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The emergence of a new paradigm in ape language research

Stuart G. Shanker[a] and **Barbara J. King**[b]
[a]*Departments of Philosophy and Psychology, York University, North York, Ontario, Canada M4S 1B4;* [b]*Department of Anthropology, College of William and Mary, Williamsburg, VA 23187*
shanker@yorku.ca **bjking@wm.edu**

Abstract: In recent years we have seen a dramatic shift, in several different areas of communication studies, from an information-theoretic to a dynamic systems paradigm. In an information processing system, communication, whether between cells, mammals, apes, or humans, is said to occur when one organism encodes information into a signal that is transmitted to another organism that decodes the signal. In a dynamic system, all of the elements are continuously interacting with and changing in respect to one another, and an aggregate pattern emerges from this mutual co-action. Whereas the information-processing paradigm looks at communication as a linear, binary sequence of events, the dynamic systems paradigm looks at the relation between behaviors and how the whole configuration changes over time.

One of the most dramatic examples of the significance of shifting from an information processing to a dynamic systems paradigm can be found in the debate over the interpretation of recent advances in ape language research (ALR). To some extent, many of the early ALR studies reinforced the stereotype that animal communication is functional and stimulus bound, precisely because they were based on an information-processing paradigm that promoted a static model of communicative development. But Savage-Rumbaugh's recent results with bonobos has introduced an entirely new dimension into this debate. Shifting the terms of the discussion from an information-processing to a dynamic systems paradigm not only highlights the striking differences between Savage-Rumbaugh's research and earlier ALR studies, but further, it sheds illuminating light on the factors that underpin the development of communication skills in great apes and humans, and the relationship between communicative development and the development of language.

Keywords: apes; ape language research (ALR); brain development; co-regulation; communication; dynamic systems; language development; symbols

1. The spreading appeal of the dance metaphor

In recent years the same metaphor has cropped up time and again in very different areas of communication studies. In ape language research (ALR), Sue Savage-Rumbaugh observes how the origins of language comprehension lie in "interindividual routines," which are like "a delicate dance with many different scores, the selection of which is being constantly negotiated while the dance is in progress, rather than in advance" (Savage-Rumbaugh et al. 1993, p. 27). In nonverbal communication research, Michael Argyle describes how "a speaker starts gesticulating and looks away as he starts to speak, and reverses this when he stops. There is an intricate co-ordination of pausing and looking within turns, followed by head-nods, smiles, and gazes. Interactional synchrony has been called a 'gestural dance,' and likened to a waltz" (Argyle 1988, p. 118). In infant development research, Daniel Stern recounts how, at the age of four months, an infant "passes into the Immediate Social World. In this world of the 'here and now, between us,' he reports on the rich choreography between himself and his mother, on the subtle moves by which they regulate their flow of feelings. Thus, Joey introduces us to the basic dance we all play out with other people throughout our lives" (Stern 1990, p. 7). What is it about this dance metaphor that so appeals to scientists who are interested in the dynamics of communication and language development?

We will explore this question through consideration of two major projects within ALR, the Animal Model Project, initiated by Duane Rumbaugh at the Language Research Center at Georgia State University, and Sue Savage-Rumbaugh's research with the bonobo Kanzi. We start by noting that the terms used to describe a dance are radically different from those used in the information-transmission metaphor that has hitherto dominated the study of communication (King & Shanker, in press). The transmission metaphor treats communication as a *sequential* process in which partners take turns emitting and processing one another's messages (Argyle 1988). The dance metaphor focuses on *co-regulated* interactions and the emergence of creative communicative behaviors within that context (Fogel 1993). Whereas the information-transmission metaphor prompts one to conceptualize communicative exchanges in terms of such constructs as *signal and response, sending and receiving,* and *encoding and decoding,* the dance metaphor leads one to conceptualize communicative encounters in terms such as *engagement and disengagement, synchrony and discord,* and *breakdown and repair.*

The chief appeal of the transmission metaphor is that communication is treated as a rule-governed system, with predictable causal sequences. Given the premise that signal A is invariably followed by response B in conditions *xyz,* one looks for the multiple variables that determine the unfolding of communicative sequences. The scientist adopts

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an objectivist stance, focusing on one individual at a time: on how she processes/responds to the signal she has received, and where these signals and responses are broken down into units that can be objectively measured.

The chief appeal of the dance metaphor is that it draws attention to how communicating partners continuously establish and sustain a feeling of shared rhythm and movement. Such an activity of mutual attunement is established through a number of different modalities. Communicating partners not only adjust to each other's specific behaviors, facial expressions, vocalizations, and so on, but may also attune to one another cross-modally. For example, an infant suddenly jerks her arms and her mother "responds with a sharp 'Oh!' that has the same temporal and intensity contour as the infant's arm movement" (Fogel 2000, p. 7), or the tone of voice prompts the other to move closer or farther away. Whereas the transmission metaphor sees communicating partners in discrete states – that is, an agent is either sending or receiving – the dance metaphor sees both participants in the communicative activity as continuously active and interactive.

The information-transmission metaphor brings to mind the image of two computers or fax machines transmitting information back and forth (Pinker 1994). The dance metaphor brings to mind several different images: it summons up a Vygotskean picture of a novice being guided by a more experienced partner; a picture of awkwardness and friction when two partners are not in harmony with one another; and a picture of fluid movement when two partners are effortlessly communicating with one another. The dance metaphor also suggests how difficult it can be to identify one partner as the "initiator" of an exchange: Is it the one who asks the other to dance? But then, how much nonverbal communication may have preceded the actual issuing of this "invitation?" Similarly, Schore describes how the young infant smiles in order to evoke her mother's gaze, and conversely, averts her gaze when she finds too much arousal unpleasant (Schore 1994, pp. 82ff). But such behaviors only occur within the context of being gently held and cooed to, or recently fed, not to mention all the previous gaze exchanges.

This type of affective interaction has been broken down into six basic levels by Stanley Greenspan; they include attention regulation, engagement, intentional affective signalling, reciprocal affective problem-solving, affectively mediated creation of ideas and affectively mediated thinking (Greenspan 1997). In *The Evolution of Intelligence,* Greenspan and Shanker show how these basic affective interactions are the foundation for human intelligence and reflective thinking (Greenspan & Shanker, in press). It is no wonder that the emotional interactions expressed by many in the context of a dance metaphor has become so popular in infant developmental research; the field has increasingly begun to look over the past two decades at interactional synchrony (Greenspan 1979; 1989; Horner 1985; Schore 1994; Stern 1990; Thelen & Smith 1994). Like two dancers who are aware of themselves and each other as a single entity, the members of a dyad are said to be "mutually attuned" when they are in a similar affective state and rhythmically synchronized with one another's movements and moods. In normal dyadic interactions this is thought to occur as much as 30 percent of the time; the other 70 percent of the time, the caregiver and infant are in various degrees of being "out of synch" with one another (Tronick 1989). Attentive caregivers are sensitive to these periodic breakdowns and good at restoring interactional synchrony. When a caregiver is poor at repairing these breakdowns there is a marked decline in interactional synchrony. Communication breakdowns are even more common with infants who have problem temperaments or who have trouble developing and/or responding to conventional communicative behaviors.[1]

There has been a great deal of research confirming the findings of Thomas and Chess' "goodness-of-fit" model (Chess & Thomas 1984): namely, when caregivers respond harshly or inconsistently to a difficult child, or to a child who fails to develop conventional communicative behaviors, periods of "dyadic dissonance" increase and the child is more likely to behave aggressively and egocentrically with peers later in life. But when caregivers are effective at adjusting their parenting skills to match the child's temperament or idiosyncratic communicative behaviors, we see higher levels of interactional synchrony. What is more, there is evidence to suggest that the more secure the attachment, the more positive the child's social interactions with her peers and the more developed her prosocial attitudes (Ainsworth et al. 1978). That is, the greater the amount of interactional synchrony between an infant and her caregiver, the better is the child able to adjust her communicative style and responsiveness to the rhythms of different dance partners as she grows older (Greenspan 1997).

What we are witnessing in the growing popularity of looking at dynamic interactions in infants and caregivers in the context of a dance metaphor, therefore, is an indication of how the information-transmission metaphor, which

Stuart G. Shanker is Professor of Philosophy and of Psychology at Atkinson College, York University. His research focuses on the role of social and emotional factors in communication and language development in young children. Among his recent publications are *Apes, Language and the Human Mind* (with Sue Savage-Rumbaugh and Talbot Taylor, 1998); *Wittgenstein's Remarks on the Foundations of AI* (1998); *Toward a Psychology of Global Interdependency: A Framework for International Collaboration* (with Stanley Greenspan, 2002); and *The Evolution of Intelligence* (with Stanley Greenspan, in press). His honors include Canada Council Doctoral and Postdoctoral Fellowships; the Marian Buck Fellowship at Christ Church College, Oxford; the Walter L. Gordon Fellowship at York University; and Research Grants from the Canada Council, The Unicorn Foundation, The Templeton Foundation, and Cure Autism Now.

Barbara J. King is Professor of Anthropology at the College of William and Mary, Williamsburg, Virginia. Her research focuses on communication and cognition in primates; King and her students currently study ontogeny of gesture in captive gorillas. Among her publications are *The Information Continuum* (1994) and two edited volumes, *The Origins of Language* (1999) and *Anthropology Beyond Culture* (with Richard G. Fox, 2002). The Teaching Company selected her to create and teach "Roots of Human Behavior," a 12-hour audio- and video-course. Her honors include the Virginia State Council of Higher Education's Outstanding Faculty Award (1998) and a Guggenheim Fellowship (2002–2003).

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treats communication as a sequential, turn-taking process, is becoming increasingly constrictive for those interested in the co-regulated dynamics of communication. More and more, researchers are employing multiple cameras and digital recorders to analyze the multi-modal coordination of movements and vocalizations in dyadic interactions, and conducting longitudinal studies in an effort to understand how an organism's particular experiences govern its behavior (Bekoff & Byers 1998).[2] The larger question here, however, is whether these technological and methodological advances can be accommodated within the conceptual framework that underpins the use of the transmission metaphor, or whether what we see here are the emerging signs of a paradigm shift in our understanding of the very nature of communication, and thence, the types of data one sets out to acquire in ALR and human communications studies, and the manner in which this should be analyzed.

2. The shift from an information-processing to a dynamic systems paradigm

The shift from the transmission metaphor to a dance metaphor represents, we believe, a fundamental shift in communications theory from an *information-processing* to a *dynamic systems* paradigm. In an information-processing system, communication, whether between cells, mammals, prosimians, apes, or humans, is said to occur "when one organism (the transmitter) encodes information into a signal which passes to another organism (the receiver) which decodes the signal and is capable of responding appropriately" (Ellis & Beattie 1986, p. 4). In a dynamic system, all of the elements are continuously interacting with and changing in respect to one another, and an aggregate pattern emerges from this mutual co-action. Hence, communication is seen as a "continuous unfolding of individual action that is susceptible to being continuously modified by the continuously changing actions of the partner" (Fogel 1993, p. 29). Thus, whereas the information-processing model looks at communication as a linear, binary sequence of events, the dynamic systems model looks at the relation between behaviors and how the whole configuration changes over time.

For example, on the information-processing paradigm there are said to be a limited number of basic emotions (e.g., interest, surprise, happiness, sadness, anger, fear) in nonhuman and human primates that are indexed by stereotypical facial expressions. The production of these facial expressions, and the responses that they evoke, are both thought to be controlled by genetically determined programs (see Ekman 1980; Izard 1997). The communication of basic emotions is treated as a composite form of reflex: namely, a stimulus triggers a neural program that controls a neuromuscular/expressive, autonomic, behavioral, and experiential sequence of events. In this framework, one studies the facial expressions that index the basic emotions by isolating a movement in a single region of the face or combinations of regional movements (Ekman 1980) and the reactions of another organism to what are construed as stereotypical facial expressions (Griffiths 1997).

The problem with this approach, stressed by dynamic systems theorists, is that facial expressions of emotion are constantly changing in response to the changing dynamics of social interactions (Wagner 1997). Furthermore, recent research by dynamic systems theorists suggests that the stereotypical facial expressions associated with "basic" emotions *develop* within the context of shared emotional experiences. For example, so-called "Duchenne" smiles are commonly treated as an index of pleasure (see Ekman 1992) and cited by affect program theorists as a paradigm example of an innate communicative signal (Ekman 1979). But Messinger, Fogel, and Dickson's micro-analysis of the emergence of Duchenne smiles in young infants revealed that sometimes the smile only occurred on one side of the face, or was stronger on one side; sometimes the smile contained negative elements (e.g., grimaces); and most important of all, that smiles were fleeting and "seemed to change in response to equally swift reactions on mother's part. It was out of this fast-paced interactional matrix that more stable, recognizable expressions seemed to develop" (Messinger et al. 1997, p. 207). Thus, on the dynamic systems approach, one studies the development of stereotypical facial expressions, or the communication of emotions, by looking at the complexity, detail, and responsiveness of subjects' changing facial expressions of emotion in the context of co-regulated interactions.

The shift from an information-processing to a dynamic systems model thus represents an important transformation in our understanding of the nature of communication, from looking at communication as an *encryption process,* to seeing communication as a *co-regulated activity.* On the information-processing paradigm, what is communicated is always information, construed in Shannon's information-theoretic terms (Shannon 1948) as something that can be quantified. The information communicated is about an *internal state* or *representation.* Whether it is a thought, an emotion, an intention, or a piece of knowledge, the information that A communicates to B must be such that it can be encoded and transmitted. Genuine communication only occurs when B decodes the message that A encoded. Hence, A and B must possess the same code in order for genuine communication to occur. Such a code must either be genetically predetermined (Chomsky 1972), imprinted during a critical period (Lenneberg 1967), or acquired through conditioning (Skinner 1957).

In the dynamic systems paradigm, what is communicated is not simply information, although, to be sure, an important aspect of communication is what kinds of information a subject can communicate. Indeed, one of the most important developments in the study of great ape communication has been learning about the surprisingly complex kinds of information that great apes communicate to one another and the complex social situations in which this occurs (see King & Shanker, in press). But, in addition to communicating various kinds of information, great apes and humans also communicate their desires and intentions, fears, warnings, invitations, attitudes, emotions, and so on, where none of the latter can be reduced to the former. That is, what one communicates when one communicates one's desires, intentions, thoughts, wishes, emotions are exactly that: namely, desires, intentions, thoughts, wishes, and emotions. Far from being trivial, this tautological statement lies at the very heart of the categorial distinction that dynamic systems theory draws between the communication of information and the communication of desires, intentions, thoughts, wishes, emotions, and so forth.

In other words, dynamic systems theory repudiates the information-theoretic assumption that all communicative

acts can be construed as the communication of "information," and even, that the particular circumstances in which great apes and humans do communicate information to one another can be construed in Shannon's information-theoretic terms (Coulter 1983). What is at stake here is the very premise that in all acts of communication one organism encodes information into a signal that another organism decodes. Great ape and human communication, according to the dynamic systems paradigm, is not a "telementational" process (Harris 1980a): to communicate a desire, intention, thought, wish, or emotion, is not to "transmit information" about an "internal state" that must be "decoded" in order to be understood. Rather, mutual understanding is something that *emerges* as both partners converge on some shared feeling, thought, action, intention, and so on. Far from following some predetermined format, such a process is intrinsically creative.

This last point bears directly on the so-called "problem of creativity" which, following Chomsky's review of Skinner's *Verbal Behavior* (Chomsky 1959), has become a defining issue in the continuity/discontinuity debate over the relationship between animal and human (linguistic) communication. For, according to Chomsky, "the fundamental distinction between human language and the purely functional and stimulus-bound animal communication systems" (Chomsky 1966, p. 9) is that only the language system is capable of generating infinitely many novel combinations, where creativity is a property of the system itself. But dynamic systems theory places "*creativity* at center stage since the most salient aspect of co-regulated interchange is the emergence of something novel, something that was not there before" (Fogel 1993, p. 31). There is thus a striking difference between where the two paradigms assign creativity, and in the very manner in which creativity is construed. Whereas the information-processing paradigm sees creativity as a property of the language system itself, of the basic principles of that system, dynamic systems theory views creativity as a property of agents' behavior in co-regulated interactions. Indeed, as we shall argue below, the significance of recent ALR lies precisely in the creative emergence of novel communicative behaviors.

This emphasis that dynamic systems theory places on *creative actions* has important methodological significance for the study of communication. Ethologists have long stressed the importance of becoming thoroughly acquainted with one's subjects in order to determine the components and significance of their communicative behaviors. Many would insist that the development of a comprehensive profile of an individual's or a species' communicative repertoire demands multiple perspectives, for example, observing the subject interacting with different partners in different settings over extended periods of time. But dynamic systems theory introduces a further critical element into this matrix. Not only must one always be prepared for the possibility that a subject's communicative acts are highly idiosyncratic, but further, one must be alert to the new dimensions of meaning that are co-created in the contexts of ever-changing and evolving interactions. Even a stereotypical movement that one has observed countless times before – say, an ape slowing extending a forearm to request a food item – might suddenly take on some whole new dimension of meaning as, together, the co-participants treat the gesture as, for example, a summons to direct their gaze at some distal event. Thus, one must adopt a *hermeneutic* stance toward one's subjects and interpret the significance of their communications for them, in terms of the context in which these activities occur, the extent to which the behavior of the interactants is rigid or ritualized, and the extent to which it is innovative and responsive.

The shift to a dynamic systems paradigm has profound implications, therefore, for our understanding of the genesis of communicative behaviors. On the information-processing paradigm, we consider the nature of animal signals as if they have been "designed" for a specific purpose. This is a shorthand way of saying that we assume that the signals we observe are the product of natural selection, which has favored those properties of signals that make them most effective at conveying information (Halliday & Slater 1983, p. 43). Thus, we seek to isolate specific behaviors ("signals") and establish (through repeated observations) the conditions that trigger those behaviors and the responses that they evoke. When we look for the reasons why an ape did such-and-such, we are looking for the reasons why that behavior was selected. On the dynamic systems paradigm, natural selection is thought to apply to the whole *developmental manifold* (see Gottlieb 1997). Highly predictable developmental outcomes are seen as the result of *canalizing influences* – that is, highly predictable environmental circumstances (van der Weele 1999) – and not as *canalized traits* (i.e., traits that are strongly buffered from environmental perturbations by information that is encoded in an organism's genes).

The "canalizing influences" that most stand out in the case of human communicative development are the close dyadic relationships in which an infant is nurtured over the first few years of its life, and the influence of social factors on her development (see Shanker & Taylor 2001). That is, the infant is exposed to a highly stable environment: first in the womb, which provides the foetus with surroundings that are highly (but not totally) buffered from external shocks, and then in close dyadic relationships with primary caregivers. The secondary altriciality of humans thus helps to ensure that infants experience remarkably similar rearing conditions during the first few years of their lives. Indeed, one of the most important lessons we have learnt from the study of developmental disorders is just how serious are the consequences for a child's cognitive and linguistic development if, for either endogenous or exogenous reasons, she is unable to undergo the typical emotional experiences involved in these early dyadic interactions with a primary caregiver (see Goldstein & Hockenberger 1991).

In general, then, the information-processing and dynamics systems paradigms can be contrasted as shown in Table 1.

One of the most dramatic examples of the significance of shifting from an information-processing to a dynamic systems framework can be found in the debate over the interpretation of recent advances in ALR. To some extent, many of the early ALR studies reinforced the stereotype that animal communication is functional and stimulus-bound, precisely because they were based on an information-processing framework that promoted a static model of communicative development. In these studies, researchers employed the sorts of techniques that one sees in behavioral modification therapy, and attempted to model ape linguistic behaviors – for instance, by molding apes' hands into closer and closer approximation of ASL signs and rewarding the apes with food treats (see Gardner & Gardner 1969; Premack 1976).

Table 1. *Information-processing dynamic systems theory*

Information Processing	Dynamic Systems Theory
All communication systems can be modelled in the same way	
Great ape and human communication is a process	Great ape and human communication is an activity
Great ape and human communication is sequential	Great ape and human communicative interactions are co-regulated
Information is transmitted from a sender to a receiver	Aggregate patterns emerge from this activity of mutual co-action
Communicating organisms are in one of two states: either active or passive, sending or receiving	Partners are continuously active, continually adjusting their behaviors to each other
The information transmitted is encoded and decoded by sender and receiver	Communication is not a process, *a fortiori,* not an encryption process
Communication is governed by fixed codes	Communicating partners are constrained by each other, their developmental history, their biology, and their environment (social, cultural, physical)
There are different categories of information but what is communicated is always information	What one communicates when one communicates, e.g., one's desires, intentions, thoughts, wishes, emotions are just that, and not different kinds of "information"
The information communicated is about an internal state or representation	Desires, intentions, thoughts, wishes, emotions, are not "internal states" or "representations." In the particular circumstances where it makes sense to say "P communicated a piece of information to Q," the information communicated is not about an internal state or representation, but rather, *that such-and-such* ("that there is some ripe fruit over there," "that John is coming," and so on)
We infer from an organism's behavior what sorts of mental events it is experiencing	An organism's behaviors serve as a criterion for describing it with such-and-such an epistemic operator
Communication is a rule-governed causal sequence, where such-and-such a signal is predictably followed by such-and-such a response in conditions *x,y,z*	Co-regulated interactions are intrinsically creative
Creativity is a property of a generative system (i.e., of its basic principles)	Agents act creatively (or rigidly)
The signals we observe are product of natural selection	Highly predictable communicative behaviors are a product of canalizing influences
The development of species-typical communicative behaviors is maturational	The development of species-typical communicative behaviors emerges in the context of co-regulated species-typical experiences
The maturation of communicative behaviors can be treated as the simple summation of hereditary and environmental factors (i.e., H X E)	The development of communicative behaviors is the result of a nonlinear, complex interplay between maturational and experiential factors
The scientist adopts an objectivist stance, focussing on one individual at a time, isolating specific behaviors (signals) and establishing through repeated observations the conditions that trigger those behaviors and the responses they evoke	The scientist adopts a hermeneutic stance, looking at what their behaviors mean to the participants involved in co-regulated interactions
The communicative significance of a particular gesture, vocalization, movement, etc., can be decontextualized	The communicative significance of a gesture, vocalization, facial expression, etc., is viewed as a function of its role within a communicative exchange (e.g., a hand movement only counts *as a gesture* in the context of such-and-such an activity)

Terrace's attack on ALR (Terrace 1979) was really an attack on the idea that such conditioned behaviors could lead to truly linguistic behaviors. But, as Terrace himself recognized (Terrace 1986), Savage-Rumbaugh's work in ALR introduced an entirely new dimension into these discussions. What is particularly noteworthy about Savage-Rumbaugh's research is that she began within, but then gradually abandoned, the information-processing framework that had hitherto dominated ALR. As we shall see, the more freely she began to interact with the apes, and the more freely they began to interact with each other, the more they became active partners in their own – and Savage-Rumbaugh's – development.

Shifting the terms of the discussion from an information-processing to a dynamic systems framework not only highlights the striking differences between Savage-Rumbaugh's research and earlier ALR studies, but further, it sheds illuminating light on the factors that underpin the development of communication skills in great apes and humans,

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and indeed, the relationship between communicative development and the development of language. From a dynamic systems perspective, the development of language cannot be understood outside of the rich and complex activity of non-verbal communication. One studies, not what linguistic gestures *represent,* but rather what they mean to participants, where meaning is conceptualized – not as the transfer of some symbolically encoded information, but as a discovery of the mutual convergence on some shared feeling, thought, action, or intention.

3. Sherman and Austin

ALR has been overshadowed by the debate over whether the capacity to acquire syntax and semantics is uniquely human. Discussions have primarily centered on the question of whether enculturated apes' communicative behaviors can be compared with early norms in child language acquisition studies. The most striking results obtained so far have been with Kanzi, the male bonobo who was born and raised at the Language Research Center. Hence, there has been a natural tendency to focus on the linguistic feats of this extraordinary ape, to the exclusion of other important areas of ALR. This tendency is unfortunate, however, for two different reasons. One is the obvious risk of overlooking important findings that have been made with other great apes. But perhaps the more important reason is that this understandable preoccupation with Kanzi's achievements may skew the significance of ALR by casting the research as a matter of establishing whether or not great apes can be brought to cross the "language Rubicon."

The danger here is that ALR might be seen as solely a matter of ascertaining whether great apes can perform "high enough" on language tests – as measured, for example, by how many words they can learn or what sorts of syntactical constructions they can master – so as to refute any lingering doubts about whether their behavior can be legitimately described in linguistic terms. The problem with such a viewpoint is that it accepts from the outset the assumption, first articulated by eighteenth-century Cartesian philosophers, that there is a bifurcation between language and nonhuman primate "natural expressive behaviors" (see Savage-Rumbaugh et al. 1998, Ch. 2), and that the goal of ALR is to see whether apes' productive behaviors can become genuinely "language-like" or remain merely instrumental. This framework has engendered a polemical atmosphere in which researchers from opposite ends of the "continuity/discontinuity" spectrum reach opposite conclusions about how the data should be properly interpreted.

By no means should the research community neglect the interpretation of the rich corpus of data that has been accumulated with Kanzi, or with the apes trained to use a version of American Sign Language (e.g., the chimpanzee Washoe [Gardner & Gardner 1969] and her ape companions [Cianelli & Fouts 1998] or the orangutan Chantek [Miles 1990]; for a recent review, see Hixson 1998). For there are many important questions in the existing data that remain to be answered: for example, what kinds of concepts have they mastered and in what order have they acquired linguistic skills (see Kako 1999)? But we must not lose sight of the fact that the driving impetus behind ALR is to discover what kinds of communicative skills great apes can master, and equally important, what sorts of skills seem to be irrevocably beyond their grasp; how environmental contingencies affect a great ape's cognitive, socioaffective, and communicative development; and, perhaps as a result of this research, the significance of various aspects of the social environment and caregiving practices on a child's cognitive, socioaffective, and language development. Thus, as exciting as the achievements of the "linguistic savants" clearly are, some of the most important findings may be in those "grey" areas where the distinction between "natural" and "linguistic" behavior is hardest to draw.

For this reason, we begin our discussion of ALR with a review of the Animal Model Project, which was conducted with the chimpanzees Sherman and Austin at the Language Research Center in the late 1970s and early 1980s. The stated purpose of this research was not to establish that a great ape could reach such-and-such a level of (age-matched) linguistic performance, in order to silence the discontinuity critic. Rather, as Savage-Rumbaugh explained in *Ape Language:*

> As Sherman and Austin moved from the simplest discrimination tasks to complex spontaneous communications, it became increasingly apparent that they were continually learning to do far more than they were being taught. The issue of whether or not they had achieved "true" human language was never the goal. The goal was to improve their communicative competence and, in doing so, to more clearly define the skills involved, both at the behavioral and at the cognitive levels (Savage-Rumbaugh 1986, p. 404).

The reason why the research with Sherman and Austin provides such an important starting-point for this discussion is because, in their case, we have a fully documented account of the steps that were taken to enable a great ape to undergo what Deacon describes as "the shift from conditioned associations to symbolic associations" (Deacon 1997, p. 84). Moreover, we have the added benefit that Deacon presents a highly sophisticated explanation of this "shift" on the information-processing model of communication, which, as we shall see, stands in sharp contrast to Savage-Rumbaugh's own explanation of Sherman and Austin's development, which is highly resonant of the dynamic systems paradigm. Thus, the contrast between their two accounts illuminates the different kinds of explanation that are pursued on the two models of communication outlined at the end of the preceding section.

Significantly, Sherman and Austin were both highly communicative with Savage-Rumbaugh from the start of the research onwards. She reports that they were especially sensitive to affects conveyed by her tone of voice, to her facial expressions, and that they frequently gestured to communicate their desires to her (Savage-Rumbaugh 1986, pp. 38, 56). What they could not do very well, however, was pair lexigrams with objects. An experimenter would train them on the association between a food item and its lexigram and later hold up the food item and ask them with which lexigram it was associated. Even though other great apes had performed well on this task, Sherman and Austin both experienced considerable difficulty when they had to choose between two keys, and despite extensive training, they were unable to perform above chance if they had to select from three keys. But dramatic advances occurred when Savage-Rumbaugh shifted to a "request task" paradigm in which the experimenter would hold up a food item that the chimp would immediately receive if he pressed the right key. Now Sherman and Austin started to make rapid gains in lexi-

gram-object pairings. But if the expectancy of receiving a food item by pressing its lexigram key was removed, their response behavior quickly became fragmented again.

Savage-Rumbaugh then attempted to teach Sherman and Austin the difference between "requesting" and "naming" by fading out their food rewards. In her view, another significant advance occurred when the apes began to use lexigrams with no expectation of receiving a food reward. Now,

> When a training task was begun, instead of waiting for the teacher to ask that certain items be given or labelled, the chimpanzees began naming items spontaneously and then showing the named item to the teacher. As the chimpanzees decided which objects were to be named and shown, they also incorporated many aspects of the teacher's role into their own behavior. They initiated trials, singled out objects, and actively engaged in behaviors designed to draw the teacher's attention to what they were saying. Moreover, these indicative behaviors, once they appeared, were not limited to training contexts (Savage-Rumbaugh 1986, p. 326).

In other words, Sherman and Austin began to demonstrate communicative behaviors that are normally seen in a one-year-old child: they imitated Savage-Rumbaugh's actions and they used lexigrams spontaneously, in novel situations, to refer to objects, direct her attention, and express their intentions. Whether or not such actions are present in the wild, these were certainly new behaviors for them, and thus, as Deacon (1997) argues, we need to explain this radical development in their behavior.

On the information-processing paradigm, communication is defined in terms of, and is limited to, the number of channels available to the sender and receiver; the quality of the medium; and the nature of the "internal states" experienced by sender and receiver. Since neither of the first two factors was significantly altered by the new training paradigm, the explanation for Sherman and Austin's communicative development must, on the information-processing approach, lie in the third factor. And this is precisely the route that Deacon takes: according to Deacon, the qualitative "shift" observed in Sherman and Austin's communicative behavior was the result of a "radical transformation in the[ir] mode of representation" (Deacon 1997, p. 87).

On Deacon's account, this *mental transformation* was induced, first by training, and then systematically extinguishing, illicit symbol combinations in a combinatorial system consisting of two "verbs" and four "nouns." Even such a simple combinatorial system allows for 720 pair sequences, most of which are nonsensical. Over the course of thousands of trials these illicit combinations were gradually extinguished. As a result, the apes learned, not simply "symbol-object" pairings, but "a set of logical relationships *between the lexigrams*" (Deacon 1997, p. 86). That is, they discovered "that the relationship that a lexigram has to an object *is a function* of the relationship it has to other lexigrams, not just a function of the correlated appearance of both lexigram and object" (Deacon 1997). And this, Deacon concludes, "is the essence of a symbolic relationship," which, once grasped, enabled Sherman and Austin to assimilate new symbols into their lexicon, quickly and effortlessly (Deacon 1997). Deacon's explanation of this phenomenon is presented in the same sort of terms as one would use to describe a Pattern Recognition program: namely,

> the shift from associative predictions to symbolic predictions is initially a change in mnemonic strategy, a recoding. It is a way of offloading redundant details from working memory, by recognizing a higher-order regularity in the mess of associations, a trick that can accomplish the same task without having to hold all the details in mind. (Deacon 1997, p. 89)

Deacon primarily bases his account on the early guidelines of the Animal Model Project (see Rumbaugh 1977; Savage-Rumbaugh & Rumbaugh 1979). But Savage-Rumbaugh has recounted how, soon after these guidelines were published, she decided that "unlike all previous ape-language projects, this one would not have as its goal the production of word combinations or sentences. I wasn't in search of the linguists' holy grail. I was going to focus on words: What does a word mean to a chimpanzee, and how can we find out?" (Savage-Rumbaugh & Lewin 1994, p. 49). She quickly abandoned the earlier approach of training the chimps on which cues were irrelevant and which combinations were illicit, and began instead to encourage the chimps to use lexigrams in their day-to-day activities.

Given this difference in their views of the goals and methodology of the Animal Model Project, it is not surprising that we should see striking differences in the manner in which Deacon and Savage-Rumbaugh conceptualize Sherman's and Austin's communicative development, and in their explanations of why this occurred. According to Deacon, Sherman and Austin experienced a sort of "gestalt-like" mental reorganization: what he calls a "recoding" or a "re-representation" of the lexigrams they had originally learned as indexical pairings. But according to Savage-Rumbaugh, the "qualitative shift" observed in Sherman's and Austin's communicative behaviors was the result of her efforts to establish "a much closer physical proximity with the apes, interacting with them in a social, preschool-like setting [that] would emphasize communicative needs rather than promoting teaching efficiency" (Savage-Rumbaugh & Lewin 1994, p. 49). For Deacon, "the food lexigrams are in a real sense 'nouns' [because they] are defined by their potential combinatorial roles" (Deacon 1997, p. 88). For Savage-Rumbaugh, "it seemed that [Sherman and Austin] really 'had words' [when] they understood that words could be used to express future intentions and thereby coordinate actions, rather than simply as a mechanism to get others to do something for them" (Savage-Rumbaugh & Lewin 1994, p. 127). That is, what licenses us to describe Sherman and Austin as "mastering 'words'," is the sorts of acts they performed with the lexigrams, not what might have been going on in their minds. Thus, for Deacon, the "shift" in Sherman's and Austin's behavior occurred as the result of a mental transition from learning isolated symbol-object pairings to learning how new symbols fit into a combinatorial system; for Savage-Rumbaugh, the development occurred as the result of switching from a behavior modification paradigm to using lexigrams in ordinary social circumstances. Whereas Deacon's explanation focuses on what went on "inside their heads," Savage-Rumbaugh's explanation focuses on the nature of the actions that the apes began to perform with the lexigrams.

For example, Savage-Rumbaugh recounts how, to teach them comprehension skills, she hid a food item in a container and used the keyboard to tell them what was hidden in the container:

> The first time I did this Sherman rushed to smell the container, but was unable to detect what was in it. He gestured for me to open the container, but I refused. Instead, I went to my keyboard, located just outside Sherman and Austin's room, and

> stated *this chow.* When I used my keyboard, it made the symbol "chow" appear on projectors located just above Sherman and Austin's keyboard. Sherman saw this information and apparently believed me because he immediately used his own keyboard to say *open chow.* On the next twenty trials of this novel situation, Sherman made just two errors, even though I used many different words. (Savage-Rumbaugh & Lewin 1994, p. 71)

In this episode, Sherman's novel communicative behavior emerged in the context of a complex routine that encompassed not only the present circumstances, but also all of the food-sharing interactions that Sherman had previously experienced. Furthermore, what Savage-Rumbaugh was doing was a crucial factor in what Sherman was doing, just as what he was doing was a crucial factor in what she was doing. The manner in which each of them was acting was part of their shared history together and the evolving dynamics of their co-regulated interactions. The fact that Savage-Rumbaugh describes Sherman as "believing her" attests to her perception of the importance of the strong affective undertones that enabled this dyad to make a significant communicative advance.

Savage-Rumbaugh reports that she soon began to observe a qualitative change in her interactions with Sherman and Austin: namely, they began to express their intentions before acting; to pay far more attention to the consequences of their own communicative actions; and to attend more closely to her actions in order to ascertain what she intended (Savage-Rumbaugh & Lewin 1994, p. 72). Deacon's argument is that there is such a dramatic shift between their behavior prior to and immediately following the above event, such that we can infer that they must have experienced some such "mental reorganization" as that described. Prior to this moment their use of the lexigram board was merely *conditioned,* but after the "qualitative shift" their uses of the lexigram board became *symbolic.* But Savage-Rumbaugh argues that the big difference was that, to begin with, Sherman and Austin were intent on using the lexigram board to control the behavior of their experimenters (Savage-Rumbaugh 1986, p. 65), and that they became increasingly intent on discerning her intentions and expressing their own. Rather than viewing Sherman's and Austin's novel communicative behaviors as evidence of a "mental transformation," Savage-Rumbaugh treats their behaviors as a criterion for describing them as developing "linguistic skills." That is, we do not infer from their behaviors that Sherman and Austin had developed such-and-such "mental representations"; it is the behaviors themselves – the *changes in the types of co-regulated interactions in which the apes began to engage* – that licenses us in describing Sherman and Austin as developing linguistic skills (see Johnson 2001).

The point Savage-Rumbaugh is making here is reminiscent of what Tomasello says about language acquisition in children. Tomasello argues that:

> At around nine to twelve months of age human infants begin to engage in a host of new behaviors that would seem to indicate something of a revolution in the way they understand their worlds, especially their social worlds. (Tomasello 1999, p. 61)

What Tomasello has in mind is the fact that, although infants are gesturing intentionally by eight months of age, it is only "at nine months of age [that they] begin engaging in a number of so-called joint attentional behaviors that seem to indicate an emerging understanding of other persons as intentional agents" (p. 61). So too, Sherman and Austin were clearly communicating intentionally with the lexigram board from a relatively early point. But, according to Savage-Rumbaugh, the critical factor in their communicative development was that they not only began to attend more closely to her actions, but also "to pay close attention to each other's communications; they engaged each other before delivering their message; they gestured to emphasize or clarify messages; they took turns" (Savage-Rumbaugh 1986, p. 84).

As a result of this qualitative change in their mutual coactions, the chimps began to engage in highly atypical activities, such as freely sharing each other's plant food and using the lexigram board to cooperate in complex food-sharing tasks. For example, Savage-Rumbaugh placed them in adjoining rooms that were separated by a clear plexiglas window. In Sherman's room there were a number of boxes, each baited with a different kind of food, and each needing a specific tool in order to be opened. Austin was then placed in another room with all of the tools. He could see all the different foods through a window, and would signal to Sherman which food he wanted. Sherman responded by using the lexigram board to tell Austin which tool he needed to open that box. Austin would select the appropriate tool (e.g., a key or a wrench) and pass this through a small hole to Sherman. Sherman would then open the right box and pass the food through to Austin (eating a portion of it along the way).

Clearly far more was involved here than "mutual instrumental behavior." Savage-Rumbaugh reports how "Joint regard, amplification of symbols with gestures, and spontaneous correction of errors were behaviors that emerged out of the interindividual interactions between Sherman and Austin" (Savage-Rumbaugh 1986, p. 203). The apes were not simply monitoring the other's behavior in order to ensure that their communicative intention had been correctly "decoded" and, when this was not the case, repeating or reinforcing the behavior in question. Rather, as is the case with children who are learning their first words, they were using lexigrams to "coordinate [their] management of a complex interactional task" (Taylor 1992, p. 245). Indeed, they even began to *correct* themselves, as well as each other. For example,

> On one trial Sherman mistakenly requested a key when a wrench was appropriate for the task, and he watched as Austin began to look over the toolkit in response to the request. Austin picked up the key, and Sherman looked surprised, turned to look at the keyboard, which still showed the *key* request he'd made, and realized his mistake. He rushed to the keyboard and corrected himself by tapping on the wrench symbol to draw Austin's attention to the changed request. Austin looked up, saw what Sherman was doing, dropped the key, and took the wrench to the window to give to Sherman (Savage-Rumbaugh & Lewin 1994, p. 82).

In other words, Sherman and Austin had reached a point where one wants to say: not only were they able to understand the meaning of the lexigrams they were using, but they were even able to understand each other using lexigrams. The problem that Deacon and Savage-Rumbaugh are both addressing, therefore, is: What justifies us in describing Sherman and Austin's behavior here in such linguistic terms?

Deacon's answer is that, given the objective structure of the combinatorial system that they were using, one can in-

fer that they had both experienced the same mental transformation, and thus, that their behavior can be described in linguistic terms. Savage-Rumbaugh's argument is that it is on the basis of his behavior, and the context in which this occurred, that one is warranted in saying that Sherman had meant to *ask* for a wrench (his gestures, the fact that he looked surprised when Austin picked up a key, that Sherman then rushed to the keyboard and tapped on the wrench lexigram). That is, one is warranted in describing Sherman and Austin as understanding the meaning of lexigram symbols on the grounds that they could do such things as use a lexigram correctly and respond appropriately to its use by others; initiate spontaneous exchanges with lexigrams; use lexigrams to express their intentions; jointly attend to lexigrams, each other, and another person or object; use lexigrams to direct each other's or another person's attention; extend the use of lexigrams to novel (but suitable) circumstances; spontaneously assign unlabelled keys to new foods; closely attend to their own, and to someone else's use of lexigrams; and correct their own or each other's mistaken uses of lexigrams.

The natural question to ask here is whether it would not be possible simply to combine Deacon's information-processing with Savage-Rumbaugh's dynamic systems arguments, that is, to argue that they operate at different, but ultimately compatible explanatory levels. To appreciate what the obstacles are to such a synthesis, it is important to understand how much the two accounts differ from one another in their most basic view of the phenomenon that we are seeking to explain. Deacon's argument represents a sophisticated reformation of the classic "language Rubicon" argument: namely, the problem is redefined as asking whether the primate brain can be brought to a point of spontaneous "re-representation" where the logical relationships between a set of lexigrams is suddenly grasped. But Savage-Rumbaugh is arguing that what matters here is whether Sherman and Austin's actions were sufficiently complex to satisfy the criteria for describing them as *understanding the meaning of symbols,* and what implications this might have for our views about the relationship between *language* and *communication.* If, as would appear to be the case from the above catalogue of behaviors, Sherman and Austin can legitimately be described as having acquired *primitive linguistic skills,* it is because of the sorts of things that they began to *do* with lexigram symbols and the sorts of co-regulated activities they began to engage in using those symbols. And herein lies the reason why Savage-Rumbaugh concluded, contrary to what is postulated by the information-processing model of communication, that: "meaning and intent are not to be found by looking 'inside' a speaker" (Savage-Rumbaugh 1986, p. 382). That is, the explanation of the fact that Sherman and Austin had begun to understand the meaning of lexigram symbols, and that they could understand each other using lexigrams, does not revolve around what (if anything) went on in their minds. Whether or not a subject understands the meaning of a symbol, or another speaker, is established by *what she says or does* in the context of dynamic interactions (see Johnson 2001).

If anything, the research with Sherman and Austin attests to just how problematic it is to draw a categorical distinction between nonverbal communicative behavior and primitive linguistic behavior (see Savage-Rumbaugh et al. 1998). For what the research with Sherman and Austin demonstrates is how verbal behavior – that is, *what we describe as "verbal behavior"* – "emerges from and with nonverbal behavior, and as it does, it provides for a new means of coordinating interindividual object-oriented behaviors" (Savage-Rumbaugh 1986, p. 31). On this line of thinking, the ontogeny of language skills lies, not in a genetic blueprint for "encoding" and "decoding" "mental states" or "representations" using a "generative" system of sounds or signs, nor in a sudden mental shift from "indexical" to "symbolic" comprehension, but rather, in "interindividual interactions [that] come to be coordinated through the use of words" (Savage-Rumbaugh 1986).

The psychological problem we are then left with on the dynamic systems approach is: What made Sherman and Austin's communicative development possible? How did Sherman and Austin acquire such species-atypical skills as using lexigrams to communicate with one another and with humans? Clearly, what was most atypical about Sherman and Austin was the environment in which they were raised and the kinds of tasks that they were encouraged to master. For example, by being physically separated but still able to see and interact with each other, they were encouraged to employ alternative means for engaging in food-sharing activities. To be sure, the technology that they employed literally forced them to engage in "turn-taking" exchanges; yet that does not mean that the communication between them during these tasks was limited to these exchanges. Rather, their use of the board was incorporated into their nonverbal "dances," and like the chimpanzees Sultan and Chica in Köhler's famous studies, they mastered the use of a tool to overcome obstacles and achieve their desired goals (Köhler 1925/1951). Unlike Sultan and Chica, however, what Sherman and Austin mastered was a communication tool. And to master that tool and the increasingly complex demands that Savage-Rumbaugh imposed upon them, required sustained attention and interaction.

But it was not just the task and the tool that made this cognitive and communicative development possible; the presence of an unusually responsive and emotionally attuned caregiver was absolutely crucial. That is, we must not lose sight of the fact that Savage-Rumbaugh herself was an essential element in Sherman and Austin's cognitive and communicative development. For what is clear from her account is that Savage-Rumbaugh was learning as much from these social interactions as were the apes. Her own development as a primatologist, and her development as an interactive partner – her growing understanding of Sherman and Austin's temperaments, their attitudes, thoughts, needs, and of course, communicative intentions – played an integral role in Sherman and Austin's development. In other words, the *socialization of attention* that was observed in Sherman and Austin was a dyadic (and triadic) phenomenon.

4. Kanzi

It is not difficult to understand why the story of the Kanzi research has so captured the general public's imagination. All of the ingredients that one looks for in a gripping scientific narrative are here: the infant of a little-known species of great ape suddenly, and unexpectedly, succeeds where his adoptive mother, despite extensive training, had failed. A research program on the brink of losing its funding is sud-

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denly reinvigorated. Paleoanthropologists begin to speculate that here at last has been found, if not the "missing link," then at any rate a plausible model of the "common ancestor" (before chimpanzees and hominids moved off on their separate evolutionary paths). Psychologists are forced to reconsider their preconceptions about the cognitive and communicative bifurcation between animals and humans. And society as a whole is forced to reassess the morality, and perhaps even the legality, of its attitudes towards apes (see Wise 2000).

Given the larger ethical as well as scientific implications that hinge on this research, it is not surprising that it has become so important to catalogue the exact nature of Kanzi's linguistic achievements. This record is now widely known (see Savage-Rumbaugh & Lewin 1994; Savage-Rumbaugh et al. 1993; 1998): When he was two years old, Kanzi could use eight symbols on a lexigram board to request various food items. By the time he was three, he was using 20 symbols, and when he was eight he had mastered the productive use of over 250 symbols. He uses these signs purposefully, without cuing or imitation, to do such things as refer to objects and locations in the immediate, present surroundings, as well as to others that are "absent," and even, to comment on events that occurred in the past, ask questions, play games, or simply provide information (both requested and unsolicited).

Even more significant than his use of signs on the lexigram keyboard is Kanzi's ability to understand spoken English sentences. When he was eight years old Kanzi was extensively tested on the same data-set as Alia, a two-year-old child (see Savage-Rumbaugh et al. 1993). The sentences on which they were tested involved such requests as to put something on or in something; to give or show something to someone; to do something to someone; to take something to a distal location; to fetch an object or objects from a distal location; or to engage in some make-believe sequence. Almost all of the sentences were new to Kanzi, and many involved somewhat bizarre requests in order to ensure that he was not able to derive their meaning solely on the basis of semantic predictability.

The results of this comparative study are fascinating: Kanzi was correct on 72 percent of the 650 sentences on which they were tested, while Alia was correct on 66 percent. Even in the cases that were classified as errors, Kanzi was usually partially correct. For example, if asked to fetch two objects from some location he might return with only one. Or he might give the right item to the wrong person, or the wrong item to the right person. In those cases where both Kanzi and Alia were completely mistaken in their responses, it was generally either because of inattention or because they responded to some atypical request (e.g., "Put the knife in the hat") with a customary action (Kanzi attempted to cut a bar of soap with the knife and Alia attempted to cut an apple).

Perhaps the most striking difference between Kanzi and Alia was in respect to their memory abilities: Alia could tolerate fairly long delays before she executed the task that had been requested, whereas Kanzi needed to act fairly promptly if he was to succeed. Yet Kanzi actually performed better than Alia when asked to go to a distal location to fetch an object. Interesting to note, both experienced difficulty when confronted with conversational implicatures. For example, they were asked to "Go outdoors and get an orange" while seated in front of an array of objects that included an orange. Half of the time Kanzi would pick up the object immediately in front of him and then go to the location named, while Alia responded in a similar fashion 25 percent of the time. But when the ambiguity was removed and he was asked, for example, to 'Go get the orange that's outdoors," Kanzi responded appropriately 91 percent of the time.

These controlled studies confirmed Kanzi's ability to understand English sentences displaying a variety of syntactic patterns. Some of these sentences exhibited a degree of syntactic complexity, including the use of embedded constructions. And many of the sentences were paired with their semantic inversion so as to ensure that Kanzi was responding to the syntactic structure, and not simply to semantic cues. This proven ability to understand spoken sentences in English represents a new dimension in ALR. But what are the implications of this advance for our understanding of nonhuman primate communicative capacities, and perhaps, for our understanding of language acquisition? Once again, the contrast between Deacon's discussion of this issue and Savage-Rumbaugh's is highly illuminating.

Deacon's argument is a definitive example of the subtlety that is possible on the information-processing paradigm. He begins by placing Kanzi's current linguistic proficiency at around the level of a three-year-old child, which is more than high enough to pose a formidable challenge to nativist theories of language acquisition (Deacon 1997, p. 125). But then, this leaves us with a paradox. Assuming that Kanzi acquired the rudiments of language whereas Matata did not, simply because he was exposed to a language-enriched environment from birth whereas she was wild-born and was ten years old when Savage-Rumbaugh started working with her, why should Kanzi's brain have evinced a "language-specific critical-period adaptation" when apes in the wild do not possess language (Deacon 1997, p. 126)? Furthermore, Deacon argues that the Kanzi research highlights the same "basic paradox" that we see in language acquisition: namely, why should Kanzi have easily mastered a task which, *prima facie,* is far more complex than simpler tasks requiring conscious memorization of novel associations?

Generativists have, of course, long capitalized on the latter paradox in order to bolster their claim that a child must possess innate knowledge of the "principles and parameters" of language (Pinker 1994). But Deacon eschews this nativist strategy and, in its stead, pursues the same sort of "less-is-more" solution as Elissa Newport does, arguing that the child's general cognitive deficit vis-à-vis problem-solving may actually be an advantage when it comes to language-learning (see Newport 1991). Perhaps this is the reason why Kanzi was able to make the categorial shift from indexical to symbolic association spontaneously, whereas Sherman and Austin required extensive training in order to experience this "mental reorganization." Perhaps Kanzi's mind worked differently, not because there is something unique about Kanzi, but because there is something distinctive about the brain of an infant primate. Indeed, perhaps there is something unique about the structure of language that renders it ideally suited for an "immature" mind to acquire.

By no means is Deacon challenging the premise that a child – or an ape – must acquire grammar when he or she acquires language; for grammar, he insists, is "essential to successful [symbolic] communication" (Deacon 1997, p. 128), insofar as it enables a subject to predict which symbol combinations are licit and which nonsensical. But Dea-

con's argument side-steps the whole controversy over whether Kanzi's achievements constitute "true" language. For, on Deacon's terms, what is clear is that Kanzi has acquired a symbolic system with a simple grammar equivalent (if not identical) to that of a three-year-old child, and that in itself is all that really matters here: that is, if we can explain the psychological mechanism that enabled Kanzi to acquire symbols and a grammar, perhaps we shall therein discover the key to the "basic paradox" in the study of language acquisition.

The crux of Deacon's thesis lies in his earlier discussion of Sherman and Austin. To review his argument: In the combinatorial system on which Sherman and Austin were trained, there were 720 pair sequences, most of which are nonsensical. Granted this is still a large number, but with enough time and patience it was possible to extinguish the illicit combinations and overcome Sherman and Austin's "natural learning predispositions that worked against their discovery of the symbolic reference associations of the lexigrams they were taught" (Deacon 1997, p. 125). But, with Kanzi, we are talking about a productive system of 250 symbols and a comprehension system of at least 650 words. Clearly any thought of systematically extinguishing illicit combinations is untenable; for the system is large enough that a version of Gold's Theorem applies here as much as to natural languages.

It was undoubtedly because he was exposed to language from birth, that Kanzi "crossed the same cognitive threshold [as Sherman and Austin] supported mostly by his own spontaneous structuring of the learning process" (Deacon 1997, p. 125). That is, *Kanzi had to undergo the same cognitive transition as Sherman and Austin* – from indexical to symbolic associations – but he did so spontaneously, because his brain was exposed to this task at a time when his prefrontal cortex was still relatively under-developed. Thus, our task is to explain what it was about "Kanzi's immaturity [that] made it easier [for him] to make the shift from indexical to symbolic reference and to learn at least the global grammatical logic hidden behind the surface structure of spoken English" (Deacon 1997, p. 137). Kanzi's cognitive deficits must somehow have resulted in a match between his "spontaneous structuring of the learning process" and "the structure of the patterns to be learned" (Deacon 1997, p. 128).

As noted above, one of the more intriguing contrasts that emerged in the Kanzi-Alia comparative study was the difference between their short-term memory capacities. But this disparity is not surprising, given the rapid pace of a child's cortical development in the first two years of life. Kanzi's short-term memory capacity would likely be closer to that of, say, a one-year-old child. This point turns out to be important for Deacon's thesis, for he wants to argue that, in general, the immature primate brain renders it difficult for infants to attend to surface details. That is, because of his short-term memory deficits, Kanzi found it difficult to store specific symbol-object associations. But this turned out to be an advantage when it came to language-learning, precisely because grammar and syntax are "surface expressions of the deep web of symbolic relationships" (Deacon 1997, p. 128).

Thus, the reason why Kanzi acquired so much more language than Sherman and Austin, and acquired it so much more easily, has nothing to do with that extra one percent of "human DNA" supposedly possessed by bonobos (see Marks 2002); nor the fact that "bonobos manifest a more intricate socio-communicative repertoire, including the use of more gestures and more vocalization, than common chimps do" (Savage-Rumbaugh & Lewin 1994, p. 125). Rather, Deacon argues that the basic difference between Sherman and Austin and Kanzi, concerns the stage of cortical development which each had reached when they were first exposed to language. Given their greater attentional and short-term memory capacities, the chimps' natural learning predisposition was to focus on the details of word-object relationships, whereas Kanzi's natural learning predisposition was to inhibit these "surface details." It was this initial "learning bias" which enabled Kanzi "to notice the existence of superordinate patterns of combinatorial relationships between symbols" (Deacon 1997, p. 136).

The reason why Kanzi's achievements are so relevant to language acquisition, therefore, is "because if *his* prodigious abilities are not the result of engaging some special time-limited language acquisition module in his nonhuman brain, then such a critical period mechanism is unlikely to provide the explanation for the language prescience of human children either" (Deacon 1997, p. 137). That is, "Precisely because of children's [general] learning constraints, the relevant large-scale logic of language 'pops out' of a background of other details too variable for them to follow" (Deacon 1997, p. 135). Deacon is comparing language acquisition to the perceptual phenomenon in which a stimulus automatically "pops out" from a field of distractors in the preattentive stage of perception (Triesman 1986). So too, Deacon wants to argue, with grammar: here, the "distractors" are the indexical associations between signs and objects, and what "pops out" are the symbolic associations that are a function of the distributed relationships among the symbols in a system.

Thus, rather than supposing that the human brain must somehow have been "hardwired" during the Pleistocene to acquire language, it must be languages that have evolved in such a way as to capitalize on this *bias* of the immature brain. That is,

> Language structures may have preferentially adapted to children's learning biases and limitations because languages that are more easily acquired at an early age will tend to replicate more rapidly and with greater fidelity from generation to generation than those that take more time or neurological maturity to be mastered. (Deacon 1997, p. 137)

But then, what the Kanzi research suggests is that what holds true for the human brain is, in fact, a more general property of the higher primate brain. Hence, if great apes in the wild lack language, this presumably must be due to nongenetic factors.

The idea of the "biases," which lies at the heart of Deacon's thesis, harkens back to AI's notion of cognitive heuristics. It is no coincidence that a computational metaphor should play such an integral role in his theory, for his argument is fundamentally mechanistic: his notion of "mental reorganization" is that of a spontaneous processing phenomenon that is brought about by the exposure to appropriate inputs during a sensitive period. Thus, Kanzi's advanced communicational abilities are the result of a "mental re-coding" – literally, the acquisition of a symbolic code – that occurs "spontaneously, without conscious effort or formal instruction [and] is deployed without awareness of its underlying logic" (Pinker 1994, p. 18). It turns out that the obstacles to this process – which most of us encounter as

adults when we try to acquire a second language – are conscious effort and attention to the details.

A crucial aspect of Deacon's argument is the idea that Kanzi acquired his language skills spontaneously, around the age of two. But Savage-Rumbaugh places great emphasis on the events leading up to that momentous day when she discovered that Kanzi had acquired eight lexigram symbols without any direct instruction. For example, she tells us that at around the age of six months, Kanzi "became mesmerized by the keyboard, staring at the symbols as they flashed onto the projectors at the top of the keyboard" (Savage-Rumbaugh & Lewin 1994, p. 129). When he was 14-months-old, Kanzi began "to press keys on the keyboard and then run to the vending machine as though he had grasped the idea that hitting keys produced food" (Savage-Rumbaugh & Lewin, p. 130). At this stage, his behavior was similar to Matata's, who had also grasped the communicative function of the lexigram board, but had great difficulty with individual lexigram-object associations. When he was 18-months-old, Kanzi started "inventing simple iconic gestures, the first of which indicated the direction of travel in which he wished to be carried. He did this not with a finger point, but with an outstretched arm" (p. 134). He even "added emphasis to his gesture by forcefully turning [Savage-Rumbaugh's] head in the direction he wished to go. . . . At other times, as he sat on [her] shoulders, he would lean his whole body in the desired direction of travel so that there was no mistaking his intent" (p. 134). And he often "vocalized while gesturing, which served to catch [her] attention and to convey the emotional affect that accompanied each request."

Around the age of two, Kanzi started to incorporate lexigrams into his communicative repertoire. For example, he "started deliberately to select the 'chase' symbol. He would look over the board, touch this symbol, then glance about to see if [Savage-Rumbaugh] had noticed and whether [she] would agree to chase him" (p. 134). Also interesting, on that first day after Matata's departure, when he was left alone at the lab with Savage-Rumbaugh, the first thing Kanzi did with the board "was to activate 'apple,' then 'chase.' He then picked up an apple, looked at me, and ran away with a play grin on his face" (p. 135). Throughout that day he repeatedly "hit food keys, and when [Savage-Rumbaugh] took him to the refrigerator, he selected those foods he'd indicated on the keyboard. Kanzi was using specific lexigrams to request and name items, *and* to announce his intention" (p. 135).

Savage-Rumbaugh's explanation of Kanzi's language development proceeds from essentially the same starting-point as Deacon's; for she too would argue that "if early exposure to language is even part of the explanation for Kanzi's comparatively exceptional language acquisition, then it must be attributable to something about infancy in general, *irrespective of language* (Savage-Rumbaugh & Lewin 1994, pp. 126–27). But, as is clear from the foregoing account, the focus of her argument is on the importance of Kanzi's communicational development for the development of his language skills. In many ways, Kanzi's acquisition of lexigram symbols is reminiscent of the effects of preliterate experiences on a child's acquisition of reading skills; for, typically, the more a caregiver reads to an infant, the more the child understands about the function of the printed word, and thus, the faster the child learns how to read (see Adams 1998). Moreover, one cannot ignore the dominant role of affect in communicative development; for the most effective speech-language therapies for children with severe language delays are those that mobilize the child's affects (see Thomson 2001). Significantly, Kanzi's first "language act" with the board the day after Matata was taken away was not to obtain a food that he wanted to eat but to engage Savage-Rumbaugh in one of his favorite pastimes.

In Savage-Rumbaugh's mind, the most important decision that they made was to "abandon any and all plans of [formally] teaching Kanzi and simply to offer him an environment that maximized the opportunity for him to learn as much as possible" (Savage-Rumbaugh & Lewin 1994, p. 137). This decision demanded that they create new lexigrams for the most important aspects of Kanzi's day-to-day activities: for example, the names of foods, caregivers, other apes, locations in the forest, toys and games. No symbols were inserted solely for the purpose of ascertaining whether Kanzi could grasp some abstract concept. If anything, we should look at the board in the same way that we look at motherese; for the board was not designed to test or to instruct: it was designed to facilitate interactions by providing Kanzi with an artificial communication tool (and a fairly cumbersome one at that). As a result, Kanzi's "communications soon began to revolve around his daily activities, such as where we were going to travel in the forest, what we would eat, the games we wanted to play, the toys Kanzi liked, the items we carried in our backpacks, television shows Kanzi liked to watch, and visits to Sherman and Austin" (p. 139). The conclusion that Savage-Rumbaugh reaches is the opposite from Deacon's: Far from being the result of a spontaneous "mental reorganization," she argues that Kanzi's language development was a prolonged process that occurred because he "was aware that we employed the keyboard as a means of communication and apparently felt keenly motivated to do so as well" (p. 139).

It might be tempting to conclude that one could simply combine these two arguments: that is, treat Deacon's information-processing account as providing the underlying psychological explanation of the communicative development described in Savage-Rumbaugh's dynamic systems approach. But it is important that we recognize just how much the two paradigms differ, not only in their explanation of *how* Kanzi acquired his prodigious abilities, but also in their understanding of the *nature* of those abilities. On Deacon's argument, the "grammar" of the system that Kanzi acquired is something that pre-existed his encounter with it, and the problem that Deacon sets out to answer is: How was Kanzi able to *see* and thus *acquire* this structure? But the problem that Savage-Rumbaugh addresses is: How did Kanzi's verbal skills emerge in the context of, and as a way of augmenting and co-regulating, his nonverbal interactions? How were Kanzi's natural communicative abilities shaped by the co-regulated interactions in which he engaged into linguistic skills: that is, into acts of reference, utterances, truths and falsities, apologies, explanations, corrections, and so forth?

Once again, Savage-Rumbaugh pursues a similar kind of interactional explanation as that which Tomasello (1999) explores in *The Cultural Origins of Human Cognition.* According to Tomasello, "sounds become language for young children when and only when they understand that the adult is making that sound with the intention that they attend to something. This understanding is not a foregone

conclusion, but a *developmental achievement*" (Tomasello 1999, p. 101; our emphasis). This developmental achievement is only realized when "the two interactants share an understanding of each other's interactive goals" in whatever the context in which they are engaged (p. 99). To see how this argument relates to Kanzi's linguistic development we might consider a few scenes from the NHK video, *Kanzi: An Ape Genius: The Cookout.* Kanzi and Savage-Rumbaugh are engaged in a clearly defined joint activity. When asked by Savage-Rumbaugh, Kanzi collects and breaks sticks for the fire. He chooses long sticks to break and picks up short sticks that he does not break. When Savage-Rumbaugh tells him that he can get the lighter from her pocket, he immediately responds by reaching one hand into her pocket. Savage-Rumbaugh stands up in order to make the pocket more accessible. The moment she says he can use the lighter to start the fire, he is already starting to do so. He tries several times to light the fire and only stops, and drops the lighter in the fire, when he sees that the flame has taken. Interesting to note, he chooses to light the paper and not, say, one of the larger branches. He then stares very intently at the growing flames.

4.1. Match-to-sample test

This is one of the most familiar tasks that have been done with Kanzi. The scene begins with Kanzi seated in front of a large lexigram board while Rose is standing behind him, and Sue and a male worker are standing at a small window in an adjacent room around five feet away, giving him instructions through a microphone. The test begins with Kanzi half-turning in his seat to look at Rose, who says "Let's listen some more." He turns back to the board, then orients to the right to the sound of his name, then immediately turns back to the board when Sue says his name, recognizing that this is his cue to look at the board. When he hears the spoken word he quickly points to the correct symbol and Rose says, "Yes." Kanzi vocalizes and it sounds like he too may be trying to make the same sound [yz]. Sue says "ice," and as he points to the key, he vocalizes [ice?]? He and Rose then interact in a play tickle (initiated by Rose). Then the male worker says "balloon": Kanzi can be seen to be leaning towards him at this precise moment, and it is not clear whether Kanzi is leaning in response to the vocalization (perhaps to hear better), or his leaning is cuing the vocalization. He scans the board left-to-right and doesn't move until he has spotted the correct key, and then extends his left index finger. (Throughout this sequence he always points at the selected key with his left index finger. He always withdraws the point as soon as the synthesizer articulates the sound.)

The sequence is repeated, with Kanzi seated before the board but looking over his right shoulder at the male worker until he says a word. Rose says "Good Kanzi" when he gets it right, and he can be seen to dip his head when she says this. The worker then says "chicken" and Kanzi methodically scans the board, left-to-right, right-to-left, then left-to-right again (6 seconds in total) before he sees the key. He does not begin to point until he has found the key. The male worker says "hot dog" and again Kanzi scans left-to-right and points with his left index finger. Now Rose says (for the first time): "Perfecto," and raises her arms and stands with her hands open. She says this with a different tone of voice, which seems to indicate that the task is over, as does the gesture (inviting hug?). Kanzi vocalizes himself, and it sounds like he's trying to imitate her vocalization. Then he turns and starts to get off the chair, suggesting that he too thinks that Rose's vocalization indicated that he was finished. But Sue interrupts saying "one more." He immediately sits back down and turns back to the board. The worker says "grapes" and Kanzi quickly points to the key. Rose has been standing with her arms raised all this time. When Kanzi points to grapes she says, in an even more emphatic tone: "Success." As she says this, she lowers her arms and starts to move towards him. He interprets this as indicating that the task really is over and stands up, but he moves to the window and not to Rose (who is advancing towards him). He looks out the window (to see if Sue is getting him food?) while Rose hugs him from behind. Hugging him Rose says "Good job," with a rising intonation, then repeats "Good job," with a falling intonation. There can be no doubt now that the session is over. He begins to look towards Rose and she fixates directly on his eyes and, with her own eyes very wide open says: "And then we'll get some more grapes." He vocalizes ([yz]?) and Rose, turning and moving away, says "How does that sound?" It is Kanzi's turn to respond excitedly, vocalizing, standing upright, and swinging his arms, which draws Rose back to him. The two of them start to move and vocalize together excitedly, their arm movements and vocalizations mimicking each other.

4.2. Kanzi and Tamuli

In this scene, Sue is seated on one side of a chain-link fence and, immediately opposite her are seated Kanzi and Tamuli, his adoptive half-sister at the LRC who has received little exposure to language. The contrast between Kanzi's and Tamuli's behavior in this scene is fascinating. It starts with Sue repeatedly saying Tamuli's name and pointing at her to get her attention. Then Sue says: "Tamuli, could you slap Kanzi? Tamuli, you (pointing), slap Kanzi." Tamuli does nothing, but Kanzi himself starts to gently slap Tamuli on her back. Then he shakes her arm (which is resting on his leg) and vocalizes quietly. Tamuli is looking all around and seems to regard all this as a game, for there is a large play-grin on her face, and she is uttering play-grunts. Sue tries something else: "Tamuli, could you give Kanzi a hug?" As Sue says this Kanzi leans forward towards Tamuli, lowers his head, and hugs her. Sue laughs, starts to say "Kanzi is . . ." But Tamuli continues to look all around with a play-grin on her face. Sue tries a third request: "Tamuli, could you groom Kanzi?" The moment she says this Kanzi picks up Tamuli's left hand and raises it to his chin while making a facial gesture (rounded lips). He looks intently at Tamuli, but when she fails to respond he shrugs off her arm. But Sue immediately repeats "He's asking you to groom him," and as she says this Kanzi engages in the exact same action, holding Tamuli's hand up to his chin and leaning forward. As he does this, Sue is saying "look, he put your hand up there . . . Isn't that nice?" But again Tamuli doesn't respond, and Kanzi drops her hand. As he does so, Sue says "look, he's showing you." But at this point Tamuli has lost all interest in this game and moves off to play with someone else, while Kanzi looks after her, then turns back to Sue and receives a treat.

In all of these scenes (and several other examples in *Kanzi: An Ape Genius*) we see Kanzi totally engaged in a joint activity for a sustained period. There is a seamless web

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of communication between him and his caregiver in multiple modalities and not just through spoken English. Much of the communication occurs without Kanzi looking directly at the researcher as she speaks. In the first of the three episodes, Kanzi clearly understands what he is being asked to do. He knows what size of stick is needed for the task and roughly how many sticks are needed. He understands that Savage-Rumbaugh isn't just informing him that there is a lighter in her pocket, but that she is giving him permission to fetch the lighter from her pocket in order to start the fire.

The second scene demonstrates how Kanzi can attend to multiple speakers and even grasps whom he should respond to at any given moment. The task itself is clearly something he can easily perform despite the complexity of the array. In a couple of cases it is clear that he doesn't know where the symbol he needs is situated, but rather than searching randomly, he systematically scans the board until he's found the right symbol. The manner in which this session ends is also interesting: rather than any one of the participants formally signalling an end to the task, we can see them negotiating with each other before they are satisfied that they have reached a closing.

In the final scene Kanzi recognizes immediately that Tamuli has not understood what she is being asked to do. It is significant, not only that he wants to help her, but also, that he repeatedly tries to show her what she is being asked to do. The difference between the manner in which this session and the previous scene ends is also interesting. Her attention no longer engaged, Tamuli simply wanders off; but Kanzi remains seated with Sue and turns back to her, at which point they jointly negotiate an end to the activity.

In each of the preceding three paragraphs we have relied heavily on the use of epistemic operators to describe Kanzi's behavior. To appreciate how such an argument differs from an information-processing approach, one has to go back to the argument introduced in the preceding section about the language-games in which these terms are used. For the argument here does not turn on what may or may not have been going on inside Kanzi's mind. Rather, the crux of this argument is that Kanzi behaves in a manner that satisfies the criteria for describing him as "*understanding* what he is being asked to do"; as "*knowing* that Savage-Rumbaugh is giving him permission to fetch the lighter"; as "*recognizing* that Tamuli has not understood what she is being asked to do"; and so on. For example, we do not *infer* that Kanzi understands Savage-Rumbaugh's request to "Go wash the potatoes, cut them up, and put them in a pot on the stove" when we observe him turn on the kitchen faucet, wash the potatoes, fetch a knife, cut the potatoes into pieces, place all these in a pot, fill the pot with water, and then place this on the stove. This behavior is just what is called "understanding the request." If one insists on further criteria before one is prepared to sanction this description of Kanzi's behavior (e.g., that Kanzi is able to do this with different kinds of vegetables, that he can pick out a potato from a table laden with different kinds of vegetables, that he understands the relation between *potato* and *vegetable*, etc.), this merely signifies that the application of "understands the request" may sometimes demand greater behavioral complexity than might be afforded by a simple or a single comprehension task, and not that comprehension is some epistemically private mental process or state underlying an organism's behavior.

There are many other scenes that we could have discussed that demonstrate equally striking examples of linguistic behavior. Scenes in which Kanzi can be seen to correct himself, or explain something, or apologize for an action; scenes in which he engages in pretend-play, or more formal types of games (like Pacman); scenes in which he engages in both imitative and creative problem-solving and tool-making; or in which he seemingly understands what someone is thinking or feeling; scenes in which he empathizes with someone else's feelings or mood. In other words, the behavior that we observe in this video is very much, as Deacon emphasizes, like that of a three-year-old child.

For this very reason, Kanzi has been widely perceived as something of an anomaly in ALR. His "prodigious abilities" have been viewed – and in some cases even dismissed – as a misleading indicator of nonhuman primate communicative capacities, precisely because of the unusual circumstances in which he was raised; that is, in which he was literally "raised" to an "unnatural" cognitive and communicative level, because he "receive[d] a kind of 'socialization of attention'" (Tomasello 1999, p. 35). That is, "responding to a culture and creating a culture *de novo*" are seen as "two different things" (Tomasello 1999, p. 36). As far as we know, "apes in their natural habitats do not have anyone who points for them, shows them things, teaches them, or in general expresses intentions toward their attention" (p. 35).

Elsewhere (King & Shanker, in press), we look in some detail at the issue of whether, or to what extent, great apes in their natural habitats have displayed elements of such behaviors. The point we would like to focus on here is Tomasello's suggestion that when apes are raised in a human-like cultural environment, in which "they are constantly interacting with humans who show them things, point to things, encourage (even reinforce) imitation, and teach them special skills," they experience a "socialization into the referential triangle – of a type that most human children receive – that accounts for the special cognitive achievements of these special apes" (King & Shanker, submitted, p. 35). Indeed, one of the principal effects of being deprived of these *socializing experiences* – whether because of endogenous or exogenous reasons – is that both a child and an ape develops the sorts of social and communicative deficits that are labelled "autistic" (Greenspan 1997; Harlow & Zimmerman 1959).

As we saw in the preceding section, Tomasello talks about the "nine-month revolution" that occurs when an infant starts to "'tune in' to the attention and behavior of adults toward outside entitites" (Tomasello 1999, p. 62). From this point on, the infant's communicative behavior is marked by gaze following, extended bouts of social interaction, joint engagement, social referencing, imitative learning, and deictic pointing (directed gaze, imperatives, declaratives). It seems likely that the regularities observed in the appearance of these behaviors are related to "critical periods" in the child's neurobiological development (see Johnson 1997). But then, one must not overlook the importance of a caregiver's behavior (e.g., smiling, facial animation, body posture, gaze, etc.) during these "critical periods" for the child's cortico-cortical and neurohormonal development (see Schore 1994). That is, here too, the significance of the *developmental manifold* cannot be divorced from what might appear to be strictly maturational events (see Gottlieb 1997).

Moreover, every one of the above communicational behaviors has been observed in Kanzi. This suggests, not only that the development of these abilities is not confined to humans, but more important, that the development of these abilities is not genetically predetermined. Rather, the child's growing ability to engage with her caregivers in complex communicational activities, express her intentions and desires, and describe and express her ideas and feelings, all develop in the context of close dyadic relationships with her primary caregivers. And herein lies the crux of the dynamic systems alternative to Deacon's information-processing view about what it is "about infancy in general, *irrespective of language*," that enables a primate to develop language skills.

We are not concerned here with what might or might not have gone on "inside Kanzi's head" that enabled him to develop language skills; nor is language viewed as a combinatorial system whose "structure" he had to "grasp." Rather, language is viewed as a particular type of reflexive activity in which Kanzi was enculturated (see Shanker 2001; Taylor & Shanker, in press). The psychological issue we are concerned with here is how Kanzi's attentional capacities, his use of lexigrams, and his comprehension of spoken English, developed as a result of being nurtured in language-enriched interactions with his caregivers. We saw in the opening section how, in the dynamic systems paradigm, communication is viewed as a "continuous unfolding of individual action that is susceptible to being continuously modified by the continuously changing actions of the partner" (Fogel 1993, p. 29). Hence, the ability to attend to another subject's actions – and all of the other executive functions (see Russell 1997) – are both vital to, and continue to develop as a result of, the "communicational dances" in which the infant engages with her caregivers.

On this line of thinking, what the research with Kanzi ultimately shows us is how an infant's cognitive and communicative development involves an ongoing and complex interplay between biological, social, and cultural factors, rendering it not just difficult, but conceptually misguided, to attempt to draw a hard-and-fast distinction between a child's communicative and her linguistic development. Language does not suddenly appear at some predetermined age, or in predetermined stages, but rather, gradually emerges as a means of co-regulating and augmenting such primal activities as sharing, requesting, imitating, and playing. The child or ape is increasingly motivated to use and develop these potential communicational tools *so that* she may achieve context-dependent, interactional goals: goals which themselves develop as a function of the child or ape's developing communicational environment, and her or his growing abilities and increasingly differentiated affects.

5. The implications of the new paradigm

The basic premise of the information-processing paradigm which has hitherto governed the study of ape communication is that, given that great apes are "penultimate" to humans (at least, in the existing natural order), we can expect them to possess sophisticated, but not quite fully human communicational abilities: for example, conscious and intentional, but not normative or creative. If one adopts a Piagetian model of cognitive development, one would expect to find that great apes consistently perform at an early stage of human cognition: for example, the sixth sub-stage of sensori-motor development. That is, one would expect to find them confined to a point that is just prior to when human infants burst forth into the world of creative thinking, human social cognition, and language.

The mind of the great ape is thus thought to be at the stage where it is just starting to become purposive and intentional, but is still characterized by poor abstract thinking (e.g., perceptually-bound) and is highly egocentric (see Parker & McKinney 1999). Hence, the challenge rendered by the information-processing paradigm is to establish the fixed parameters of great ape communicative behaviour: for example, the basic manner in which they communicate (both send and receive/respond); the kinds of messages that they communicate; the stimuli that prompt them to communicate; and the functions of their communications. It is thought that, in answering these questions, we shall deepen our understanding of great apes' cognitive and communicative capacities and perhaps thereby illuminate the more "primitive" elements of human communication that underpin or accompany linguistic communication; that is, those aspects of human communication that are said to be "paralinguistic" (see Argyle 1988).

The problem with the information-processing paradigm is that, not only does it set an upper limit on the communicational complexity of the species being studied, it also shapes how that complexity is conceptualized. In the early days of information-processing studies, when great apes were still being viewed as stimulus-bound creatures, it was natural enough to think of their communicative behaviors in linear terms. But it is difficult to see how great apes could engage in the complex and sustained interactions that have been documented over the past decades if their communicative behaviors were limited to a signal/response mode. The idea of creative communicative behaviors emerging from such discrete interactions is rendered virtually impossible; but some of the most striking advances in recent years have been in regard to learning just how creative is the mind of the great ape (see Allen & Bekoff 1997).

As useful as the information-processing paradigm might be for cybernetic systems, or for some simple organisms, it is difficult to account for the kinds of communicational complexity that we observe in nonhuman primates in terms of turn-taking sequences in which the subject is either sending or receiving, either active or passive. For, great apes are continuously active when communicating, continually adjusting their gestures and vocalizations to one another (see Johnson 1993; 2001). The quantity, type, and manner of gesturing vary with age, social conditions, activity, and the subject's wishes. Species-typical gestures, such as slapping, clapping, pounding, and chest-beating, have been seen to vary from one group to another, from one individual to another, and from one age to another (Tanner & Byrne 1999). Infants have demonstrated a growing awareness of the communicative significance of their own gestures (King 2000; Parker & McKinney 1999). Individuals have been observed to create new gestures, and possibly, to generalize these idiosyncratic gestures to other communicational situations (Tanner & Byrne 1999). Individuals have been observed to restrain their supposedly innate and automatic gestures (Tanner & Byrne 1999). And extremely subtle variations have been observed in "species-typical" behaviors: for example, whether an ape touching another moves its hand vertically or horizontally;

quickly or slowly; lightly or more heavily; with a pushing or a pulling motion.

Furthermore, we have to absorb the implications of the startling advances that have been made in ALR. In addition to the surprising number of symbols and syntactical patterns that bonobos have mastered, it is also important to note that they have demonstrated the ability to play games that are based on complex rules; engage in sophisticated make-believe and role-playing; solve complex tasks imitatively and creatively; perform remarkably well on match-to-sample tasks (even when the instructions are delivered through earphones or by different speakers); deal easily with simple Theory of Mind tasks; and even engage in normative behaviors such as justifying or explaining their own actions, or trying to teach or correct another ape's actions (see Savage-Rumbaugh et al. 1998; Shanker & Taylor 2001). One might object that such studies tell us little about natural great ape abilities, insofar as it is only human intervention that has enabled apes to rise to these cognitive and communicative levels (see Tomasello 1999). But then, that is surely the point of such studies; for by demonstrating the plasticity of great ape capacities, we are learning about the significance of caregiving practices for nonhuman primate development.

Taking all these factors together we can see how, far from being fixed and invariant, great ape communicative behaviors in the wild, as well as in research facilities, are carefully nurtured and culturally variable. In place of the information-processing model that has hitherto dominated the study of ape communication and ALR, therefore, we believe that it is imperative that we shift to a dynamic systems paradigm, which places the emphasis on the dyad rather than on the isolated individual; which sees great ape communication as a co-regulated activity rather than a linear and discrete sequence; which focuses on the creativity of ape communicative behaviors rather than treating them as phenotypic traits; which is better able to account for both the social complexity and the developmental character of nonhuman primate communicative abilities; and which looks at how language skills emerge as a means of co-regulating and augmenting such primal activities as sharing, requesting, imitating, and playing.

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NOTES

1. We follow conventional practice in referring to the caregiver-infant *dyad* as the primary context for infant development, but endorse Fivaz-Depeursinge and Corboz-Warnery's (1999) approach to studying *triadic interactions* in early infancy.

2. As in the case in child developmental research, we see ample evidence in the primatological literature of the effects of good versus poor caregiving on an infant's development. In particular, as we shall discuss below, Savage-Rumbaugh's work with the chimpanzees Sherman and Austin, and with the bonobo Kanzi, has dramatically demonstrated just how significant the effects of caregiving can be on an ape's communicative development (*infra*). Primatological research is amassing ever more extensive evidence of social modifiability in several different modalities (see Seyfarth & Cheney 1997; Snowdon 1999).

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Ethnography should replace experimentation

David F. Armstrong

Editor, Sign Language Studies, Gallaudet University, Washington, DC 20002.
David.Armstrong@gallaudet.edu

Abstract: This paper points to the need in ape language research to shift from experimentation to ethnography. We cannot determine what goes on inside the head of an ape when it communicates with a human being, but we can learn about the nature and content of the communication that occurs in such face-to-face interaction. This information is fundamental for establishing a baseline for the abilities of an ape-human common ancestor.

> *Bishop Berkeley hinted darkly,*
> *if I can't see you, you can't be you.*

I begin my commentary with this bit of philosophical doggerel because it reflects the important epistemological and methodological issues that are raised by Shanker & King (S&K). Moreover, it points to an equally important question of perception and judgment that their article also attempts to answer. The epistemological issue concerns the impossibility of knowing what goes on inside an ape's head when it engages in communicative behavior with other apes or with human beings. Critics of ape language research (ALR) generally raise this issue and go beyond it by asserting that apes cannot have the sort of mental representation systems that are presumably required to support the use of language. Understanding these putative systems in human beings has been a major goal of cognitive science. S&K want to move us beyond this epistemological barrier by proposing that, at some level, the question is irrelevant. This may be especially true for those of us interested in how the communicative behavior of apes bears on the origins of human language. We will never know what went on inside the heads of australopithecines or early representatives of genus *Homo* either, but it is vitally important to know what sorts of *behaviors* these hominids might have been capable of, and ALR can help us to establish a baseline.

In proposing that we move from an information-processing to a dynamic systems paradigm, S&K are also hinting at a methodological shift in ALR from formal experimentation to ethnography as the principal means of gathering data, a position that has been argued forcefully by Savage-Rumbaugh (1999). Ethnography is here construed as "participant observation." We may not be able to get inside the ape's head, but at least we can enter into its social circle in a meaningful way, and that is the location from which shared meaning emerges. It is only by interacting with another human being or with an ape that we can judge whether or not communication has taken place. As S&K put it (target article, sect. 4.2) when evaluating Kanzi's appropriate responses to complex requests: "This behavior just is what is called 'understanding the request.'" For many years now, anthropology has insisted that a great

deal of this sort of interaction must precede any judgments about the language and culture of exotic groups of humans.

Let us now move to the questions of perception and judgment that I introduced above. How do we perceive apes, how do we judge their behavior, and why are these questions important? I begin with this quotation from Sapir (1921) on the signed languages of the deaf:

> Still another interesting group of transfers are the different gesture languages, developed for the use of deaf-mutes, of Trappist monks vowed to perpetual silence, or of communicating parties that are within seeing distance of each other but are out of earshot. Some of these systems are one-to-one equivalences of the normal system of speech; others, like military gesture-symbolism or the gesture language of the Plains Indians of North America (understood by tribes of mutually unintelligible forms of speech), are imperfect transfers, limiting themselves to the rendering of such grosser speech elements as are an imperative minimum under difficult circumstances. (p. 21)

This is interesting because it represents two prejudices that were shown to be wrong by Stokoe (1960) and subsequently by many others: Language is spoken and the signing done by deaf people is simply a substitution code for speech (if it is even that sophisticated). An early goal of sign language linguistics was to prove to a skeptical world that these languages of the deaf were full-fledged human languages, and the early descriptions of them tended to stress their structural similarities to speech, while asserting that they were not simply codes for particular spoken languages (see Taub 2001, p. 3). In a linguistics dominated by the generative approach, this was taken as further proof of the existence of universal grammar because these languages were expressed in a novel medium.

It was only possible to learn how non-Western societies actually functioned by going out and participating in them in situ. And only the involvement of linguists fluent in sign, including deaf signers, could lead to the fuller and richer descriptions of signed languages and their cognitive and neural underpinnings that are now emerging (Emmorey 2002; Taub 2001; Wilcox 2000). If we had accepted the prejudgments of Sapir (1921) we would not have bothered to study these languages in the first place, and if we had accepted the prejudgments of a later generation of grammarians there would have been no need to study them in depth because we could have assumed that they were simply products of universal grammar, hence just like spoken languages. The truth has turned out to be vastly richer and more interesting than either of these sorts of assumptions would have led us to believe.

Non-Western people and deaf people living in industrial societies are all members of species *Homo sapiens,* and we *should* be willing to license their communicative behavior as linguistic. But we should recognize, by the same token, that only fairly recently have the human "sciences," as developed by Westerners, been willing to so license their behavior. Previously, their behavior, communicative and otherwise, was dismissed as inferior, and it was easy for European scholars to find justifications for this. So, we should be suspicious of similar offhand dismissals of the behavior of animals that are as closely related to us as chimpanzees and bonobos.

What can all of this tell us about the status of ape-human interaction and ALR? Linguistics could be properly defined as the study of human languages, and language could be defined as something that is only done by members of species *Homo sapiens.* But to adopt this position also seems like something of an intellectual cop-out. Of course, human language must be different from whatever apes do when they communicate among themselves or with human beings. S&K have moved us considerably in the direction of understanding what it is that occurs during these interactions, and they have rightly insisted on the importance of acquiring this knowledge.

A new paradigm?

John D. Bonvillian[a] and Francine G. P. Patterson[b]

[a]*Department of Psychology, University of Virginia, Charlottesville, VA 22904-4400;* [b]*The Gorilla Foundation/koko.org, P. O. Box 620530, Woodside, CA 94062.* **jdb5b@virginia.edu penny@koko.org**
The Gorilla Foundation/Koko.org (www.gorilla.org)

Abstract: Shanker & King argue for a shift in the focus of ape language research from an emphasis on information processing to a dynamic systems approach. We differ from these authors in our understanding of how this "new paradigm" emerged and in our perceptions of its limitations. We see information processing and dynamic systems as complementary approaches in the study of communication.

Shanker & King (S&K) assert that a new research approach has emerged, one in which the interaction between organisms is conceived as continuously changing, multifaceted, and mutually coordinated (a dynamic systems approach), as opposed to the linear, rule-governed information-processing approach. The authors depict this dynamic systems approach to language as a partnered dance and recommend a research focus on the interactional synchrony of communication partners.

How new is this approach? The importance of focusing on the interaction between a caregiver and her infant has a long history in developmental psychology, particularly in attachment research. Nearly 50 years ago Mary Ainsworth pioneered this approach in her longitudinal studies of child development. Earlier investigators of maternal caretaking typically followed one of two approaches: making global impressionistic accounts of the quality of care, or counting the number of discrete behaviors produced by a caregiver. The latter approach had advantages over the former in that counting discrete behaviors often resulted in high intercoder agreement and numerical data amenable to statistical analyses. Ainsworth's important insight was that a critical aspect of a caretaker's behavior is not the total number of times a caregiver produces a particular behavior, but whether the caretaker produces the behavior as a response to the infant's signals. The research focus thus should be on the infant's signals and the caregiver's responses to these signals.

Ainsworth and her associates subsequently showed that a child's development is dependent in part on the nature of the interaction between mother and infant. Infants typically experienced more or less successful outcomes depending on the nature and quality of their interaction patterns with their caregivers. This focus on the nature of the interaction between caregiver and child also proved effective in discerning maltreating parents (Crittenden 1981) and in the development and assessment of intervention programs for maltreated children.

Although S&K allude to Ainsworth's contribution (cf. Ainsworth et al. 1978), no mention is made of the pioneering research in interactional synchrony in psycholinguistics. Beginning in the early 1960s, William Condon conducted studies using very fine-grained analyses of the movements of a speaker and listener in accord with speech production. Condon showed that there are numerous movements by the speaker (self synchrony) and the listener (interactional synchrony) in association with the speaker's utterance. This research approach not only underlined the precise and complex pattern of human communication, but also provided compelling evidence that the processing of spoken language input to children with autism may be markedly abnormal (Condon 1976).

If Condon's approach of examining self and interactional synchrony was such a substantial breakthrough in the understanding of communication, then why was it not adopted more widely by investigators? We speculate that one important reason is that carrying out the microkinetic analyses involved is extremely time-consuming. Condon often made dozens of measurements of different head, limb, and body movements in association with vocal production for each second of film. Although these interactional- and self-synchrony approaches revealed a very rich psycholinguistic fabric in human communication, many investigators may have been reluctant to invest the numerous hours needed to conduct such de-

tailed and complex analyses. Other reasons for not embarking on this approach may be the lack of agreement among investigators about the size and scope of the processing units that should be analyzed, confusion about the statistical procedures to employ, and lack of awareness about the interactional-synchrony approach.

If S&K want additional investigators to use the interactional-synchrony approach, then they will need to clearly delineate the methodological techniques that investigators should employ. This information is absent from their paper. Should one use the same microkinetic analyses as Condon? Defining and analyzing the nature of the interactional units is an important and difficult step, but one that is necessary before such an approach will be widely adopted.

The title of the target article ("The emergence of a new paradigm in ape language research") seems to imply that the focus on interactional synchrony is new (it actually has a long history in psycholinguistics) and that it will supplant information-processing approaches. We doubt that the latter will happen. The information-processing approach has been an effective methodological approach in linguistics for a long time. The interactional-synchrony approach underscores the richness of communication, but it is limited in many linguistic analyses. Rather than seeing the two approaches as incompatible, we see them as complementary.

S&K also advance the idea that the best way to study the linguistic behavior of apes is by abandoning rather artificial training plans and offering a more natural, immersive, and nurturing learning environment. The authors champion Savage-Rumbaugh's work as being a watershed in this regard. Yet, other investigators in ape language research, particularly those employing sign language (e.g., Gardner & Gardner 1994; Miles 1997; Patterson 1980; Patterson & Linden 1981), reached this conclusion long before. Their work has much to offer to this discussion.

Finally, we have reservations about the use of an interactional-synchrony approach with the lexigrams used in Savage-Rumbaugh's research. The interactional-synchrony approach in psycholinguistics emerged from analyses of speech and the listener's movements associated with its production. To the extent that sign language use involves the sequential production of linguistic symbols in a communicative setting, we would expect it to be a worthwhile candidate for interactional synchrony analyses. We are not as confident about whether microkinetic analyses of a communication partner's movements would yield equivalent information when lexigrams are the linguistic units. Lexigrams are relatively discrete units that are activated by touch and do not vary in production characteristics. Before recommending analyses of interactional synchrony through investigations involving lexigrams, the authors should examine how the information obtained from lexigrams resembles that obtained from other language modes. Moreover, whereas speech and sign communication emerged within human social interaction over many eons, the same evolutionary communicative history would not apply to lexigrams. Even in spite of our concerns, however, we believe that the interactional-synchrony approach, properly applied, will make an important contribution to ape language research.

Language evolution in apes and autonomous agents

Angelo Cangelosi

Plymouth Institute of Neuroscience and School of Computing, University of Plymouth, Plymouth PL4 8AA, United Kingdom.
acangelosi@plymouth.ac.uk
http://www.tech.plym.ac.uk/soc/staff/angelo

Abstract: Computational approaches based on autonomous agents share with new ape language research the same principles of dynamical system paradigms. A recent model for the evolution of symbolization and language in autonomous agents is briefly described in order to highlight the similarities between these two methodologies. The additional benefits of autonomous agent modeling in the field of language origin research are highlighted.

The shift from information processing to dynamical system paradigms has recently interested numerous research fields, in addition to that of ape language. In particular, the interdisciplinary field of cognitive and behavioral modeling has not only undergone such a change but has greatly contributed to its development. This has been strongly influenced by the development and dissemination of computational approaches based on autonomous agents. Such methodologies have been employed in the study of evolutionary and adaptive behavior (Meyer & Wilson 1991), cognitive science (Beer 2000; Parisi 1997), neuroscience (Ruppin 2002), and language research (Cangelosi & Parisi 2002; Steels & Kaplan 2000). The autonomous agent methodology is characterized by the following principles: (1) agents are situated and embodied systems, such as robots, continuously interact in their environment; (2) the environment is in constant change due to the action of agents, the dynamical properties of objects in it, and evolutionary and learning phenomena; and (3) complex behavioral and cognitive patterns emerge from this mutual interaction among agents and between agents and environment. Additionally, in models used for studying language, the communication between agents is the result of a dynamical process depending on the sensorimotor, social, cognitive, and neural abilities of autonomous individuals.

All of these principles are remarkably common with the properties and examples of the ape language dynamical system paradigm discussed by Shanker & King (S&K). It is therefore surprising that the authors do not acknowledge the contribution of this computational approach. As a matter of fact, a recent autonomous agent model of language evolution (Cangelosi 2000; 2001) has directly referred to the relationship of ape language studies and the evolution of symbolization. This model is briefly summarized here and is used as an example that reinforces the similarity between the dynamical systems paradigms of ape language research and that of evolutionary autonomous agents. In addition, the benefits of this comparative approach for language origins and evolution are highlighted.

Some experiments on language acquisition in chimpanzees have explicitly investigated symbolization abilities in apes (Savage-Rumbaugh 1986; Savage-Rumbaugh & Rumbaugh 1978; Savage-Rumbaugh et al. 1980). These studies make a clear operational distinction between nonsymbolic and symbolic languages. When animals use language in a nonsymbolic way, they simply learn conditional associations linking signals and objects. Alternatively, real symbolic languages are based on the learning of symbolic (e.g., syntactic) relationships between communication signals (Deacon 1997; Harnad 1990). Ape language studies have consistently shown that chimpanzees can learn real symbolic relationships, although only under specific experimental conditions. Moreover, it has been shown that apes can also invent new symbolic rules themselves (Greenfield & Savage-Rumbaugh 1990). These data, and other literature on symbolization, were used by Deacon to propose that the gap between spontaneous animal communication systems and human languages is explained by significant differences between the cognitive abilities and neural structures of such species. Only animals that are evolutionarily close to humans, such as apes, can partially overcome some of their cognitive and symbolic limitations, under specific and extensive experimental conditions.

Autonomous agent modeling has been recently used to compare the evolution of language in apes and simulated agents (Cangelosi 2000; 2001). This model investigates the symbol acquisition abilities of evolutionary autonomous agents. An artificial neural network constitutes the "brain" that controls the agent's behavior. The task performed by simulated organisms is directly inspired by well-known ape language experiments (Savage-Rumbaugh & Rumbaugh 1978; Savage-Rumbaugh et al. 1980). A population of agents evolves according to their ability to navigate and forage in a simulated mushroom world (Cangelosi & Harnad 2000). In addition, agents undergo a language acquisition stage during which they learn to name foods by listening to their own parents. They learn the names of three edible food categories and also a com-

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mon verb for the edible mushrooms. The same learning takes place for three categories of toadstools. An important aspect of such a model is that researchers do not impose a predefined lexicon. Instead, each population evolves its own set of meanings and the corresponding signals. This is achieved through the dynamic learning interactions between children and parents, and through the interdependence between the evolving behavioral abilities (e.g., categorization of mushrooms) and the learned linguistic skills.

The simulation results showed that populations evolve shared lexicons that optimally facilitate the foraging task. The majority of such languages are compositional. They contain two words to name the action (e.g., avoid/approach) and three words to name the three individual categories of mushrooms. To test whether these apparent compositional languages are actually based on real symbolic relationships, a symbol acquisition test, similar to that in Savage-Rumbaugh and Rumbaugh's (1978) chimpanzee experiments, was used. The test consisted of three learning stages. In the first stage, organisms learn to label only four foods (two edible and two poisonous mushrooms). Subsequently, they learn to associate the four names with two new verbs. In the final stage, the names of the remaining two foods are taught. The association with previous verbs is not explicitly taught because, in a real symbolic language, the logical relationship between new names and verbs is expected to be made by generalization. Data showed that the majority of populations successfully generalize the association of verbs with new names, thus demonstrating that a real symbolic language has been acquired.

This simulation shows that it is possible to build autonomous agent models that manifest behavioral, cognitive, and neural phenomena similar to those observed in experimental studies. In addition to sharing with new ape language research the benefits of a dynamical system paradigm, this modeling approach provides other advantages. This is especially true in the field of language origin research. Computational models require that language origin theories be defined in clear operational terms (necessary to implement the computer program) so that hypotheses can be verified during simulation (Cangelosi & Parisi 2002). Autonomous agent models permit the simulation of past language origin scenarios by manipulating various evolutionary, behavioral, neural, and social variables. This also helps in overcoming the limits of other computational approaches, such as classical connectionism (Rumelhart & McClelland 1986), which can only study ontogenetic changes. Finally, constraining the models to known empirical facts related to language evolution (Tomasello 2002) produces a virtuous circle. Models generate new predictions and insights, and subsequent experimental studies verify them and generate new predictions, which can be tested again in simulation.

Dynamic systems theory places the scientist in the system

Alan Fogel, Ilse de Koeyer, Cory Secrist, and Ryan Nagy

Department of Psychology, University of Utah, Salt Lake City, UT 84112-0251.

{alan.fogel; ilse.dekoeijer; cory.secrist; ryan.nagy}@psych.utah.edu
http://www.psych.utah.edu/alan_fogels_infant_lab/

Abstract: Dynamic systems theory is a way of describing the patterns that emerge from relationships in the universe. In the study of interpersonal relationships, within and between species, the scientist is an active and engaged participant in those relationships. Separation between self and other, scientist and subject, runs counter to systems thinking and creates an unnecessary divide between humans and animals.

What does it mean to have an individual mind? One could say it means to have the ability to think and reason, to make decisions independently of others, and to have a unique point of view and the ability to formulate intentions and actions consistent with that point of view. But most human minds work along the channels of thought and reason acquired by speaking and acting with other people (Piaget 1965; Vygotsky 1978). Our "private," "original," and "independent" thoughts are always phrased in cultural lexicons of imagery, myth and story, gesture, language, or mathematical formalism. To demonstrate the independence of our minds, we say things, do things, and build things with words, gestures, and materials that are sociocultural in origin.

Being-in-relation, participating in an interpersonal relationship, is a fundamental, irreducible, primary, way of being. Individuals are born into interpersonal relationships. We never, not for a single moment of life, exist outside of relationships even when we are physically alone. Our thoughts, our movements, the artifacts carried with us are all grounded in cultural processes that were conceived, composed, and codified by individuals-in-relation (Fogel 1993).

It is ironic, then, that Western cultural and linguistic imagery gives the illusion that individual minds take precedence over being-in-relation. As scientists of mind and communication, we can use the cultural lexicon to distinguish human and animal worlds, self and other, inner and outer, emotion and cognition, and verbal and nonverbal. On the other hand, as human beings immersed in the act of communicating, such distinctions become arbitrary and meaningless. Shanker & King (S&K) suggest that we can either gain knowledge *about* the phenomenon under study or – by our own experience – gain intimate knowledge *of* the same phenomenon (James 1890). Is it possible to be a scientist while relating to our subject matter as a fully participating human being?

S&K compare and contrast two paradigms aimed at answering such scientific inquiries. They observe the behavior of individualistic world-view thinkers who come back from their solitary journeys into the mind and attempt to communicate their insights. Deacon is chosen to represent this style of science, studying phenomena from a distance, attempting to break them into independent parts, and subsequently reassembling them into a model of reality. Deacon is asking, "What are the limitations of an ape's language capacities? What abilities for communication does an ape have?"

Savage-Rumbaugh, on the other hand, was not content to approach apes from a distance. She asked, "What are the possibilities for connection between me and Kanzi? How can I change myself in order to deepen the relationship between us?" Kanzi and the other apes become active participants in a courageously alive interpersonal relationship – not separate minds contemplated from afar. Savage-Rumbaugh entered the apes' world by altering her behavior both physically and emotionally. She gave the apes the opportunity and tools to communicate, learn, and grow with a wonderfully engaged partner. By allowing herself to be moved and changed, she demonstrated the emergence of communicative capabilities in nonhuman species in ways that were heretofore unimagined.

The fundamentally different paradigms of Deacon and Savage-Rumbaugh represent different theoretical approaches: information processing versus dynamic systems. These paradigms also afford entirely different kinds of knowledge: knowledge from reason and knowledge from the fully human experience of direct engagement.

Dynamic systems perspectives assume a fundamental relatedness at the heart of the universe, implying that the scientific observer is part of this relatedness. But this raises a central question. Having gone into a direct relationship with the apes, what kind of a scientific story can be told? From a dynamic systems perspective, it is sufficient to know with whom, how, and under what conditions individuals can relate with and connect to each other. For example, Smuts (2001) described surprising and mutually enriching encounters with creatures as diverse as dogs, baboons, birds, and rodents that emerged from the scientists' "sensitivity and humility" (p. 301).

Dynamic systems is not a new behaviorism. It is not uninter-

ested in mental and emotional processes. Rather, dynamic systems suggests that knowledge of the other can arise only in relation to the other. Acts of separation run counter to dynamic systems thinking, creating a sense of human *versus* apes rather than humans *with* apes. Thinking about the ape's mind in the absence of a close relationship is not the same as the direct experience of another mind through sharing actions and feelings, such as playing games with mutual delight or aligning intentions to achieve a common goal. These shared experiences give the observer a sense of direct certainty that she is engaging with another intentional being.

We now know that Kanzi and other bonobos can – with an engaged human partner – comprehend spoken English, produce English-based speech sounds, make stone tools, and write lexical symbols (Savage-Rumbaugh et al. 2001). It is difficult to imagine how these scientific discoveries could have been made outside of a relationship of engagement and mutual commitment between scientist and ape. The theory arises from this observation. Communicative skills like language and gesture, within and between species, can only emerge by engaging in meaningful interpersonal relationships built up over time (Bruner 1983; Fogel 1993). The telling of what happened in those remarkable relationships is all one needs to know in order to replicate the findings – that is, to recreate a similar relationship.

But this is not the whole story. As Kanzi and the other apes changed in relation to the scientists, those scientists changed in relation to the apes. Instead of being humans who viewed animals as separate and different, they became humans who changed their ways in order to invite animals into the realm of engaged, intelligent, feeling beings. This is a form of moral courage: to open our own minds to change, to expand what it means to be human by acts of love that transcend the ordinary.

The proper study of chimpkind

R. Allen Gardner

Department of Psychology, University of Nevada, Reno, NV 89557.
gardner@unr.edu

Abstract: The target article issues a stirring call for more appropriate treatment of chimpanzees in experimental laboratories. This commentary heartily endorses that position with examples of methods and results found in sign language studies of cross-fostered chimpanzees.

It is heartening to read this eloquent call for more appropriate research with great apes. Through nearly three decades of articles and books, Beatrix Gardner and I, and our collaborators, have insisted that judicious comparisons between chimpanzee and human intelligence depend on comparable laboratory conditions. Savage-Rumbaugh's (1984) published description of her work with Sherman and Austin seems to contradict the description in the target article. If Savage-Rumbaugh has indeed treated Kanzi with the warmth described in the target article, that would be a step beyond the usual laboratory conditions of caged apes. But, it would fall far short of cross-fostering in which infant chimpanzees are maintained from birth under nearly human conditions.

Washoe at about 9-months, followed by Moja, Pili, Tatu, and Dar within days of birth, entered a laboratory of rigorous cross-fostering. They had all the usual toys, games, and picture books that human infants get. They lived in quarters in which humans had lived, ate human food at tables from dishes with forks and spoons, and drank from cups. They also helped set and clear their tables, clean their quarters, and put away their playthings. They dressed and undressed themselves and used human toilets, wiping themselves, flushing, even asking to go potty during lessons and naptimes. Most important, at least one human member of their foster families stayed in close attendance throughout their waking hours. Never caged, they were as free as human infants to move about the world with supervision. When they slept, human family members listened on intercoms to comfort troubled infants during the night (Gardner & Gardner 1989).

We, Gardners, always acknowledged our debt to Kellogg and Kellogg (1933) and Hayes and Hayes (1951), who pioneered rigorous cross-fostering. Chimpanzees lack human vocal apparatus and vocal habits, dooming the Kellogg and Hayes projects because rigorous cross-fostering entails conversational give and take in a common human language. Our innovation was to use American Sign Language, a naturally occurring human language without speech. A common human language is essential for comparisons with human children who grow up with spoken and signed languages (Gardner et al. 1989).

The only advantages of Rumbaugh lexigrams are those claimed for other synthetic studies of "ape language." It is impossible to know what relationship, if any, these synthetic codes have to natural languages that human children acquire in homes. Unlike Washoe, Moja, Pili, Tatu, and Dar, who could sign to themselves and to each other, Kanzi needed a lexigram panel and a human interlocutor. Excursions from the computer room required a portable panel with painted patches for lexigrams. When a human attendant unfolded the panel, Kanzi could touch the lexigram patches (Savage-Rumbaugh et al. 1986). Human observers reported which patches he touched. In Kanzi's fourth year, a videotaped sample of sessions with the portable panel showed 80% agreement between observers reading Kanzi live and observers reading from tapes (Savage-Rumbaugh et al. 1986, p. 217).

In their fourth year, we video taped informal conversations of Tatu and Dar with a human interlocutor. Independent transcribers agreed on 81% to 84% of the chimpanzee signs in these tapes. Tatu signed at the rate of 441 and Dar at 479 utterances per hour. On average, 35% of Tatu's and 49% of Dar's utterances included two or more signs (Gardner & Gardner 1998b). These numbers are within the range of human infants (Bloom 1993). By contrast, in his fourth year sample, Kanzi averaged 10.2 utterances per hour. In a late sample from Kanzi's sixth year, Greenfield and Savage-Rumbaugh (1990) reported that on average 10.7% of Kanzi's utterances included two or more lexigrams. They also report that the experimenters themselves failed to achieve fluency with lexigrams. As in other research that the target article rightly criticizes, lexigrams may be suitable for testing fashionable theories about "ape language" but they prevent spontaneous conversation.

Size of vocabulary, appropriate use of sentence constituents, number of utterances, proportion of phrases, inflection, all grew robustly throughout five years of cross-fostering. Growth was patterned growth and patterns were consistent across chimpanzees. Wherever there were comparable measurements, patterns of growth for cross-fostered chimpanzees paralleled characteristic patterns reported for human infants. As for phrase patterns, nominative and action phrases appeared first, attributives second, and experience/notice appeared latest in the developmental samples of each chimpanzee – the same sequence that appears in studies of child development (Gardner & Gardner 1998a).

In extensive vocabulary tests, cross-fosterlings named objects they had never seen before under double-blind conditions. Because they could answer questions with any sign in their vocabulary, rather than having to choose from among a few forced alternatives, their errors were informative, often more informative than their correct responses (Gardner & Gardner 1984; Gardner et al. 1992). Conversational skills were systematically tested in experiments that inserted prescripted probes seamlessly into common daily conversations. Van Cantfort et al. (1989) showed how they grew and developed like human children in using sentence constituents to answer Wh-questions. Bodamer and Gardner (2002) and Jensvold and Gardner (2000) showed how the contents of chimpanzee rejoinders depended on the contents of probes in patterns like those found in children, and they showed that chimpanzee patterns resembled patterns of older and more competent children. Bodamer and Gardner (2002) and Shaw (2001) showed

the chimpanzees' skill and human-like development in pragmatics of turn-taking. Chalcraft (2002), Drumm et al. (1986), and Rimpau et al. (1989) showed how they used pronomial inflections and stress, devices that are integral to human sign language.

Warm attachment to committed, long-term human experimenters made it easy to adapt cross-fosterlings to rigorous experimental procedures. Gardner et al. (1989) described sign language studies of cross-fostered chimpanzees in detail. All nine of the reviews of this book that I have found commented favorably on its rigorous research methods (Anderson 1990; Boysen 1990; Byrne 1990; Chizar 1991; Hauser 1990; Hunt 1990; King 1991; Lieberman 1991; and Miles 1991). According to Chizar, "the book could be used in courses on measurement and experimental methods just as well as it could be used in courses dealing with comparative psychology, communication, and psycholingusitics" (p. 439). Perhaps the target article's stirring call for appropriate comparative methods will attract readers to research cited in this commentary.

Is there empirical evidence for the dynamic nature of communication systems?: The role of synchronization and inferential communication

Karl Grammer

Ludwig-Boltzmann-Institute for Urban Ethology at the Insitute for Anthropology, University of Vienna, A-1090 Vienna, Austria.
karl.grammer@univie.ac.at Web: evolution.anthro.univie.ac.at

Abstract: Communication is a multichannel, multiunit process that works on different levels. It is sequential with specific information carriers on a cognitive accessible level, and dynamic for the regulation of relationships at the same time. One function of communication is the broadcasting of internal states that can be assessed by inferential communication.

The assumption that communication is a dynamic system is not entirely new. The failure of many research attempts to clearly link signals to outcomes, social cognitions, and changes in the relationship between the communicators has led many to declare that the information processing approach is too simplistic (Grammer 1991; Grammer et al. 1997; 1999). The reasons for this failure are manifold, including the fact that the traditional behavior and signal description uses categories, and that we already impose theories about behavior on the structure of behavior in the analysis. I believe that although the theory of communication presented in the target article provides a thought-provoking contrast to the traditional sequential theory of communication, it does not present a viable, empirically testable alternative because it does not take into account the possible coexistence of multilayered communication systems.

Markl (1985), in his seminal analysis of communication theories, drew attention to the fact that stimuli have releasing effects (behavior consequences) and motivating effects (changes in the receiver's motivations), which are two ends of one communicative spectrum: an information processing communicative system and a dynamic system, which operate on the level of pairs. The two systems coexist on the simple level of behaviors and the resulting function for relationships. Communication is a process continuously changing in time and composed of multisignal and multichannel units (a string of many events interrelated in "communicative" space and time) that are highly graded and context-dependent (e.g., depending on the sex and age of the sender, the number and identity of spectators, etc., a smile may convey friendly readiness for contact, embarrassment, or cynical satisfaction at a mishap). On the unit level, nonverbal behaviors may be coded either as language-like (a smile conveys happiness), or as very loose probabilistic associations between movement patterns and external situational referents (Scherer 1982).

Central to the dynamical system communication approach is the synchronization of body movements and mutual rapport, but the road to the empirical analysis of synchronization in social interaction is filled with unsuccessful attempts. Grammer et al. (1998) showed that rhythmical patterning of movements alone, and not their content, is responsible for creating positive feelings in interactions. In these analyses we were unable to demonstrate a relation between synchronization defined in terms of sequential units, and subjective experience of pleasure and interest in another person. Significant results were found only for a phenomenon we describe as *hierarchically patterned synchronization.* If a female is interested in a male, highly complex patterns of behavior with a constant time structure emerge. The patterns are pair-specific and independent from behavioral content. This clearly underlines that such dynamic processes exist in social interactions and that communication is a dynamic pattern creation process. However, one result did not fit into this picture: Generally none of the behaviors commonly denoted as human courtship signals by Moore (1985) went into the patterns, suggesting the existence of a parallel information processing communication system running on another level.

The second problem dismissed by the dynamic communication systems approach is the communication of internal states. Shanker & King (S&K) explicitly deny this possibility. In the context of the biological functions and constraints of communication, this does not make sense because any communicative agent should try to access this type of information. The reasons for this are many. First, information about the internal states of a communicative agent would allow a receiving communicative agent to assess the action tendencies of an opponent. Both the sender and the receiver would benefit from this because they would be able to use this information to structure their own future actions, and communicative agents would be able to break down the search spaces for action in planning. Second, the ability to track the internal states of an opponent would help to prevent deception, as well as help in the search for honest "signals" (Zahavi & Zahavi 1997). Markl's analysis of mutualistic communication within groups of socially living organisms concludes that these organisms should inform their partners only as accurately as necessary to achieve the communicative purpose.

Grammer et al. (1997) showed that the quality of female body movements, and not their content, changes with female estrogen levels, and that males react to these changes with higher excitement (i.e., they start to move more). Therefore, it is not the cognitive signal processing itself that induces a communicative effect; it is the way a movement is performed in terms of speed, emphasis, information content, and complexity. We concluded that the assessment of internal states by movement inference might be crucial to interaction and so cannot be easily dismissed. We hypothesized that the recently discovered mirror neurons (Gallese et al. 1996) appear to form a cortical system matching observation and execution of motor actions. Such a system may provide a necessary bridge from "doing" to "communicating" and also could be responsible for the inferential communication of internal states. Rizzolatti and Arbib (1998) offer a hypothesis on how this gap might have been bridged. Whenever an individual is about to perform an action or observes another individual performing an action, pre-motor areas are activated. The actor recognizes an intention in the observer, and the observer will notice that his response affects the behavior of the actor. We thus predict a simple communicative system in which communicative agents can assess internal states directly from the perception of motion by inference through body movement.

There is empirical evidence for a dynamic communication system, but it appears to be coexisting with an information processing system. I would suggest that there are two systems working in parallel: a high level system where signals are exchanged and a low level system that regulates relationships. If a new model for communication is to emerge, it will need rigid formalization and this formalization should allow for empirical testing. As it is presented

here, and as convincing it might be, anecdotal evidence is not sufficient. Communication research must rely on empirical research on the level of physical information exchanged in interaction.

Information, information transfer, and information processing

Ulrike Hahn

School of Psychology, Cardiff University, Cardiff CF10 3YG, United Kingdom.
hahnu@cardiff.ac.uk

Abstract: Shanker & King (S&K) fail to provide substantive reasons for a paradigm shift in the study of communication because nonstandard and equivocal use of terminology obscures and undercuts their arguments.

Shanker & King's (S&K's) article advocates a shift in the study of primate communication from an information-processing systems to a dynamic systems paradigm. However, their use of these terms is problematic, obscuring both what we are meant to be shifting from and what we are meant to be shifting to, and why.

The supposedly standard paradigm of communication is information transfer or information transmission, whereby communication is said to occur when "one organism encodes information into a signal that is transmitted to another organism that decodes the signal" (target article, Abstract). "Information" in this paradigm is explicitly equated with Shannon's information theoretic notion: "On the information-processing paradigm, what is communicated is always information, construed in Shannon's information-theoretic terms (Shannon 1948) as something that can be quantified" (target article, sect. 2). But is there really such a paradigm? These terms are used widely, and typically loosely; definitions of *communication* are legion (e.g., see Hauser 1997, Ch. 1) as are uses and definitions of *information* (e.g., Kay 2000, Ch. 3).

Undoubtedly, the notion of communication as information transfer has widespread intuitive appeal, but how many researchers are committed to the particular interpretation of "information" attributed here? Doubts arise because the notion of information in information theoretic terms does not incorporate semantics: "The word 'information' in this theory is used in a special sense that must not be confused with its ordinary usage. In particular, information must not be confused with meaning" (Weaver 1949/1962, p. 99). In particular, Shakespeare's *Hamlet* and a meaningless jumble of words could potentially be exactly equivalent from an information theoretic standpoint, because in this framework information is a measure of freedom of choice in selecting the message (Weaver; see also Dretske 1981 for extensive discussion). Hence, this seems unlikely to be the meaning of "information" ubiquitously used by supposed proponents of a communication-as-information-transfer approach. Hauser (1997), for example, speaks of communication in terms of information, senders, and perceivers, but explicitly disavows the abstract information theoretic sense, and a closer reading of Ellis and Beattie (1986), who are quoted by S&K as proponents of this view, also suggests that they are using the term in a different, namely semantic, sense. Just how far the information theoretic notion is from the notion of semantic content is exemplified by the extensive (and deeply problematic; see, e.g., Godfrey-Smith 1989) attempts of Dretske (1981) to build on the information theoretic notion in order to derive a new informational semantics.

An immediate consequence of S&K's misattribution of a highly restrictive notion of information to the supposedly dominant communications paradigm (see target article, sect. 2) is that any argument for a paradigm shift (sect. 2) on the grounds that communication is more than just communication of information (e.g., it is emotions, wishes, and desires) breaks down. For such an argument to have substance, it would have to be shown that it actually applies given standard usage of the term, which seems unlikely.

Confusion is further compounded by the fact that the chosen terms for the two communication paradigms, *information-processing* and *dynamic systems,* are those used in a debate about the nature of *agent internal processes:* information-processing versus dynamical systems accounts of human cognitive behavior (see, e.g., van Gelder 1998a). Are these two things – information-processing views of communication and information-processing accounts of cognition – meant to be the same thing? Is communication as information transmission meant to imply "cognition as information-processing" on the part of the participating agents, and is "communication as a dynamical system" to imply a dynamical systems view of agent internal processes involved?

At the very least, the relationships between the various uses of these terms need to be explicitly clarified. According to S&K, communication is a dynamical system in that "what is communicated is not simply information" (sect. 2) and "all of the elements are continuously interacting with and changing in respect to one another, and an aggregate pattern emerges from this mutual coaction" (target article, Abstract). It is not clear that this has anything to do with the use of "dynamical system" in the context of agent internal behavior, defined by van Gelder (1998a) as a system that is "quantitative in state" – that is, with distances in state or time as determined by an independent metric, such that these distances matter to behavior (see van Gelder for further discussion and alternative definitions; but see also Chater & Hahn 1998). What is clear is that "information" in the Shannon-Weaver sense, is no more routinely implied in information-processing accounts of cognition than in information transfer characterizations of communication (see Dretske 1981, for extensive discussion of the relationship between "information" as used in information theory and as used by cognitive scientists; for an example of the latter, see Marr's classic work *Vision* [Marr 1982]).

The equivalence between the communications paradigm and the paradigm for agent internal explanation is never explicitly stated, but seems implied from the use of much of the rhetoric associated with the debate about agent internal cognitive explanation. In particular, the paper repeatedly adopts an anti-representational stance (but see, e.g., van Gelder's Author's Response on internal representation; van Gelder 1998b).

Useful dynamical systems accounts of behavior provide an alternative, but equally detailed, nuts-and-bolts explanation of a particular behavior. Simply telling us not to "look into the head" neither answers, nor does away with problems. What mechanisms are required in order to acquire language? In particular, how do we determine the references of terms which, logically, are vastly underdetermined? What cues (social and other) operate here? The problem persists for both the acquisition of conventionally regulated meanings (i.e., words of English) and novel, emerging referential terms. Likewise, how do we, or primates, extract, represent, and productively apply syntactic patterns? These are standard questions that proponents of information-processing views of language processing and development have been concerned with, and the fact that communication is dynamic and co-regulated neither resolves them nor makes them redundant. S&K may be interested in other problems but that does not make these problems go away, and without any clear outline of specific alternative explanations there are no grounds for a shift to a new mode of explanation.

Certainly, the mere fact that communication between two agents is "dynamic" in some sense of the word says nothing about the means by which this process is internally mediated. There is no clear general inference from the nature of the interaction between agents to the nature of agent internal processes. Nor is it feasible to argue that two *particular instantiations* of a class of account are incompatible in order to establish an incompatibility between the respective *classes:* Even if one were to grant that Deacon's (cognitive) information-processing explanation of Kanzi was incompatible with Savage-Rumbaugh's dynamical account of communication (cf. target article, sect. 4), this would not suffice to establish that the class of dynamical views of communication is incompatible with the class of information-processing views of agent internal processing.

Thus, there is no real argument for a paradigm shift even if the equivocations on terms were removed.

Communication and communion

Tim Ingold

Department of Anthropology, School of Social Science, University of Aberdeen, Aberdeen AB24 3QY, Scotland. **tim.ingold@abdn.ac.uk**

Abstract: Shanker & King's (S&K's) dynamic systems approach converges with developments in social anthropological studies of communication which were long ago anticipated in the writings of Volosinov and Schutz. Following a review of these writings, this commentary suggests that a dynamic systems approach should distinguish communion from communication. It concludes with a remark on the evolutionary implications of the approach.

I am much in favour of the dynamic systems approach, of which I count myself as an advocate (e.g., Ingold 2000a; 2000b; 2001). Therefore, where Shanker & King (S&K) systematically contrast this approach with the alternative, information-processing approach, I find myself nodding in agreement on every point. I am also encouraged by the news that a paradigm shift towards theorising in terms of dynamic systems is well underway. Such a shift will not only release biological and psychological studies of communication from the straitjacket of hard-core cognitivism and neo-Darwinian fundamentalism in which they have been confined for so long, but it will also provide the basis for effective collaboration between practitioners in these fields and those in my own discipline of social and cultural anthropology, which in recent years has witnessed a shift in theoretical focus almost identical to that reported by S&K. In her recent, wide-ranging review of anthropological studies of communication, Ruth Finnegan noted how a number of approaches, some of which date back some time, are converging on a "fresh view of speech and communication: not independent systems of signs for conveying decontextualised pieces of information but modes of social action, created by interacting human agents in specific situations" (Finnegan 2002, p. 7).

Yet, talk of paradigm shifts sometimes exaggerates the novelty of perspectives that have been around for generations, albeit as subversive undercurrents to mainstream positions. In what follows I first draw attention to two foundational texts that long ago anticipated the advent of "dynamic systems" thinking, and then move on to consider whether the concept of communication should really have a place in this way of thinking at all. I conclude with a remark on its implications for our understanding of evolution.

In his remarkable study *Marxism and the Philosophy of Language*, first published in Russian in 1929, V. N. Volosinov argued that the idea of language as a formal system that is somehow installed in the mind, independently of its instantiation in speech, is a pure abstraction. In reality, language is not expressed in the current of speech, it *exists* in it. Moreover, this current is not monological but always (at least) dialogical; thus speech exists only in contexts of social interaction. And through this interactivity, forms and meanings are not so much revealed as continually brought into being. All these points are succinctly tied together in Volosinov's proposition that "language is a continuous generative process implemented in the social-verbal interaction of speakers" (Volosinov 1929/1986, p. 98). A contemporary rereading of Volosinov's work would be worth the effort, lest we expend undue effort in painfully rediscovering what he had taught us all along.

Coming from a quite different direction, and pioneering a phenomenological approach to human sociality, Alfred Schutz's classic paper "Making music together" (1951) suggested that at the core of all social interaction is a "mutual tuning-in relationship." According to Schutz (1951), to listen to the speech of a consociate is not so much to decode an utterance as to enter into the very process of its production. We understand one another because, having made the other's purposes our own as well, we converge on the same meaning, a meaning that is drawn from our joint involvement in a specific context of interaction. As the title of Schutz's paper implies, the prototype for the "tuning-in relationship" is the mutual attentiveness of the members of a musical ensemble, for each of whom both listening and playing are integral to one and the same current of action. The speaker in a social setting, just like the musician in the ensemble, is able continually to adjust his or her gestural movement to resonate with the gesturing of others without ever interrupting the flow, because the process of action is itself a process of attention (Ingold 2000a, p. 415).

The musical analogy, however, raises a doubt in my mind as to whether the mutual understanding that emerges from shared feeling and action should be regarded as an instance of communication at all. Do musicians really *communicate* with one another and with their audience? The music, after all, inheres in the sound itself; it is not an idea that is conveyed *by means of* sound. And this sound is there in the environment for anyone who would attend to it, just as there are all the other sounds that envelop us as we go about our lives. We may attend and respond to the sounds of the wind, or the rain, or the waterfall, but we do not imagine that these phenomena are communicating with us. Rather, their sounds are simply *there,* as the acoustic signatures of their phenomenal *presence* in the world. Likewise, if I sing a wordless song, however full of feeling, is this not just one way in which I manifest my presence in the world – a presence to which others may or may not attend and respond? Singing, like music-making of any kind (or like dancing or gesturing), is a mode of presence, not a mode of communication (Ingold 2000a, pp. 104–105).

Why, then, should it be any different with speech? In the West, we routinely distinguish between speech and song on the grounds that what counts in speech are not the sounds themselves but the ideas that are encoded in sound. But that is to invoke a Cartesian distinction between inner mental states and their outward physical expression, which S&K rightly reject. In everyday experience people are present for one another through their voices and gestures just as much in their speech as in their song, if indeed the two can be distinguished at all. (In many of the non-Western societies studied by anthropologists, they are not distinguished or, if the distinction is drawn, it runs along quite different lines.) Bronislaw Malinowski (1923) classically described the mutual copresence established through speech and song as "phatic communion." His point was that all communication of information is founded in a communion of copresence. That point is S&K's too, but I feel it might be made more convincingly by keeping the concepts of communion and communication distinct (on this distinction, see Ingold 1986, pp. 272–77), for, however much we might wish to avoid it, mere mention of the word "communication" tends to conjure up the image of a sender, a receiver, and a message.

Finally, what are the implications of a dynamic systems approach for our understanding of evolution in general, or, more specifically, for the evolution of language? One implication, as S&K rightly observe, is that the linguistic capacities of apes and humans are not genetically determined. But it is not particularly helpful to attribute their development, instead, to "an ongoing and complex interplay between biological, social, and cultural factors" (target article, sect. 4.2, last para.). What is a biological factor, as distinct from a social or cultural factor? It is difficult to see how such factors could possibly be isolated, let alone brought into play with one another. One other implication of the dynamic systems approach, surely, is that we should cease slicing up the world of interacting organisms in this arbitrary way. If the capacities (including linguistic capacities) of organisms are emergent properties of the entire system of relations or "developmental manifold" set up by virtue of their positioning within an environment, then the evolution of these capacities must be understood in terms of the properties of dynamic self-organisation of the manifold itself. I do not

deny that natural selection occurs, leading to cumulative changes in the genome along ancestor-descendant sequences. But (*pace* Gottlieb 1997; Griffiths & Gray 1994) it occurs *within the context* of the evolution of dynamic systems, rather than bringing this evolution about (Ingold 2001, p. 125).

The Vygotskian advantage in cognitive modeling: Participation precedes and thus prefigures understanding

Christine M. Johnson

Department of Cognitive Science, University of California San Diego, La Jolla, CA 92093-0515. **johnson@cogsci.ucsd.edu**

Abstract: Shanker & King's (S&K's) proposal is consistent with a Vygotskian model of development which assumes that cognition is first social and visible, and only later internalized and invisible. Rather than slipping into positing "epistemic operators" like *understand* or *intend* as generative of behavior during language learning or theory of mind tasks, this approach profits from keeping its focus on charting the ontogeny of embodied interactions.

Shanker & King (S&K) are to be lauded for taking on the daunting task of applying the dynamical systems model to cognitive phenomena, and they do a commendable job of grappling with the subtleties of the argument. Their reinterpretation of ARL research makes clear how such efforts can yield rich dividends in revealing insights heretofore hidden in existing data and in generating a host of new questions to address. Even so, like most of us, they were no doubt originally trained to think of cognition as the invisible, individual events responsible for behavior, and of its study as essentially an inferential science. Such training is difficult to overcome, and undertaking its revision demands that we challenge each implicit assumption that may lead to a confounding of the models that S&K are attempting to contrast.

As one case in point, consider their use of the following from Tomasello's (1999) *Cultural Origins of Human Cognition:* "sounds become language for young children *when and only when* they understand that the adult is making that sound with the intention that they attend to something" (Tomasello 1999, p. 101; emphasis mine). Given that Tomasello has perhaps done more than any other contemporary investigator to advance our understanding of the relation between culture and cognition (although see Cole 1996; D'Andrade 1980), citing him in a discussion of the interactive nature of language development is highly appropriate (cf. target article, sect. 4). And yet the particular quote chosen by S&K represents, to my mind, the one aspect of Tomasello's approach that runs counter to the model the authors are advocating. That is, suggesting that some kind of shared understanding is a *prerequisite* for linguistic behavior places what is "inside the head" as the source and cause of that which is observable, rather than emphasizing that behavior – or, more accurately, interaction – is the source and cause of what ultimately must end up "inside the head." What makes this difference in emphasis significant enough to merit treatment in *BBS* is its consequences for how science is done, both in terms of the methods that are employed and the interpretations that are supported.

Adopting the stance that cognition is best studied as an observable, distributed event (e.g., Hutchins 1995a; Johnson 2001; Vygotsky 1978; Wertsch 1985) rather than as an invisible, mental one, need not prevent us from recognizing that positing the latter may be necessary to a complete account of human, or ape, cognition. However, positing mental states and then designing experiments to discriminate the behaviors required to confirm them has led both human and comparative cognitive research down many a garden path. For example, because our working definitions of mental states have tended to be based on adult introspection and language use, they are often rendered in the shorthand *mentalese* (e.g., "know," "desire," "intend," etc.) that adults adopt after years of experience with interpersonal and environmental interactions. In the "theory of mind" literature, for example, subjects (usually children or nonhumans) are typically hypothesized to know or not know what another person knows or does not know.

Furthermore, it is this mental state of knowing that is presumed to endow each subject with the capacity to perform correctly on a critical experimental task. However, this presumption becomes suspect when we realize that adults can, and do, experience a variety of mental states, at different times and sometimes simultaneously, while engaging in such tasks. These can range from explicit propositional reasoning involving terms like "know," to cued heuristics (like an expression of puzzlement predicting prolonged searching behavior), to intuitive affect-laden biases that are not readily articulated or experimentally assessed. Similarly, implementing such research has demonstrated that context (the particular phrasing of the experimenter's question, the use of three-dimensional dolls vs. drawings, etc.) can have significant effects on, for instance, the age at which a child is likely to succeed (see Astington et al. 1988). In addition, the performance of nonhuman primates on related tasks are replete with complex ambiguities (e.g., Heyes 1993; Povinelli 1994) that, together with the above, suggest such models and methods may obscure, more than reveal, the role of ontogeny and phylogeny in the emergent adult abilities.

In contrast, one of the great advantages of S&K's approach is its integration of the Vygotskian insights that cognition is first social and visible and only later internalized and invisible, and that the study of changes in the relative contributions of novice and expert (observable, e.g., in detailed video analysis of interactions over time) best capture cognitive development. As a result, this approach can inform models of mental representation by charting its ontogeny through embodied interactions in the infant and its caretaker (e.g., Fogel 1993), the juvenile and its cohorts (e.g., Rogoff 1990), and the adult and its society (e.g., Hutchins 1995a; 1995b). In this view, "theory of mind" shifts from being a "representation of the representations of others" to being a complex set of propensities and sensitivities in a range of related contexts. For example, in the first few years of life, a child participates with more proficient others in an array of interactions in which attention, and especially co-attention, directed at an object early in an interaction tends to correlate later with efficient searching for that object, and movement of that object without the accompanying co-attention correlates with circuitous and often unsuccessful searching (see Karmilov-Smith 1992). As the child becomes more of an active agent in such interactions (e.g., taking on the roles of mover or searcher) as well as engaging in a variety of other co-attention-mediated activities, there is improvement in the child's sensitivity to the relevant cues, the timing and relevance of the child's actions and associated affect, and the range of contexts in which the child can appropriately participate. From this perspective, what the child who succeeds at a theory of mind task has learned is not what others know or do not know, but the nature of effective (and ineffective) coordination for any player in that class of interactions (see Wittgenstein 1953).

More than a substitution of one set of jargon for another, such a restructuring reveals aspects of cognition that are not liable to be detected under the traditional approach. S&K's analysis of Kanzi's performance, for example, highlights many qualities of his coordination during language interactions (e.g., that he can attend to multiple partners, respond in and to a variety of media, scaffold the activities of more novice participants, etc.) that have been little noted in the literature on ape language research and yet open a whole vista of important research questions that beg further investigation. However, even as they strive to explain that epistemic operators such as *understand* or *intend* are best used to refer to sets of behavior rather than to mental states, I fear that their frequent use of such terms may only make it harder for us to focus as we should on the stuff that we can actually see. Until we get better at doing systemic cognitive analyses of embodied interactions, I recommend that we limit our use of such terms. If we confine

ourselves to using them only when we must, their meanings should, in time, become clearer to us all.

What ape language research means for representations

Edward Kako

Department of Psychology, Swarthmore College, Swarthmore, PA 19081-1390. **ekako1@swarthmore.edu**
http://www.swarthmore.edu/socsci/ekako1

Abstract: Shanker & King (S&K) rightly stress that recent ape language research has important implications for language development and origins. But the evidence does not warrant their conclusion that we can dispense with representations. Indeed, their own discussion of the nature of communication highlights the central role that representations must play in our models of communicative competence, in and out of language.

Shanker & King (S&K) quite rightly claim that recent ape language research should be taken seriously, and that it has important implications for the origins of human language. As I have argued elsewhere (Kako 1999), Kanzi's abilities (particularly his comprehension of spoken English) are indeed impressive. His ability to distinguish "Put the ball on the hat" from "Put the hat on the ball" suggests that he understands the links in English between the syntactic position of an object label and the role it plays in the event that the sentence describes. This, in turn, suggests that the prerequisites for such abilities may antedate modern humans quite substantially. However, the major implication that S&K wish to draw from the success of these projects – specifically, that models of communicative competence can do without representations – is not warranted by the evidence.

S&K point out that communication can take place even when information about mental states is not being transmitted. Facial expressions, for example, can communicate fear or surprise; a movement of the hand can communicate affection or a desire to reconcile after conflict. These communications do not involve the transmission of information per se, nor do they occur embedded in any kind of elaborate code. For the communicative act to succeed, the participants need not represent their own mental state or that of their partner. These acts are, nonetheless, meaningful to both participants in the interaction.

The evidence that some communication is distinct from information transmission and can take place in the absence of representations does *not*, however, warrant the claim that we are permitted to dispense altogether with the notions of information transmission and representation. I do not mean to say that human communication takes place *only* in language, or that it always requires codes and the representations that participate in those codes. On the contrary, human communication, most broadly construed, takes place in multiple channels. Through some of these channels, communicative partners can converge on shared meanings without the benefit of any structured representations. But for some forms of communication, structured representations and the codes that assemble them are absolutely indispensable.

In fact, the contrast that S&K try to make between information processing and dynamic systems theory (DST) reinforces the central role that representations must play. In their Table 1, they write that, according to DST,

> Desires, intentions, thoughts, wishes, emotions, are not "internal states" or "representations." In the particular circumstances where it makes sense to say "P communicated a piece of information to Q," the information communicated is not about an internal state or representation, but rather, *that such-and-such* ("that there is some ripe fruit over there," "that John is coming," and so on). (target article, sect. 2; S&K's emphasis)

Tellingly, both of their examples require the thinker to represent entities (fruit, John), the relationship between those entities and the thinker (roughly, "not here"), and properties of those entities, either now or in the future (ripe, coming). It is not clear what these are, if they are not representations of the world as it is or soon will be. Any system capable of communicating the content of these statements must rest on representations. Moreover, the system itself must be structured in a way that allows the listener to reconstruct in his or her own mind the representation that exists in the mind of the speaker.

S&K consider at length the achievements of the chimpanzees Sherman and Austin, whose facility with lexigrams was indeed quite impressive. They are right to emphasize the importance of the situations into which Savage-Rumbaugh placed the chimpanzees and they are right to conclude that the success was not due entirely to the information Sherman and Austin extracted from the combinations in which the lexigrams appeared. But again, S&K's conclusion that we can dispense with representations in accounting for their success is unwarranted. Even if the lexigram combinations provided no help whatsoever to Sherman and Austin, a proper description of their competence requires the admission of representations. Once again, S&K's own words are telling:

> one is warranted in describing Sherman and Austin as understanding the meaning of lexigram symbols on the grounds that they could do such things as . . . use lexigrams to direct each other's or another person's attention; extend the use of lexigrams to novel (but suitable) circumstances; spontaneously assign unlabelled keys to new foods; closely attend to their own, and to someone else's use of lexigrams; and correct their own or each other's mistaken use of lexigrams. (target article, sect. 3, fifth last para.)

In order to use lexigrams in these ways (and *use*, for S&K, is critical), they must have been able to represent the attentional state of others relative to their own, such that they could detect a match or mismatch; and they must have been able to represent both the links between lexigram and meaning and intentions (their own and others), again in such a way that they could detect a match or mismatch.

To claim that representations are essential is not to deny or devalue the importance of social interaction for Sherman, Austin, or Kanzi (or, for that matter, for human children). Nor is it to deny that rich communication can take place in the absence of information transmission traditionally conceived. But, as S&K's own words strongly suggest, it is impossible to eliminate representations altogether. Indeed, I would argue that Sherman, Austin, and Kanzi are such extraordinary cases precisely because we cannot escape the notion of representations when we talk about them. The lesson of ALR is not that we should declare humans and their primate-relatives "representation-free," but that we should embrace nonhuman primates as "representation-rich."

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Dancing on thin ice

Stan A. Kuczaj, Joana A. Ramos, and Robin L. Paulos

Department of Psychology, University of Southern Mississippi, Hattiesburg, MS 39406-5025. **{s.kuczaj; joana.ramos; robin.paulos}@usm.edu**
http://www.usm.edu/psy/kuczaj/

Abstract: The "new" paradigm proposed by Shanker & King (S&K) is neither new nor a significant advance in our understanding of communication. Although we agree that social interaction is important, ignoring the roles of mental processes and the significance of information exchange is theoretically dangerous. Moreover, the "communicative dance" is sequential. If one partner does not lead, how is the other to follow?

Shanker & King (S&K) propose a "new paradigm" for ape language research and boldly state that the "shift from an informa-

tion-processing to a dynamic systems model . . . represents an important transformation in our understanding of the nature of communication" (target article, sect. 2). We believe that their paradigm is neither new nor a significant advance. To the contrary, the dynamic systems approach they advocate raises more questions than it answers. Consequently, over-reliance on such an approach would be a serious theoretical mistake.

S&K note that early attempts to train apes to use language relied on operant conditioning principles and that the results of such attempts were typically disappointing. The failings of early ape language research are not surprising, given that the principles that underlie behavioral modification play minimal roles in human children's acquisition of language. In fact, the inadequacy of simple stimulus-response explanations of language development has been recognized for decades (Brown 1973; Chomsky 1959; Kuczaj 1977; McNeill 1970). S&K's characterization of contemporary information-processing approaches as simple linear stimulus-response systems is misleading (see Klahr & MacWhinney 1998), and sets up information-processing as a flimsy straw man for them to knock down.

Much of the rationale for the arguments in the target article comes from the successes reported by Sue Savage-Rumbaugh once she began to interact in a more fluid fashion with Kanzi. We agree that her work has produced remarkable results, but we are stunned by the claim that the work with Kanzi provides a "new" basis for understanding human child language development. Despite everything that Kanzi has accomplished, he has not acquired the language skills of a normal human child. Attempting to equate Kanzi's accomplishments with those of human children is misleading and inappropriate. It takes away from both Kanzi's and human children's significant achievements, and muddies the water in terms of understanding and comparing Kanzi's acquisitions with those of human children.

The dynamic systems approach advocated in the target article is based on a dance metaphor, where "communicating partners continuously establish and sustain a feeling of shared rhythm and movement" (sect. 1). In the communicative "dance," partners adjust their communicative efforts based on each other's behaviors, which might include gestures, facial expressions, vocalizations, and so on. For S&K, the appeal of this approach is the liberation from sequential analyses and the recognition that aspects of the communicative process are in a state of constant flux. But what does the dance metaphor actually gain us?

According to the dance metaphor, communication partners "adjust" to each other. Adjustments require that each partner pay attention to the other, and so they do not occur in a temporal vacuum. Adjustments are inherently sequential unless one assumes that partners share some sort of amorphous psychic connection; therefore the dance metaphor does not "free" us from the need to consider and understand the sequential flow of information between partners. Consequently, S&K's proposal to neglect the sequential flow of communication is based on the unfounded assumption that the communicative dance is not sequential.

The most important aspect of the dance metaphor is the emphasis on social interactions. Such interactions are significant aspects of communicative development, and are likely essential for normal development to occur, as others have noted (e.g., Bruner 1975; Messer 1994). However, even though social interaction may be necessary for normal development, it is not sufficient (Kuczaj 1982; Pinker 1994). Thus, any explanation of language development that excludes all factors outside of the social realm is destined to fail.

We are also confused by S&K's disparaging use of the term *information.* They suggest that the notion that information is exchanged in communication is flawed, and that instead of communicating information, partners communicate desires, intentions, thoughts, wishes, and so on. We disagree wholeheartedly with their proposal that communication exchange does not involve information. If one communicates one's disappointment, one is informing another (or others) of this disappointment. This is information. We admit that we do not understand the distinction that they are trying to make between information and noninformation. Because they never define *information,* it is not clear what they mean when they use this term; however, it is clear that they do not think information is exchanged in communication, whatever information means to them.

S&K also suggest that communication should be viewed as an activity rather than a process. The distinction they wish to draw between activity and process is not clear, but given the general themes in their argument, we suspect that they would view activity as some sort of overt behavior and process as some sort of internal phenomenon. If this is the case, it seems that the distinction they wish to make concerns the level of description rather than the best way to characterize and explain communication. An adequate explanation of language, be it ape language, child language, or full-blown adult human language, will require appreciation of what happens during discourse events (which seems to be their emphasis), understanding what the organism brings to the task of learning language, consideration of the processes involved in development, and the determination to integrate these components into one coherent explanation. Simply sweeping untidy components of the phenomena under the proverbial rug will not get us closer to a satisfactory explanation.

You can dance if you want to

Valerie Kuhlmeier and Paul Bloom

Department of Psychology, Yale University, New Haven, CT 06520.
{Valerie.kuhlmeier; Paul.bloom}@yale.edu

Abstract: We argue that the dance metaphor does not appropriately characterize language. Indeed, language may be a red herring, distracting us from the intriguing question of the nature of apes' social interactions.

We are not dancing. As you read this, you are learning about our beliefs and desires – we are communicating – but there is no nontrivial sense in which we are now dynamic systems engaged in mutual coaction. This is not because of the abstract nature of this topic. When we communicate about more concrete matters – the berries in the forest are poisonous, give us food or we will kill you, nice day today, isn't it? – we are still not dancing. It is also not because this is written; we could convey all the same information by speaking. Of course, we *could* gesture, and respond to your gestures, and engage in all sorts of synchrony, but we certainly do not have to. We could simply say "the berries in the forest are poisonous," and you would understand what we mean.

Clearly, dancing is not necessary for language. It is also not sufficient. There is an important difference between asking your child "Do you want milk?" versus "Do you want cookies?" and it has nothing to do with dynamic systems or coregulated interactions. It has to do with the words "milk" and "cookies." Similarly, the difference between "I like milk better than cookies" versus "I like cookies better than milk" emerges not from dynamic interaction but from the ordering of the words and phrases – in other words, from the syntax.

On the face of it, knowing a language has much to do with knowing the meanings of words and how they are combined to form meaningful utterances. Languages must be learned because words and syntax vary across different languages, and any account of how speakers successfully communicate must make reference to knowledge of the words and rules of a given language. We do not see these claims as particularly controversial, but it is not clear how they are captured in the dynamic systems paradigm that Shanker & King (S&K) describe. In any case, it is clear that dancing and language, though possibly somehow related, are quite different things.

In fact, language might well be a red herring here, one that distracts the reader from the theoretical and empirical insights that

lie within this very interesting paper. There is plainly a sense in which apes really do seem to be engaging in a dance, and the study of the nature of this dance can tell us much about how their minds work (hence our title, inspired by the song by "Men Without Hats"). The obvious question is: What goes on in this dance? At what level do we interpret the apes' behavior? You can imagine scenarios of different complexity, from the instinctive mimicry of a newborn baby to the conscious debating of polished politicians. Where within this range do the apes' social interactions fall?

S&K are generous in their interpretation. When describing the actions of Kanzi, Sherman, and Austin under the dynamic systems paradigm, they assume underlying mental attribution abilities such as a recognition of another's intentions, feelings, desires, and knowledge states. The chimpanzees must discover the "mutual convergence on some shared feeling, thought, action, or intention" (target article, sect. 2). They are portrayed in some circumstances as beginning to "attend more closely to [Savage-Rumbaugh's] actions in order to ascertain what she intended" (sect. 3). Indeed, in one situation, Kanzi is described as recognizing that "Tamuli has not understood what she is being asked to do" (sect. 4.2).

Is this characterization of the dance correct? Perhaps, but it has not been shown yet. Apes *are* good candidates for such attribution abilities. Recent studies, for example, have suggested that chimpanzees follow the gaze of conspecifics (Tomasello et al. 1998), know what conspecifics can and cannot see (Hare et al. 2000), and perhaps even recognize what a conspecific knows and does not know (Hare et al. 2001). But this is all controversial; we have only begun to demonstrate these abilities and still need to examine whether they represent a theory of mind like that assumed to exist for humans, or whether chimpanzees solve such tasks using alternative problem-solving strategies. They might dance, but not know that they are dancing, or even that they are interacting with intentional agents. To make assumptions regarding the level of interaction without first examining what is happening in the mind of the ape subjects is putting the cart before the horse. Only with careful empirical research may we be able to discover the nature of the dances S&K report.

On the public nature of communication

David A. Leavens

Psychology Group, School of Cognitive and Computing Sciences, University of Sussex, Falmer, East Sussex BN1 9QH, United Kingdom.
davidl@cogs.susx.ac.uk **http://www.cogs.susx.ac.uk/users/davidl/**

Abstract: Comparative and developmental psychology are engaged in a search for the evolutionary and developmental origins of the perceptions of "intentions" and "desires," and of epistemic states such as "ignorance" and "false belief." Shanker & King (S&K) remind us that these are merely words to describe public events: All organisms that can discriminate states of "knowledge" in others have learned to do this through observation of publicly available information.

In this thought-provoking target article, Shanker & King (S&K) invite us to reconsider several of the widespread assumptions that underpin much of contemporary research into the comparative and developmental psychology of communication. Among these assumptions is that intentions and desires are invisible causal factors that can be invoked to explain or predict overt behavior. Bateson (1972b) pointed out that mental phenomena work differently from physical phenomena, insofar as in the physical world nothing cannot cause anything but in the mental world, in the world of communication, inaction can carry a great deal of meaning with measurable consequences. To borrow his example, "the income tax form which you do not fill in can trigger the Internal Revenue boys into energetic action" (p. 452). Failure by a low-ranking chimpanzee (or wolf or human) to exhibit submissive behavior in the presence of more dominant animals will earn rebuke. When every social encounter constitutes an episode of negotiation of our relationships, then, as Bateson frequently noted, we cannot not communicate. A snub is defined as the absence of an expected overture. Thus, a particular social interaction is the *Ding an Sich,* and what we might label an "intention to communicate" is also the thing itself as manifested in coordinated social activity or, in other words, publicly observable behavior (e.g., Bateson 1972b).

A great deal follows from the kind of shift in perspective advocated by S&K, not the least of which is that, in this view, it becomes difficult to cling to an essentialist definition of humanity in violation of established principles both of natural history and ontogeny. Folk psychologies appeal to unseen causative agents that evolved uniquely within the human lineage, be they language acquisition devices, intentions to communicate, or demonic possession (cf. Thompson 1997). To suggest that "an intention made me do it" is as scientifically relevant as to suggest that "the Devil made me do it," as both statements are equally immune to independent verification. Folk psychologies are neither geographically nor historically constant (e.g., Lillard 1998; Vinden 1999). Thus, as S&K suggest, the behavioral sciences would be better grounded empirically if they adopted a more agnostic stance toward the intentional stance. In other words, that we can and often do talk about "intentions," "demonic possession," and other such occult mental phenomena does not constitute evidence of their existence.

Whence intentions and desires? The fullest answer to this question will derive from the richly descriptive practice required by dynamic systems theory. What we trade off in data reduction today will pay dividends when comparative and developmental psychology mature tomorrow. S&K are heralding an imminent revolution in comparative psychology, long overdue, which will be manifested in the realization that just as reference is not a property of words but of people in action (people refer to things sometimes with words and sometimes through other means), neither is intentionality a property of minds; rather, it is manifest in what people and other organisms do in relation to each other (cf. Bateson 1972a; Reddy 2001). For a case in point, consider pointing.

Only a decade ago it was widely held that only humans pointed (e.g., Corballis 1991; Donald 1991). Long-term field studies had failed to identify pointing as a major constituent of the gestural repertoire of any ape species (e.g., Goodall 1986), though more recently there have been reports of some pointing by wild chimpanzees (Inoue-Nakamura & Matsuzawa 1997) and bonobos (Veá & Sabater-Pi 1998). Recent large-scale laboratory studies have shown that apes in captivity very commonly point to food outside their reach when, and only when, a human observer who is in a position to retrieve that food for them arrives on the scene (e.g., Leavens 2001; Leavens & Hopkins 1998). They do this with no explicit training whatsoever. Thus, apes in captivity spontaneously exhibit the capacity to capture and redirect the attention of a human observer to a distal entity; their gestures are manifestly "about" objects in the environment and therefore are intentional, by definition. Their pointing constitutes an attempt to manipulate human behavior – to condition their human observers or to "train" their trainers – not by virtue of their possession of some unseen and scientifically unverifiable abstract representation of others' mental states, but because that is what pointing does in the real world and they've learned this, presumably, through observations of the consequences of their actions, which, also presumptively, is how people learn to manipulate other people.

There is extraordinary variation between captive apes in the opportunities they will have had to experience coordinated activities with humans. Consider the typical environmental circumstances of chimpanzees housed in a biomedical research center: Their feeding schedules are largely divorced from their own communicative activities (i.e., food delivery is not typically contingent on their communicative bids) and their often vigorous, even hysterical, appeals to dissuade veterinary technicians armed with tranquilizer dart guns are utterly futile. Chimpanzees housed in these conditions experience on the order of four minutes per day of positive face-to-face interaction with their human caregivers (Bard,

personal communication, 1998). Is it at all surprising that apes raised in such circumstances often exhibit less facility or skill in tasks involving the establishment of joint attention with humans than do human children, who have both profound emotional bonds with and, in comparison with most captive apes, extraordinarily more experience interacting with humans? What is astonishing is that these apes, in the face of so much futility, persist in their attempts to manipulate people through intentional communication.

S&K remind us that the facility evinced by chimpanzees to train their human observers to act as extensions of the chimpanzees' instrumental goals is related in a systematic way to their developmental histories, their biology, and their social, cultural, and physical environments. They invite us, therefore, to reconsider the significance of the ape language studies; to wit, when we raise apes in environments that mimic, in important particulars, the rearing circumstances of our children, they start to behave a lot like our children do.

Dancing with humans: Interaction as unintended consequence

John L. Locke

New York University, New York, NY 10003. **JLL7@nyu.edu**

Abstract: Parallels to Shanker & King's (S&K's) proposal for a model of language teaching that values dyadic interaction have long existed in language development, for the neotenous human infant requires care, which is inherently interactive. Interaction with talking caregivers facilitates language learning. The "new" paradigm thus has a decidedly familiar look. It would be surprising if some other paradigm worked better in animals that have no evolutionary linguistic history.

Shanker & King (S&K) advocate a model of language teaching that values dyadic interaction. The need for such a model is surely understandable where the learners, anthropoid apes, are highly communicative but lack the evolutionary history, hence genes, for language. Here I will discuss instructive parallels in a different group of learners who are also highly communicative but lack the maturation and experience for language.

My 1996 article in the *Journal of Child Language* asked, "Why do infants begin to talk?" and a subtitle promised to treat language as "an unintended consequence" (Locke 1996). I asserted that when infants begin to talk they draw on vocal material that is learned in the first year of life, building on attentional biases present in the first few weeks. A corollary was that infants store strings of sound long before they can possibly be aware that ambient figures, in talking, are actually exercising a system of arbitrary sound-meaning relations that is regulated by an equally arbitrary mental grammar. Thus, it was proposed, the actions and developments that nudge infants along the path to language could only involve proximal benefits that are inherently nonlinguistic (Locke 1996).

The central thesis of the "Why" article was derived from more basic evolutionary processes relating to bipedalism, pelvic narrowing, and truncated gestation, leading to "premature birth" and a protracted period of postnatal dependency and care (Locke, submitted). It is significant that the unusually intense stimulation associated with routine caregiving – feeding, comforting, carrying, and, in modern humans, talking – is experienced during a period of equally intense brain development (Locke 1993).

With weaning and ambulation – changes that potentially reduce interactions between infants and their caregivers – attachment grows stronger. This ensures continued valuation of faces, eyes, and voices, and therefore persistent sensitivity to the organs of spoken language. Conveniently, the faces that command infants' attention also move when people talk, the eyes that offer opportunities to share emotional space also link words and environmental referents, and the voices that identify caregivers and their feelings happen to be what vowels and other speech sounds are made of. Thus, infants, merely by dancing with mature humans – who happen to spend most of their time talking – take on linguistic knowledge.

Not that systems for the analysis of linguistic material are unavailable to the infant, for they are, or soon will be. But mechanisms that operate on stored utterances must be supplied, and this is where social observation and interaction come in. This becomes evident when one considers a nearly opposite circumstance. In the case of autism, faces and possibly voices are largely ignored, if not avoided. These dispositions make it almost impossible to know whether there is something wrong with the usual linguistic machinery, since – lacking an interest in people who talk – there are too few experiences of the sort that, in normally developing children, produce material for linguistic processing.

In the apes, a great deal more of the brain develops prior to birth, at a time when the fetus is shielded from ambient vocal stimulation and, one deduces from studies of vocal behavior (cf. Tomasello et al. 1985; 1989), is only rarely exposed to maternal calls and screams. After birth, neonatal apes begin independent movement and foraging at an early age. In the unusual case of the captive bonobo, Kanzi, talking humans appeared on the scene immediately, with the training success that Savage-Rumbaugh and her colleagues have documented.

Savage-Rumbaugh's training program, like the ocular and vocal interactions of human infants and their caregivers, produced a number of unintended consequences. As S&K point out, Kanzi "volunteered" to interact in a number of instances. He ad-libbed and he responded to speech even when doing so was not in the empirical script. These voluntary interactions hint at reasons why Kanzi learned the sounds and meanings of his caregiving talkers, but they also present us with an apparent paradox. Whereas human infants, while interacting with talkers, typically happen to learn a language, an infant ape, when enrolled in a language-training program, happens to develop a taste for spontaneous human interaction. So what was the "ape language project" really about?

I have argued elsewhere that children develop the capacity for language gradually, in several discernible phases (Locke 1997); these include vocal learning and storage, phases that precede the child's first use of words. At this point, there is usually little for the investigator to observe but social behavior. Researchers on the Genie project learned the hard way, if they learned at all, that one cannot get to grammar without passing through social interaction, a passage they may have inadvertently blocked – thwarting their own theoretical goals – when they removed the matron who became "too close" to Genie at the beginning of the project (Rymer 1994). Was the purpose of the research to encourage a modern-day "wild child" to develop trust and affection for her caregivers, or to create novel utterances through the application of grammatical rules? The theoretical stakes were clearly linked to the latter, but there is no way the "critical period" hypothesis can be tested if the learner lacks previous success in the area of emotional development and attachment.

When Savage-Rumbaugh tried to teach language she got interaction, just as the human infant's interaction with talking caregivers naturally produces vocal learning and the storage of speech. Thus, one sees reasons for concern about definitions of language that emphasize the information that is *sent* rather than the thoughts and feelings that are *shared*. The informationally oriented paradigm that was, or seemed to be, available when ape research began was inspired by theoretical mathematicians – Shannon and Weaver (1949) – at least one of whom was also a cryptographer. These theorists designed a measure according to which communication was impossible if the "receiver" already knew the information "contained" in a message, but in reality people frequently interact precisely so they can have the experience made possible by their shared knowledge (Locke 2001).

Definitions of language grew up around such rational assumptions. Little surprise, then, that S&K would suggest, a half century later, that a different paradigm is needed for apes, a species in

which parents rarely vocalize or donate information to their young (King 1994a; 1994b). The new training procedures, they suggest, should be based more on social interaction (it is not clear that dynamic systems theory is actually needed here) and less on information processing. But this "new" paradigm has a decidedly familiar look, for as every good developmentalist knows, interaction – to the neotenous members of a gregarious species – is the only game in town. It would be surprising if some other "paradigm" worked better in animals that have no evolutionary linguistic history.

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Can dancing replace scientific approach: Lost (again) in chimpocentrism

A. Miklósi

Department of Ethology, Eötvös University, Budapest, Pázmány P. s. 1c, H-1117, Hungary. **miklosa@ludens.elte.hu**

Abstract: In communication studies, in contrast to the approach of the information-transmission hypothesis, the dynamic systems theory tackles the problem of continous feedback between interactors. However, Shanker & King's (S&K's) account seems to lack methodological elaboration, for the reader is presented with anecdotes. Furthermore, in contrast to the authors' beliefs, chimpanzees (and humans) are not the only animals able to show coregulated communicative interactions, for similar phenomena can be found in other animals, as for example in dogs.

Contrasting the "information-transmission" and the "dynamic dance" metaphors is not a good way of approaching the study of communication. First, as the authors correctly recognize, "information" is just as much a metaphor or a hypothetical scientific construct as is the "dance" metaphor, so the existence or nonexistence of information or dancing is not the main question. Any model of communication should be able to form predictable and testable hypotheses for experiments. The information-transmission hypothesis (ITH) was successful in providing researchers with testable problems in both animals and humans, the assumption being that communication occurs if the uncertainty (as perceived by an external observer) of the receivers' behaviour decreases after the sender emits a signal. Note that the receiver's behaviour is used here only as an indicator because, for an ethologically based approach to communication, it is the sender who invests and therefore should get the gains (Slater 1985). But in the target article, when S&K state in Table 1 that "communication is governed by fixed codes," "what is communicated is always information," and "development of species-typical communicative behaviors is maturational" (target article, sect. 2), the ITH is misleadingly presented because these features are not necessary attributes of this theory and are not fulfilled for most mammalian species.

Second, ITH is portrayed as relying on sequences of behavioural actions, and dynamic system theory (DST) communication is described as a coregulated process. This simply cannot be true because any string of actions by definition occurs in time. Even parallel actions are not immune to time, and this is even truer if we observe communicative (or any other) interaction between two or more individuals. Coregulation does not free one from the passing time in which we all live. Nevertheless, the authors correctly feel that there is difference between the two models in how they treat communicative interactions. In my view, the main difference is that ITH takes communication as a *one-way process* without being interested in the effect of the receiver's feedback on the sender. In contrast, the DST model places more emphasis on the feedback effects by modeling communication as a *two-way process.* Naturally, in one-way communication there is no need for regulation; in contrast, if the role of the sender or actor changes over time (turn taking) then regulation becomes a critical issue. Therefore the problem is not whether any information has been transmitted, but at which level of complexity the communicative interaction is modeled.

The authors apparently feel that chimpanzees have been left out of the *Homo* club (again), and by appealing to DST they could be smuggled in through the backdoor. But for ethologists there are no clubs and backdoors. Instead, there are selection processes and functional compatibility to the environments for species, as well as behaviour mechanisms and ontogenetical processes that allow individuals to interact with their environment on a daily basis.

The assumption that the DST approach reveals special abilities for apes and humans reminds me of the classic error of chimpocentrism. Some examples are given here from our research with dogs (for examples of related research projects with parrots, see Pepperberg 1991, and for projects with dolphins, see Herman & Tavolga 1980). Dogs have been selected for living in human social setting, and today most individuals of this species are members of family groups of humans. Over time, dogs must have been transformed to some extent to be able to survive among humans. We (Miklosi & Soproni 2003) and others (Hare & Tomasello 1999) have hypothesized that there has been a genetic adaptation of dogs to enable them to live in human society, and changes in their ability to communicate with humans could be one of them. First, dogs might have acquired novel abilities to "understand" human communicative actions. We have shown that dogs can perform better than most "socialized" apes in situations where humans are signaling hidden food by pointing (e.g., Soproni et al. 2002). Second, it seems that dog's communicative behaviours are less rigidly constrained by genetic influences, and can be molded by interaction with humans, leaving room for developmental ritualisation (Tomasello & Call 1997). This is supported by many everyday observations (Pongrácz et al. 2001) and staged experiments (Miklosi et al. 2000) showing highly variable idiosyncratic processes of coregulation in communicative situations between owner and dog. Although these interactions might not be as complex as those indicated for Kanzi or other apes, the difference is clearly not in quality but in quantity.

To take another example, S&K note that the ability to take turns is an important aspect in regulation of communicative interactions. We observed this ability in dogs when they were working for their blind owner (Naderi et al. 2001). Analyzing the behaviour of the human and the dog we found that there is a high frequency of turn taking as the role of the initiator changes in the dyad. Based on this we have hypothesized that blind leading is not solely the task of the dog but is a result of coregulated cooperative interaction of both participants. All this suggests that dogs, and not only chimpanzees, possess prerequisites for taking an active role in dynamic, creative, coregulated interactions.

Although one should not blame the authors for taking the dancing metaphor too seriously, I mention it because of their poor presentation of supporting data to the DST approach. What is needed here are clear questions and experimental tests (and not film extracts), and, most of all, statistically processed data. As a better example, take the research on courtship dance in humans, which can be viewed in a *functional* framework by hypothesizing that it is a means for males to signal (send information about) their quality to attractive females (Grammer 1990). Thus, the female can judge the male's competence and decide whether she finds him attractive. Although one could insist that some information has been transmitted, the metaphor breaks down because the result is the outcome of a joint interaction, and no clear one-to-one transmission can be traced. Therefore, we need theories and means to describe the *mechanism* that contributes to dancing behaviour. Because humans usually find synchrony more pleasurable that asynchrony, one could assume that the synchrony achieved by the dancers plays a key role in the maintenance of this activity. Grammer et al. (1998) supposed that the higher the interest of the dancers in each other, the more synchronized the dance should

be. Using a newly developed search algorithm by Magnusson (1996), they found little evidence overall for one-to-one synchronization by position mirroring or echoing in the partners' behaviour, but a female interested in a specific male showed longer and more elaborate dancing patterns that resulted in synchronization to the male's behaviour. Thus, female interest was expressed as an active contribution to the level of synchrony in the dance. This analysis should be a warning that verbal descriptions of communicative interactions should not be taken at their face value.

Clearly, what is needed in this research are new methods that collect data for the assumptions provided by DST, and what is not needed are anecdotes that are focused on whether or not one likes to dance with chimpanzees.

Could dancing be coupled oscillation? – the interactive approach to linguistic communication and dynamical systems theory

Erik Myin and Sonja Smets

Centre for Logic and Philosophy of Science, Department of Philosophy, Free University Brussels (VUB), B1050 Brussels, Belgium.

{emyin, sonsmets}@vub.ac.be http://homepages.vub.ac.be/~emyin/ http://www.vub.ac.be/CLWF/SS/

Abstract: Although we applaud the interactivist approach to language and communication taken in the target article, we notice that Shanker & King (S&K) give little attention to the theoretical frameworks developed by dynamical system theorists. We point out how the dynamical idea of causality, viewed as multidirectional across multiple scales of organization, could further strengthen the position taken in the target article.

The importance of this target article obviously outstrips the domain of animal language.

In fact, the article forms a further step in the unfolding of the dynamical, interactivist paradigm in which cognitive activity is seen as the exercise of a potentially ever-developing set of skills (Myin & O'Regan 2002), rather than as a computational process in a disembodied brain. It adds to the recent wave of detailed accounts from that paradigm on, for example, (visual) perception (O'Regan & Noë 2001) and social understanding (Gallagher 2001). Against the common claims that language research is all about specific neurocomputational processes in brain modules unique to humans, Shanker & King (S&K) offer a convincing plea for conceiving of language as a primarily interactive social practice. In this sense the work of earlier theorists such as Merleau-Ponty (1945) and Wittgenstein (1953) is being further pursued within a respectable contemporary scientific context.

We noticed that the authors do not offer a formal detailed elaboration on their position within the framework of dynamical systems theory (Abraham & Shaw 1992; Kelso 1995). Therefore, we ponder whether they would tend to follow recent cognitive theorists (Beer 1995; Keijzer 2001; Thelen et al. 2000) in attempting to connect the descriptions they offer at the behavioral level with a multiscale spanning explanatory dynamical account, where scales range from environmental events, including actions by other organisms, to nervous and muscle systems.

A typical dynamical systems story concerns the interactions between variables and parameters at different scales of organization and how these create a dynamical set of highly interlinked and interdependent patterns. In the established vocabulary, higher-level orderly patterns, which are described by collective variables, depend on (possibly simple) control parameters, which in turn often depend on the collective variables. Such is the case in the by-now classic example of finger movements described in Kelso (1995), where the simultaneous movement of the same fingers of the two hands, when started out of phase (the collective variable), automatically shifts to synchrony when the speed of movement (the control parameter) crosses a threshold.

Four aspects of the dynamical systems account seem particularly relevant. The first concerns the emphasis on emergence of a higher scale order, where emergence can be understood as a novelty because it arises out of interactions with parameters that are nonspecific, in the sense of apparantly unrelated (the specific value of the speed threshold in the finger example has no straightforward relation to synchrony). Second, changes at any level can have effects in any direction, upward or downward, implying the necessity to replace the notion of hierarchical level by the notion of scale (Alexander & Globus 1996). As such, the influence from higher to lower levels is mutual or, in the term of Kelso (1995), it can be described as "circular causality." Third, the dynamical systems approach seems to apply in particular to systems in which there are a large number of (nonlinear) interactions between subsystems. This last aspect entails a possible compatibility of the dynamical approach with the dance metaphor, for a dance almost literally can be seen as a coupling of two systems of coupled oscillations: here we have many coordinated patterns at different scales in one system (the whole body, the legs, the feet, the toes, and whatever nervous events that are involved) and harmonious synchronization effectively depends on the strength of the coupling of two such systems. This dance metaphor can be framed in dynamical systems terminology where, for example, changes in lower scale coupling strength or the range of individual oscillator frequencies that modulate the large-scale systems order are described in terms of a control parameter (Keijzer 2001). Fourth, though it is possible to develop dynamical accounts by referring to abstract variables and parameters that do not touch on the actual physical components of systems, some cognitive theorists try to develop dynamical accounts that specify parameters and variables that do directly refer to tangible aspects of the environment, other agents, and various scales of bodily, nervous, and muscular organization (see the references cited above).

Though there is some passing mention of the cognitive dynamical systems literature in the target article, we feel the authors' claims could be strengthened by it at various points, of which we single out two. First, there are sharp discussions of why one cannot interpret what the authors say as merely a higher scaled account of the phenomena, which still could be implemented through lower scaled mechanisms of a cognitivistic nature (see S&K's repeated discussion of Deacon). Basically, the authors' reply to the cognitivist seems to consist in re-emphasising that cognitivism turns the explanatory order upside down by misconstruing linguistic development as an internal event inside individual heads. However true this is, such a reply could be strengthened by reference to multiscales spanning dynamical accounts. As long as nothing is said about lower scales (and how they relate to higher ones), it remains possible for the cognitivist to insist there *must* be some relevant story to tell at the "subpersonal" symbolic level about representational processes in individual brains. It can be validly pointed out that this cognitivist move is empty, in the absence of further constraints, of what it is for an event at the subpersonal level to count as a representation. But the multiscale dynamical account allows for the further argument that mapping into subpersonal symbolic representational structures is impossible precisely because this account is inherently dedicated to multiplying directed causality rather than to delegating it to a single symbolic or representational level.

Second, we also think the dynamical multiscale approach could do justice to the nonreducible aspects of language development emphasized by S&K when they refer, for example, to the open-endedness and the possibility for continuous modification inherent in (proto-) linguistic communicative behavior. It is explicit in the dynamical literature that higher scaled order has a certain degree of autonomy vis-à-vis lower scales in at least two senses. First, it arises as a consequence from nonspecific triggers (in contrast to this, in the cognitivist paradigm, higher level events invariably emerge from lower level events that are isomorphic to events at

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the higher, behavioral, level). Second, there is the possibility of circular causality flowing down from higher scales. Perhaps this is the theoretical counterpart of the fact, brought prominently to the fore by the story of Kanzi, that achievements in linguistic development, such as a new understanding, can serve as the driving force for ever-novel linguistic development.

On the other hand, the multiscale picture might also remind us of the possibility of lower scale constraints; probably the specifics of the human vocal apparatus missing in other species impose limits on linguistic development – for example, by making it impossible for them to speak out loud to themselves (the importance of which, for the development of thought, has been stressed by many theorists; see Clark 1997, Ch. 10).

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Does the new paradigm in ape-language research ape behaviorism?

Joseph J. Pear

Department of Psychology, University of Manitoba, Winnipeg, Manitoba, R3T 2N2, Canada. **pear@cc.umanitoba.ca**

Abstract: Although Shanker & King (S&K) disregard the behavioral paradigm, their arguments are reminiscent of those in Skinner's *Verbal Behavior* (1957). Like S&K, Skinner maintained that communication is not appropriately characterized as the transmission of information between individuals. In contrast to the paradigm advocated by S&K, however, the behavioral paradigm emphasizes prediction and control as important scientific goals.

Shanker & King (S&K) emphasize the differences between their paradigm and the cognitive one, while basically ignoring the behavioral one. In their only substantive reference to *Verbal Behavior* (Skinner 1957), they imply that Skinner's behavioral approach requires the conditioning of a code whereby one individual transmits information to another (sect. 2, para. 4). Skinner, however, opposed the use of terms such as *idea*, *meaning*, and *information* that refer to inner things that are transmitted through speech:

> Idioms and expressions which seem to explain verbal behavior in terms of ideas are so common in our language that it is impossible to avoid them, but they may be little more than moribund figures of speech. The basic formulation, however, has been preserved. The immediate successor to "idea" was "meaning," and the place of the latter is in danger of being usurped by a newcomer, "information." (Skinner 1957, p. 7)

S&K's critique of the transmission metaphor echoes Skinner's treatment. For example, Skinner discussed an instance of one person telling another that there is gold in the Klondike. The traditional account of this episode is that the knowledge (or information) about gold being in the Klondike has been transmitted from one individual to the other. However, Skinner rejected this account:

> The fact [that there is gold in the Klondike] is not transmitted from one speaker to another. What is "made common" to both listener and speaker (to take the etymology of *communicate*) is either a verbal response or a resulting nonverbal tendency (to go to the Klondike when gold is reinforcing). (Skinner 1957, p. 364)

S&K's statement that "mutual understanding is something that *emerges* as both partners converge on some shared feeling, thought, action, intention, and so on" (sect. 2, para. 6, emphasis in the original) is reminiscent of the above reference to the etymology of *communicate* (to make common).

S&K state that, in addition to various kinds of information, "great apes and humans also communicate their desires and intentions, fears, warnings, invitations, attitudes, emotions, and so on" (sect. 2, para. 5). Skinner would have agreed that these communications are not transmissions of information. To conserve space, I consider only the first item listed – desires, which Skinner dealt with in his concept of the "mand." He illustrated with the example of someone manding candy with the exclamation "Candy!", which would traditionally be regarded as having the referent (or "meaning") candy:

> But "what is communicated" would appear to be "the speaker's need for candy," which refers to the controlling state of deprivation. The concept of the mand . . . explicitly recognizes both contingency of reinforcement and deprivation or aversive stimulation and is free to deal with these variables in appropriate fashion *without trying to identify a relation of reference or a process of communication.* (Skinner 1957, p. 44; emphasis added)

In addition, in striking concordance with S&K, Skinner described the kind of analysis required by verbal behavior as follows:

> We need separate but interlocking accounts of the behaviors of both speaker and listener if our explanation of verbal behavior is to be complete. In explaining the behavior of the speaker we assume a listener who will reinforce his behavior in certain ways. In accounting for the behavior of the listener we assume a speaker whose behavior bears a certain relation to environmental conditions. The interchanges between them must explain all the conditions thus assumed. (Skinner 1957, p. 34)

Speaker and listener roles alternate, and could be described as a kind of dance (although I am unaware that Skinner ever used that particular metaphor). In any case, this description clearly fits "coregulated activity" as described by S&K.

S&K make much of the tendency for language-trained chimpanzees to correct themselves or others. These are editing processes, which Skinner discussed extensively in *Verbal Behavior* (e.g., Ch. 15). It is interesting that humans are not the only species that engage in this behavior.

In addition to the above consistencies with Skinner's *Verbal Behavior*, S&K frequently make behavioral-sounding statements, as illustrated in two examples: "Whether or not a subject understands the meaning of a symbol, or another speaker, is established by *what she says or does* in the context of dynamic interactions" (sect. 3, 4th last para., emphasis in the original) and "Kanzi behaves in a manner that satisfies the criteria for describing him as '*understanding* what he is being asked to do'" (sect. 4.2, para. 5, emphasis in the original). Also characteristic of behaviorists, S&K eschew the concept of mind in a manner that is reminiscent of Skinner's critique of mentalism.

There are differences between the new paradigm and the behavioral one. The question that needs to be addressed, however, is whether these differences justify the new paradigm. To answer this, we need to decide what purpose we expect a paradigm to serve. Skinner's purpose, as expressed in *Verbal Behavior*, was to understand verbal behavior in the following sense:

> The "understanding" of verbal behavior is something more than the use of a consistent vocabulary with which specific instances may be described. It is not to be confused with the confirmation of any set of theoretical principles. The criteria are more demanding than that. The extent to which we understand verbal behavior in a "causal" analysis is to be assessed from the extent to which we can predict the occurrence of specific instances and, eventually, from the extent to which we can produce or control such behavior by altering the conditions under which it occurs. (Skinner 1957, p. 3)

S&K's purpose seems to encompass only the limited understanding referred to in the first sentence of this quotation. This is suggested by their emphasis on creativity as an unanalyzed feature of verbal interactions and by their advocacy of a hermeneutic stance. Certainly perfect prediction may be an impossible goal, especially with regard to something as complex as verbal behavior; however, given how far S&K have gone in describing their subject

matter behaviorally, why not continue to the next step, why not explore how the prediction and control of verbal behavior can flow from their paradigm?

Research scientist

Irene M. Pepperberg

School of Architecture and Planning, Massachusetts Institute of Technology, Cambridge, MA 02139. **impepper@media.mit.edu**
http:// www.alexfoundation.org

Abstract: Viewing communication as a dynamic system is laudable; arguing that the approach is novel is questionable. Some researchers studying nonhuman communication other than ape language have been using such an approach for decades. A brief description of an avian system provides one such example. Interestingly, the dynamic social system described in the target article may have a developmental neuronal basis.

I applaud Shanker & King (S&K) for examining communication as a dynamic system. I disagree, however, that Savage-Rumbaugh originated dynamics in ape language research (ALR), and I disagree with S&K's primate-centric view. Many nonprimate studies have long championed this approach (see the reviews in Pepperberg 1999; 2001).

ALR dynamics began with Gardner and Gardner (1969). The Gardners, unfortunately, were not lauded, and in fact were so strongly attacked (e.g., Sebeok & Rosenthal 1981) that they could not continue their research. I have no desire, however, to rehash that controversy.

Concerning avian studies, Todt (1975) was the first to examine Grey parrot vocal learning dynamics. He tested his hypothesis that Greys, like songbirds who also live in large social groups, learn by eavesdropping: watching and listening to a network of social and vocal interactions, making deductions and assumptions about the results of such behavior (e.g., Peake et al. 2002). Via his Model/Rival (M/R) technique, whereby two humans demonstrate vocal behavior to be acquired, Todt's Greys learned German phrases in less than 72 hours. His data strikingly contrasted with previous failures to train mimetic birds using operant techniques and little, if any, social interaction (Grosslight & Zaynor 1967; Mowrer 1954). Todt's parrots, however, replied only to the human trainer initiating interactions and they learned phrases from only the human M/R respondent (the Model for the bird's responses and its Rival for the trainer's attention).

My subsequent interspecies communication research with Greys adapted Todt's technique: I interchanged the roles of M/R and trainer, included birds in dialogues, and instituted referential rewards. A bird observed two humans handling an object; the trainer then queried the M/R as to "What's here?", "What toy?" The trainer rewarded correct responses with the item, demonstrating referential and functional label use, respectively, by providing a 1:1 correspondence between label and object, and modeling label use as a means to obtain the object (Pepperberg 1981). The M/R now also erred and was vocally chided by trainers (e.g., "No, you're wrong"), who temporarily hid the object. When responses were garbled or incorrect, the M/R was told to speak clearly or to try again. M/R and trainer reversed roles, showing how either party could request information or effect environmental change. After humans modeled interactions 2 or 3 times, the bird was queried about the object. Initially, any novel utterance related to the target label (e.g., /i/ for "key") was rewarded; object labels/sounds used for other purposes were not. Thus, the bird was part of a dynamic, synchronous, triadic, interactional network. (Parallels should be evident with Kanzi's subsequent experience watching his mother's training.) Parrots' results, again, were striking. The birds began using English labels referentially, acquired concepts once thought limited to primates, responded to anyone, and learned all parts of interactions (Pepperberg 1981; 1999).

I then investigated why M/R training succeeded. Previous interspecies communication studies compared different training methods' efficacies (Baptista & Petrinovich 1984, 1986; Fouts 1972), or described conditions enabling allospecific learning (Savage-Rumbaugh 1991), without determining what input aspects were necessary and sufficient for referential acquisition. Beginning in 1991 with naive parrots, students and I varied presence/absence/relative amounts of functionality, reference, and social interaction using video and audiotapes, and testing the effects of joint attention (note Baldwin 1995) and one trainer. We found that only birds exposed to the full complement of interaction, reference, and functionality in M/R training acquired communicative competence (Pepperberg 1999; Pepperberg et al. 2000).

Additional dynamic input, however, was important. Our parrots spontaneously produced utterances initially lacking language value (sound play), just as young children do. Children's vocalizations quickly acquire value when interpreted by caretakers as meaningful and intentional; so did those of our birds (Pepperberg 1990). For both birds and children, repeated interactions "conventionalize" both sound patterns and sound-meaning connections toward standard communication. And, like children (e.g., Brown 1958; 1973), birds use caretakers to provide referential information for relatively novel labels (Pepperberg 2001). For example, our youngest subject uttered a label previously used in a specific context – "wool" for a woolen pompon – while beaking a trainer's sweater. Our responses – including high affect and excitement – stimulated him further, revealing the potential power of his utterances and encouraging further categorization attempts. Even birds' initial erroneous categorizations engendered information about a correct, new label for an item (e.g., we stated that almonds aren't "cork" but "cork nut"; Pepperberg 1999; 2001). Thus parrots, like children, seemingly have a repertoire of desires and purposes, driving them to form and test ideas in dealing with the world; these ideas may be early stages of representation and categorization in cognitive processing (Pepperberg 2001). Baldwin (2000) proposed that human infants (12–18 months) use clues to others' intentions to guide their interpretation of language, emotion, and action; with such information, infants avoid potential errors and quickly learn relevant new skills. Research data suggest that parrots act similarly in learning from human trainers.

Finally, recent mirror neuron (MN) data (Arbib & Rizzolatti 1996; Fadiga et al. 1995; Fogassi 2000) suggest a basis for S&K's coregulated activities. Briefly, when a subject does not overtly replicate an observed action, MNs fire as though it had – creating the chance for synergy. The human MN system likely evolved to analyze and developmentally recreate actions (including speech) to which it is exposed, wherein neurons react which activate muscles not directly observed (Sundara et al. 2001). I propose that exposure to complex speech patterns and everyday combinatorial behavioral acts in (at least) humans initiates recruitment of neurological paths (involving MNs) that are critical for simultaneous complex combinatorial behavior, including verbal-nonverbal communication; emergence is a matter of achieving physical competence and choice to execute such behavior. Although coregulated behavior seemingly occurs without ostensible training, training in fact begins at the organism's birth.

How might avian systems work? Does hearing human speech and seeing human actions initiate comparable patterns in parrot brains? If action planning is the ability to select (even unconsciously) appropriate neurons and combine them into patterns of appropriate temporal activation (Arbib & Rizzolatti 1996), M/R training could strengthen or help form connections in whatever system does exist (Brauth et al. 2001).

In sum, the dynamic approach favored by S&K is likely correct, but it is neither new nor unique to primates.

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Metaphor muddles in communication theory

Drew Rendall and Paul Vasey

Department of Psychology and Neuroscience, University of Lethbridge, Lethbridge, Alberta T1K 3M4, Canada. **{d.rendall; paul.vasey}@uleth.ca**
http://home.uleth.ca/~d.rendall
http://www.psych.uleth.ca/People/Vasey/Vasey.html

Abstract: Shanker & King (S&K) argue that information-theoretic approaches to communication are too rigid to capture the ebb and flow of communicative interactions. They advocate instead a dynamic systems approach based on the metaphor of dance. We focus on two problems arising from the dance metaphor: first, that its inherently cooperative tone contradicts basic tenets of behavioral biology; and second, that it risks obscuring rather than clarifying the details of communicative interactions.

Shanker & King (S&K) propose "a fundamental shift in communications theory from an information-processing to a dynamic systems paradigm" (target article, sect. 2). The dynamic systems paradigm they advocate is based on the metaphor of dance in which "communicating partners continuously establish and sustain a feeling of shared rhythm and movement" (sect. 2). Accordingly, they stress the importance of interactional synchrony, mutual attunement, and affective resonance between participants. They liken the processes to the choreographed harmony of a waltz. They illustrate their approach with a thorough redescription of ape language studies, especially those involving the chimpanzees Sherman and Austin and the bonobo Kanzi, in which synchrony and attunement were apparently critical in the emergence of effective communication between the animals and their human instructors.

S&K are probably right to stress that communicative interactions are inherently dynamic. They may also be right that participants' affective and motivational systems play an important role in regulating communicative dynamics. However, their additional specific emphasis on mutual attunement between participants seriously limits the scope of their proposal to situations in which the motives and interactive goals of communicating parties are largely coincident. This may be approximately true for many of the communicative contexts involved in the ape language studies they review, or, at the least, it may be a productive ideal to foster in future efforts to teach language to another species. It may also be approximately true in some other natural communicative contexts. For example, in some baboons, pairs of males perform ritualistic greeting displays that involve a series of coordinated actions that include reciprocal genital inspection and handling (Smuts & Watanabe 1990). Considerable behavioral synchrony and possibly mutual attunement may be central to these greeting interactions. However, the intrinsically cooperative flavor of the communication dynamic that S&K stress inherently limits its general applicability to the broad sweep of natural communicative interactions in these and other species.

If the past 40 years of research and theorizing in behavioral biology have taught us anything, it is surely that although the interests and interactional goals of interacting parties are sometimes overlapping, they are seldom completely symmetric and are often profoundly asymmetric (Hamilton 1964; Trivers 1971; 1974). Consequently, many social encounters are expected to involve negotiating conflicting motives and objectives, with associated undertones that are the antithesis of mutual attunement. In communication theory, alternative frameworks have been developed recently that embrace this point and also incorporate the dynamic nature of communicative interactions. For example, Owings and Morton's (1997) "Manager-Assessor" framework explicitly characterizes communication as a back-and-forth dynamic between signalers trying to influence others as part of a broader effort to manage their environments to individual advantage and receivers assessing both signals and the signaler's other behavior as part of its own broad management effort. Similarly, Owren and Rendall's (1997) "Affect-Induction" framework views signalers and receivers engaged in a recursive and reciprocal effort to influence each other through signals that induce affective responses conducive to their own respective interests. Both frameworks allow for symmetry in participants' interests. However, neither framework requires it. Both predict the communicative dynamic as often to involve negotiation of conflicting goals, each party pursuing its own objectives while resisting its partner's. If the metaphor of dance had to be used, the appropriate step would as often be a slam-dance[1] as a waltz.

The more general problem in S&K's proposal is the explanatory reliance on metaphor at all. Both the dynamic systems approach that S&K advocate and the information-theoretic approach they critique lean heavily on metaphor. S&K invoke the metaphor of dance. The information-theoretic framework frequently invokes the metaphor of *meaning*. As an analogy to the words of language, animal signals are held to contain meaning. Communication is proposed to involve the encoding, transmission, and decoding of this meaning. These and other metaphors can make useful explanatory shorthand for processes whose details are well understood. The "selfish-gene" metaphor in behavioral biology is a good example (Dawkins 1976). However, there is the risk of seriously misrepresenting those details when the metaphor is interpreted literally or when it is used as an a priori explanation of them.

Consider the following example. Young rodent pups that are separated from their nest produce ultrasonic vocalizations. The vocalizations are a by-product of the pups' physiological response to cold stress that involves non-shivering thermogenesis in brown adipose tissue. The increased metabolic requirements associated with this process involve constriction of the larynx following inhalation that functions to increase air pressure and oxygen transfer in the lungs. Ultrasonic sounds result during prolonged expirations against this constricted larynx, a phenomenon referred to as "laryngeal braking" (Blumberg & Alberts 1997). Mothers are responsive to the ultrasonic calls produced from this laryngeal braking and rapidly retrieve the pups. Pups do not call in other circumstances if they are not also cold-stressed, even though retrieval in those circumstances would often be appropriate. Here then is an example of functionally integrated communication between mothers and pups in which the ultimate interests of both parties are likely to be quite overlapping if not actually symmetric. Yet, even here there is no requirement of, or evidence for, mutual attunement or interactional synchrony. Nor is it easy to assign meaning to the pups' ultrasonic calls: "I'm cold . . . I'm stressed . . . I'm anxious . . . I'm out of the nest . . . Help me . . . Retrieve me . . . Warm me. . . ." The calls are an incidental by-product of a physiological response to cold stress for which selection has favored maternal perceptual sensitivity and behavioral responsiveness. Neither the metaphor of dance nor the metaphor of meaning accurately captures either the mechanisms underlying signal production and maternal responding or the functional consequences for either party. Surely these are exactly the phenomena that a theory of natural communication aims to explain.

In sum, if S&K's approach is to represent a paradigm shift in communication theory, they will need to demonstrate its applicability beyond the limited cases where participants' interests and goals are expected to be mutually attuned. They will also need to cash-out the dance metaphor for a far more concrete explanation of communicative processes.

NOTE

1. Slam-dancing, or "moshing," was popular in the 1980s and early 1990s. There were few formal rules or "steps." Instead, participants attempted to "slam into" others, at the same often trying to avoid being slammed into by them.

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Great ape communication: Cognitive and evolutionary approaches

Anne E. Russon[a] and David R. Begun[b]

[a]*Psychology Department, Glendon College of York University, Toronto, Ontario, M4N 3M6, Canada;* [b]*Anthropology Department, University of Toronto, Toronto, Ontario, M5S 3G3, Canada.* **arusson@gl.yorku.ca begun@chass.utoronto.ca www.yorku.ca/arusson http://www.chass.utoronto.ca/anthropology/Faculty/Begun/**

Abstract: There are good arguments for examining great ape communicative achievements for what they contribute to our understanding of great ape cognition and its evolution (Russon & Begun, in press a). Our concern is whether Shanker & King's (S&K's) thesis advances communication studies from a broader cognitive and evolutionary perspective.

Communication has considerable importance for understanding great ape mentality. Across domains, communication included, great apes have been shown to achieve cognitive abilities in the rudimentary symbolic range (alternatively, representational, hierarchical, or second-order) – notably, beyond the sensorimotor level 6 that S&K suggest (e.g., Byrne 1997; Langer 2000; Parker & McKinney 1999; Russon et al. 1996). The great apes' cognition may reach beyond modularity, in that abilities in different domains may facilitate one another. Communicative experiences are therefore probably important contributors to great ape cognitive development, insofar as their cognitive abilities are emergent products constructed on the basis of experience (Parker & McKinney 1999; Russon 1999; Suddendorf & Whiten 2001; Tomasello et al. 1993).

Comparative studies represent cognition in great apes as broadly intermediate between other anthropoids and humans, and linked with information processing challenges in the realms of diet, socioecology, and social relations (Russon & Begun, in press b). It is then important to identify what evolutionary forces rendered valuable cognitive systems like these, reliant on communication ontogenetically and so highly flexible, loosely channeled, and highly complex. Because some of great apes' achievements, communicative achievements among them, are in the range traditionally considered uniquely human, answers to these questions have implications for understanding the evolution of human mentality.

In short, great ape cognitive prowess is manifest in many contexts, including communication. It probably owes to numerous interrelated and interacting causes, one of them being selection for centralized generative mechanisms that construct abilities in individual problem areas on the basis of communicative as well as individual experiences. Here we consider S&K's information processing (IP) and dynamic systems (DS) metaphors, the latter proposed as a new paradigm, in light of communication as an important dimension of cognitive research.

The information processing metaphor, to S&K, turns on Shannon's concept of information. Views they attribute to the information processing metaphor are hard to square with Shannon's mathematical concept. Attitudes and concepts from classical ethology, behaviorist learning theory (much of it predating Shannon's work), and discontinuity models of communication are included, for instance, along with views from models of complex cognitive and brain processes. S&K characterize information processing by contrasting it with dynamic systems in terms of the qualities of behavior implied, as innately versus experientially developed, tightly versus loosely channeled, simple versus complex, sequential versus dynamic, and rigid versus creative. Modern models of complex cognitive and brain processes, however, concern developmentally labile, flexible, and complex behavior – that is, behavior less consistent with information processing than with dynamic systems metaphors. Further, for us, an equally if not more important contrast is between proximate and ultimate causes of communicative capabilities.

S&K's evaluation finds the information processing and dynamic systems metaphors incompatible rather than complementary – hence, their hailing a paradigm shift (from "Deaconesque" to "Savage-Rumbaughian") and the demise of the information processing metaphor. This evaluation rests on several factors. The authors dismiss internal states as unknowable and unimportant factors in language or communicative phenomena: The meaning of behavior is the behavior itself. This is reminiscent of strict behaviorist or postmodernist positions that eschew consideration, or even the existence, of nonobservable phenomena. We understand behavior to imply associated internal states within individuals. It is essential to investigate in great apes the internal states that accompany their linguistic achievements *as well as* the communicative dynamics that affect them. S&K's criterion for evaluating Deacon's and Savage-Rumbaugh's explanations is sufficiency in explaining the linguistic achievements of a small number of individuals of *Pan.* However, there are other questions about ape language for a new paradigm to tackle. In the target article, several cognitive issues are acknowledged but put aside, including what kinds of concepts language-experienced apes have mastered and in what order they are acquired, what communicative skills great apes master and what seem to be beyond their grasp.

S&K's dynamic systems metaphor appears to create an impasse for the study of cognitive and evolutionary dimensions of communication. If this is so, this metaphor for communication offers little to these realms of enquiry. As presented in the target article, the dynamic systems metaphor is a contextual, performance-oriented communicative dance model that applies to most species; therefore, no basis is available for comparing communicative processes across species or assessing their relationship to one another.

This leaves us asking whether multiple approaches can be brought together. Features of communication which are of interest to S&K and which are brought out by the dynamic systems metaphor are context, quality, flexibility, and complexity. These are all descriptors amenable to cognitive interpretation and analysis. Some cognitive developmental models consider the role of social forces; some, for instance, have incorporated Vygotskian-like concepts of the sort that mesh with S&K's dance metaphor (e.g., Fischer et al. 1993). These models have been applied, fruitfully, to great ape cognition (Parker & McKinney 1999). Efforts like these suggest integration may be possible.

If a paradigm shift is indeed imminent, as a valuable guide to research it must inform many approaches. S&K's dynamic systems metaphor is so focused on the minutiae of the proximate causes of communicative achievements that it is doubtful whether it can be applied usefully to questions of the nature of cognition or of cognitive, communicative, or language evolution. Without informing our understanding of comparative communication or of the evolution of communication, there is no way to get from observations of chimpanzee or bonobo communication to insights into the development of language, as claimed by S&K. Integrating generative and experiential approaches with comparative and evolutionary approaches may one day produce a fruitful new paradigm.

Contribution of plasticity of sensorimotor cerebral cortex to development of communication skills

Barry J. Sessle and Dongyuan Yao

Faculty of Dentistry, University of Toronto, Toronto, Ontario M5G 1G6, Canada. **{barry.sessle; a.yao)@utoronto.ca**

Abstract: Several lines of evidence have underscored the remarkable neuroplasticity of the primate sensorimotor cortex, characterizing these cortical areas as dynamic constructs that are modelled in a use-dependent manner by behaviourally significant experiences. Their plasticity likely provides a neural substrate that may contribute to the dynamic systems paradigm argued by Shanker & King (S&K) as crucial for development of communication skills.

Shanker & King's (S&K's) discussion of the recent advances in ape language research and the emerging conceptual shift to a dynamic systems paradigm, suggests that such a paradigm sheds light on the factors underpinning the development of communication skills and the relationship between communicative development and the development of language. The authors note that facial expressions constantly change in response to the changing dynamics of social interactions, which is an example of what they term "the plasticity of great ape capacities." Our comments focus on recent neuroscience research into the neuroplasticity of primate cerebral cortical areas contributing to communication and other orofacial motor skills.

The transformations in behaviour outlined by S&K reflect alterations to higher brain neural circuitry related to cognitive and affective behaviour, and to output pathways from higher brain centres that drive the muscles participating in facial expression and other communicative behaviours. In the case of the primary somatosensory (SI) and motor (MI) cortices, most studies of their organization and neuroplasticity in the human and nonhuman primate have focussed on limb sensorimotor control (for review, see Buonomano & Merzenich 1998; Chen et al. 2002; Sanes & Donoghue 2000; Weinberger 1995). Recordings of neuronal activity in limb MI of monkeys and neuroimaging studies in humans have revealed remarkable neuroplasticity in the motor representations and neuronal properties within MI as a result of a variety of manipulations and behaviours ranging from lesions of sensory and motor nerves to the learning of motor skills and cognitive motor events. This plasticity can occur when already learned movement sequences are repeatedly performed from memory or when they are performed in response to sensory cues. Some of these changes in motor representation or neuronal activity within limb MI can occur quickly (within minutes) and are reversible, indicating that MI actively and dynamically participates in the neural changes required for the learning of new motor behaviours. Sensory manipulations or behavioural task acquisition can, in addition, be associated with neuroplastic changes also in limb SI (e.g., an expanded somatosensory representation of those sensory inputs that are most critical to the learning of a skill). Moreover, comparable neuroplasticity has been documented in auditory and visual cortices (Buonomano & Merzenich 1998; Syka 2002).

However, the organization and learning-associated plasticity specifically of the face sensorimotor cortex and associated cortical regions have received little attention in the primate, although findings in humans and monkeys and subprimates attest to the plasticity or progressive return of function of face sensorimotor cortex following cortical damage or trauma to peripheral tissues (see Buonomano & Merzenich 1998; Chen et al. 2002; Martin & Sessle 1993; Sanes & Donoghue 2000; Weinberger 1995; Yamamura et al. 2002) and its participation in some aspects of speech (Bookheimer et al. 2000; Cabeza & Nyberg 2000; Price 2000; Rumsey et al. 1997). We have recently begun investigating the possible role and plasticity of the face sensorimotor cortex in the monkey's acquisition of orofacial motor skills, building on our earlier findings from intracortical microstimulation (ICMS) mapping and recording of neuronal activity in the awake monkey's face MI. The mapping had revealed an extensive representation of the orofacial muscles participating in facial expression and other communicative behaviours as well as in cyclic jaw and tongue movements associated with ingestive behaviours (e.g., licking, mastication, swallowing; Huang et al. 1988, 1989; Martin et al. 1999). The cyclic movements can be evoked not only from face MI but also from cortical regions more lateral to face MI (e.g., the "cortical masticatory area," CMA). These orofacial motor behaviours are severely disrupted when the face MI or adjacent cortical regions are transiently inactivated (e.g., Murray et al. 1991; Narita et al. 2002; Yamamura et al. 2002), emphasizing the crucial role that these cortical areas play in the expression of these diverse motor behaviours. This view is further supported by findings that many neurones in face MI discharge in relation to trained tongue or jaw motor tasks, and those in CMA also are active in relation to cyclic movements associated with ingestion or facial expression (e.g., Luschei & Goldberg 1981; Martin et al. 1997; Murray & Sessle 1992; Yao et al. 2002a). Furthermore, similar neuronal discharge patterns occur in face SI, and inactivation specifically of face SI disrupts both learned motor behaviour and cyclic orofacial movements (e.g., Lin et al. 1993; 1994; 1998), underscoring the importance of somatosensory processing in cortical mechanisms contributing to the guidance, and potentially the learning, of articulatory as well as masticatory and other orofacial motor skills. In our most recent studies of the primate face sensorimotor cortex, we have tested for its plasticity during the monkey's learning of a novel tongue-protrusion task (Yao et al. 2002b). The orofacial motor representations and neuronal activity patterns within face MI were determined before and after the animal learned this orofacial behaviour. Consistent with findings in limb MI (see above), we found that before the tongue-task training only one-quarter of neurones showed tongue-protrusion related activity, whereas three-quarters of those recorded after training had tongue-protrusion related activity. Furthermore, the proportion of loci in tongue MI from which tongue protrusion could be evoked by ICMS was significantly higher than that before training. Such preliminary data are indicative of face MI neuroplasticity and provide evidence of a role for MI in orofacial motor learning in primates.

These various findings in primate face as well as limb sensorimotor cortex emphasize that MI and SI are dynamic and adaptive neural constructs that are modeled by behaviourally significant experiences. It seems highly likely that they are involved in mediating, at least in part, the complex sensorimotor and cognitive phenomena associated with the development and acquisition of communication skills, and that they contribute to the neural substrate underpinning the dynamic systems paradigm outlined by S&K. Future research needs to explore this possibility more directly, by examining the plasticity of face MI, SI, and adjacent cortical areas involved in orofacial motor behaviour, in relation to the development of primate orofacial communication.

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Information processing and dynamical systems approaches are complementary

David Spurrett

Department of Philosophy, University of Natal, Durban, 4041 South Africa.
spurrett@nu.ac.za **http://www.durbanphilosophy.nu.ac.za**

Abstract: Shanker & King (S&K) trumpet the adoption of a "new paradigm" in communication studies, exemplified by ape language research. Though cautiously sympathetic, I maintain that their argument relies on a false dichotomy between "information" and "dynamical systems" theory, and that the resulting confusion prevents them from recognizing the main chance their line of thinking suggests.

Speaking very generally, information theory is primarily concerned with *structure* (understood via concepts like compressibility, probability, entropy, etc.) and dynamical systems theory (DST) with *change* (making use of differential equations, trajectories through state-spaces, etc.). For the purposes of science, the two, thus understood, are pragmatically complementary and probably conceptually inextricable (Collier 1999). The behavioral sciences, in particular, are simultaneously about some distinctive types of structure and the processes (themselves structured through time) by which these structures are produced and maintained. Not only is it not *required* that we chose between the two, Shanker & King's (S&K's) conception of them as being alternatives is probably incoherent.

As a mathematical apparatus, information theory makes no claims at all about the nature of cognition. In particular, it does not demand an episodic turn-taking model of communication between cognitive systems, require that such communication make use of conventionalized encryption, require a theory of cognitive processing dependent on passive "internal" representations, or entail anything at all about whether nonhumans are capable of language. Confusing information theory with these commitments means relinquishing crucial tools for rejecting them as requirements for a model of cognition (e.g., that they demand the execution of unlikely encoding and decoding tasks), and for developing the details of a distinctive alternative, insofar as these involve specifying what acts of discrimination are to take place, under what time constraints, in what ways different streams of information processing relate to one another, and how they manifest in behavior.

Conversely DST, itself a mathematical apparatus, can in principle be used to describe *any* distribution of cognitive labor, including highly internalist approaches involving just the types of representation and communication S&K reject. This fact is obscured by S&K's tendencies to conflate dynamical with "distributed" as though dynamical systems approaches *automatically* involve control parameters in the wider environment, and to limit much of what they say about DST to metaphorical claims about "dance," and impressionistic references to coregulation, canalization, and so forth.

S&K, then, draw a coherent-seeming contrast, by means of two expedients: first, saddling information theory with a range of nonessential commitments amounting to a restrictive and intrinsically implausible conception of mind; and second, providing almost no nonmetaphorical detail about DST. Acceptance of the resulting false dichotomy makes all references to information, encoding, and representation seem suspect, thereby leading S&K to miss their main chance.

I take it, for present purposes without argument, that some of our most empirically and theoretically powerful treatments of language proceed on the assumption that language is in some sense symbolic and systematic. Many take the fact that language presents this "digital" aspect as justification for thinking that it *is* fundamentally digital. Even those wary of viewing language *as* digital (due to worries about neural implementation, adaptive explanation, developmental plausibility, etc.) nonetheless regard the digital aspects of language as setting an important explanatory target for cognitive science. What motivates this caution is recognition that even if any given theory of language turns out to be incorrect, we would be unwise to relinquish the data, or the powerful and economical ways of representing it made possible by treating language as in some sense digital. This type of conservatism is entirely proper, and furthermore provides a key constraint on any "paradigm change" likely to affect the course of scientific research. By ignoring or undervaluing it, S&K demand too destructive a revolution.

S&K's positive case involves drawing our attention to a variety of reasons (some from well-established research programs) for thinking that nondigital aspects of interactive, situated behavior are crucially important for language learning and for the functioning of language. I agree, and also grant that taking these aspects of behavior seriously (as increasing numbers of researchers do), could lead to significant changes in how language is understood. The crucial question that S&K's destructive radicalism prevents them from taking seriously, though, is *how do the digital and nondigital aspects of language relate to one another*?

Emotions, which are sometimes coregulated, can be seen (Ross & Dumouchel 2001) as strategic signals encoding preference intensities (typically more difficult to infer than orderings) in ways that, unlike standard commitment devices, do not require explicit construction prior to strategic interaction. By having preference intensities thus represented (even if roughly), otherwise intractable strategic problems can be negotiated. I speculate that dance-like aspects of interactive behavior could support the digital aspects of language, in effect by constraining the computational searches required for handling symbolic and systematic tasks.

To evaluate this hypothesis would require exploring the relationships between different streams of information processing (some overlapping or interacting), different types of control system (some enabling coregulation), each with particular processing capacities, time-budgets, varyingly distinctive histories, and so forth. It is *interesting* to know that "cross-modal" matching (target article, sect. 1, para. 4) takes place, for example, when dynamical properties of an infant's gesture are copied in its mother's responding vocalization. But saying we should eschew talk of information in favor of viewing such activities as part of a "dance" is unhelpful. As behavioral scientists we want to know how such matching is possible, and what difference it makes to learning and using language. This demands asking about the (information) processing capacities and properties of different organisms and their cognitive subsystems, and how these relate to the (dynamical) control of behavior. If research in this direction is to be pursued, though, we need to eradicate the reflex to go on the offensive whenever we hear reference to "signals," "codes," or "representations," let alone "symbols" or "grammar."

Information, representation, and the dynamic systems approach to language

John Symons

Department of Philosophy, The University of Texas at El Paso, El Paso, TX 79968. **jsymons@utep.edu** **http://www.utep.edu/philos/symons**

Abstract: Shanker & King (S&K) provide a criticism of information-theoretic approaches to language, but the real obstacle to their dynamicist approach is the argument that representations are an indispensable part of any cognitive theory. Since the dynamicist approach has a prima facie anti-representationalist bent, the authors must show why dynamicist views can provide adequate explanations of intelligent behavior.

By now, it should be well beyond doubt that dynamic systems theory (DST) provides a set of mathematical tools that can be applied to many areas of investigation in a fruitful manner. A growing number of researchers are finding DST particularly well-suited to the study of the complex, and often nonlinear processes that constitute cognition in biological settings (Port & Van Gelder 1995). However, many philosophers, linguists, and cognitive scientists shy away from DST because of its antirepresentationalist implications (Eliasmith 1997). With their target article, Shanker & King (S&K) contribute to the effort to win over the wider community to the dynamical approach. While supporting their effort, I think they do not fully appreciate the reasons so many researchers resist DST.

Objections to DST are obviously not objections to the mathematical tools used in the study of dynamical systems by physicists and others. Instead, the principal objections involve the explanatory merits of the metaphors, models, and explanations derived from these tools. Much of the target article describes research inspired by the dynamicist metaphor of dance, so the authors are clearly not relying on the authority of established techniques in physics or mathematics to make their case. Instead, by arguing for the usefulness of the dance metaphor and for the effectiveness of the ALR methods inspired by those metaphors, the authors are taking a clear position on what they think counts as a good way to look at language. Language, according to the authors, is better viewed as a dance than as an encryption process.

As the article convincingly demonstrates, the dance metaphor has led some researchers to treat language development with a greater degree of sensitivity than do those researchers guided solely by more traditional approaches. At the very least, one can agree that it has caused researchers to discover new connections, to rethink the relative importance of certain details, to construct revealing new experiments, and so forth. However, the real challenge for the authors involves going beyond simply showing that

the dynamical approach provides an interesting and potentially fruitful new way of looking at linguistic phenomena. If this is to be a genuinely new paradigm in language research, the difficult conceptual work ahead involves defending the explanatory and predictive power of the dynamical approach *over* its major competitors.

By contrasting the dynamical approach with information theoretic approaches, the authors have been less successful in making their case than they might have been had they tackled head-on their real adversary: the representationalist model of language and cognition. The article repeatedly mentions the anti-representationalist implications of the dance metaphor, but it does not make the case for the reason why representationalists should risk losing as powerful an explanatory notion as representation. Information theory per se is not what supports the explanatory power of traditional theories. Rules and representations, not information, lie at the core of traditional explanations of cognitive phenomena. In Deacon's (1997) case, for instance, very little rests on the notion of information; instead, it is the establishment of higher order relations between representations that he claims marks the transition from iconic to symbolic languages in animals.

By targeting information-theoretic approaches, the authors have chosen the wrong opponent. In part, this is because, in the brain and behavioral sciences at least, *information* is such a slippery notion. Fred Dretske (1981) put it well when he wrote that "its use in telecommunications and computer science gives it a tough brittle and technical sound, and yet it remains spongy, plastic and amorphous enough to be serviceable in cognitive and semantic studies" (p. ix). Like Dretske, Ken Sayre (1976) pointed out that uses of the term *information* in cognitive science are almost never connected with the mathematical definition of information as provided by Shannon and Weaver. Historically, contexts in which this term has been given a precise definition include Shannon's well-known mathematical theory of communication, the theory of semantic information of Carnap and Bar-Hillel, and later the theories of informational complexity associated with Kolmogorov and Solomonoff. For the most part, uses of *information* by linguists, cognitive scientists, and neuroscientists conform to none of these three contexts. In my view, dynamicists are right to argue for their approach over the unrealistic rules and representations models we find in many areas of the brain and behavioral sciences; however, S&K are wrong to pin the failings of traditional cognitive and linguistic studies on information theory per se.

The principal issue at stake in recent philosophical debates surrounding applications of DST is whether the anti-representational models, metaphors, and explanations it inspires can shed any light on distinctively *cognitive* phenomena. Traditionally, explanations in cognitive science have been understood to differ from explanations in biology, physics, or chemistry insofar as most cognitive scientists (at least traditionally) have held that genuine explanations of cognitive phenomena, unlike explanations in the noncognitive sciences, *must* involve semantically evaluable *and* etiologically involved entities. So far, the representationalist strategy has been the best way to handle the systematic properties that mark intelligent behavior. Specifically, as Fodor (1987) and others have emphasized, any psychological theory worth its salt must account for satisfaction conditions, compositionality, and intentionality. Representationalist views can do this, but at the expense of biological plausibility. So far, we dynamicists seem to have biological and behavioral plausibility on our side, but have been less successful in capturing the systematicity of intelligent behavior. The challenge for dynamicists is either to show that we too can account for the same phenomena as the representationalists (and that we can do it in a more biologically plausible manner) or that Fodor and company have emphasized the wrong set of properties. The ape language research described in this target article goes some way toward doing the former.

My only complaint is that, by focusing on information theory (which in principle could be compatible with many dynamicist accounts), S&K have not chosen the right target for their criticism.

Doesn't a dance require dancers?

N. S. Thompson[a] and Jaan Valsiner[b]

[a]*Departments of Psychology and Biology,* [b]*Department of Psychology, Clark University, Worcester, MA 01610.* **{nthompson; jvalsiner}@clarku.edu**

Abstract: The dance metaphor of ape-human communication is valuable and needs to be pressed to its logical conclusion. When couples dance, they are both choreographers and dancers, and the dance arises dialectically out of the "peractions" of the dancers. We suppose that the way in which scientists communicate with their apes emerges by an analogous process.

We are not friends of the information-transfer metaphor of social interaction. Thompson (1997), an ethologist, referred to this metaphor as a commodification of interaction that leads to a distracting search for mythical substances shuttled between mythical storehouses. He argued that our urgent task should be to discover how "communicating" animals go about designing one another's behavior through their actions. Valsiner (1989; 2001) has demonstrated that all culture "transfer" is a bidirectional, coconstructive process whereby both the sender and the receiver are reconstructing the message. The notion of reconstruction (rather than reception) of communicative messages is the norm in human communication research (cf. Karl Bühler's [1934/1990] "Organon Model").

We deplore information transfer talk in part because its popularity is dependent on a pernicious confusion between the intensional and extensional uses of the word *information* – that is, between information in the ordinary, language sense ("information that . . .") and information in the technical sense of bits of information. The intensional use is the most common and is in play anytime a speaker refers to "information about some state of affairs" or "information that such and so is true." The extensional meaning arises from information theory (Shannon & Weaver 1949), and here the sender has a set of options to choose from (e.g., letters of the alphabet) and the receiver has the same set of options. By the use of a channel (such as dots and dashes), the sender reduces the number of options in the behavior of the receiver. *Information* is just this reduction in uncertainty concerning the options of the receiver. Information theory is about the efficient use of the channel: maximum reduction of uncertainty, with minimum use of channel. In information theoretical talk, there is no question of "aboutness," of what the message is "saying." Despite being fundamentally incompatible, the two uses of *information* have sustained each other in the cognitive sciences, the extensional use providing a dash of quantitative legitimacy and the intensional use giving the impression that information talk can accommodate the complexities of human and animal transaction.

In contrast to the commodity-transfer model of animal interaction, the dance metaphor is wonderfully consistent and evocative. Instead of worrying about what the participants pass between them, we are led to wonder what they mutually produce. We therefore applaud Shanker & King (S&K) for their persuasive presentation of that viewpoint. But we wish they had pursued their metaphor more faithfully. Their reliance on dynamic systems theory obscures the contribution of *agency* to the phenomenon they are trying to explain (Valsiner 2002). To call a social interaction a dance is to stress the *peraction* of social agents. When agents peract, they act *through* or *by means of* one another. Each has a state of affairs toward which his or her behavior is directed, and that state of affairs requires certain actions on the part of the social partner. The behavior of each actor is therefore directed toward using the other as a tool to produce a particular desirable result. The dialectic between their peractions *is* the dance. From an observer's standpoint, the best dances, like the best chess games and the best tennis matches, are those in which neither peractant entirely gets his or her own way.

Some peractants use signs as handles by which to grasp their social partners. Let's say that my wife and I are at dinner with my Republican cousins and the topic of the table turns to politics. My

wife, who has a long experience with my reactions to "the self-serving, right-wing politics of the rich," feels my whole body tense up as I start to rise to the bait. She is uneasy about what is about to occur, and she places her hand very lightly on my hand, where I have gripped the table, ready to pull myself forward in my seat. The warmth and intimacy of her gesture is utterly inconsistent with the publicness and tenseness of the social context in which it occurs. My attention is sharply refocused on her, on the human (rather than the political) dimensions of the conversation, and, ultimately, on the humor and absurdity of the situation.

But let us now suppose that in addition to placing her hand on my hand, she had leaned over to whisper in my ear, "Easy does it!" It is still possible to accommodate this new move – the speech move – within the dance metaphor, speech being just another of the many modalities that are tuning my behavior, instant by instant, as the dinner party goes forward. But it is not easy to avoid the feeling that *this* dance move is special. True, the intimacy of a whisper, the softness of the tone with which it is delivered, even the *tickle* of it, all are akin to the intimacy of her hand placed on mine. But in whatever way these features have meaning, the words "Easy does it!" seem to have meaning in a further sense. It is as if your partner on the dance floor, wishing the two of you to swing about, instead of leaning into your body and throwing you ever so slightly off balance, had spoken the command, "Let's swing!"

The difference does *not* seem to be in the tool relation between the two peractants. If my momentary goal in the dance is to twirl my partner glamorously under my arm, then her twirling is a tool in my grand design. If her momentary goal is for us to swing around together in an exalting display of our "coupleness," then my swinging is a tool in *her* grand design. Thus, for a dancing couple, the place where each grips the other is the *handle* of a tool. What makes a spoken command a different sort of move in the dance is that the handle in that case is a *sign,* instead of the actual grip on the partner's body.

Nobody would deny that all chimpanzees are adept at using social partners as tools. What seems to set the ape language of research chimpanzees apart from that of their wild conspecifics is their use of signs as handles. We heartily agree with the authors that the dance metaphor does more than the information-transfer metaphor to help us understand these animals' laboriously acquired talent at sign use. Even more useful would be an elaborated version of their dance metaphor in which the dance is a square dance and every participant is both caller and dancer.

Does the use of the dynamic system approach really help fill in the gap between human and nonhuman primate language?

Jacques Vauclair

Centre for Research in Psychology of Cognition, Language and Emotion, University of Provence, 13621 Aix-en-Provence Cedex 1, France.
vauclair@up.univ-aix.fr **http://www.up.univ-mrs.fr/wpsycle/**

Abstract: The highly recommended transposition of the dynamic system approach for tackling the question of apes' linguistic abilities has clearly not led to a demonstration that these primates have acquired language. Fundamental differences related to functional modalities – namely, use of the declarative and the form of engagement between mother and infant – can be observed in the way humans and apes use their communicatory systems.

I can only be in sympathy with Shanker & King's (S&K's) proposal that dynamic systems paradigms are better suited than information processing models for describing and explaining language achievements in any species. In effect the kind of approach advocated in the target article places the investigation of linguistic abilities in its proper context, namely, that language and related behaviors are worked out in humans through a dynamic system of coregulations and coconstructions of cognition between social partners. In that respect, the paradigms on which S&K rely in their propositions are reminiscent of the Vygostkian approach to human cognition and language (e.g., Vygotsky 1962), also known as the "distribution cognition" approach (Johnson 2001).

Two remarks are in order before discussing the use and output of the dynamic system approach in ape language research (ALR) studies. Although S&K do an excellent and fair job of summarizing ALR studies, two aspects of potential importance for the paradigm they defend are lacking in their review. First, the comparative literature provides evidence that marine mammals possess abilities of a complexity comparable to that reported for chimpanzees and bonobos. The California sea lions trained in a symbolic matching technique using gestural signs (e.g., Gisiner & Schusterman 1992), could comprehend at least 20 signs (modifiers, objects, and actions) in about 200 three-sign combinations. Experiments with dolphins that followed the same basic experimental procedure also demonstrated excellent abilities of these animals in comprehending imperative as well as interrogative strings (e.g., Herman et al. 1993). Interestingly, the studies with marine mammals were carried out without apparently following the dynamic approach, for those studies were based on the more traditional information-processing models.

Second, the achievements concerning language-trained apes are characterized by a gap between the comprehension of the communicative signals they have been exposed to and their abilities to produce strings of gestures or lexigrams or both. For example, the bonobo Kanzi can comprehend quite complex instructions given to him in English (Savage-Rumbaugh et al. 1993), but his productions of gestures and lexigrams are rather poor compared to his comprehension. Thus, the most complex of Kanzi's "word" combinations involves three items (usually made of two gestures and one lexigram; Savage-Rumbaugh et al. 1986). In humans, lexical comprehension and production work in parallel (e.g., Harris et al. 1995; Werker & Tees 1999). The gap observed in the bonobo makes it difficult to interpret the ape's achievements in evolutionary terms unless we assume that comprehension has evolved in the absence of language, in which case we may question the adaptive function of comprehension in the absence of production. These issues raise the problem of the role of the human-defined environment (through a process described as enculturation; Tomasello et al. 1993) on the emergence of the cognitive and communicative skills involved in language that may have no equivalent in spontaneous behaviors and on which no selective pressure has supposedly acted (Vauclair 1996).

The main point I want to make bears on the linguistic abilities reported for the bonobo Kanzi, which are undoubtedly, as S&K correctly state, the most impressive among those exhibited by any nonhuman species. Considering that the primary function of language concerns the exchange of information about the world, and following the pioneering work of Bühler (1934), two principal functions (or modalities) can be distinguished in the linguistic as well as prelinguistic communication among humans (Bates 1979). The first function is the *informative function* and it takes two forms: a declarative form that represents states of the world (e.g., "Jim comes") and an interrogative form. The second function is *injunctive* (imperative) and *exclamatory* and mostly expresses itself with requests and demands (e.g., "Come!"). Developmental work with young children has shown that the use of declaratives (e.g., Bassano & Maillochon 1994; Wetherby et al. 1988) becomes the dominant mode of communication between 1 and 2 years of age (about 60% of all utterances). Declaratives (Bates et al. 1975) can be words or gestures, and they function not primarily to obtain a result in the physical world but to direct another individual's attention (its mental state) to an object or event, as an end in itself.

It is quite clear from the published work that the imperative function appears to be the main (if not exclusive) mode used by the "linguistically" trained apes. In the case of Kanzi, studies reveal (Savage-Rumbaugh et al. 1985; 1986) that, unlike human chil-

dren who use language to make indicative or declarative statements, about 96 percent of Kanzi's productions are requests. Thus, the difference between Kanzi's modalities of communication and the typical declarative mode observed by humans is striking. In effect, communication in the apes has essentially an imperative function (this appears indeed to be the rule for all animal species and this mode is sufficient to fulfill the biological requirements of, for example, warning again predators). In contrast, humans use not only linguistic signs but also prelinguistic communication means such as gestures (e.g., pointing) for both imperative and declarative purposes, as when two persons share an interest toward a third person (Vauclair 1996; in press). This is further illustrated in a study in which we assessed the behavior of human-human, human-ape, and ape-ape pairs during object manipulation (Bard & Vauclair 1984; Vauclair 1984). One of the pairs we observed involved the bonobo Kanzi, his adoptive mother Matata, and a human caretaker (Kanzi was 8 months old when the observations started). We found that the communicative styles expressed by the adult toward the infant differed between species; thus, in contrast to the human adults, the ape adults rarely acted on the objects in ways that would direct or engage the infants' attention. Because our study was devised to focus on the factors of coattention and coregulation between infants and competent partners, it fit exactly with the principles of the dynamic system paradigm or distributed cognition (Johnson 2001). Our results clearly suggested that the patterns (e.g., joint attention to objects) which characterize mother-infant communication in humans were missing in the chimpanzees and the bonobos examined within our comparative framework.

Human expression and experience: What does it mean to have language?

Yves-Marie Visetti[a] and Victor Rosenthal[b]

[a]*LATTICE-CNRS-ENS, 92120 Montrouge, France;* [b]*INSERM XR-324, Centre Paul Broca, 75014 Paris, France.* **yves-marie.visetti@ens.fr**
victorro@broca.inserm.fr

Abstract: We support Shanker & King's (S&K's) proposal for a dynamic systems approach in ape language research, but question their vision of what it means to have language. Language plays an essential role in the making of the human mind. It underlies any kind of human interaction and codetermines perception and action. Moreover, what gives human thought the very characteristic architecture of textuality criterially requires a third party.

We are sympathetic with Shanker & King's (S&K's) plea for a dynamic systems approach in ape language research and share their critique of the information processing model. But we find their proposal too general and unspecific with respect to communicative abilities of humans and apes, and we question their vision of what it means to have language. In our view, the dance metaphor may be an illustration of interactional synchrony in a dynamic system, but it captures little of what may characterize human communication, and we argue the insufficiency of a dyadic relationship for the emergence of language skills in favor of a more complex triadic structure. What S&K call a "new paradigm" is reminiscent, both with respect to the idea of dynamic systems and with respect to the principles of ape cognition research, of the Gestalt paradigm formulated by Wolfgang Köhler and his Gestalt colleagues.

Wolfgang Köhler, the author of the most extensive early studies of "the mentality of apes" (Köhler 1925/1951), first formulated a dynamic systems approach to brain and cognition (Köhler 1920). A good deal of what S&K describe as "new" observations of coregulation, dyadic interactions, creativity and mutual adjustments, and of the social background of action and interaction, are modern replica of what Köhler described in his famous studies.

Gestalt psychologists opposed the idea of internalization of the "outer world" and worked out an original and extremely fertile concept of unity of mind and the physical world. This concept was most explicitly formulated in Köhler's theory of *physical* and *phenomenal* forms which applied, in the same terms, to the realm of mind and to the "outer world." The Gestaltists stressed the physiognomic character of perceptual experience, where physiognomic refers both to the *expressive* character of percepts (as though dynamically expressing "an inner form of life"; Werner 1957, p. 69) and to the conative dimension of perception whereby the *readiness for action* imbedded in perceptual experience "urges" us to act upon, or use, perceived objects. Perception, action, and expression were thus assumed to be closely intertwined, making up a unified "practical field" (see Rosenthal & Visetti 1999; 2003). It is precisely this unitary view of perception, action, and expression that may be requisite for semiotic interaction, forming a practical basis for communication since whatever apes, or we, perceive and do is intrinsically expressive. Yet, in order to communicate, it is necessary to have something to transmit. Interactional synchrony illustrated by the dance metaphor falls short of this requirement because it may amount to simple harmonic adjustments between dynamical systems. The need to communicate may arise instead when synchrony breaks down – that is, when dissynchrony occurs and generates instability. Moreover, we do not merely communicate in order to get hold of, point to, or share a novel experience or object, but also with the aim of stimulating the ongoing exchange or simply adding an episode to the history of our selves.

What does it mean to have language, then? Although Gestalt psychologists failed to properly appreciate the importance of language and categorization in the making of human mind, Köhler perceptively noted that it was the absence of language that prevented his most clever apes from *giving an elaborate structure to time.* Language provides the only direct means for entertaining an explicit relationship to the past or remote future. In the absence of language, it would indeed be difficult to have a history, to make up a narrative, or to carry out truly elaborate programs. The emergence of language endowed the human mind with extremely rich temporality and thereby permitted, inter alia, the incomparable development of *memory.*

Narrative, which is inconceivable without language, has been argued to play an essential role in the making of the human mind. As Bruner put it:

> Narrative deals with the stuff of human action and human intentionality. It mediates between the canonical world of culture and the more idiosyncratic world of beliefs, desires and hopes. It renders the exceptional comprehensible and keeps the uncanny at bay. . . . It reiterates the norms of the society . . . can even teach, conserve memory, or alter the past. (Bruner 1990, p. 52)

According to Bruner, narrative requires four crucial constituents to be carried out: (a) agentivity; (b) establishment and maintenance of sequential order (event structure); (c) sensitivity to the canonical and the exceptional; (d) adoption of a narrator's perspective (because narration cannot be voiceless). There is no doubt that some apes, especially Kanzi, can handle direct agentivity, and may be able to establish short sequences necessary for the execution of simple plans. But what is the extent of their capacity to express and characterize the ordinary and the exceptional? Can they explicitly adopt another being's perspective? How much of role reversal can they handle; can they cast a third party to perform an action that is not directly observable?

Although we agree with S&K that a dyad may give substance to interactional synchrony, we emphasize that a dyad does not make a society and that language use presupposes a complex *triadic* relationship. The issue is not how many individuals are involved in interaction, but the very necessity of a *third party viewpoint,* whether the third party is *present* or *absent, real* or *fictitious.* What gives human thought the very characteristic architecture of textuality criterially requires a third party. Grammatical devices (person, time, space, modality) universally reflect

this triadic structure of human symbolic and linguistic experience (see Rastier 1997).

The pervading use of metaphors, semantic creativity, and polysemy are but examples of the irreducibility of language to simple pragmatics or communication. Language is not only the vehicle but also the very means of formulating and becoming aware of our own intentions, desires, or thoughts. We do not convert preexisting thoughts, desires, or intentions into language; rather, it is via linguistic expression that we *discover, realize,* and *ratify* our thoughts, desires, and intentions as they develop and take shape. Conversely, language itself evolves and negotiates its own linguistic values as they are deployed in speech or writing.

During the past half-century several linguistic theories contended that language and the world of life are two systems that evolved independently of one another and then came to interact. But interaction falls short of social norms and rites, couched in the very structure of what we have called *semantic forms,* and meaning involves both usage and Gestalt transformation (Cadiot & Visetti 2001; Visetti & Cadiot 2002). Language hangs behind any kind of human interaction as it codetermines perception and action. The world we experience is a world proper to language-endowed beings, so that even our most ordinary actions are fashioned by language use (e.g., naming modifies our very perceptual experience). There is no doubt that Kanzi's being raised in contact with language-endowed society where language always played an important role in everyday interactions had a profound effect on his cognitive development. Even Kanzi's perception of objects, qualities, or events was fashioned by language. Yet Kanzi did not *really* acquire language and never crossed the frontier of narrativity, textuality, and many other characteristic features of language that make up the human mind. The manipulation of lexigrams allowed Kanzi to become a champion of a complex semiotic tool. This, however, falls short of what it means to have language.

Making meaning

Gabriel Waters and Sherman Wilcox

Department of Linguistics, University of New Mexico, Albuquerque, NM 87131. **{watersg; wilcox}@unm.edu** **http://www.unm.edu/~wilcox**

Abstract: This commentary discusses the dynamic systems (DS) approach to communication over an information-processing (IP) model. The commenters suggest that the authors of the target article, in their treatment of the issue, do not identify the central failing of the IP model. Further, it is suggested that the DS approach should include examination of mechanisms in the emergence of symbolic communication.

We commend Shanker & King (S&K) for recognizing that the information-processing (IP) model is inadequate for explaining the nature of communication, and we very much support their call to abandon that model. We also find their application of the dynamic-systems (DS) paradigm innovative. In fact, we believe the dynamic systems approach may be even more powerful as a model than suggested in the target article, particularly in regard to the genesis of complexity and the emergence of symbols in communication systems.

We agree with S&K in their general approach, but we suggest that their argument could be strengthened in two ways. First, they should target more precisely what is wrong with the IP model as an explanation for communication. Second, we urge them to explore more fully the DS paradigm as an explanation for how symbols emerge.

How is meaning communicated? The IP model as applied to the study of communication and language is based on the pervasive conduit metaphor (Reddy 1979) for understanding how communication works. The conduit metaphor assumes that words are containers for meaning, that speakers place meaning into their words and receivers extract these meanings and thereby understand what the speaker meant; thus, meaning is transmitted in words. As S&K note, although the IP model may help us to understand how certain mechanical systems work, it leaves unexplained how meaning is communicated, for signals may be transmitted but meaning is not.

By not addressing the question of what the IP model fails to explain – how communication works – S&K are left dancing around the edges of the problem. The main problem with the IP model is not that it focuses on sequential turns, on sending and receiving signals, or even on encoding and decoding. Challenging those aspects of the IP model leads S&K astray, for communicative behavior does after all often proceed in turns, communication does require the production and perception of signals, and expressing a concept does entail coding "the relationship between a conceptualization one wishes to express and the linguistic structures activated for that purpose" (Langacker 1991, p. 294).

The central failure of the IP model, which is left unaddressed by S&K, is its assumption that meaning is transmitted *in* signals. We believe that a cognitive approach offers a better explanation for how communication works, for words and signals, although informative, do not "contain" meaning; rather, meaning is constructed. The construction process relies on environmental signals (words and gestures) and conceptual knowledge and processes. Signals, words, gestures, and expressions do not *mean;* they are prompts for the construction of meanings (Turner 1991).

In this alternative, cognitive model of communication and language, grammar is not a device for generating structure. Grammar is an inventory of knowledge used by those communicating to sanction (both in production and perception) utterances, the "prompts" that are used to construct meaning.

What is left unexplained in the IP model, and in S&K's rejection of it, is *how meaning is communicated.* We believe that clarification requires examining the cognitive abilities that are recruited in the construction of meaning, and how it is that symbols, by which we mean mappings of form and meaning, emerge. Though we concur that a sudden restructuring of the brain of the type proposed by Deacon does not seem compatible with the data, we would urge S&K not to throw out the cognitive baby with the information-processing bathwater.

How do symbols emerge? The target article suggests that communication is creative even when stereotypical gestures are used in novel contexts. We contend that these situations are more important to the development of symbolic communication than is implied in S&K's analysis. Stereotypical or ritualized gestures are often automatized and reduced forms of a functional behavior. An extended arm used to request an item is, both for apes and preverbal children, an automatized reduction of grabbing at the item. Once ritualized, this gesture is already emancipated from its functionality. Creative use of these ritualized gestures is the mechanism that allows for their emancipation from typical contexts, and is therefore an early stage in the shift from association to symbolic representation (Haiman 1998).

The systems introduced to the apes, whether lexigrams or signs, are a body of ritualized gestures. When researchers introduce these gestures in communicative contexts beyond requests for an item or simple naming, they are in a sense performing a guided emancipation of the gestures. Crucial to this guided emancipation is the response to the ritualized forms by others in the new context.

Given that joint attention and coregulated communication is not particular to the apes involved in ape language research (ALR), and that the ritualization and emancipation of functional behaviors into communicative gestures have been observed in free-ranging apes (Plooij 1984), what may be special in the examples from ALR is the breadth of the ritualized gestures that are incorporated into the apes' repertoires and that these gestures are readily familiar to the researchers.

Finally, we address the implication that the Gardner studies utilized a behavior modification model. In both of the Gardners' projects, cross-fostering rather than a system of rewards served as the dominant methodology (see Gardner et al. 1989). Given that nat-

ural social interaction across multiple contexts is central to cross-fostering, it would seem that this body of research would provide a fertile area for the proposed dynamic systems paradigm. Of note is that research from these projects has concentrated on such dynamic systems approaches as topic maintenance and repair (Jensvold & Gardner 2000).

We applaud S&K for recognizing the shortcomings of the IP model and the utility of the DS approach as a method of examining communication, but we believe that the mechanisms of understanding and the emergence of symbols deserve a deeper analysis. We urge S&K to incorporate both the mental and the social into their DS model. Communicative gestures occur in social settings; guided emancipation is a social behavior. Gestural signals, whether acoustic or optical, do not contain meaning; rather, meaning is assigned to gestures by ape and human. The assignment of meaning to perceptual events, though it may be socially constructed, dynamic, and emergent, is also essentially a conceptual task. As Fauconnier and Turner (2002) note: "The illusion that meaning is transmitted . . . is possible only because there is a brain on each end to handle the construction of meaning" (p. 5).

Blind men, elephants, and dancing information processors

Chris Westbury

Department of Psychology, University of Alberta, Edmonton, Alberta T6G 2E9, Canada. **chrisw@ualberta.ca**
http://www.ualberta.ca/~chrisw/Westbury.html

Abstract: Whatever else language may be, it is complex and multifaceted. Shanker & King (S&K) have tried to contrast a dynamic interactive view of language with an information processing view. I take issue with two main claims: first, that the dynamic interactive view of language is a "new paradigm" in either animal research or human language studies; and second, that the dynamic systems language-as-dance view of language is in any way incompatible with an information-processing view of language. That some information is defined in coregulated social interaction guarantees the dancing. That all information is composed of relevant differences guarantees the information processing.

In a tale attributed to the twelfth century Sufi Hakim Majdud Sanai, three blind men describe an elephant. One strokes the animal broadside and pronounces that an elephant is a warm soft wall. A second grasps the pachyderm by the tail and opines that an elephant is a short thick rope. The third grabs hold of a tusk and declares that an elephant is a cold smooth pillar.

The fable captures the position in which I find myself with respect to this target article: I agree completely, yet consider it misleading. None of the blind men is incorrect. Each has honestly captured an aspect of elephantness. It is the implication that a part is the whole that misleads. The implication I will focus on here is the one most concretely laid out in the target article: the claim that a dynamic interactive emergence view of language and an information-processing view are in conflict, and that the former is right. In fact, both views focus on different but compatible aspects of language.

As surely as an elephant is a warm soft wall, language is a dynamic interactive emotional process – a dance, to use the preferred metaphor of this paper. The experimental and observational empirical evidence and the philosophical motivations behind that claim are all compelling, multidimensional, and voluminous (for summaries of some empirical evidence, see Bloom 2000; Hendriks-Jansen 1996). It is hard to imagine that there are many people interested in language who question the importance of coregulated social interactions in language learning and use. Among the many who have advocated their importance, I here briefly mention three from different disciplines: the philosopher Ludwig Wittgenstein, the psychologist Jerome Bruner, and the anthropologist Gregory Bateson.

Ludwig Wittgenstein reminds us how far back the roots of this "new paradigm" go. As surely as an elephant is a short thick rope, Wittgenstein's (1958) *Philosophical Investigations* is a compilation of different arguments explaining why linguistic semantics could only be grounded by interacting organisms sharing nonlinguistic experiences and behaviors. Shanker & King's (S&K's) failure to acknowledge these roots for their ideas seems almost willful at times, given that one of them authored a book on Wittgenstein. One hardly knows what to make of a philosopher who would cite Savage-Rumbaugh (1986) as an authority on the point that "meaning and intent are not to be found by looking 'inside' a speaker." This is a point that Wittgenstein (1958) made 28 years earlier: "Nothing is more wrong-headed than calling meaning a mental activity." Similarly, a single citation to C. M. Johnson (2001) for the claim that "whether or not a speaker understands the meaning of a symbol . . . is established by what she says or does in the context of dynamic interactions" seems odd when that claim is exactly the main point of *Philosophical Investigations.* I didn't understand in reading this target article why we should be more impressed by the fact that ape language researchers have tossed aside a few such comments today, than by the fact that the theoretical force behind those comments was laid out in a compelling manner 50 years ago.

The so-called new paradigm championed in this article was dubbed "transactional contextualism" by Jerome Bruner, one of the founding fathers of the information processing paradigm with which S&K suppose transactional contextualist accounts to be incompatible. Over a decade ago, Bruner (1990) wrote:

> transactional contextualism [is] the view that human action could not be fully or properly accounted for from the inside out – by reference only to intrapsychic dispositions, traits, learning capabilities, motives, or whatever. Action required for its explication that it be *situated,* that it be conceived of as continuous with a cultural world. The realities that people constructed were *social* realities, negotiated with others, distributed between them. The social world in which we lived was, so to speak, neither "in the head" nor "out there" in some positivistic aboriginal form. And both mind and the Self were part of that social world. If the cognitive revolution erupted in 1956, the contextual revolution (at least in psychology) is occurring today. [emphasis in the original]

Hendriks-Jansen (1996) explicitly tried to situate Bruner's transactional contextualism inside an ethological perspective that allowed it to encompass not only human action and language but also a wide range of other behaviors issued by humans, nonhuman animals, and intelligent artifacts. He was not the first to try to meld biology, computation, language, and situated interaction. In the 1970s, Gregory Bateson wrote a series of books in which animal communication (including human language) was described as a dance within what we would now call an information-processing (but he called a "cybernetic") theoretical structure. Bateson would not understand why anyone would argue that there was a contradiction between language as interactive dancing and language as information processing. Information is any difference that makes a difference (Bateson 1972). That some information is defined in social interaction guarantees the dancing. That all information is composed of relevant differences guarantees the information processing.

Although I do not believe that Deacon (1997) is a good representative on which to focus the criticism, S&K do correctly identify a common narrowness of view in today's siliconcentric information processing paradigm. The paradigm tends to judge perceptually mediated information about objects as more important or even more real than socially mediated information about conspecifics. This is probably because many of the paradigm's metaphors are computer-inspired. Computers don't yet have emotions or social norms, and so don't yet use many cues from their conspecifics to modulate their behavior. However, the fact that information processing theorists are computer geeks does not negate the fact that carbon-based information processors dance.

Perhaps I have misread the intent of this article. Perhaps, as their title suggests, the authors' goal was never to proclaim a new paradigm in language or communication research in general, but

only to underscore that some old and well-studied themes from human language research have been underemphasized by ape language researchers. If that is the case, I'd just like to add for their benefit: As surely as an elephant is a cold smooth pillar, language is information processing.

A multichannel information-processing system is simpler and more easily tested

Thomas R. Zentall

Department of Psychology, University of Kentucky, Lexington, KY 40506.
Zentall@uky.edu
http://www.uky.edu/AS/Psychology/faculty/tzentall.html

Abstract: The dance metaphor for the communication between two organisms may be an appealing image because it appears to capture the intricate synchronization of their interaction; however, it is neither parsimonious nor easily tested. Instead, a multichannel information-processing model, even one that can process only serial events, provides all of the flexibility required to account for the complex temporal coordinated action observed.

Shanker & King (S&K) use a dance metaphor to describe the communication between two organisms because a coordinated dance between two people gives the impression of a single organism in movement. But is it accurate? The transmission of a television image provides the illusion of three dimensions moving seamlessly through time. But in reality it is a linear, one-dimensional transmission. To create this illusion, a serial signal sweeps across the screen and back again almost 300 times a second. And then that static image is repeated with minor variations so quickly that it gives the further illusion of a moving picture. Similarly, the interaction between two organisms gives the appearance of a coordinated dance when in reality it is very likely the continuous response by one organism to a stream of stimulus input (information) from the other (and the rest of the environment).

The oversimplified information processing model presented as a foil by S&K is analogous to a single-channel communication system used by some push-to-talk communication systems. These relatively simple systems can send or receive information but cannot do both at the same time. Thus, the listener must wait to respond until the sender has completed his part of the communication. A better, but still oversimplified system, is a typical telephone line that allows the sender to be verbally interrupted by the listener at any time. Although this system does not allow for the rich visual channel provided by face-to-face communication it does allow for simultaneous (auditory) input and (oral) output. Such a dual-channel processing system could involve the parallel processing of input and output (listening while speaking) but it is also quite possible that a single central processor could switch rapidly between output and input channels, thereby giving the impression of parallel processing. The functioning of such a system would be facilitated by input and output buffers that briefly store information to be acted on the next time that processing attention is switched back. Such rapid shifting between output and input channels of a serial processor can provide all that is needed for the coordinated dance described by S&K.

According to S&K, communication can take two forms, the transmission of factual information and the communication of desires, intentions, fears, warnings, invitations, attitudes, and emotions. Although S&K claim that information-processing models deal with the transmission of factual information whereas dynamic models include the communication of emotions, an information-processing model can handle both kinds of communication. Knowledge and emotion may differ in content but there is no reason for the processes that underlie them to be different.

S&K propose that an example of the information processing approach is the use of traditional instrumental training procedures (e.g., Rumbaugh 1977), whereas dynamic systems use a more functional approach involving the communication of needs which produce reinforcers specific to those needs. This distinction is really not useful. When conditions are arranged such that the outcome that follows a response is specific to that response, it is typically called a differential outcomes procedure. This is a very effective training procedure that generally facilitates acquisition and yields improved retention in a variety of species (see, e.g., Trapold 1970), but a dynamic model is not needed to explain it. It is explained more simply in terms of the added cue value of the expectancy of differential reinforcement at the time of choice.

A more serious problem with the dance metaphor is that it takes on a magical, poetical quality. We don't ask how the graceful ballroom dancers manage to coordinate their movements to produce the elegant results of that orchestration. To do so would be to spoil the beauty of the performance. We can also marvel at the beauty of Kanzi's language skills but it doesn't further our understanding of the mechanisms involved in those skills in either chimpanzees or humans. It may make us feel good to view the complex symbolic and gestural communications between the young chimp and the experimenter as an elegant dance, but it is not likely to be a productive research strategy. In fact, it is quite clear from the section that describes (in great detail and in quite human terms) the complex interactions between Kanzi and his trainers (target article, sect. 4) that the dynamic approach takes great liberties in drawing analogies between the interactive behavior of a young chimpanzee and that of a young child. Thus, the real danger inherent in this dynamic systems approach is not in its appreciation of the beauty of the interaction between organisms but in its appeal to anthropomorphism. To draw on a counterexample to illustrate this point, when an African grey parrot is presented with an array of objects that differ in shape, color, and material and the parrot is asked, "How many green?" (Pepperberg 1999), what is impressive about the response is not that it is given in clearly understandable English that could easily be mistaken for that of a young child, but that when asked a question about category and number to which there are many possible answers but only one is correct, the parrot answers correctly!

The scenarios described by S&K are sufficiently ambiguous that they provide ample opportunity for alternative explanation. And although the described interactions certainly reflect a high level of cognitive functioning, there is no reason why an information-processing account could not provide an equally reasonable and perhaps more easily testable account of the behavior. Although in the future it may be demonstrated that the language skills acquired by Kanzi are beyond the scope of an information-processing account, I would want to be sure that a less caricatured version of information processing was exhausted before abandoning it to a vaguely formulated dynamic systems approach.

Authors' Response

The emergence of a new paradigm in ape language research: Beyond interactionism

Stuart G. Shanker[a] and Barbara J. King[b]

[a]*Departments of Philosophy and Psychology, York University, North York, Ontario, Canada M4S 1B4;* [b]*Department of Anthropology, College of William and Mary, Williamsburg, VA 23187.* **shanker@yorku.ca**
bjking@wm.edu

Abstract: We group the issues raised in the commentaries into five major sections. In the first, section R1, we consider some of the antecedents to dynamic systems (DS) in psychology, biology, anthropology, and primatology and note the key changes that have occurred in DS over the past ten years. Next, in section R2, we ex-

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plain the ways in which *co-regulation* differs markedly from *interactional synchrony,* focusing in particular on the creation of meaning inherent in co-regulated communication. The following section (R3) clarifies the challenge that DS poses to Cartesian assumptions about the nature of communication and contrasts this position with behaviorism. In the next section (R4) we reject the notion that IP and DS may be, in fact, compatible paradigms. Finally, we explain the exciting future we envision for using DS to facilitate consideration of evolutionary questions, particularly those concerning the comparative evolutionary development of socio-emotional dynamics between partners (section R5).

We are grateful that so many colleagues provided commentary on our target article. Our contrasting of the information-processing (IP) and dynamic systems (DS) models in the context of ALR clearly touched on issues central to many diverse disciplines. In organizing our response around five major questions, we hope to clarify our position on key concepts, introduce some new material to support our portrayal of DS as contrasted with IP, and respond to a number of more specific points raised in the commentaries.

R1. Is our paradigm really so new?

In commentaries representing a wide range of viewpoints, including polar-opposite opinions, we were struck by the number that converged on the point that DS is by no means a recent phenomenon (**Bonvillian & Patterson**, **Grammer**, **Ingold**, **Kuczaj**, **Ramos & Paulos** [henceforth **Kuczaj et al.**], **Locke**, **Westbury**). We certainly do not wish to deny the antecedents of DS. Gilbert Gottlieb has written an invaluable book (Gottlieb 1992) surveying the development of dynamic systems ideas in biology through such critical figures as August Weisman, Sewall Wright, and Zing-Yang Kuo (see in particular Kuo's highly influential *The Dynamics of Behavior Development: An Epigenetic View;* Kuo [1976]).

Equally, the last thing we wanted to suggest is that the psychological interest in DS originates with Alan Fogel's seminal work, *Developing through Relationships* (1993). Fogel himself would be the first to stress the importance of the thought of Jerome Bruner on his own development, as is the case for SGS. Moreover, we would strongly support the argument that dynamic systems owes much to the work of developmental interactionists tracing back to Vygotsky (and we are grateful here to **Ingold** for bringing to our attention the work of Volosinov and Schutz); indeed, we have argued for this position in print as well (see, e.g., Shanker 2002). We would also note here that we embrace the four factors that **Myin & Smets** talk about, which we have addressed in varying degrees in other places, and agree with them that the work on multi-causality done by DST developmentalists over the past decade adds considerable support to our argument; and we welcome **Sessle & Yao**'s neuro-synopsis, which also strengthens our perspective considerably.

Coming from anthropology, King credits her theoretical development to the work of G. Bateson and others (see e.g., King 2002). Though the DS perspective is newer in primatology, antecedents are clearly visible in the work of Hinde (1985) and Bateson (1978). That some anthropologically trained primatologists today are at the forefront of the interdisciplinary push to apply DST to the study of the behavior of monkeys and apes outside the ALR context (**Johnson**; see King & Shanker 2003), attests to the significance of these roots.

Concerning nonhuman language research, our reason for confining the discussion to the studies conducted at the Language Research Center relates to the fact that, as we spelled out in the target article, the differences between Deacon's and Savage-Rumbaugh's views of the goals and methodology of the Animal Model Project and the Kanzi research are particularly illuminating for the stated purposes of our paper. We are pleased to acknowledge the important work of **Gardner** and Fouts with the chimpanzee Washoe, **Bonvillian & Patterson** with the gorilla Koko, and **Pepperberg** with the parrot Alex. Elements of DS are starting to emerge in these research programs; however, they have operated in the absence of any formal commitment to working out DS principles.

Having noted some important precursors of dynamic systems theory, we reiterate our contention that there has been a significant shift in dynamic systems writings in biology, psychology, philosophy, evolutionary theory, and anthropology over the past ten years. From this shift may develop the "imminent revolution" predicted by **Leavens**. We identify, in the next sections, further questions arising from the commentaries in order to discuss some of the key issues related to this shift.

R2. Isn't co-regulation the same as interactional synchrony?

The interdisciplinary shift towards a new understanding of DS is owed in part to the growing recognition of the distinction between the concepts of *co-regulation* and *interactional synchrony.* Given attempts to conflate the two (**Bonvillian & Patterson**, **Grammer**, **Rendall & Vasey**, **Visetti & Rosenthal**, **Zentall**), we wish to review the basis for drawing this distinction.

As some of the commentators have noted, developmental interest in the mutual social bond and its expression in human infants and their caretakers can be traced back to Bowlby's theory of attachment (Bowlby 1969) and to early research on dyadic interaction (e.g., Trevarthen's proto-conversations [Trevarthen 1979] and Bruner's formats [Bruner 1983]). Both of these research programs take into account the nuanced back-and-forth between human infants and their caretakers and are united in their use of some version of an interactional synchrony (IS) framework.

Condon and Sanders, in an influential paper in *Science,* defined the shared rhythmicity of IS as occurring when "the configurational organizations or 'units' of the listener's body motion are synchronous with the speaker's speech" (Condon & Sanders 1974; see Fogel 1993 for analysis of possible flaws with Condon & Sanders' data). Though these authors intend IS to refer primarily to bodily actions in relation to speech, the term is frequently extended to include other synchronies (for example, body motion to body motion, or vocalization to vocalization). Though the definitions of IS may vary in emphasis, as Fogel (1993) makes clear, IS in its focus on combining elements from two individuals misses out on aspects of social relating that are critically important in co-regulation.

Members of a dyad are said to be in IS when they are in a similar affective state and attuned to one another's communicative behaviors. In normal dyadic interactions this is

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thought to occur 30% of the time; the other 70% of the time the caregiver and infant are "out of sync" with one another and thus experience interactional dissonance (Tronick 1982). Attentive caregivers are said to be highly sensitive to these periodic breakdowns and good at restoring IS. When a caregiver is poor at repairing these breakdowns – say, because of internal or external stresses – or an infant has what is referred to as a "problem temperament," there is a marked decline in IS.

Co-regulation differs from IS in its recognition that social partners do not just synchronize or harmonize when they communicate, but actually *make meaning* as they act together. We can do no better by way of explanation of this point than to refer back to **Waters & Wilcox**, whom we thank for pushing to the foreground this aspect of DS. Co-regulation highlights the role of creativity as social partners transform each other's behavior, in a way that IS does not. It is a mistake to equate the two. Research methodologies formulated on the IS framework will not allow for the same questions to be asked of ALR that might get asked on the DS framework. We can see this if we consider claims that turn-taking is inherently sequential (**Kuczaj et al.**, **Miklósi**, **Waters & Wilcox**). In one sense, say, measuring micro-movements using a finely calibrated time clock, it is at times true that movements follow one upon another with short lags in between. That this is not invariably the case is clear from observing any interaction in which two social partners are intimately engaged with each other: the partners' vocalizing (or verbalizing) and gesturing may overlap, and when turn-taking is preserved, it is marked by contingency and co-construction. DS emphasizes, not microtemporality (as does IS, and as does **Zentall** in his claim that rapid shifting between input and output of a serial processor is sufficient to create a coordinated dance), but *the mutual transformation in the thought and actions of two partners who are seen as a single unit with internally related elements.*

Furthermore, co-regulation fundamentally alters the manner in which we view interactional dissonance. IS attempts to determine which of the two partners is "responsible" for periodic breakdowns, that is, to ascertain if the caregiver is inattentive or insensitive to the infant's cues, or if the problems are due to the infant's temperament. But co-regulation looks, not at the individuals *per se,* but at the sorts of patterns that have evolved in a dyadic relationship. That is, it sees breakdowns, not as the result of the caregiver's poor parenting skills or the child's problem temperament, but rather as the result of the patterns that have developed in a dyad, that, over time, may become increasingly entrenched.

R3. What about mental representations?

One of the key respects in which there has been a pronounced shift in dynamic systems thinking over the past ten years lies in the attempts that have been made, as **Johnson** spells out, to develop DST within the broader framework of a nonCartesian epistemology (see Shanker 2001). In this respect, one of us (Shanker) would almost be tempted to cite Wittgenstein here as yet another extremely important precursor of DST, were it not for Wittgenstein's well-known antipathy to any suggestion that his philosophy is tainted with theory-construction. In place of models of what must be going on inside an agent's *mind/brain* when they perform some action, DS places the focus on *agents* and the skills they must acquire in order to master their society's normative practices. For this reason, we strongly agree with **Symons** that it is critical that DST theorists develop a comprehensive response to the "rules and representations" model of cognitive phenomena. We particularly welcome **Cangelosi**'s noting that the computational approach, with its focus on autonomous agents, complements our DS approach; we agree that the two dovetail nicely, each adding power to the other. Similarly, we recognize the value in **Thompson & Valsiner**'s concept of "peraction," though we feel that co-regulation itself sufficiently reflects the concept of agency.

Our focus on agents relates also to a palpable concern expressed in some of the commentaries that we are in danger of relapsing into behaviorism. One way in which this charge is expressed is that we seek to ban all talk of mental representations (**Russon & Begun**, **Kako**). But, in fact, our concern was to clarify what it is that developmentalists or comparative primatologists have in mind when they speak of a subject's "representational capacities" by looking carefully at how a nonhuman primate or a child behaves in socially-constructed activities. Such behaviors and propensities serve as criteria for describing the nonhuman primate or child's capacities, not as *evidence* of an "underlying representation" that *enables* them to do such-and-such (see Coulter 1983).

The point of such an exercise is not, however, to demonstrate the "inter-translatability" of Cartesianism – which encompasses both behaviorism and cognitivism (see Shanker 1998) – and DS; rather, it is to clarify how the framework of Cartesian assumptions about the nature of cognition and communication "has lead both human and comparative cognitive research down many a garden path," as **Johnson** puts it. A short list of the sorts of garden paths she likely has in mind here would include: attempts to develop reductionist and even eliminativist models of cognition and communication; to postulate models of the preconscious "inferential processes" that *must,* according to cognitivist theories, enable a subject to do such-and-such; to see the mind as compartmentalized into separate faculties or modules, each of which operates according to its own internal logic; to treat development as a maturational process that is guided by internal biological constraints; to treat cognitive and emotional processes as independent phenomena (where the latter are seen as complex reflexes that impinge upon the former); to restrict the role of emotional development on the growth of the mind to motivational factors that can impede or promote cognitive development, but have no intrinsic bearing on the nature of cognitive development (see Greenspan 1997); to search for discontinuities to the exclusion of continuities, whether these be between nonhuman primates and humans, or between prelinguistic and linguistic children, or between imitation and "mindreading," and so on; to treat enculturated abilities and skills as generative processes; and so on.

At the opposite end of the spectrum, we actually received enthusiastic support from one commentator for what are seen as our behaviorist proclivities. **Pear**'s commentary serves as a salutary reminder that Skinner's views about verbal behavior were, perhaps, too peremptorily dismissed by the linguistic and psycholinguistic communities (see Skinner 1956). Skinner's critique of mentalism undoubt-

edly deserves more careful reading than has hitherto been the case. Certainly we would agree with Skinner's opposition to the use of terms such as "idea," "meaning," and "information" as referring to inner things that are transmitted through speech. But that hardly means that psychologists should be barred from using these mental terms.

Wittgensteinian philosophers have gone to some lengths to clarify the important differences between the type of behaviorist approach that **Pear** sketches and the Wittgensteinian critique of Cartesian epistemology (see Shanker 1998). We will restrict ourselves here to commenting on two of the points that Pear raises. First, to argue that A's telling B that there is gold in the Klondike does not amount to the same thing as saying that the knowledge or information about gold in the Klondike has been *transmitted to B,* does not entail that A may not, in fact, convey some piece of knowledge or information to B. But whether or not A conveys such information to B depends on what B knows about the Klondike, not what is going on inside the mind of A. What we are challenging is the Cartesian view of epistemic terms, but certainly not the use of epistemic terms (see Hacker 1990). Second, we would argue that, not only is it difficult to see how the "prediction and control of verbal behavior could flow from the DST paradigm," but, in fact, such an outlook runs completely contrary to the DST paradigm, for the reasons spelled out in the target article.

Certainly there are many situations in the study of animal communication in which one does indeed have to scrutinize whether the use of epistemic terms is warranted. **Rendall & Vasey** illustrate the importance of this point with their rat pup example. The upshot of their biological explanation of the rat pups' behavior is that it forces us to consider in what sense the rat pups are communicating that they are cold. If a laptop starts to beep, is it communicating that its battery is almost spent? Such a comparison is precisely the sort of example that made the IP approach so appealing; for here one would argue that both are communicating in exactly the same sense and that great ape and human communication are just more complex manifestations of this signal transmission process. By presenting such a thorough biological explanation – one that, as they explain, removes any role for the concept of meaning (or epistemic operators) – Rendall & Vasey show how, as we list at the top of our chart, there is a *categorial* difference between this mechanical use of "communication" and the co-regulated activity of communicating thoughts, desires, intentions, and so on. What makes this issue particularly complex is that there are gray areas where it is not at all clear whether the behavior in question is closer to the mechanical or the intentional sense of "communicate."

Related to this point is **Grammer**'s defense of a version of the *continuum picture,* which he sees as ranging from simple information processing systems "at one end of the communicative spectrum," to complex dynamic processing systems at the other. The continuum here is supposed to be both phylogenetic and characteristic of the various elements involved in the communicative acts of higher social creatures. It is not entirely clear whether Grammer regards the latter as simply a more complex, hierarchically-structured matrix of multiple information-processing channels that somehow interact with one another dynamically, or whether he sees the latter as a dynamic process accompanied and influenced by sub-information-processing systems. Both ideas are interesting. The fundamental problem with this argument from the DST perspective that we have sketched is that, to defend this more sophisticated version of the continuum picture, Grammer treats the "primitive" acts of communication in inferential, linear terms. Thus, lower organisms are treated as inferring the internal states of other organisms from overt signals and then responding. In the case of humans, there is a subtle shift from describing the brain as making such inferences about an observer's intentions, which raises the specter of the "Homunculus Fallacy," to describing the actor as unconsciously making these inferences, which raises the specter of the "cognitive unconscious" (see Shanker 1998).

The examples that **Grammer** cites from his research about how the unconscious patterning of movements affects agents' hedonic state, or their behaviors, are fascinating; however, the argument that we presented in the target article is that these reflexive elements of communication are to be explained, not in the inferential, linear terms of the information-processing framework, but in terms of an agent's developmental history and the co-regulated nature of dynamic interactions (which, as Grammer explains, are both pair-specific and context-specific). This was the point that we sought to clarify in the discussion of the development of facial expressions. Co-regulated facial expressions of affect are not only a critical example of the kind of movement patterns that Grammer is discussing, but indeed, one of the primary mechanisms in a child's functional emotional development (see Greenspan & Shanker, in press). We agree with Grammer that these micro-elements play an integral role in linguistic communication, and for that reason, that it is fundamentally misguided to attempt to segregate "linguistic" from paralinguistic and nonlinguistic factors in the analysis of speech acts (see Toolan 1996).

A second respect in which we are charged with harboring dangerous behaviorist proclivities is in our failure to acknowledge the compositional nature of language and the role that constraints must play in a child's acquisition of language (**Hahn**). In regards to the former, it is absolutely correct that we wish to challenge the compositional theory of meaning and a generativist view of syntax. For reasons that are familiar in the literature, we are uneasy about attempts to model the nature of language on the paradigm of reading (**Kuhlmeier & Bloom**; see, e.g., Harris 1980b). The skills involved in the former gravitate, in John Austin's justly celebrated phrase, around learning how to do things with words. What we have to keep in mind when we speak about a child as learning "how to do things with words" is that *what* a child comes to acquire are *cultural techniques.* That is, a child is not simply learning how to put words together according to certain kinds of rules in order to express different kinds of thoughts; a child is learning the ways of behaving that *count,* within the cultural environment, as the performance of particular culturally conceived acts. A child learns those forms of behavior that those in the culture speak of as *particular kinds of acts* (see Shanker 2002; Shanker & Taylor 2001; Taylor 1997). Therefore, the difference between a child describing how cookies are better than milk, and how milk is better than cookies, does not reduce to syntax. Without taking into account such factors as tone of voice, facial expression, gestures, and other actions, we cannot describe the child as having mastered these different moves in the language game of describing one's preferences – a point, incidentally, which the critics of ape language research made quite forcefully (see Wallman 1992).

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In regards to "internal constraints," we must direct the reader to other publications in which we look at this issue in much greater detail (see e.g., Shanker 2001). Here we would point out that **Hahn**'s substitution of "information theory" (which he discusses) for "information processing" (which we discuss) may be the reason why he urges us to pursue the very issues that DS would steer us away from (e.g., "mechanisms" by which language is "acquired" and social "cues" that impact this process). In fact, we see the demand for "internal constraints" as one of the hallmarks of a Cartesian view of language development. In this view, the child is construed as engaged in a "translation" process (a "mapping" problem), which would be radically underdetermined were it not constrained by innate concepts or word-learning biases. But what if we should abandon the Cartesian premise that meaning and reference are primate mental phenomena, on which this picture of language acquisition rests, and instead view children developing language skills as *learning the techniques required to engage in different kinds of practices*? Children develop their communicative intentions within a richly structured interactional context, which involves a "continuous unfolding of individual action that is susceptible to being continuously modified by the continuously changing actions of the partner" (Fogel 1993). By such a non-Cartesian outlook, the child is seen, not as *inferring* what adults mean when they use such-and-such a word, but rather, as participating in joint activities that involve a continuous process of mutual adjustment. The child acquires through this co-regulated process those skills *that we describe as "language skills"* (Hymes 1974; Taylor 1997).

This brings us to another of the major issues raised in the commentaries: the implications of DST for our views about the nature of language development (**Hahn**, **Kuhlmeier & Bloom**, **Locke**). We were delighted that Locke contributed a commentary, as we see his work as pivotal to the dynamic systems approach to language development. It is beyond the scope of this response to explore the intricacies of this issue in any detail; for present purposes we would merely note that we fully agree with the emphasis that Locke places on the secondary altriciality of our species for our understanding of the processes involved in language development (see Shanker & Taylor 2001). In fact, it was Locke's *A Child's Path to Spoken Language* (Locke 1993) that first brought the importance of this issue to our attention. For dynamic systems theorists, there is a particularly interesting parallel between Locke's research on how a fetus becomes attuned to the characteristic speech sounds of its community, and Gottlieb's experiments on wood ducklings (see Gottlieb 1997), in which Gottlieb demonstrated how the expression of what would normally be classified as *instinctive* or *innate* behavior in newborns is in fact regulated in nonobvious ways by prenatal *experience* (in particular, how the duckling's auditory experience of its siblings' vocalizations during the embryonic period contributes to its identification of the maternal call of its species). Clearly, much important work remains to be done here on how language development and socialization begins very early – long before the child begins to babble, let alone utters or even understands their first words.

R4. Are IP and DS compatible paradigms?

At this point it should be abundantly clear that we cannot subscribe to the suggestion by **Bonvillian & Patterson**, **Spurrett**, and **Westbury** that IP and DS are in fact complementary paradigms. Not only do they ask different questions of the phenomena under study, IP and DS are also based on different primary principles, as we have shown and as is reflected in the commentary by **Fogel**, **de Koeyer**, **Secrist & Nagy** (**Fogel et al.**). To the points we made in R2, we add only some remarks in response to **Rendall & Vasey**. It is a misunderstanding of co-regulation to assume it requires symmetry, or that mutual attunement always results in cooperation. Social events between children and their caretakers typically include elements of pronounced asymmetry; that adults have everything from greater motor control to greater social power is recognized and incorporated into DS-grounded studies of both nonhuman primates (King 2002) and humans (see Fivaz-Depeursinge & Corboz-Warnery 1999 for an additional example to Fogel 1993). Whereas DS seeks joint maintenance of action (and transformation of action as we have outlined) between social partners, it does not insist on *equivalence* of action.

Similarly, the fact that asymmetry within primate social relationships may involve conflict as well as cooperation is wholly accommodated by DS. One reading of our dance metaphor suggests a focus on "positive" outcomes via cooperation, but in fact we intended a broader perspective. One of us (King), having assessed hundreds of hours of filmed social interactions in captive bonobos and gorillas for the presence of co-regulation, readily finds co-regulation between social partners engaged in conflict. The contingency and creativity so intrinsic to a dance are just as fundamental to a threat-mediated aggressive interaction or to an outright fight (King, in preparation) Indeed, if DS were not equipped to embrace conflict as well as cooperation, and asymmetric as well as egalitarian events, it would be of limited use for understanding the inevitable power differentials that characterize social communication in primates, nonhuman and human.

R5. Does DS facilitate consideration of evolutionary questions?

Russon & Begun suggest that the DS paradigm creates an impasse for the study of cognitive and evolutionary dimensions of communication because "no basis is available for comparing communicative processes across species or assessing their relationship to one another." We see, on the contrary, enormous potential in DS for illuminating evolutionary issues. It is for this reason – and not because of any a priori judgments about the quality of comparative social communication – that we have limited our work to analysis within the primate lineage. Indeed, we welcome suggestions that co-regulation exists in the social communication of other mammals and birds (**Pepperberg**, **Vauclair**).

Within primatology, we badly need fine-grained, qualitative work describing details of the contingency and creativity that mark co-regulation in great ape vocalizations, gestures, and body movements, both in spontaneous ape-ape interactions in the wild and captivity, and in the patterned routines that emerge between enculturated apes and their human caregivers. **Armstrong** and **Leavens** clearly grasp the need for richly descriptive ethnographies of ape social lives. In highlighting the importance of studying spontaneous communication between apes, though, we mean to acknowledge our disagreement with Armstrong's sugges-

tion that communication can be judged to exist only by humans' interacting with another human or with an ape (King & Shanker 2003).

In considering research methods, we would caution against reducing rich qualitative research to the status of "anecdote," as does **Miklósi**, or assuming that "the pain" of rigorous scientific work is linked differentially to statistical analysis. In the experience of one of us (King), only qualitative work can answer certain questions about the contingency and creativity of co-regulated primate social communication. Further, when working from filmed records, transcription and careful qualitative analysis is time-consuming, requiring immense attention to detail (King, in preparation). Our intent, however, is not to dichotomize qualitative versus quantitative methods. We envision a vital role for quantitative research, for example, in documenting the relative proportions of co-regulated versus more rigid, noncreative social events across various contexts. We envision a collective enterprise aimed at discovering which conditions promote, and which constrain, full expression of co-regulated social communication in primates and other animals.

Given the principles of DS, key evolutionary questions would not prioritize tracing either the development of individual cognitive capacities that support mental representations, or the path by which certain human vocalizations or gestures might have developed from precursors in our closest living relatives. In line with DS's shift away from the transmitted signal "in the head" of the individual, primary attention would be given to the comparative evolutionary development of socio-emotional dynamics between partners. Unlike **Russon & Begun**, we do not wall off the study of such socio-emotionality from the study of cognition, and believe that the study of one would greatly enhance understanding of the other.

Applying DS to primate communication will, we predict, lead to a series of "articulated propositions" (Latour 2000), that is, propositions able to establish connections across elements of a system in new ways. In sum, just as we acknowledge a robust history for the development of the DS framework, now emerging as a genuine and vital paradigm, we envision an exciting future in which various DS-inspired approaches (a blend of qualitative, quantitative, computational, and other methods) discover unexpected *riches* in ALR and the study of primate social communication in general.

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