed to my fourth principle: *Use the right baseline.* Microanalysis is an attempt to fabricate an experimental design within observational data that one was unable to subject to an actual controlled experiment. An experimental manipulation is always preferable; it is just not always possible without distorting the phenomena in which one is interested. A smile presented upon the investigator's cue, for example, may not have the same effect upon a child's or parent's behavior as smiles in the real world have. Our job, therefore, is to organize the data as if the events A constituted trials in an experiment.

The baseline problem has two parts: One is the decision as to which two (or more) circumstances to compare. In the foregoing example, the hypotheses involved the effects of various states (e.g., attending to the mother) upon the structure of a series (the organization of facial expressions in time). The next example involves the effect of discrete events, the onset and offset of mothers' jiggling their babies during feeding, upon the likelihood of occurrence of another discrete event, the babies' resumption of sucking. The "experimental" condition is therefore the moments immediately following jiggling, and the baseline or "control" condition consists of moments when the babies have paused from sucking but when the mothers have not jiggled.

This raises the second part of the baseline problem: The two conditions have to be comparable in terms of some starting point in time. In this case, the question is whether the jiggling makes the baby start sucking sooner. Sooner from when? Because the question has to do with effects on the duration of the pause in sucking, all conditions have to be compared as a function of time since the beginning of the pause.

The resulting *contingency function,* a conditional probability as a function of time, is shown in Fig. 10.2. Functions of this kind have been used for some time in other fields, for example in comparing the effects of chemotherapy upon life expectancy as a function of time since the onset of cancer. Fig. 10.2 shows that jiggling itself suppresses the likelihood of a new burst of sucking, but that the cessation of jiggling accelerated the burst, even above what its likelihood would have been had the mother not jiggled at all.

An explanation of exactly what is plotted in Fig. 10.2 will allow the reader to translate this technique into his or her own research questions involving conditional probabilities. Notice that transitional or Markov probabilities are of no interest here: We already know that the next thing the baby is going to do after pausing is to start sucking again. The question is whether the mother's jiggling has any effect on how long it will be before the sucking

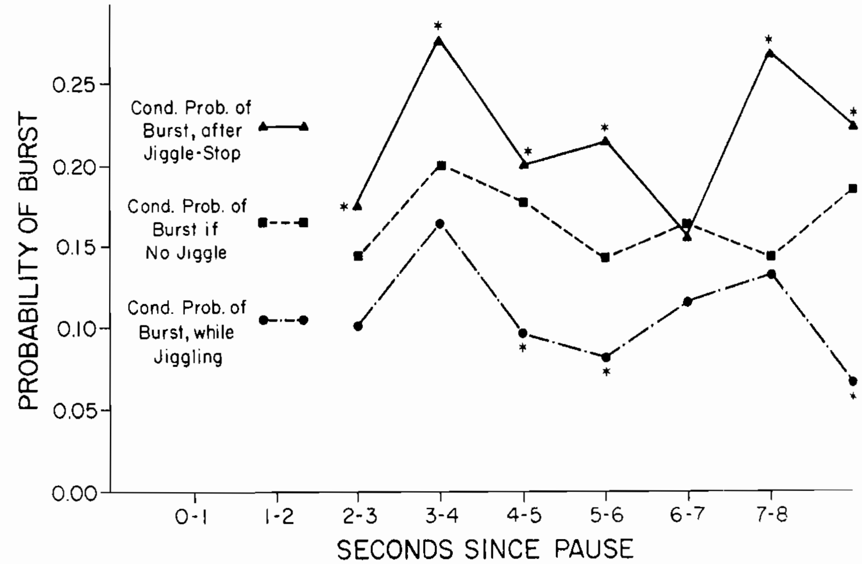

FIG. 10.2 Contingency functions showing conditional probability of a burst of sucking (end of the pause), as a function of time since onset of the pause, depending on mothers' jiggling. N of mothers and babies = 52, N of pauses = 1814. (From Kaye & Wells, 1980.)

resumes. This effect is expressed as the conditional probability of a burst, within any 1-second interval. (The analysis begins in the third second of the pause because intersuck intervals of less than 2 seconds were not defined as pauses). Of all those pauses that have lasted x seconds, what proportion will end in the next 1-second interval, and is this likelihood affected by whether the mother has jiggled? After $x + 1$ seconds, a smaller number of pauses remain for consideration, because some have already ended. For any 1-second interval, Fig. 10.2 shows the pauses divided into three conditions: those in which the mother had not yet jiggled and the infant had not yet resumed sucking; those in which she was jiggling; and those in which she had previously started and stopped jiggling. Each point represents bursts per opportunities to burst at that point in time.

In practice, one chooses the interval to be plotted along the X axis, then has the computer find all the latencies from the starting event to the conditional (A) and/or the terminal event (B), sort them into piles determined by the interval to be used, and compute (by arithmetic on the contents of those piles) the proportions for each point. These proportions can then be compared, at any point on the X axis, by a simple chi-square test. The asterisks in Fig. 10.2 show where the conditional probability of a burst under one of the two jiggling conditions was significantly different from the baseline no-jiggling condition. Because the conditions are mutually exclusive, the assumptions of the chi-square are met even though the cases do not come from independent subjects, so long as nothing has biased the sorting of cases into conditions along the way. For example, if mothers had jiggled their babies just because they could tell the pause was not going to end, the statistical comparison in Fig. 10.2 would be meaningless. In that particular study (Kaye & Wells, 1980), we had to do a controlled experiment in which we fed the babies ourselves and jiggled according to a predetermined schedule, in order to show that it was really an effect of jiggling and stopping upon the onset of the next burst, rather than the other way around. This brings up an important point: The fact that time passes from left to right in these graphs does not necessarily exclude the possibility that the direction of effects is from right to left—that is, from the terminal event B to the condition A. This is because the subjects can anticipate their own as well as each others' behavior. When we controlled the onset and offset of jiggling ourselves (Fig. 10.3), we could exclude that possibility.

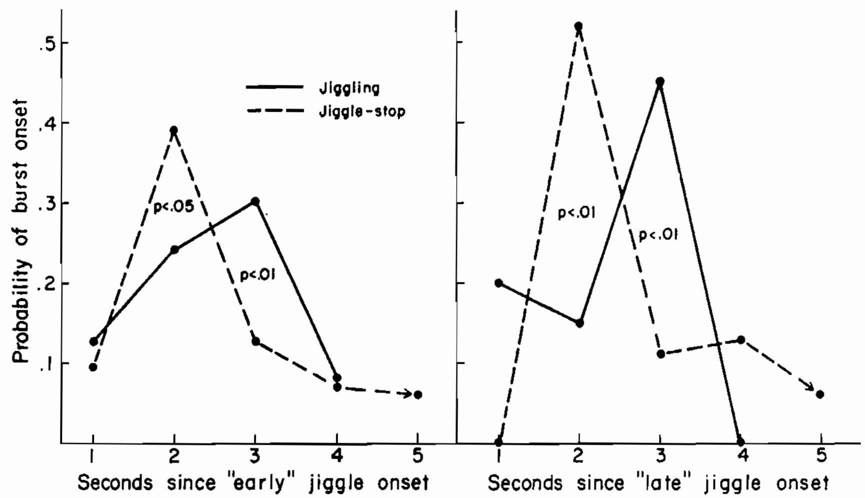

FIG. 10.3 Conditional probability of a burst of sucking, depending on the experimenter's jiggling: (1) 2 seconds after the onset of the pause; (2) 4 seconds after the onset of the pause. N of babies = 12, N of pauses = 899. (From Kaye & Wells, 1980.)

The choice of intervals on the X axis is important. They have to be: (1) a simple unit, like seconds or weeks; (2) large enough so that at least 10 cases (e.g., "opportunities to burst") occur in each interval under each condition (otherwise one will not get anything like smooth curves); (3) larger than one's confidence interval, based on reliability studies, for how close together in time two events can be and still allow one to be sure that they occurred in the order in which they appear (Kaye, 1980); and (4) otherwise as small as possible.

A fine point: One might decide to exclude all cases from the analysis whose latencies from A to B were less than the reliable confidence interval. Similarly, with time-sampled data, it might be wise to exclude cases in which both events occurred during the same interval. If so, then the data should only be excluded from that time onward; everything that happened (or did not happen) up to the moment the given condition became true should still be retained in the baseline data, to prevent bias.

Fig. 10.4 shows a variation of this technique, a contingency function both forward and backward from the onset of a pause, showing how it doubles the likelihood of a mother's jiggling.

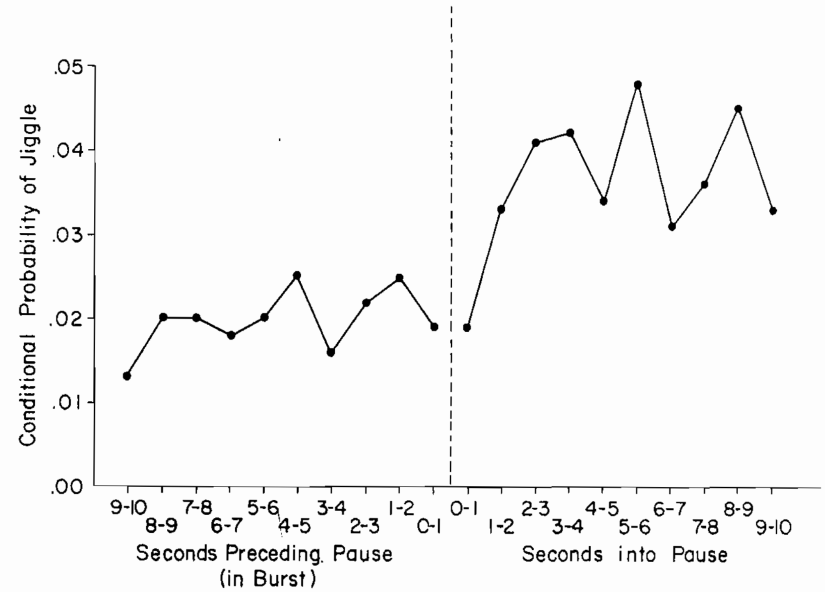

FIG. 10.4 Contingency function forward and backward from a target event. N = 52 mothers and babies, 1814 pauses. (Data from Kaye & Wells, 1980.)

My fifth principle is one that I need not belabor, because it is commonly known even though it is commonly violated: *Exploratory findings must be confirmed in a second sample.* It is reasonable to reject all manuscripts submitted for publication, and all dissertations submitted for doctoral degrees, whose results consist of nothing more than behavioral relationships found through exploratory microanalysis without confirmatory analysis. The fact that B is "significantly" contingent upon A does not test how fortuitous might be the finding of that particular contingency, among all the hypotheses explored, among all the different kinds of A and B analyzed, in that particular sample. However, if it is fortuitous it will not emerge again in a new corpus of data.

The obvious exception to that last statement brings me to my sixth and last principle: *Always suspect artifacts.* The time to dwell on possible spurious relationships in the data is not after the analysis has been done, as an attempt to account for only those results that are unfriendly to our theory. The time is before designing the study, especially before designing the procedures of observation and coding.

Perhaps the most difficult kinds of artifact to avoid are those having to do with the reïfication of categories. We tend to assume that those behavioral units we can identify and code independently are independent acts in the subjects' repertoires of behavior. If, in fact, they are neurophysiologically linked to other acts, and if (as must always be true to some extent) the boundaries around our categories fail to correspond to the actual units of behavior, we can easily find significant contingencies between our categories that reflect nothing except our poor understanding of where the psychological boundaries actually are between events. Or the confusion can have the opposite result: failing to find contingencies because of our inadequate definition of categories.

More avoidable types of artifact include three different ways of failing to code events independently:

1. Both series of events, A and B, are coded by the same coder at the same time. When an A and a B occur in close proximity, the coder misses one or the other, producing a spurious exclusivity. Alternatively, by only a few unconscious coding errors, the coder introduces an hypothesized regularity into the data.
2. Both series are coded by the same coder, but at different times. Nonetheless the coder biases the data because he or she cannot help watching both kinds of events even while coding only one.
3. Two different coders are employed for A and B, and are kept as unaware as possible of one another's work. But the coder of the baby's facial expression keeps the audio on and is unconsciously biased by the mother's vocalizations, and the coder of the latter keeps the video visible and is biased by the baby's face.

My students and I have regrettably discovered many other means of introducing artifacts into microanalysis, and it is likely that the reader will independently rediscover them. Although it is obviously preferable to think about the possibility of artifacts in advance, the specific results of each study are an occasion for checking back to rule out artifacts before publication.

INTERPRETATION VERSUS EXPLANATION

Not all of my colleagues feel comfortable about the attitude implied by the foregoing principles. Some, I think, hearing Goethe's (1808) lines:

> Mysterious in the light of day,
> Nature will not be denied her veil,
> And what she does not make manifest to your spirit
> Cannot be forced from her with levers and screws [p. 188].

assume the converse, that whatever Nature does make manifest to their spirit must be true and must be defended *against* levers and screws. Their main rhetorical weapon is the assertion that human minds and social relations transcend science. This is not the place to refute that controversial assertion, but I do want to devote the remaining discussion to at least raising the issue. It is of concern to all developmental psychologists, but microanalysis especially forces us to come to terms with it.

One of the themes of my own work has been that even though parents naturally overinterpret expressive-looking acts in young infants, psychology ought to guard against doing so. It is one thing for a mother to say to a father, "He says, 'Who are you?'," when the baby seems to be making a quizzical face. It is quite another thing for psychologists to talk that way when our purpose is to discover when and how the infant actually comes to use gestures with meaningful intent (Kaye, 1982). Yet some authors claim that human thought and communication are beyond the domain of what they consider to be hard-nosed science. Rather than imposing systematic procedures to test the validity of interpretations of events, they would treat the interpretations themselves as data, arguing that acts between human beings are always, by definition of their humanity, meaningful gestures. Harré (1974) wrote:

> I shall assume that all forms of social behaviour have the same level of sophistication, and indeed that human beings are unable to perform in a socially unsophisticated way [p. 16].

This troubles me because I should have thought it was an investigable question, not something to be assumed; and it seems to me that the evidence on young infants shows that it takes them some time, with adults playing an important role, before their social behavior becomes sophisticated in any sense of the word.

Elsewhere in the same article Harré castigates what he calls "outmoded positivistic ideas about what science is [p. 14]." Trevarthen (1977) takes up the same theme:

> I believe that a different kind of research, less analytical *at the start* is a necessary complement to experiment in scientific study of intelligence, especially for the early developmental stages when great impressionability of memory is controlled by

> innate forms of action. This alternative method attempts to capture regular patterns in spontaneous action and tailors experimental intervention to what is discovered . . . . The essential difference resides in an emphasis on generative or structural and functional complexity in the subject who thus becomes a free-acting agent [p. 229].

This "alternative method" sounds like just the kind of methods I have discussed previously, including lip service to confirmatory analysis. Yet we find in Trevarthen's studies only the conclusions from his observations, illustrated by anecdote and by selected photographs and drawings, without any attempt to test hypotheses.

I am not ready to be classed among those who reduce psychology to the "behavior of organisms" or those who allow similarities across species to blind them to the more important qualities that make humans unique. We do need theories of *human* development. The issue, as I see it, is whether those theories ought to be tested by traditional rules of evidence or whether the rules should be suspended when we turn our lenses upon our own species. On that issue I take a conservative view. Innovative, painstaking observation and analysis of parent–infant interaction is possible within the format of ordinary scientific investigation; we have neither the need nor the right to suspend the rules.

A great deal has been written in philosophy and in the theory of literary criticism about *interpretation*. This referred originally to the interpretation of texts, such as the Bible; thence it was applied to analysis and criticism of literature in general, thence to a theory of symbolic meaning. It is not a big leap from there to interpretations about the mental and social life of babies. But must we leave the realm of science altogether, in favor of "hermeneutics" (Heidegger, 1962), whenever we study our own species? Enmeshed in this question is the problem of what we mean by "study." Are we trying to make plausible sense out of individual cases, or are we trying to explain processes common to all? I argue that the latter is our goal. Interpretation, by itself, may be the goal of idiographic analysis but is never sufficient for explanation.

I see our present tension, then, as the same one that has existed in psychology since the 19th century (Allport, 1962) between idiographic and nomothetic research. The proper goal of one is to interpret a particular event in depth without reducing it to a mere instance of categories already known. The proper goal of the other is to generalize about a whole set of things, by adequately sampling them. The biggest difference between the two approaches is in the relationship between the data that must be presented and the prior beliefs of the intended audience. One can show that something exists by demonstrating one instance of it, so long as the reader agrees that it is an instance. That depends on his or her prior belief that what is asserted is possible. If I point to a clam opening and shutting, and say, "This clam is communicating with its fellows," your acceptance of the proposition depends on your belief in the more general proposition "clams can communicate." In no way could my one clam, or even thousands of clams doing the same thing, be taken as proof that what we are seeing is communication. However, the converse is true: A prior belief that this form of communication exists will usually function as sufficient basis for acceptance of the particular instance.

A nomothetic approach is required when the communication abilities of clams are precisely what the audience doubts. The prior beliefs that the audience must share with the researcher are beliefs about procedures for identifying communication (or whatever the behavior in question is) when it exists.

Within the work of one investigator or a group of like-minded investigators, these two modes of questioning can coexist quite peacefully. But between two authors whose theories are at odds (for example, myself and Trevarthen), different predilections towards idiographic versus nomothetic arguments tend to drive them further apart and make fruitful dialectics impossible. The nomothete regards the idiograph as an unscientific storyteller; the idiograph regards the nomothete as a pseudoscientific reductionist. One says, "Let us see your data." The other replies, "Data lie; look at my videotape." "But your videotape is not representative; I don't see the same things in my videotape." "Because you have blinded yourself."

A long-standing version of this debate has to do with psychoanalytic interpretation. An interpretation is offered about the meaning of a person's behavior, about the symbolic meaning of a dream, or about something the patient says during a session. The most important evaluation of the analyst's interpretation comes from the patient. If the case is written up, the interpretation will also be evaluated by a different audience: the readers. In both cases, the validity of the interpretation is measured only by its success in eliciting recognition from the patient or colleague. So it is *doubly* idiographic: It does not have to be true *of* people in general, and it does not have to be true *to* people in general. Still its acceptance is not an arbitrary matter. Psychoanalytic interpretations only appear plausible to those of us who are already prepared to believe that all aspects of human behavior, including unconscious acts, are intentional and significant. The psychoanalytic literature, which is almost entirely in the form of case studies, does not convince anyone who is skeptical about the basic principles of psychoanalytic psychology.

The same is true of illustrative cases as evidence about the development of affective expressions in infants. Consider the following interpretation of a 7-month-old child who had been left with his grandparents for 2 weeks (Mahler, Pine, & Bergman, 1975):

> When his mother returned, he at first had a rather severe crisis of reunion, crying unconsolably for quite a while and not allowing her to either feed him or put him to sleep. However, by the next day he was his old smiling and tranquil self. This reaction to brief separation, which is peculiarly specific to mother-infant reunions in the second half of the first year, might be understood metapsychologically in terms of the split that still exists in the internal part-images of the mother. This split is easily activated by such brief absences; the mother of separation must be reintegrated as the "all good" symbiotic mother so as not to hurt or destroy the good object [p. 67].

Whatever the authors may mean by "metapsychologically," they obviously make some controversial assumptions about what goes on in the 7-month-old's mind. I wish only to point out that a report of this kind cannot possibly be considered data in support of such assumptions. That is essentially the criticism I have made elsewhere of the blanket applications of notions of "systems" and "intersubjectivity" to the young infant (Kaye, 1982). Whether we accept those interpretations depends on our prior beliefs about gesturing by infants; anecdotes and photographs are not evidence.

In summary, it is not sufficient for an investigator to fall back upon the familiar excuse "this is an exploratory study," as though that were to sanction any and all informal, inspired, or intuitive manifestations of the spirit. Microanalysis, properly understood, belongs to nomothetic science.

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