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Species and individual differences in communication based on private states

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Abstract: The way people come to report private stimulation (e.g., feeling states) arising within their own bodies is not well understood. Although the Darwinian assumption of biological continuity has been the basis of extensive animal modeling for many human biological and behavioral phenomena, few have attempted to model human communication based on private stimulation. This target article discusses such an animal model using concepts and methods derived from the study of discriminative stimulus effects of drugs and recent research on interanimal communication. We discuss how humans acquire the capacity to identify and report private stimulation and we analyze intra- and interspecies differences in neurochemical mechanisms for transducing interoceptive stimuli, enzymatic and other metabolic factors, learning ability, and discrimination learning histories and their relation to psychiatric and developmental disabilities.

Keywords: animal communication; animal models; autism; communication; developmental disabilities; drug states; emotion; experimental analysis of behavior; individual differences; language; private events; symbols; verbal behavior

A visitor to a zoological garden witnesses chimpanzee behavior that bears striking resemblance to human behavior. The ape's facial expressions, manual gestures, and howls of displeasure as it wraps its head in its arms remind the viewer of our own efforts to make our feelings understood by others around us. These similar behavioral patterns have led many human observers to speculate that the chimpanzees' behavior and their own share important common factors.

Young children also convey feelings to parents and caregivers in nonverbal ways, for example, with tears or sobs. Yet, unlike chimpanzees and other nonhuman species, most children somehow seem to learn to identify their feelings and to report them to others through natural socialization processes. The present target article is about the nature of this process and the mechanisms responsible for creating individual differences in communicative behavior involving one type of private stimulation – emotional or feeling states. We will propose a nonhuman laboratory animal model for this process.

Figure 1 illustrates our animal model: a pair of birds was trained to exchange arbitrary cues, "letters," based on drug-induced state variations in the internal environment of one of them. The drug-cue bird received cocaine, pentobarbital, or saline and was trained to communicate with another bird by pecking response keys corresponding to these drug-induced state variations. As far as we know, this interaction represents the first laboratory demonstration in a nonhuman species of the exchange of arbitrary communicative cues based on the internal state of one of the participants. We believe this model can help us understand how humans exchange information about private stimulation, even when it is novel. It may also shed light on human individual differences in the skill and tendency to engage in this kind of communication.

Ours will be an animal model of one way humans may acquire the ability to report on their private stimulation. To better understand it, we will have to review relevant variables from a number of areas that were drawn upon and combined to generate the performances illustrated in Figure 1, including animal models of human psychological phenomena, both natural and artificial animal communication, the status of private events in psychology, and mechanisms of interoceptive stimulus control (with particular emphasis on pharmacological agents). Our second objective is to show that the forms of behavior used to generate our model are also applicable to much more sophisticated behaviors in later-evolved species. Our model was based on the more limited response capabilities of birds; subsequent animal models of communication based on private stimulation will undoubtedly display more complex features.

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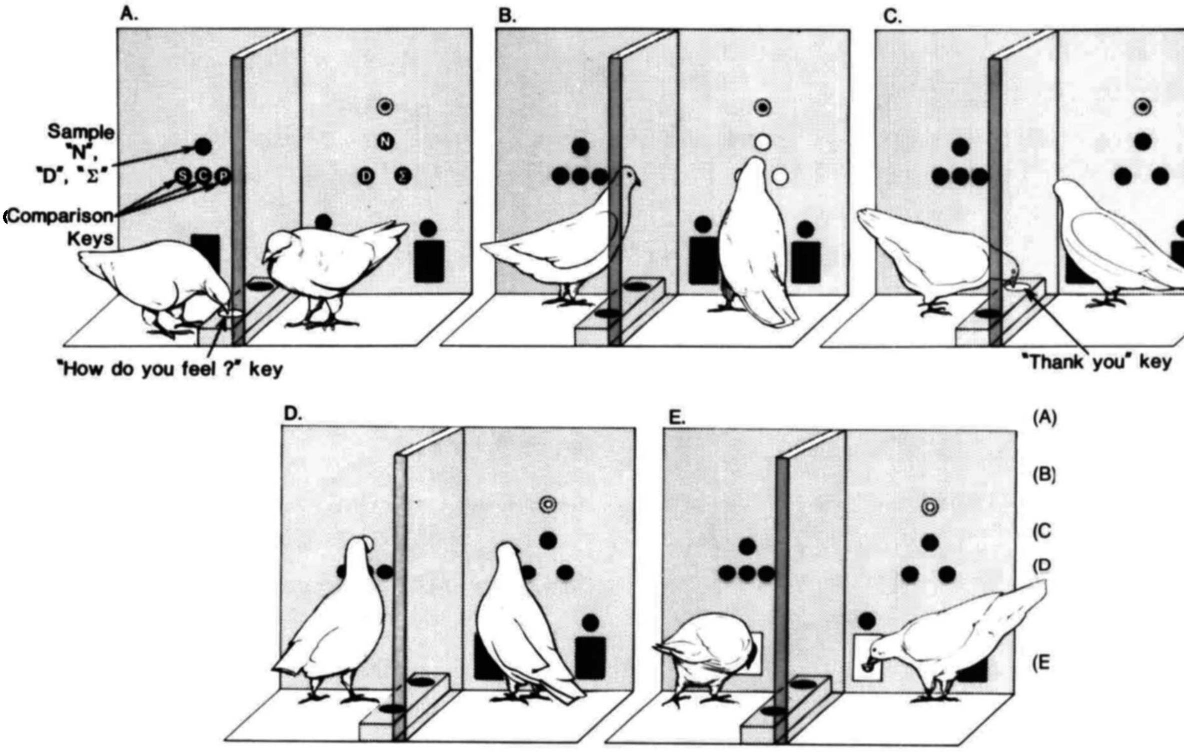

Figure 1. A two-pigeon communicative exchange based on the internal state of one of the birds (Decoder, left; Drug-cue bird, right). A: The Decoder pecks the "How do you feel?" key. B: The Drug-cue bird pecks the drug-class letter corresponding to its internal state. C: The Decoder pecks the "Thank you" key, which presents the flashing blue light to the Drug-cue bird. This response also presents to the Decoder the drug-class letter previously pecked by the Drug-cue bird. D: The Decoder matches the drug-class letter (projected on its sample key) by pecking it and then pecking the letter representing the specific drug that the Drug-cue bird is currently experiencing. The Drug-cue bird attends to the flashing blue light. E: The Decoder receives food upon correctly completing the communicative exchange and the Drug-cue bird receives water (adapted from Lubinski & Thompson 1987).

1. Models of human psychological phenomena

The notion that inanimate and animate systems may serve as models for human phenomena is very old (Gunderson 1985; Keehn 1986). Leonardo da Vinci (Keele 1983) drew mechanical devices he believed could model human actions if they were actually built and René Descartes (1662) hypothesized that the automata he designed captured key components of human behavior. Both models were inanimate, however. By the middle of the nineteenth century, Darwin's (1859) theory of evolutionary continuity became a basic assumption of the rapidly developing field of biology. Darwin's work suggested that to gain biological insight into human beings it may be more illuminating to study nonhuman animate systems rather than the inanimate models of da Vinci and Descartes. Claude Bernard (1885), the founder of experimental medicine, used dogs in laboratory preparations as models of human physiology, assuming basic continuity in physiological functions across species. Both Darwin and Bernard argued that anatomy, physiology, and behavior not only look similar in different animals but often share common evolutionary origins and current regulatory mechanisms.

In modeling human psychological phenomena using nonhuman species, one accordingly assumes functional continuity in behavior. Thorndike's (1911) model of learning in chickens and cats, Pavlov's (1927) description of associative conditioning in dogs, and Tolman's early studies on "insight" in rats (Tolman & Honzik 1930) were all thought to have important implications for human conduct. In the middle of the twentieth century, however, biological continuity was questioned in linguistics and some areas of psychology (cf. Miller 1990). Human behavior (especially communicative behavior) was thought by many to differ qualitatively from the behavior of other animals. Even among some behaviorists, the human tendency to communicate thoughts and feelings was frequently mentioned as falling beyond the realm of cross-species generalization and laboratory experimentation.

H. S. Terrace (1985, p. 1026), for example, wrote: "The ability to name is also relevant to a basic aspect of human consciousness. As part of our socialization, we learn to refer to various inner states: our feelings, emotions, thoughts, and so on (cf. Skinner 1945). If one applied to internal states the same distinction one makes between perceiving an external event and naming that event, one is left with a very interesting difference between animal and human consciousness. Human beings are able to name their inner states, animals are not (e.g., Gallup 1977; Griffin 1976)."

This target article will explore the possibility that arbitrary, non-species-specific communication between organisms based on private events may extend beyond *Homo sapiens* and does not require language. We will present evidence that these are differences within and across species in the disposition to respond discriminatively to and communicate based on stimuli that are accessible only to the subject, such as events beneath the skin. This extension of biological continuity to communication based on private events may not only facilitate our understanding of human development but it may also elucidate other psychological phenomena. We will discuss animal models of communicative behavior sharing features of the relationships between "speaker" and "listener." We believe these communicative interactions, which were formed in the laboratory using the experimental tactic of *behavioral synthesis* (Catania 1983; Epstein 1984), accurately model some aspects of human interpersonal communication.

Behavioral synthesis is the methodology underlying our model. Behavioral constituents are combined into novel combinations that have some of the functional properties of complex human behavior. Laboratory animals are trained to display several independent repertoires that are hypothesized antecedents to a complex human performance. Once established, cues for each are presented in the proper temporal arrangement (e.g., Findley 1962; Thompson & Lubinski 1986) to determine whether an integrated pattern resembling human behavior emerges. The final product of a behavioral synthesis is a plausible reconstruction of some aspect of human behavior. Such behavioral interpretations (Day 1976; Schnaitter 1978; Skinner 1969) parallel evidence provided by simulations in other scientific domains, providing a "plausibility proof" (cf. Epstein 1984; Kordig 1978)[1] of the variables underlying complex behavioral phenomena, especially those that are not directly accessible or cannot be investigated for ethical reasons.

Antecedents to this methodology are found in Köhler's (1925/1976) classic experiments on "insight" learning in chimpanzees, which motivated one of the first contemporary demonstrations of the utility of behavioral synthesis. Köhler suspended a banana out of a chimpanzee's reach and placed in his cage a large wooden box that was sturdy enough to stand on. The chimpanzee solved the problem by pushing the box below the banana and standing on the box to reach it. At the moment when Köhler's chimpanzees detected a new instrumentally useful rearrangement of preexisting behaviors (viz., pushing large objects + reaching for food), they were said to have experienced an "insight" analogous to insight learning in humans. In the present context, *insight* may be interpreted as a behavioral synthesis of two previously independent behaviors that are now components of a more complex behavioral form.

Köhler's finding led Epstein et al. (1981a) to reason that apes could solve the problem without specific training because they had previously learned all the constituent behaviors necessary for this novel performance (reaching for edibles, pushing or moving large objects). The problem of how to reach the fruit provides the opportunity for both constituents to come into close temporal conjunction. Thus, a new form of instrumental behavior emerges as a result of the simultaneous presentation of cues relevant to both repertoires. Indeed, many instrumentally effective behaviors that appear totally novel, or wholly creative, actually stem from recombinations of existing behaviors (cf. Lubinski & Dawis 1992; Lubinski & Thompson 1986; Skinner 1957). Epstein et al. set out to determine whether pigeons equipped with relevant constituent repertoires could solve the "insight" problem too. They showed that the complex primate behavior observed by Köhler (1925) could be synthesized in the laboratory with pigeons.

Using grain delivery as reinforcement, Epstein et al. (1981a) trained pigeons to peck a small box in their experimental chamber. Pecks that moved the box to various areas of the chamber were reinforced. The box was then removed from the apparatus and in its place a small plastic banana was suspended from the ceiling of the chamber. Subjects were trained to peck this small plastic fruit, again with grain delivery as reinforcement. After both component repertoires were trained, birds were placed in their chamber with both the box and the banana. The box was located off to the side, while the banana was suspended near the top of the ceiling of the apparatus (out of pecking range, but not if subjects were standing on the box). Eventually, the birds began pecking at the box until they had moved it directly below the banana, hopped up on the box, pecked the banana, and earned a reinforcer.[2] As in the case of Köhler's apes, the constituents of the pigeons' behavior were synthesized in a novel way to gain access to food. Outside the laboratory, of course, feral species frequently confront unique problem-solving situations with familiar components that set the occasion for nonexperimental forms of behavioral synthesis. Like the behavioral syntheses observed in the laboratory, these are interpreted as novel responses to unique stimulus configurations.

Epstein and his colleagues have also used pigeons to synthesize performances they describe as "symbolic communication" (Epstein et al. 1980), "tool use" (Epstein & Medalie 1983), "self-awareness" (Epstein et al. 1981b), and the spontaneous use of "memoranda" (Epstein & Skinner 1981), as found in humans and other primates. Until recently, however, laboratory simulations of covert behavior (i.e., thoughts, feelings, or images) have received limited attention, because of experimental difficulties in controlling the relevant variables. To set the stage for our synthesis, we first must turn to the study of animal communication in general.

2. The study of animal communication

To understand communication is difficult; communication based on private stimulation is even more perplexing. With regard to the arbitrariness of the medium of communication, communicative behavior falls along a continuum. Behavior that is elicited by pheromone release or by the visible presence of a conspecific that subsequently evokes a behavioral change in another member of the species lies near one end of this continuum (Salzinger 1973). Near the opposite end are human vocal utterances which, by virtue of their symbolic relationships and organization, produce unique and characteristic responses from other members of that community (e.g., I love you). In this target article, we are concerned with nonhuman communicative behavior that shares properties of human behavior, but is unlike either end of this communicative continuum. The communicative behaviors we will be discussing are arbitrary (non-species-specific) behavior patterns that are taught to animals to enable them to communicate in non-species-specific ways. Such learned communicative behavior shares fea-

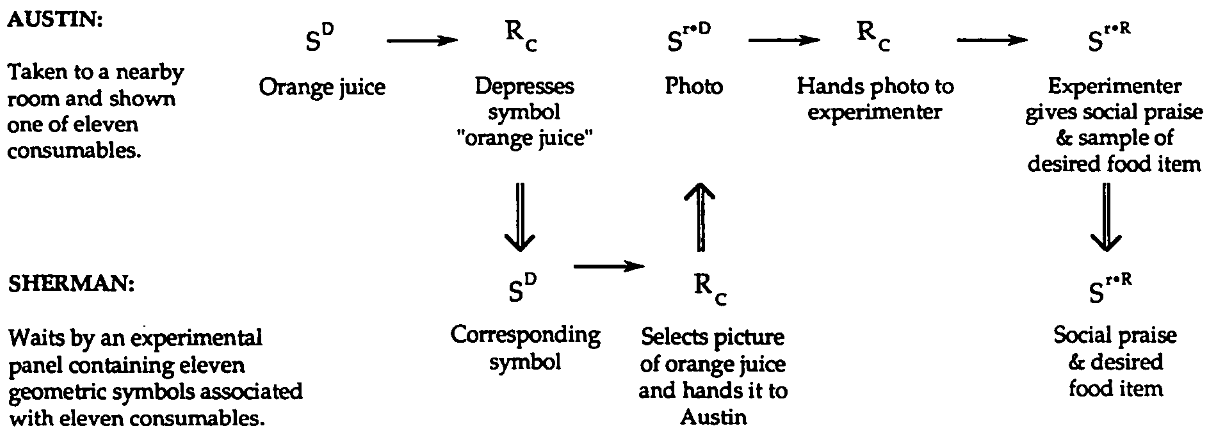

Figure 2. An interlocking paradigm of a communicative exchange between two of Savage-Rumbaugh's (1984a) chimpanzees (Austin and Sherman). The notational system follows: S^D = discriminative stimulus; $S^{r\cdot D}$ = a conditioned reinforcer, which also functions as a discriminative stimulus; $S^{r\cdot R}$= a conditioned reinforcer plus an unconditioned reinforcer; R_c = a communicative response.

tures with complex human communicative behavior and represents a form of verbal behavior according to Skinner (1957), but it is not language.

Traditional accounts of animal communication usually have referred to species-typical exchanges of inherited signal systems. Numerous definitions exist; for example, "the term 'animal communication' has often been used to refer to the kinds of signals which pass to and fro between social animals and help to mold each other's behavior towards some goal which is to their mutual advantage" (Cullen 1972, p. 101). By "kinds of signals," Cullen meant species-specific, response-produced stimuli controlled by the presence of food or a predator. Dawkins and Krebs (1978) argued that communicative interactions evolved by natural selection; the actor (their term for the sender of a signal) is selected to change the behavior of a reactor (typically a conspecific) to the advantage of the former. Other definitions proposed by comparative psychologists and ethologists are similar.

Recent developments in the laboratory synthesis of interanimal communication, however, reveal that such descriptions are unduly restrictive. Many species have been trained to communicate in non-species-specific ways and have continued to interact in this manner without unconditioned reinforcement, aversive stimulation, or deprivation of a primary reinforcer (described below). These conspecific interactions are based on experimentally imparted (and subsequently synthesized) communicative repertoires that differ from the species-typical, biologically-shared signal system of the participants.

In these experiments, communicative behavior can be viewed as an exchange of arbitrary discriminative stimuli that subsequently results in the opportunity for reinforcement (conditioned or unconditioned) for at least one of the participants. (A discriminative stimulus, notated "S^D," is defined as an environmental event that sets the occasion for reinforced responding; cf. Skinner 1974.) This definition of communicative behavior incorporates important elements of traditional ethological theory (enhanced instrumental effectiveness as a function of interacting with another species), as well as recent developments in laboratory synthesis (viz., the arbitrariness of the cues exchanged).

Figure 2, based on interactions between two of Savage-Rumbaugh's (Savage-Rumbaugh 1984a; Savage-Rumbaugh et al. 1978) chimpanzees, Austin and Sherman, illustrates how communicative exchanges may be analyzed. Communication can be described as a series of interanimal exchanges of response-produced stimuli. In *Verbal behavior,* Skinner (1957) referred to the outline of Figure 2 as an *interlocking paradigm.* The interlocking paradigm highlights the multiple functions of response-produced stimuli exchanged between organisms during communication. A response can serve as a conditioned reinforcer (S^r) for preceding behavior as well as a discriminative stimulus (S^D) for a subsequent response, hence the notation "$S^{r\cdot D}$". These response-generated stimuli are linked socially by communicative responses (R_c).

Skinner (1957) extended the "three-term contingency," $S^D \rightarrow R_v \rightarrow S^r$, to verbal behavior (where R_v = verbal response as opposed to communicative response R_c) and he stipulated that reinforcement for emitting an R_v is provided by another organism. It is clear that the interlocking paradigm (shown in Fig. 2) amounts to the interspecies connection of three-term contingencies (so that reinforcements for each participant's behaviors are interdependent or socially linked). This approach has been used in a number of primate studies, as well as more recent experimentation using pigeons, for establishing experimental syntheses of interanimal communication (Epstein et al. 1980; Lubinski & MacCorquodale 1984; Lubinski & Thompson 1987). These studies provided the foundation for the communicative features of our synthesis.

Before we apply the interlocking paradigm to our model, we will briefly review experimentally imparted communicative repertoires to illustrate how sophisticated some of these behaviors have become, and how novel the emerging behaviors have been despite their not having been specifically trained.

3. Experimentally synthesized communicative behavior in nonhuman organisms

Roger Brown (1970) wrote: "It is lonely being the only language-using species in the universe. We want a chimp to talk to so we can say: 'Hello, out there? What's it like, being a chimpanzee?'" (p. 211). Investigators have been trying to impart arbitrary communicative behaviors to other species for a very long time. Over a century ago, the first scientific article on this topic appeared in *Nature*. In his "Teaching animals to converse," Lubbock (1884) described a method for interacting with dogs via pieces of cardboard containing printed words such as "water," "out," "bone," and "food." Other attempts followed; they even involved training apes to vocalize (Furness 1916; Hayes 1951; Kellogg & Kellogg 1933). Furness (1916), for example, trained an orangutan to vocalize the words "cup" and "Papa" (in the presence of an actual cup or himself, respectively). The training of this two-response verbal repertoire was arduous, however. The ape's vocal productions were cumbersome, frail, and, quite frankly, unimpressive.

These attempts achieved only marginal success because the apes were trained to engage in vocal behavior (Furness 1916; Hayes 1951) rather than more appropriate nonvocal communication. In the early 1960s, Beatrix and R. Allen Gardner were the first to point this out, hypothesizing that because chimps are not well equipped anatomically to engage in spoken verbal behavior a different modality might enable them to communicate more effectively. They abandoned the vocal medium and adopted American Sign Language (ASL), a communication system used by people with profound hearing impairment. ASL is a manual language based on hand and finger movements; the great apes could execute these relatively easily. The Gardners taught their female chimpanzee, Washoe, more than 130 different word signs (Gardner et al. 1989). Washoe combined signs into phrases, which generalized to novel situations spontaneously. In addition, through closed circuit television, she was occasionally observed signing upon seeing pictures of objects in books even when no one else was present.

3.1. Experimentally synthesized symbolic systems in primates

Other nonvocal modalities have been used with comparable success in attempts to address other kinds of questions (cf. Premack 1970; Rumbaugh 1977; Rumbaugh & Gill 1976; Terrace 1979). One of Premack's chimpanzees (Sara), for example, using an experimentally trained symbolic communication system, displayed behavior consistent with Piaget's concept of conservation for both liquids and solids (Woodruff et al. 1978). In subsequent studies, chimpanzees were taught to perform elementary numerical operations using symbols (Boysen & Bernston 1989; Woodruff & Premack 1981; see Davis & Pérusse 1988 for a general discussion) and to use symbols to form higher-order conceptual classes (Gardner & Gardner 1984; Savage-Rumbaugh et al. 1980). Savage-Rumbaugh et al. (1980), for example, first trained chimpanzees to ask for various tools and food items symbolically. Then she taught her apes to further classify these items, as well as some new ones that had not been introduced before, into food and tool categories. Gardner and Gardner (1984) found that their chimpanzees could perform a similar task by classifying pictures (again both novel and familiar ones) into various categories. Thus, we can conclude from these studies that apes are capable not only of using nonvocal communicative modalities but also of extending their repertoire of signs and lexicons to form higher-order conceptual classes.

Gardner and Gardner (1984) reported that once three stimulus equivalence relationships were established, that is, the ability to exchange "objects" for "signs of objects" and for "photographs of objects," their chimpanzees would naturally extend their repertoire to new photographs of familiar objects, signing "orange," for example, in response to a previously *unseen* photograph of the fruit. In addition to generalizing to novel photographs, the chimps spontaneously displayed "creative" stimulus/response relationships without being specifically taught. Great apes equipped with these communicative repertoires used symbols in uninstructed ways, combining strings of symbols in meaningful combinations to communicate integrated concepts (e.g., "sweet" and "water" to report on the eating of watermelon, Gardner & Gardner 1984; Premack 1976). These findings oppose the idea that "man is the only animal to have combinatory productive language . . . a species-specific form of behavior" (Miller 1967, p. 83).

Conspecific tutoring has recently been observed in the Gardners' laboratory (cf. Gardner et al. 1989). Over a five-year period, the Gardners raised an infant chimpanzee ("Loulis"). Loulis lived with Washoe and other chimpanzees who were proficient in ASL signs. During these five years, human experimenters refrained from signing in the presence of Loulis, ensuring that the chimpanzees were the only ones who had the opportunity to communicate with signs in Loulis's presence. The Gardners wanted to ascertain whether the chimpanzees would spontaneously try to teach Loulis to sign. Washoe, indeed, appeared to instruct Loulis actively in using signs; during the early stages of instruction, she even molded Loulis's hands to form the shape of certain signs. At the end of the observation period, Loulis had not only acquired 51 signs, but had learned to combine them spontaneously in novel ways (cf. Gardner et al. 1989).

3.2. Communication based on private states

Chimpanzees also appear to have the ability to communicate about emotional states using their acquired symbolic repertoires. For example, one of Savage-Rumbaugh's chimpanzees, Austin, upon seeing two men in white coats carrying an anesthetized chimp, signed "scared." Along with this behavior, Austin displayed pilo-erection, which was interpreted as a sign of anxiety in the chimpanzee (Savage-Rumbaugh 1984a). The manual sign "scared" had been taught as a request to play a game in which the teacher dressed up in a costume and pretended to scare the chimps (Savage-Rumbaugh 1984a, p. 242). One of Terrace's chimps (Nim) signed "angry" or "bite" rather than physically attacking (Savage-Rumbaugh et al. 1983, p. 458). These anecdotes and others suggest that it may be possible to train primates and other species to report to one another on their internal emotional states in non-species-specific and perhaps even very sophisticated ways (cf. Mackintosh 1987).

The accumulation of this impressive evidence led Griffin (1976; 1982; 1984) to state: "the possibility of animal introspection is more than a will-o'-the wisp; it is a potential method which has already been employed to a very limited degree by the Gardners, Fouts, and other students of chimpanzees, and one that is ready for development and exploitation with other species to roughly the degree that they employ flexible communication systems" (Griffin 1976, p. 534). Terrace and Bever (1976) have speculated along similar lines: "monkeys can discriminate between the expression of different emotional states, as shown on video displays. . . . Thus, learning to label emotional displays may not prove difficult for chimpanzees. . . . While we have no bases at present for asserting that a chimpanzee could engage in the kind of introspection that is entailed in a description of its own feelings, or emotions, we find this possibility intriguing" (p. 581).

4. Laboratory synthesized symbolic and communicative exchanges between nonhuman organisms

"It would be astounding to discover that insects or fish, birds or monkeys, are able to talk to one another. . . . [because] Man is the only animal that can talk . . . that can use symbols" (Black 1969, p. 3). The first convincing demonstration that Black's assertion is incorrect was provided by Savage-Rumbaugh et al. (1978) – the interanimal exchange illustrated in Figure 2 [see also Savage-Rumbaugh et al.: "Linguistically Mediated Tool Use and Exchange by Chimpanzees" *BBS* 1(4) 1978]. They successfully taught chimpanzees to use symbols to report to other chimpanzees the presence or absence of the objects in a nearby room that only one animal could see. This ground-breaking study demonstrated that organisms other than humans can learn to interact communicatively by exchanging arbitrary symbols with one another. Nonhuman animal/animal exchanges of symbols, as well as experimenter/nonhuman exchanges turned out to be possible.

Savage-Rumbaugh et al. (1978) taught a pair of chimpanzees, Sherman and Austin, a long sequence of chained behaviors: they first were taught to select 11 different geometric forms (e.g., square, circle, triangle, etc.) from an experimental panel. These forms corresponded to 11 distinctive food and drink items (bean cake, orange juice, banana, etc.). Subsequently, Sherman and Austin learned to select photographs of those 11 consumables, for which they were given the corresponding food items (see Fig. 2). This completed the first phase of the experiment. In the second phase, the animals were taught to communicate with one another. One chimpanzee would be taken to a nearby room and shown one of the 11 consumables while the other waited by an experimental panel. Sherman and Austin were then reunited. The chimp who had seen the item informed the other by pressing the corresponding geometric form on the response panel. The second chimpanzee, upon observing this response, was taught to point to the photograph of the hidden item from among 11 pictures of the items located nearby. The symbolic exchange was thereby completed. If both subjects performed the symbolic exchange correctly, they were rewarded with social praise or food or drink, though not with the specific consumable item that had been presented in the adjacent room.

Following Savage-Rumbaugh et al. (1978), Epstein et al. (1980) set out to determine whether interanimal symbolic exchanges could be learned by pairs of pigeons if they were provided with the proper training and experimental medium. Their goal was to teach two pigeons to reliably exchange discriminative stimuli that had been matched to an aspect of their external environment. Only three symbolic relationships were taught to the pigeons, however, in contrast to the 11 taught to the chimpanzee. Their experimental apparatus (Fig. 3) consisted of two contiguous chambers separated by a transparent divider. Therein, two pigeons (Jack and Jill) were trained to exchange information regarding colors (red, green, and yellow) using letters (R, G, and Y, respectively). First, the pigeons were trained independently with traditional fading and shaping techniques (Catania 1992) to match arbitrary discriminative stimuli: the 3 colors matched the 3 letters. These colors were recessed behind a curtain in one chamber and hence available only to one of the pigeons. Jill was taught to match colors to lettered discriminative stimuli; Jack was trained to request a color by pecking the "What color?" key and then the "Thank you" key after having received this information. Both birds were food-deprived and received mixed grain for correct responses.

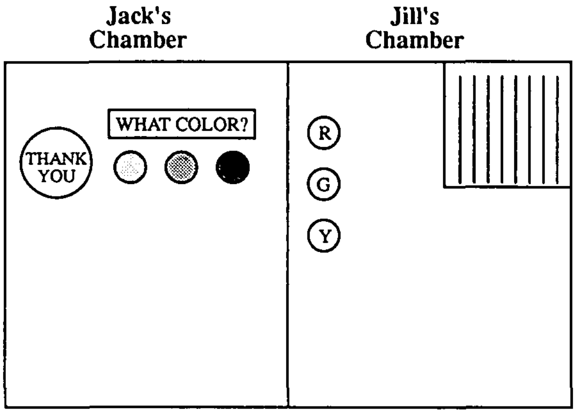

Figure 3. Epstein et al.'s (1980) experimental arrangement: Jack was trained on the left, Jill on the right. Jack needs information about the color behind the curtain in the upper right-hand corner of Jill's keyboard. The R, G, and Y letters on Jill's keyboard correspond to the colored lights projected behind the curtain. The three keys below the "What color?" key on Jack's keyboard are yellow, red, and green, from left to right. The birds are separated from one another by a Plexiglas divider.

Once each bird had learned the string of components necessary for the targeted synthesis, they were placed in their experimental chamber simultaneously and the apparatus was programmed so that reinforcement for each component was contingent on the other bird's behavior. That is, the constituent components of the birds' independent behaviors were programmed experimentally to be interdependent, comparable to the interlocking paradigm illustrated in Figure 2 for the chimpanzees. The following experimental synthesis was then observed.

Jack started the communicative exchange by pecking a rectangular key labeled "What color?" (see Fig. 3). This response illuminated one of the 3 colors behind Jill's curtain; Jill would then thrust its head through the curtain to observe what color was illuminated and then peck one of the 3 lettered keys corresponding to the illuminated color. This response automatically illuminated the letter pecked by Jill. Next, Jack pecked a key labeled "Thank you," thus rewarding Jill with mixed grain. Jack then observed the illuminated letter on Jill's response panel and pecked the appropriate corresponding color on its response panel. If Jack pecked the correct key, it received mixed grain; the cycle would then continue with different colors appearing behind Jill's curtain (following the "What color?" response) quasi-randomly. Hence many of the important social components of the Savage-Rumbaugh interanimal communicative exchange were structurally preserved in the Epstein et al. study. Indeed, some intriguing nonprogrammed communicative behaviors emerged over time and appeared to contribute to the integrity and efficiency of the birds' exchanges. The birds seemed to attend closely to the discriminate stimulus changes in each other's chamber, after each had completed a response. If either bird was at all sluggish after completing the next component of the interaction, the other would peck at the Plexiglas, which appeared to "hurry the other along."

It should be noted, however, that the stimulus interrelationships were more complex for the chimpanzees than for the pigeons. Only a subset of the criteria for symbolic meaning (with respect to the letters exchanged between subjects) was met by the birds; the letters and colors had only a unidirectional functional significance, rather than the bidirectional/transitive symbolic relationships discussed earlier in the primate studies (Gardner & Gardner 1984; Savage-Rumbaugh 1984a; 1984b; Savage-Rumbaugh et al. 1980). This critical difference between the chimpanzee and the pigeon preparations does not affect the structure of the social interaction, however, only the nature of the stimulus equivalence relationships between elements of the exchange. The stimulus (cue) relationships were more complex for the apes than for the pigeons.

Following the Epstein et al. (1980) demonstration, Lubinski and MacCorquodale (1984) reported a second two-pigeon communicative exchange, with birds trained to interact in the absence of food deprivation and without material rewards corresponding to such states. One of the objectives of this study was to answer the objection of several commentators who had noted that, unlike most human transactions, Epstein et al.'s tasks (like many primate tasks) were tied to specific biological drive states and material rewards (cf. Mounin 1976; Savage-Rumbaugh 1984b; Walker 1983). Lubinski and MacCorquodale wanted to determine whether a comparable performance could be maintained through conspecific social stimulation (i.e., without specific drive states and material rewards).

Lubinski and MacCorquodale constructed an experimental apparatus similar to Epstein et al.'s: two adjoining experimental chambers, separated by a Plexiglas divider, each supplied with an individual response panel (see Fig. 4). Two pigeons served as subjects, the "tacter" (trained in the right chamber) and the "mander" (trained in the left chamber).[3] Three colors (red, white, and yellow) served as discriminative stimuli; these were arbitrarily matched to the letters "R," "W," and "Y," respectively (see Fig. 4).

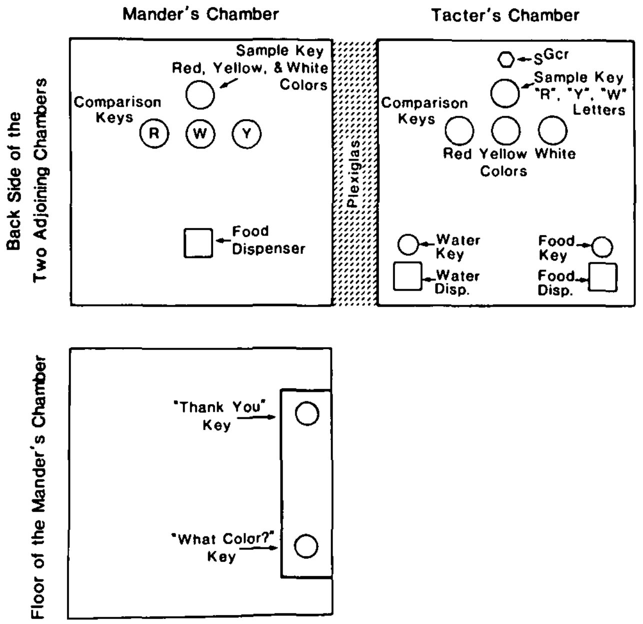

Figure 4. Lubinski and MacCorquodale's (1984) experimental arrangement for training an exteroceptive communicative exchange, ultimately without deprivation and material rewards. The top two panels are the front wall of each chamber. The bottom panel is the top view of the floor of the left chamber. Adjoining keyboards for the two birds are separated by a Plexiglas barrier. The mander's keyboard is on the left; the tacter's is on the right.

The experimental design was similar to Epstein et al.'s, except that when the mander pecked "Thank you," the tacter received an intermittently flashing light, which had previously been paired contingently with both food *and* water (multiple commodities making it a more powerful consequence, a *generalized conditioned reinforcer*, notated S^{Gcr} in Fig. 4). When the flashing light appeared, the tacter could receive food or water by pecking the corresponding food or water keys. (The mander was food-deprived to 85% of its free-feeding weight throughout the entire experiment.) Initially, the tacter performed this interaction while deprived of either food or water. Subsequently, however, its performance was observed when it was satiated for food and water and unconditioned reinforcement was absent (the food and water keys were inoperative); under these conditions, the presentation of the flashing light, contingent on a correct response, operated as the sole programmed consequence for completing the interanimal exchange. Some intriguing communicative responses emerged in this study as well, and these appeared to contribute to the integrity and efficiency of the exchange in, if anything, more ways than the emergent behaviors observed by Epstein et al.

When satiated, the tacter's performance was noticeably more sluggish than when it was deprived of either food or water. On those days, following the mander's response "What color?" the tacter would frequently not respond immediately to the discriminative stimulus change in its chamber. At this point, the mander would peck the Plexiglas repeatedly and exhibit species-characteristic aggressive displays apparently directed toward the tacter. Often, the tacter would then approach the keyboard and match the projected color to the appropriate letter. Because of the dynamic characteristics of this interaction, Lubinski and MacCorquodale (1984) decided to assess the significance of the mander's social facilitation in a second experimental phase.

For this phase of the synthesis, all contingencies regulating the interanimal exchange were maintained except the following: the birds were prevented from seeing one another by an opaque divider that covered the Plexiglas barrier. The tacter's deprivation and satiation conditions were alternated (ABAB) and the birds' communicative performance was observed. When satiated and without visual access to the mander, the tacter stopped matching letters to colors and this condition was eventually terminated. Subsequently, however, the opaque barrier was removed for a final condition (with the tacter deprived and satiated in alternate ABAB sessions, as before). With visual access to the mander reinstated, the tacter's matching behavior reappeared, following the mander's request, even though the tacter was food and water satiated. That is, when satiated and unable to see the mander, the tacter stopped matching colors to letters; however, when satiated but with visual access reinstated, the tacter continued to communicate with the mander by exchanging letter discriminative stimuli matched to colors. This matching behavior appeared to depend on the mander's display behavior (i.e., nonprogrammed emergent communicative phenomena), which was now readily available to the tacter through the Plexiglas.

Savage-Rumbaugh (1986) and Terrace (1985) have argued that pigeon preparations model communicative behavior only superficially – that subjects appeared to influence each other but, on closer scrutiny, their behavior was simply a function of the programming equipment. The Lubinski and MacCorquodale (1984) study demonstrates that the presence of a conspecific *is* necessary for social facilitation under certain circumstances (i.e., when the behavior of one of the participants of the dyadic exchange has low probability – because of satiation and the absence of material rewards). Under these circumstances, emergent communicative phenomena *independent of the programming equipment* appeared to be critical for maintaining communication. We next review the nature and status of private events in psychology in order to develop the rationale for this component of our synthesis.

5. The causal significance of private events and the accuracy of verbal reports of private events

Private events have been defined as "[t]hose phenomena of psychological interest, taking place 'inside the skin,' at a covert level, observable beyond the first person through indirect means, if at all. 'Feelings,' 'thoughts,' and 'perceptions' are terms covering the majority of these phenomena" (Schnaitter 1978, p. 1). A causal role of some sort of stimulation, arising from within the skin, has been assumed in much of psychological and psychiatric theory. That is, many theorists, as well as most lay people, assume that the primary stimuli for human action arise from inside the body. Moreover, the putative causal role of internal stimulation has been explored at length (cf. Boring 1953; Lyons 1986), with major theories of psychological adjustment and maladaptivity assuming that thoughts and feelings prompt actions. Indeed, the psychological significance of private phenomena is noted in nearly all comprehensive views on human behavior, from James (1890), Wundt (1894), and Titchener (1899) through the humanistic movement (Maslow 1962; Rogers 1961), psychoanalysis (Freud 1895; Jung 1959), phenomenological psychology (Allport 1937), and learning theory (Hull 1943; Skinner 1957; Spence 1956; Tolman 1932). Interest in covert behaviors has also emerged in the applied areas of psychology, where counselors and therapists (using, for example, Beck's [1976] cognitive-behavioral therapy or Ellis's [1970] rational-emotive therapy) have attempted to moderate clients' affect by restructuring their thoughts (cf. Kendall & Hollon 1981). In spite of psychology's ubiquitous interest in covert events, however, it is only recently that nonhuman models of communication based on such phenomena have been attempted, although a possible paradigm for imparting primitive vocabularies to species has been available for several years.

More than 20 years ago, Kenneth MacCorquodale (1969) wrote: "It is somewhat curious that Skinner, the most thoroughgoing behaviorist, is the only one who has been willing to discuss private stimuli, which he has done with characteristic consistency since 1945" (p. 837). MacCorquodale was referring to Skinner's treatment of verbal reports of private events, which first appeared in Skinner (1945). This original account has been augmented by Skinner and various writers over the last 50 years (Catania 1972; 1980; 1992; Day 1976; MacCorquodale 1969; 1970; Moore 1980; 1984; Schnaitter 1978; Segal 1977; Skinner 1953; 1957; 1974; Winokur 1976; Zuriff 1979; 1985). These writers have argued that we come to know our environments, both external and internal, through two types of sensory modalities – exteroceptive and interoceptive receptors.

Stimuli emanating from our external (public) environment are sensed through the exteroceptive receptors, which include the common sensory modalities (e.g., vision, audition, gustation, and olfaction). In contrast, internal (private) stimulus events (i.e., stimulus events happening underneath the skin) are sensed through interoceptors. It is commonly accepted that interoceptive sensory events emanating largely from smooth muscles and glands are involved in ("phenomenological") stimulation accompanying verbal reports of "anxiety," "fear," "joy," "depression," and "excitement." Although not responsible for all subjective components of emotional states, the circulatory, digestive, proprioceptive, and respiratory systems are involved in providing interoceptive stimulation on which statements about affect are based; and covariations in the properties of these physiological events as a function of reinforcing stimuli, emotionally charged punishers and reinforcers, are well documented (Buck 1987; Carlson 1986; Tuma & Masur 1985).

5.1. CNS interoceptive transducers

Discussions of discriminative performance based on exteroceptive cues typically proceed without reference to their transducers (e.g., the photoreceptors of the retina, or the hair cells of the basilar membrane), because these mechanisms, well known for nearly a half century, have been taken for granted. By contrast, transduction mechanisms involved in brain-mediated interoceptive discriminations were largely unknown until recently, and had been the subject of a good deal of speculation. Advances over the past two decades in neurochemical receptor assays and in behavioral and neuropharmacology research methods have begun to clarify some of the transduction mechanisms in such internal discriminations (e.g., Colpaert & Balster 1988), including those involved in familiar affective states. Since this literature is not as well known as the transduction literature on external discriminative performance, it is worth noting that research in this area has uncovered a number of intriguing findings.

For example, mu, kappa, and sigma opioid receptors of the central nervous system are each associated with distinct stimulus properties (cf. Woods et al. 1988); and the GABA receptor complex plays a critical role in anxiety (Gray 1982). Recent research with neuropeptide Y suggests that it acts on brain chemical receptors controlling aspects of hunger sensations, though these interoceptive stimuli are not isomorphic with those associated with hypoglycemia (Jewett et al. 1991; Schuh et al. 1991). Interneurons are not typically thought of as afferent transducers, but their responsiveness to chemical stimulation appears to be not unlike those in taste or olfaction; the difference is that the chemical compounds of the former are carried to their source of effect by the circulatory system. This may provide yet another way of illustrating Natsoulas's (1983; 1985) point that certain internal states ("sensations") may occur without stimulation being *carried to* the CNS by peripheral channels.

5.2. Establishing exteroceptive and interoceptive discriminations

Skinner (1945; 1957) argued that the three-term contingency used to explain exteroceptive stimulus control and interanimal communicative exchanges thereof (namely, exteroceptive discriminative stimulus/verbal response/conditioned reinforcer), notated $S^D \rightarrow R_v \rightarrow S^r$, could be extended to the functional control of behavior by interoceptive stimulation. This extrapolation, however, exchanges S^D = *public* exteroceptive stimulus events with S^D = *private* interoceptive stimulus events. The same paradigm used to explain discriminations based on (public) exteroceptive stimuli, along with properties of discrimination/generalization, is thereby extended to (private) interoceptive stimulation. A problem arises, however, regarding the way in which interoceptive stimulus control is learned, compared to exteroceptive stimulus control.

Exteroceptive stimulus control is relatively easy to explain; discriminations of public objects (e.g., cats and dogs) can be taught to a child by reinforcing appropriate verbal responses based on conventional correspondences (e.g., responding "it's a cat" when a cat enters the room and reinforcing a child for doing so). Extinguishing inappropriate responses further facilitates learning the discriminations. Thus, if parents and teachers implement the following four contingencies:

$$S^{D(\text{Dog})} \rightarrow R_v^{(\text{Dog})} \rightarrow S^{r(\text{"Yes"})} \text{ and } S^{D(\text{Cat})} \rightarrow R_v^{(\text{Cat})} \rightarrow S^{r(\text{"Yes"})}$$

versus

$$S^{D(\text{Dog})} \rightarrow R_v^{(\text{Cat})} \rightarrow S^{r(\text{"No"})} \text{ and } S^{D(\text{Dog})} \rightarrow R_v^{(\text{Cat})} \rightarrow S^{r(\text{"No"})}$$

most children will readily learn to discriminate these two classes of animals. How children learn to discriminate and report on distinct private events (affective states like anxiety versus excitement versus joy – private events emanating from interoceptive stimulation) requires a more complex explanation, however. Given that parents and teachers never have direct access to these events, how can they teach children to discriminate them?

Skinner hypothesized that people (teachers) infer private stimulation based on collateral behavior and attendant contextual factors. For example, the presentation or removal of punishers and reinforcers provides cues for inferring how people feel. Following a positive reinforcer (e.g., when a person receives praise or an expression of love), we infer feelings of satisfaction or joy on the part of the recipient. On the other hand, removal of powerful positive reinforcers (e.g., death of a loved one, being dismissed from a valued job) is associated with feelings of depression or discouragement. Anxiety and fear, moreover, are related to the presentation of conditioned and unconditioned aversive stimuli, respectively, whereas the removal of these negative reinforcers is associated with experiencing relief. Thus, observing what punishers and reinforcers are presented or removed yields useful cues for inferring an individual's emotional state. In addition to these cues, people often exhibit characteristic responses associated with attendant emotional states (e.g., laughing when happy, crying when sad). Darwin (1872) discussed these cues in detail, often observing cross-cultural consistency between specific facial expressions and inferences of distinct emotional states made by observers.

According to this line of reasoning, people infer and differentially reinforce verbal reports that discriminate between contrasting forms of internal stimulation by observing the events responsible for generating the states directly or by observing the behavioral correlates of such states, or both. Contextual cues (e.g., the social setting: academic seminars, cocktail parties, family gatherings) can also contribute to accurate inferential assessments of private states and are important correlates of relevant changes in internal conditions.

Although verbal reports of private events involve discriminations of real physical events generated by interoceptive stimulation (or possibly direct interneuronal activation) as well as observing one's own behavior, the accuracy of such reports must be interpreted with caution. People who differentially reinforce verbal responses in the presence of public cues (correlated with distinct internal happenings) never have direct access to the attendant private states or their intensity. People must rely on fallible public exteroceptive stimuli associated with private events and they must differentially reinforce correspondences between such events and appropriate verbal responses (the R_v's in Skinner's three-term contingency). This is far removed from the precision found in teaching exteroceptive discriminations (like our earlier example of discriminating cats and dogs). Although collateral behaviors and contextual factors may be correlated with distinct forms of private stimulation, their association with such stimulation is often less than consistent.

If, however, we could somehow achieve reliable access to private stimulation (i.e., technically, if we could gain access to the functional relation $S^D \rightarrow R_v$, private event → verbal response), as we believe we can, and differentially reinforce it, as in the establishment of an exteroceptive discrimination, there is no reason to suppose that interoceptive stimuli would be incapable of generating interoceptive discriminations as precise as exteroceptive stimuli are in establishing exteroceptive discriminations (Overton 1971). Our next section provides empirical support for this position.

6. Experimental demonstrations of interoceptive stimulus control

Just as the classical (Pavlov 1927) and instrumental (Thorndike 1911) conditioning paradigms lead organisms to successful discrimination of exteroceptive stimuli (Catania 1992; Honig & Staddon 1977; Mackintosh 1974), they can lead to the same capability when interoceptive stimuli are used. A substantial literature has demonstrated how private stimulus events can control the behavior of laboratory animals. Bykov and Ivanova (cf. Bykov 1928), in the first study of this kind, revealed that interoceptors responded to classical conditioning in the same manner as exteroceptors. In this pioneering work, Bykov and Ivanova used infusions of saline in the stomach and diuresis (or urine formation) as the unconditional stimulus (US) and unconditional responses (UR), respectively. They demonstrated, through Pavlovian pairings, that the diuretic response could be brought under the control of irrigation injections of saline solution injected into the stomach. Saline irrigation injections acquired the role of a conditional stimulus (CS). Thus, autonomic behavior not under direct instrumental control can be classically conditioned [see also Turkkan: "Classical Conditioning: The new Hegemony" *BBS* 12(1) 1989 and Engel: "An Essay on the Circulation as Behavior" *BBS* 9(2) 1986]. Several subsequent reports revealed that instrumental responses could also be controlled by interoceptive stimuli (Hull 1933; Kendler 1946; Leeper 1935). Amsel (1949), for example, using a T-maze, conditioned rats to escape shock by going to one arm of the maze when they were hungry and the other when thirsty. Thus, the differential stimuli associated with food versus water deprivation acquired discriminative stimulus properties for differential responding that was negatively reinforced.

Laboratory animals can also be trained to differentiate between the internal state produced by a behaviorally active drug and that associated with a vehicle (usually saline) injection or placebo (Schuster & Brady 1964; see Thompson & Pickens 1971 and Colpaert & Balster 1988 for summaries). In these methods, a food-deprived animal is injected with a training drug (e.g., morphine) and given the opportunity to respond by pressing one of two levers or pecking one of two keys, depending on the species, and this leads to food reinforcement. The appropriate response following drug administration is the drug cue lever or key; responses to the other alternative (when in a drug state) produce no reinforcer. On alternate days, however, the animal receives a vehicle injection. On those days, the opposite response is defined as correct: presses on the drug lever go unreinforced, whereas presses on the vehicle cue lever are reinforced. With this procedure, animals rapidly learn to respond only to the drug cue lever on days when the training drug has been administered and to the alternative lever on days when the vehicle has been administered. Differential responding with this procedure has been demonstrated using a wide array of behaviorally active drugs (Colpaert 1978; Goldberg & Stolerman 1986; Griffiths et al. 1985; Holtzman 1982; Overton 1971). These agents also engender stimulus generalization gradients similar to those of exteroceptive stimuli as a function of modifications of their chemical composition. So unfamiliar or novel drugs can be "classified" by laboratory animals provided they have learned to discriminate a compound with similar effects. Moreover, drugs classified by animals as members of the same class (through interoceptive stimulus generalization studies) generate similar verbal responses on conventional mood questionnaires designed to assess distinct emotional states in humans.

6.1. Discriminative stimulus properties of drugs

Many of the psychophysical properties of exteroceptive stimuli have been replicated using drugs as interoceptive stimuli (Colpaert & Balster 1988). Across a variety of species (pigeon, rat, rhesus monkey, squirrel monkey, and primates), specific agents in a given pharmacological class, for example, amphetamine and cocaine ("stimulant"), alcohol and pentobarbital ("depressant"), and morphine and heroin ("opiate analgesic"), generalize to one another in the two-choice (drug versus saline) discrimination tasks described earlier (Goldberg & Stolerman 1986; Griffiths et al. 1985; Seiden & Balster 1985; Thompson & Unna 1977). Drugs generalizing from one to another in nonhuman laboratory studies typically create similar subjective effects in humans (see below). Moreover, the drugs with the greatest reinforcing efficacy in laboratory animals generate, in humans, the highest subjective ratings of "euphoria" or "liking." Thus, the aptness of pharmacological agents for our animal model is confirmed by a variety of cross-connections bridging human and nonhuman behavioral pharmacology.

In human studies, for example, subjective effects of drugs have been assessed via verbal reports in response to standardized questionnaires (Beecher 1959; Schuster & Johanson 1988). The content of these inventories is very similar (often identical) to that of conventional mood inventories designed to assess common affective or emotional states (cf. McNair et al. 1971; Nowlis 1953; 1970; Nowlis & Nowlis 1956; Tellegen 1985; Watson & Pennebaker 1989; Zevon & Tellegen 1982; Zuckerman & Lubin 1965). In these drug studies, subjects are asked to report on their mood or the likelihood of certain behaviors in ways that can be objectively scored (such as, true/false): "I feel like going for a walk" or "My stomach feels funny." Different subsets of items that correlate with one another (for internal-consistency reliability) *and* with common feeling or mood states (for external validity) are clustered and allowed to function as scales. These scales are then used to assess the subjective strength of various classes of behaviors and mood while subjects are experiencing influences of different drugs (Fischman 1977; Fischman et al. 1976; Griffiths & Balster 1979; Johanson & Uhlenhuth 1980a; 1980b; 1981; 1982; Martin et al. 1971; Schuster & Johanson 1988; Uhlenhuth et al. 1981).

One of the more widely used instruments of this type is the Addiction Research Center Inventory (ARCI) (Hill et al. 1963). This inventory consists of 550 (true/false) items combined into seven scales that measure reports of mood fluctuations and internal conditions associated with contrasting drug states. Drug states that can be assessed with this instrument appear in quotes (which represent prototypes of the drug class measured by the scale) and are followed by (true/false) item exemplars: "morphine" (I have a pleasant feeling in my stomach; my nose itches), "amphetamine" (My thoughts come more easily than usual; I feel as if I would be more popular with people today); "alcohol" (I feel like joking with someone; my appetite is increased).

Verbal responses on the ARCI vary as a function of common drug states (and familiar emotional states). Drugs with which a person has had no experience but that share pharmacological properties with familiar drugs generate similar patterns of verbal responses. Thus, administering barbiturates to human subjects generates patterns of verbal responses on the ARCI similar to those of benzodiazepines (Griffiths et al. 1985); cocaine generates a response pattern comparable to that of amphetamine (Schuster et al. 1981). Results like these led Schuster et al. (1981) to the following generalization: "it is possible to determine whether an unknown drug belongs to the opiate, psychomotor stimulant, sedative-hypnotic, or hallucinogenic drug class on the bases of its subjective effects" (p. 116). They concluded that "[T]he drug classes based upon discriminative effects in animals and upon subjective effects in humans are in striking concordance" (p. 121).

Just as the precision offered by acoustical and optical engineers for controlling exteroceptive stimuli has enhanced our understanding of corresponding exteroceptive systems, the precision offered by pharmacology in controlling interoceptive stimuli may help to sort out individual and species differences in the nature of interoceptive systems and to build an animal model of interpersonal communication based on private stimulation. Indeed, it is generally agreed that some pharmacological stimuli generate in laboratory animals interoceptive states that share components with affective states in humans; there is also evidence that comparable subcortical structures are involved in the mediation of these phylogenetically shared states (Gray 1982; Schuster & Johanson 1988; Tuma & Maser 1985).

Given that certain drugs are capable of controlling one class of private events (feelings) with a high degree of reliability, they seem ideally suited for the present experimental study of interanimal communication. Through behavioral- and neuropharmacology, compounds have been identified that selectively bind to specific interoceptors and may be useful in resolving the problem of the inaccessibility of private events (and inaccuracies regarding verbal reports of such events). Behavioral pharmacology offers a technology for establishing interoceptive discriminations that serve as a stimulus for communicative exchanges in which one animal reports to another animal how it feels. We now have a way to gain precise access to the $S^D \rightarrow R_v$ (private event → verbal response) relation.

7. An animal model of the interpersonal communication of interoceptive ("private") states

We now return to an explication of the model illustrated in Figure 1. Our idea was to train pigeons to discriminate three contrasting states in their internal milieu (Lubinski & Thompson 1987). The experimental apparatus was a modified version of the one used by Lubinski and MacCorquodale (1984) (see Fig. 5). The experimental chambers were preserved, but the colored lights and letters corresponding to specific colors were replaced by symbols representing specific drugs and drug classes. The drug class symbols corresponded to "stimulant," "depressant," and "no drug," while symbolic names for specific drugs corresponded to "cocaine," "pentobarbital," and "saline," respectively. (As in all the preceding pigeon models, these specific response-key names were chosen for clarity in experimental exposition rather than for imparting symbolic meaning.) The experiment involved two groups of pigeons: drug-cue birds (three) and decoders (two).

The individual performances of the decoders were acquired rather quickly: pecking the "How do you feel?" key followed by the "Thank you" key and then matching a specific-drug letter to a drug-class letter (they learned this sequence at greater than 90% accuracy over the course of approximately three months). The drug-cue birds were required to learn a complex interoceptive discrimination. The subjects' performance was established and maintained under only one drive state, although in the literature at the time there was one example of a three-key interoceptive discrimination using pigeons (France & Woods 1985). We wanted to impart a three-key interoceptive discrimination across *two* drive states, thirst *and* hunger, hence, 2(drive states) × 3(drug states) = 6 conditions.

The training procedure for the drug-cue birds was the following: While deprived of either food or water, drug-cue birds received an intramuscular injection of cocaine (3.0 mg/kg), pentobarbital (8.0 mg/kg), or normal saline and were then placed in their darkened experimental chamber. After a 20-minute interval (to allow the drug to become absorbed and distributed), the lights were illuminate and, simultaneously, the three response keys in the drug-cue birds' chamber were transilluminated (see Fig. 5). Pecks matching the birds' interoceptive state (viz., saline injection = "no drug," pentobarbitol = "depressant," and cocaine = "stimulant") were followed by the presentation of the flashing blue light (indicating that food and water pecks would be reinforced with corresponding reinforcers), just as correct exteroceptive matching behavior was reinforced by Lubinski and MacCorquodale (1984). If the drug-cue birds made an incorrect response, a mild punisher ensued: their lights were dimmed for 4 sec and the trial would start over. This interoceptive discrimination took approximately 10 months to establish (with training sessions conducted six days per week). Nonetheless, 90% accuracy was eventually achieved across all six conditions.

Finally, the experimental contingencies for the decoders and drug-cue birds were programmed to be interdependent, as shown by the interlocking paradigm in Figure 6. The interanimal exchange was synthesized to resemble all relevant features of the Lubinski and MacCorquodale (1984) study, but instead of matching letters to colors, the drug-cue birds matched arbitrary visual symbols to pharmacologically manipulated interoceptive states. Decoders were food-deprived throughout the experiment, whereas drug-cue birds were (initially) de-

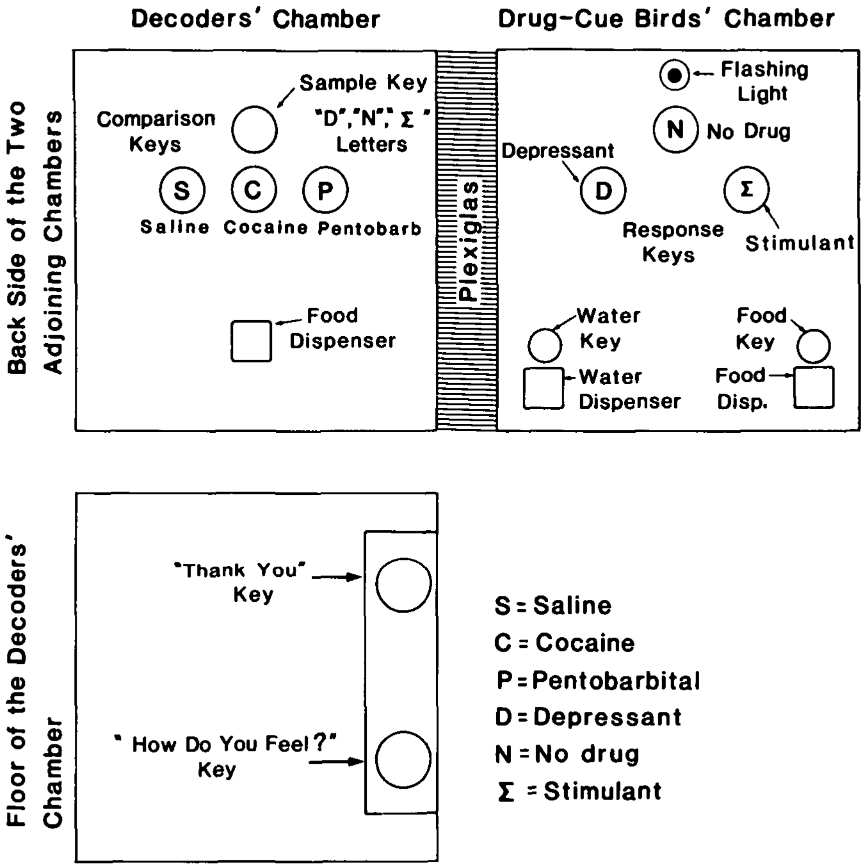

Figure 5. The details of the response operandi, stimulus configurations, and reward mechanisms structuring the experimental synthesis in Figure 1 (adapted from Lubinski & Thompson 1987).

prived of either food or water. The drug-cue birds received an injection of one of the aforementioned agents and were then placed in their darkened experimental chamber while, at the same time, a decoder was placed in the adjacent chamber. Following the 20-minute pretreatment interval, the overhead lights in both chambers were illuminated along with the decoder's "How do you feel?" key. The ensuing performance followed.

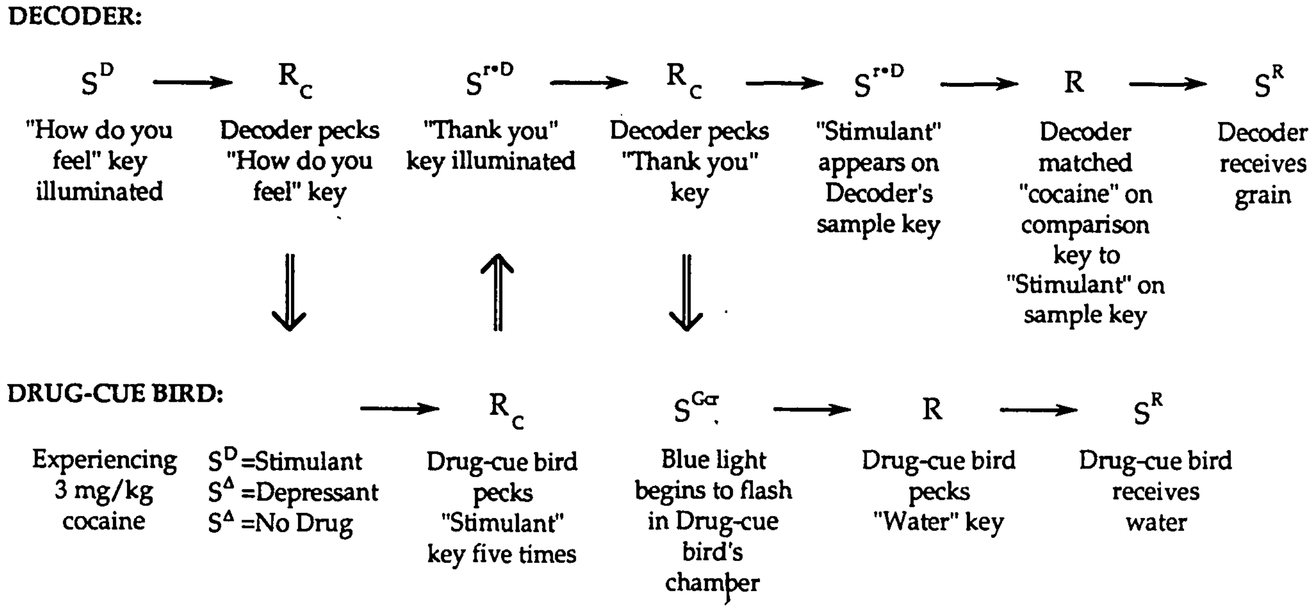

Figure 6. Interlocking communicative paradigm illustrates the technical features of the stimulus events exchanged between subjects. The notational system follows: S^D = discriminative stimulus; S^Δ = S - delta (i.e., nonavailability of reinforcement); $S^{r \cdot D}$ = a conditioned reinforcer, which also functions as a discriminative stimulus; R_c = a communicative response; S^R = unconditioned reinforcer; R = response; S^{Gcr} = generalized conditioned reinforcer.

7.1. The synthesis

7.1.1. Phase 1. The synthesized communicative sequence began when the decoder pecked its "How do you feel?" key. This response illuminated the drug-class names, one on each of the drug-cue bird's three response keys (i.e., "Depressant," "No drug," and "Stimulant"). The drug-cue bird then pecked the response key corresponding to the drug it had received. This response in turn illuminated the "Thank you" key in the decoder's chamber. When the decoder pecked the "Thank you" key, two events ensued concurrently. The drug-cue bird's blue light began to flash and the drug class name previously pecked by the drug-cue bird appeared on the decoder's sample key. From this point on, the remaining response sequences of the two birds were independent of each other. With the blue light flashing, the drug-cue bird could receive food or water by pecking the appropriate key, whereas the decoder could receive food by correctly matching the specific drug (among its comparison response keys) to the drug class (on its sample key). Overall, subjects performed this interaction with a high degree of accuracy. For all groups of birds, correct correspondence on the first trial of each experimental session (i.e., a correct discrimination of both birds) across all six states, 2(deprivation) × 3(drug), ranged from 70% to 100%.

At first, the birds performed in a mechanical fashion: they appeared at times to be uninterested in each other's behavior. Over the course of a number of days, however, each bird's behavior came more and more under the control of its counterpart. After completing a component of the interlocking sequence, each bird gradually began to orient toward the stimulus change in the adjacent chamber, which was in close proximity to the other bird. After consuming food or water, for example, the drug-cue bird approached the area near the decoder's "How do you feel?" key. If the decoders were at all sluggish in pecking the key when this light became illuminated, the drug-cue bird would peck rapidly on the Plexiglas directly above the key while orienting toward the decoder. At this point, the decoders typically would approach and peck the "How do you feel?" key and then move toward the area by the "Thank you," key, standing in position until the drug-cue bird finished matching its state to the associated letters. If the drug-cue birds were at all sluggish in performing the interoceptive discrimination, the decoders would peck the Plexiglas and emit species-characteristic aggressive displays directed toward the drug-cue bird.

We have not found other examples in the literature of interanimal exchange of discriminative stimuli based on the interoceptive state of one of the participants. The drug-cue birds' performances in this interanimal exchange involved discriminating interoceptive stimuli. Their key-pecking performance was controlled by events inside their skin, which did not covary with the particulars of their state of food or water deprivation or aversive stimulation, nor was it reinforced with a characteristic unconditioned reinforcer (e.g., only food or only water) corresponding to a particular state of deprivation or aversive stimulation.

7.1.2. Phase 2. A second objective was to determine whether the discriminative performances established in Phase 1 would generalize to similar states (i.e., private events induced by pharmacological agents that subjects had not previously experienced but were of the same class as those used during their training). This phase of the synthesis was predicated on the idea that the reason humans can describe novel stimulation (e.g., unique "mixing of emotions") is because these states often share components with familiar states they have learned to report. Using chlordiazepoxide (Librium) and d-amphetamine (Dexedrine) as novel agents for the generalization probe, drug-cue birds reliably generalized from cocaine by responding on the stimulant key after amphetamine administration; they generalized from pentobarbital by responding on the depressant key after chlordiazepoxide administration. One can attribute meaning to these behaviors by suggesting that what the pigeons were reporting was that the interoceptive cues from the amphetamine were more similar to cocaine than to pentobarbital or saline and, conversely, that the cues produced by chlordiazepoxide were more similar to pentobarbital than to cocaine or saline. (The dyadic accuracy ranged from 84% to 92% correct correspondence during this phase.) Technically, what we observed in this phase of the experiment was stimulus generalization. It is through a similar process, according to Skinner (1945), that humans have the capacity to verbally describe novel exteroceptive *and* interoceptive stimulation.

7.1.3. Phase 3. In *Mind, self and society* (1934), George H. Mead wrote: "It is quite impossible to assume that animals do undertake to express their emotions. They certainly do not undertake to express them for the benefit of other animals" (p. 16). To ascertain whether the type of communicative behavior displayed by the drug-cue birds in Phase 2 would continue when the animals were satiated with food and water and received no deprivation-relevant reinforcement (with only the flashing light contingent on correct responding), the following experimental probe was conducted.

The same procedure described for Phase 2 was maintained; however, on several intermittent days, the drug-cue birds were placed in their experimental chambers after receiving 24-hour free access to both food and water but subsequent to receiving an injection of one of the pharmacological agents. Their food and water keys were also inoperative during these sessions. When they were food- and water-satiated and had no consumable rewards but only the flashing light contingent on correct responding, the drug-cue birds continued responding correctly to decoder's requests by accurately reporting on their internal states 83 – 100% of the time. The emergent communicative behavior reported in the earlier phase of this experiment also appeared in this final phase and seemed to maintain the integrity of the exchange.

7.2. Limitations of our model

Linguists and philosophers single out the *intent* of the communicative exchange as an essential element in human communication. It is in this regard that there are important differences between the pigeons' communicative exchanges in Lubinski and Thompson's (1987) study and reports from one person to another about their feelings. When people tell one another that they are "feeling depressed" or "feeling anxious," we have good reason to believe that the listener's expression of concern and interest motivates the person to disclose feelings. Much of the interview process in psychotherapy, for example, is based on an assumption that clients intend to reveal their feelings, and the listener's interest in those reports maintains the exchange. We have no reason to believe that the pigeons' discriminative responding is primarily controlled by its impact on the other pigeon's behavior. Savage-Rumbaugh (1986) and Terrace (1985), for example, have asserted that the exchanges reported by Epstein et al. (1980), which did not involve interoceptive stimuli, could have proceeded purely mechanically, without the second bird being present. Yet this was clearly not the case in the Lubinski and MacCorquodale (1984) study, in which it was found that when the other bird was not present and motivation for communicating was low, reporting the color of the hidden light ceased. Subsequently, however, when the paired bird was reintroduced into the situation, the bird doing the reporting resumed the interanimal interaction. Critical components of the interaction between the pigeons appeared to come under the control of one or the other's behavior; these were not programmed by the experimenter, yet they emerged, and were essential for maintaining the exchange. The same emergent behaviors were also observed by Lubinski and Thompson (1987).

The controlling relation, nonetheless, remains different for the pigeon and the human under most circumstances. For the human, the listener presumably provides social reinforcement, which is contingent on reports of internal stimulation. For the pigeon, the "listener" behaves aggressively in order to maintain the communicative behavior. Specifically, it flaps its wings and pecks against the transparent partition separating the two compartments until the satiated bird reports the color of the hidden light. Whereas this is very unlike the typical human exchange based on private stimulation, it resembles exchanges seen in certain human situations. Examples would come from confrontational forms of psychological therapy, involving clients who are unwilling or unable to report feelings. Even though such clients may have good reason to feel angry, they are unable to report that feeling. In this situation, the therapist, or possibly other members of a therapy group, gesticulate, raise their voices, or in other ways present an aversive setting to client-listeners until they finally express their feelings. Functionally, this is the same as the pigeons acting aggressively until its conspecific counterpart responds. Just as pigeons may "have no interest in reporting feelings" to fellow pigeons, humans may also either not recognize their feelings or be reluctant to report them. A difference remains, but the difference is perhaps less than one would think.

8. Implications and human parallels

The pigeon experimental situation shares some features of the interactions between parents and children with autism or other developmental disabilities (Keogh et al. 1987; Reichle et al. 1986). Children with autism are often minimally verbal and unable to report their private experiences to others. Indeed, a common diagnostic feature of autism is the failure to use gestures or other symbolic communicative devices to indicate needs or wants. Thus, to create the desired behavior or outcome in their parents, children with autism may pull or push their parents to the refrigerator and then begin to scream or to hit them until they open the door and retrieve the orange juice. Yet, as with pigeons, extensive training can often be used to teach children with autism to discriminate familiar objects correctly (e.g., food items, articles of clothing, household objects, places). Children with autism have been taught to use words or signs to report on states of hunger or emotional arousal (e.g., "angry") but they seldom do so spontaneously. Their performance often retains a mechanical quality that requires, at least intermittently, an extrinsic reinforcer to maintain symbolic responding. Communicating their internal feelings to another person does not seem to be a significant factor for maintaining the exchange (apart from the primary reinforcer provided by the listener). The same seems to hold for the pigeon. A final parallel between the present model and children with such disabilities is that the latter seldom develop the capacity to report refined descriptions of the components of their internal experiences (e.g., novel mixings of emotions) whereas most well-socialized adults readily generalize their reporting to novel internal stimulations. The interoceptive discriminative repertoires of some humans, like those of our pigeons, tend to be restricted to broad categorizations.

Individual differences in the ability to report on interoceptive stimulation may emanate from endogenous (biological) as well as exogenous (experiential-learning) sources of influences. For example, both human and laboratory animal studies indicate that specific neurochemical receptors are associated with familiar affective states (e.g., the GABA/benzodiazepine receptor and anxiety). Thus, differential numbers of such receptors, selective affinity, differences in intrinsic activity, or differential release of endogenous ligands binding to such receptors could all account for individual differences in the tendency to report feelings. These would modulate the intensity of inner feelings and thereby the likelihood of their being reported. These sources of variability are akin to how differences in retinal cell function relate to visual perception.

There are also basic individual differences in conditionability. People with personality attributes characterized as "personality disorder" or those who score very high on the Psychopathic Deviate (4) scale of the Minnesota Multiphasic Personality Inventory seldom report feeling guilty or anxious. In 1957, Lykken demonstrated that people with such characteristics are not well suited to developing classically conditioned responses to neutral stimuli (e.g., tone), which precede unconditioned aversive stimuli (e.g., skin shock). Lykken and others (Lykken 1968; 1982; 1984; Tellegen et al. 1988) have argued that these differences stem from individual differences in genetic constitution and may hence help explain (1) such individuals' apparent inability to learn from repeated aversive stimulation or punishment as well as (2) certain individual differences in the readiness to "introspect." Some people have internal economies that are simply more emotionally active than others (Watson & Pennebaker 1989), and, as such, are predisposed to experience intense forms of anxiety in *relatively moderate* anxiety-provoking situations.

There are other likely sources of variability too. Children with mental retardation often find learning visual or auditory discriminations very difficult, especially those involving multiple stimulus elements, (Zeaman & House 1979). Even if the child's retinal or cochlear functions are within the normal range, a child with an IQ of 50 will often take far longer to learn a visual or auditory discrimination than an age mate of IQ = 100 (Baumeister 1967). Some children with moderate to severe intellectual disabilities may never learn conditional discriminations involving several elements (e.g., when the red light is on, a vertical line is correct; but when the green light is on, a horizontal line is correct). Although we are not aware that it has ever been tested, it seems likely that similar difficulties in the learning of discriminations based on interoceptive stimuli would confront the individual with a significant intellectual deficit, especially given the lack of consistency between private stimulation and the associated public covariates present throughout the learning process. Conditional discriminations involving interoceptive cues should be even more difficult to learn (e.g., "When I have just won a prize and I feel this way, it's called 'excited,' but when I have been knocked down by another child and I feel this way, it's called 'angry'").

People with autism tend to respond selectively to one element in a complex stimulus array in which all elements are associated with reinforcement. This phenomenon has been called "stimulus overselectivity" (Schreibman & Lovaas 1973). Children with autism often attend to an element of the stimulus situation that has been irregularly correlated with correct responding but may actually be irrelevant (e.g., the tone of a teacher's voice or the angle of a teacher's head during the presentation of an educational task). Presented with several stimuli, one of which is interoceptive and the rest exteroceptive, the child with autism may selectively attend to the interoceptive stimulus. The parent, teacher, or therapist would have no way of knowing that the child's behavior was under the control of the interoceptive stimulus, because children with autism have extremely limited verbal ability to report what they are attending to. A child with autism may persist in responding on the basis of an interoceptive discriminative stimulus, and could present an unsolvable puzzle to an adult searching for the external cue to which the child is responding.

When one teaches children to respond discriminatively to interoceptive stimuli and then to report on such stimulation one assumes that the people in the children's environment attend to cues usually correlated with affective states and that they then use those cues to teach children the appropriate label for their feeling. The child cries when hurt, has a gleeful expression when receiving a gift, or wears an unhappy expression when an anticipated enjoyable event is postponed. Some parents may be less effective than others in attending to such cues or in taking the time to teach children to selectively respond to their internal states in the presence of associated cues. The child with the disappointed expression upon learning that a trip to the circus has been delayed would find it difficult to learn the name of the feeling being experienced unless an adult takes advantage of the opportunity to teach it. Parents who are totally preoccupied with other matters (e.g., the chemically dependent person seeking drugs) or who devote much of the time responding to their own private stimulation (e.g., the person with schizophrenia who is embroiled in ruminations and delusional thinking) would probably be ineffective in teaching children to discriminate and accurately report their own private stimulation. In other instances, the children's reports may be punished by a parent who rejects them (e.g., the parent who believes that feeling discouraged is a sign of weakness or that feeling angry when frustrated is unacceptable). Under such circumstances the ability to accurately report feelings would be attenuated. This could lead to difficulties for which a counselor may be sought.

Finally, just as people have moderated their public experiences with the aid of external prostheses (e.g., eyeglasses and hearing aids), they have used pharmacological prostheses to modulate their private experiences. Controlled interoceptive prostheses directed at attenuating, increasing, or sharpening discriminative control of interoceptive stimuli have emerged over the past 35 years with the advent of modern psychiatric drugs. Neuroleptic medications have been used to control thought disorders in schizophrenics, and benzodiazepines have been used to reduce preaversive interoceptive stimuli among people with serious anxiety problems. Patients with panic attack disorder respond discriminatively to their own interoceptive anxiety stimuli, which are attentuated by administering tricyclic antidepressant medications. People with obsessive compulsive disorder respond discriminatively, not only to the contaminants that lead them to wash their hands, but also to their own feelings of anxiety when they fail to do so. Since washing their hands only reduces the intensity of the interoceptive anxiety stimuli for a short period, the avoidance behavior resumes at frequent intervals until the source of the interoceptive stimulation is attenuated pharmacologically. Individual biological differences contributing to such internal events are the foundation upon which variations in private stimulation are built. So even when parents, teachers, and therapists are consistent in their attempts to impart interoceptive discriminations, individual differences in underlying mechanisms contribute to individual differences in private states and their reports.

Perhaps Catania (1990) was well justified in hypothesizing that language functions primarily as a means of changing others' behavior rather than for communicating information about internal feelings. In the nonhuman world, overt behavioral manifestations may be sufficiently correlated with internal cues experienced by the organism in a state of emotional arousal to render supplementary communicative information based on these private stimuli superfluous. This does not mean that chimpanzees (or other organisms) cannot learn such discriminative repertoires, only that there is typically little adaptive utility in their doing so. As Pinker and Bloom (1990) noted in *BBS*, private affective stimuli are not well suited as bases for linguistic (grammatical) communication: what T. S. Eliot (1943, p. 16) referred to as "the general mess of imprecision of feeling." The human tendency to conceal emotional stimulation for social advantage makes it understandable that such language-based repertoires would be more common in humans than in other organisms. In the overt behavior of adult humans, the lack of consistent correlates of affective stimuli means that some other correlated behavior (language) is needed for this useful information. Individual differences in biological substrates (Tellegen et al. 1988) and learning histories (Skinner 1945) give some people an adaptive advantage in this respect over others.

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NOTES

1. Kordig (1978) has argued that the distinction between discovery and justification is often ambiguous and that acquiring scientific knowledge involves three tiers of credibility: initial thinking, plausibility, and acceptability. Initial thinking occurs prior to plausibility and acceptance (i.e., in the context of discovery). Plausibility and acceptance require unassailable logic and good evidence; both concepts are analyzed in the context of justification. Plausibility is necessary but not sufficient for achieving acceptability. Concepts achieving the status of the latter must satisfy more stringent criteria, but the requirements for both levels of justification are of the same logical character. Plausibility proofs provide good reasons for attaching scientific merit to a posited entity or a particular interpretation of the data; when adequately conducted, they achieve the first level of Kordig's two-stage conceptualization of justification.

As a case in point, the "hot soup" theory of organic evolution achieved plausibility in the 1950s (Miller 1953; Urey 1952). Using geological information about the early inorganic properties of earth and conjectures about electrical storm activity in the earth's prebiotic atmosphere, Miller and Urey simulated the conditions hypothesized to give rise to early life forms (see Miller & Orgel 1973) and produced amino and hydroxy acids, which are important antecedents of life. Although this did not confirm their hypothesis, it enhanced its plausibility.

2. A film of this performance is available to educators: "Cognition, creativity, and behavior" (1982). Producers, N. Baxley. Champaign, IL: Research Press.

3. Mands and tacts are neologisms used to describe two classes of verbal behavior in Skinner's (1957) analysis. Mands are verbal operants controlled by a state of deprivation of aversive stimulation and they specify their reinforcer. (They are the most primitive form of verbal behavior in Skinner's analysis.) A tact is defined as "a verbal operant in which a response of a given form is evoked (or at least strengthened) by a particular object or event or property of an object or event" (Skinner 1957, pp. 81–82). These terms were chosen because, technically, the "mander" was trained to emit *mands*, whereas the "tacter" was trained to emit *tacts* (for details see Lubinski & MacCorquodale 1984).

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Perhaps Sisyphus is the relevant model for animal-language researchers

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The central point of the Lubinski & Thompson (L&T) demonstration and argument is that any one of three internal states of a pigeon could serve as a discriminative stimulus controlling one of three corresponding responses in the pigeon. In their study it was also true that each of these responses created a distinctive discriminative stimulus controlling a corresponding response in another pigeon, a response that, if correct, presented stimuli that reinforced the relevant responses of each pigeon.

However, the demonstration that one animal's response can control another's, and reciprocally, is not new; that was programmed here only to parallel in function some portion of that human interaction we commonly call communication (or, sometimes, verbal behavior). The significant finding is that three of a pigeon's internal states could be given that function by straightforward discrimination-training techniques – no uniquely linguistic, uniquely human, or even uniquely early-lengthy history was required. Thus, the important implicit point is that this demonstration does parallel, and therefore straightforward conditional-discrimination-training procedures do analyze, some part of the human interaction we call the communication of private feelings.

If prior argument on this issue is predictive, however, the important psycholinguistic counterargument will be that this demonstration does not parallel *enough* of what we call communication to truly analyze or model it. An earlier *BBS* article on animal language by Premack (1983), together with its commentaries, modeled *that* kind of argument, and revealed it to be a quasi-Sisyphean game (the "Not Enough" game) that can be played forever. The "Not Enough" game is not quite one in which a demonstration, pushed to the top of the hill, attracts critical counterargument and immediately rolls back down, waiting to be pushed up again. Instead, with each demonstration of animal interactions that parallel part of what gets called communication in humans, some of us argue that the demonstration has been pushed far enough uphill to be at its conceptual top. Others then move the conceptual top to a far higher point on another hill; still others argue that conceptual tops are demonstrably a dime a dozen, and they want the empirical top, and in the organism in question. Meanwhile, each demonstration stays where it was left on its hill. A little later, another worker pushes an even better demonstration a little farther up this hill, or another one, only to reveal that the same three audiences still exist (and that some of us belong to more than one of them). At some point, researchers begin to see the three audiences as eternal, and the hills in question as more littered than challenging, and abandon them to students writing review papers.

The L&T demonstration, even if seen as admirably high on a relevant and not yet severely littered hill (as I do), is still open to a few questions:

(1) That the pigeon's responses were under the discriminative control of the pigeon's internal states is plausible but unproven. We know only that those responses were controlled by what substance was injected into the pigeon 20 or more minutes

earlier. The *sequelae* of those events are still called private events, in that they were not measured here. Is it possible that the control somehow lies *only* in some differential events surrounding the injections, rather than in their internal *sequelae*? Could there have been any differential events surrounding the three different kinds of injection? If so, this could be a subtly different demonstration.

(2) If the pigeons' responses are indeed controlled by their internal states, the parallel to how children get taught to label their private events is not only very good, but perhaps a little too good. When we teach children how to label what we suppose they must be feeling, we are rarely as accurately informed about the referent as the experimenters claim to be in this demonstration. Mainly, we seem to be teaching children some additional cultural labels for certain classes of their current or immediately prior situations; that we are teaching them to label their feelings is problematic (which may be why human communication on such topics is so poor). These pigeons may be better at their three bits of self-revelation than even veteran consumers of psychotherapy; is it possible that they are an unrealistically accurate model for this argument?

(3) We see clearly why and how animal-language researchers create contingencies to yield accurate reporting by pigeons of their presumed internal states; but is it likely that ordinary language-users create teaching contingencies for their children's reporting of internal states for the same reasons? What is the model, not for how we teach them that skill class, but for why we teach it to them?

Communication and internal states: What is their relationship?

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Common folks "have" emotions and talk to others; and sometimes they make "their" emotions the topic of such talk. The emotions seem to be "theirs," since they can be conceived of as private states (or events); and they can be topicalized, because we seem to be able to attribute or lend a conventionalized public form (such as a linguistic label or name) to some inner (and therefore nonpublic) state or event. This is the way much of our folk-talk and folk-thinking about emotions, the expression thereof, the role of language in these expressions, and communication in general are organized. However, as we have shown (Bamberg & Lindenberger 1984), such talk serves the purpose of communicating effectively and reaching mutual understanding.

If, however, someone wants to argue that this folk-model adequately represents reality, even if this reality remains limited to the relationship between emotion and communication, some of us feel extremely uneasy because we know, particularly from research outside behavioral psychology, that this relationship is more complex.

First, "having" an emotion is more than being interoceptively stimulated. Without the ability to interpret and integrate interoceptive and exteroceptive stimulations[1] into a conceptual system and a system of social actions, those stimulations remain isolated and meaningless. And human actions as well as their concepts are part of a sociocultural (interpretive) system that has historically evolved, and is in a process of flux (Wierzbicka 1992). Emotions in people's everyday lives are integral parts of a communicative system within which we use emotion talk to make sense of one another, and only as a consequence of this, also of ourselves.

Second, the ability to label a state or event that co-occurs with the time of the utterance forms one of the many facets of people's communicative competence. The (linguistically) adequate labeling of states as states (and of events as events), however, is part of a linguistic system – a system that has evolved out of discourse practices involving referral to states/events that do not co-occur with the time of the utterance, that is, as separate entities from the here and now. In addition, the communicative intent of referring to states or events (whether they take place concurrently or are bounded or unbounded events, taking place in "the past" or "the future") coexists with other communicative intentions. There is no reason to believe that developmentally (onto- as well as phylogenetically) the ability to refer to objects ("naming") could serve as a precursor to referring to states and events as a more complex sort of naming, which could in turn serve as precursors to syntax and ultimately to communicative intentions other than referring.[2]

In sum, Lubinski & Thompson (L&T) present a view of the relationship between emotions and communication that rests on a definition of communication as primarily "referential" and emotion as primarily "private"; both being linked by no other particular motive than to express or signal an emotion to someone else. Although I have no objections to using these assumptions to model particular occurrences of animal communication, I object to the assumption that this model can be used to represent "human communication" for three reasons.

First, L&T's model faces difficulties in incorporating the fact that humans often report "having" two emotions at the same time. Among the fascinating problems here (which we are currently investigating in different cultures/languages) are that children are willing at a relatively early age to report two simultaneous emotions such as "being happy" and "being sad" because of what are believed to be the same causing events; or having two simultaneous emotions of the same kind (though those usually are reported to vary in degree and are traced to different precipitating events). What is at stake in L&T's model is that their animals do not have to account for what emotions mean to them. This, however, is at the core of why and how we topicalize "our" emotions in everyday communicative events.

Second, L&T's assumption that children – by analogy with the animals they used in their experiments – acquire emotion talk through (parental) teaching of discriminative responding to interoceptive stimuli with an additional translation of these responses into accurate reports (sect. 8, para. 6) reveals L&T's naive model of developmental processes. Though most theories of emotional development in humans have started from the assumption that cognitive and cultural factors only elaborate and "interpret" the biological core of programs, this assumption has recently been challenged by a number of social constructivists and system theorists (cf. Averill 1980; Fogel et al. 1992; Gordon 1990), resulting in increasing endeavors to work toward a developmental theory that can integrate brain, body, and the sociocultural context within which all interact.

A third objection to L&T's analogies between animal and human communication involves the lack of discussion of the discourse/emotion interface. System theorists as well as social constructivists have examined the extremely relevant question of when (and where) emotion talk occurs in dialogue, and why people communicate (or better: topicalize) "their" emotions at all (Lutz 1990; White 1990). Such research concludes that emotion talk is an integral part of "the emotion" itself. It may even constitute the core component insofar as it reveals what "the emotion" means to the experiencer. It seems to me that L&T seriously underestimate the role of language and discourse in the constitution of such phenomena as private states or events for us humans. For these collective reasons, I see no future for any transference of L&T's animal model to the explication of human communicative practices.

NOTES

[1] Using the terms *interoceptive* and *exteroceptive stimulation* I would want to caution that the skin is by no means a boundary that keeps these two separated, as pointed out by Skinner himself (see Hineline 1992, p. 1284).

2. The idea to view general pragmatic intent as secondary to and derived from the pragmatics of referring, as suggested by Terrace (1985) and taken by L&T as an established fact, again seems to reflect folk-talk and folk-theorizing about language and language learning in our Western cultures. This, however, is in a process of slowly being replaced by more complex ways of conceptualizing this relationship.

The status of private events in behavior analysis

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There are two key questions about private events. I shall call them the privacy question and the causality question. Lubinski & Thompson (L&T) failed to address either of them directly, perhaps by design, but a full discussion can hardly avoid them.

The privacy question concerns the meaning of the word "private." There are two possible uses. In the first, often considered to be common sense, a private event can only be known to its possessor. It seems self-evident to some people, for example, that thoughts can only be known to the one who thinks them. According to this notion, private events are private *in principle,* can never be known to another, and therefore are qualitatively different from public events. The claim, made by some behaviorists, that private events are exactly like public events except in the size of the audience – private events always having an audience of one and public events having an audience greater than one – cannot remove the dichotomy. For example, how does one distinguish between a public event that happens to have an audience of one (i.e., occurs when the actor is alone) from a private event? If size of audience were the only criterion, then my yawning when I am alone would be a private event but would become a public event if my wife were there to see it. This would contradict the notion that private events are private in principle, because it is a practical matter whether my wife happens to be there or not. Thus, if private events are private in principle, there remains some unstated, unanalyzed other criterion.

The alternative use of "private" makes it a purely practical affair. In this view, there really *is* no difference between the privacy of a yawn when I am alone and the privacy of a thought or feeling. No private events are private in principle; thoughts and feelings can be known in principle, if only we take the trouble to observe them or invent apparatus to observe them. This idea depends upon an article of faith, the faith that with enough technical advance, even the subtlest thought or feeling in one person can be observed by another. One has to imagine, for example, hooking electrodes to a person's head, connecting the electrodes to a machine with a screen on it, and if the person thinks *I feel tired,* the words "I feel tired" appear on the screen along with a display of neural inputs showing whether this was a true statement of the person's feeling or not. This view at least has the advantage that it truly makes no distinction between private and public events, thereby leaving no mysteries. Its disadvantages are that it contradicts common sense by seeming to trivialize the word "private" and that it rests on an article of faith that cannot be disconfirmed.

The second big question about private events, the causality question, concerns their causal status with respect to behavior. In the commonsense view, it seems self-evident that thoughts cause behavior. Behaviorists usually deny this, maintaining that order of occurrence need not imply causality; if I think about walking and then walk, there is no necessity that the thought cause the action. Skinner often insisted that behavior cannot originate within the organism, not even in a private event. Instead, he maintained, behavior always originates in the environment, in the public domain. The validity of his assertions rests crucially on the word "originate." On the one hand, it conveys the importance of history, because origins of behavior are always in the past, never in the present. On the other hand, it may represent an attempt to avoid using the word "cause." If "originate" means nothing different from "cause," then Skinner's assertions would contradict his proposal that private events are of the same kind as public events.

Perhaps the most consistent policy for behaviorists would be never to use the word "cause." Emphasis would then go onto the trio of genetics, history, and present circumstances to explain behavior. This would favor functional explanations instead of mechanical ones. A difficulty arises, however, with the behavior analysts' substitute for immediate causality, the concept of "stimulus control," central to L&T's discussion. If stimulus control has anything to do with "originating" behavior, then stimulus control by private events suggests that private events, in some sense, *do* "originate" behavior. You cannot have it both ways, insisting that private events cannot originate behavior and at the same time insisting that private events are just like public events and can exert stimulus control over behavior just like public events.

We return inevitably to the first question: Just what distinguishes private events from public? Behavior analysts are caught on the horns of a dilemma. On the one hand, if the distinction is retained, it preserves the very dualism that radical behaviorists thought they had escaped. No mental causes seems to mean no mind-body problem. Does the mind-body problem merely surface again as the question of how a private event can exert stimulus control over public behavior? On the other hand, if the public-private distinction is dismissed, we run the risks of implausibility and inadequacy. Methodological behaviorism is attacked because it disregards the inner world of thoughts and feelings. Radical behaviorists claim they *do* treat thoughts and feelings – as private events. Has this ploy really worked?

The great strength of radical behaviorism is its avoidance of dualism. If it fails in that, it can hardly claim superiority over commonsense psychology. Even if one might wish it otherwise, the only way to preserve this superiority is to deny *in-principle* private events. The resultant faith in instrumentation and ingenuity might be compared with faith in determinism. If all events are in-principle public, however, how different is the position from methodological behaviorism?

The outside route to the inside story

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Lubinski & Thompson (L&T) have performed a valuable service by arguing and demonstrating that the study of communication of private events is feasible in nonhumans. Their provision of an empirical model that captures many of the features of such communication in humans illustrates the optimistic view that emanates from considering experience as a crucial determinant of behavior. That outlook may be contrasted with more nativist perspectives that ascribe communicative function largely to genetics. The former allows us to be hopeful that manipulable variables may be isolated and studied, and that such study offers the promise of being able to improve communication in those who have deficits in that domain. The latter suggests that one adopt a "that's the way things are" view, and that little can be done to improve the lot of those who suffer from impaired or deficient communicative skills. To adopt the nativist view categorically makes it unlikely that manipulable factors will be detected, whereas taking the position that experience is crucial makes it more likely that important influences will be discovered.

One avenue toward such discoveries is the development of useful models, and here L&T fare well. By taking knowledge concerning stimulus control of operant behavior, behavioral chaining, conditioned reinforcement, and drug-discrimination procedures, all of which have emerged via an experimental analysis of behavior, they have managed a potentially useful behavioral synthesis. The development of the model allows investigation of a variety of crucially important questions about the development and maintenance of behavior that communicates private information. For example, the model could be used to examine how accurate the original discrimination of private states must be to promote effective social interaction, or it could be used in a "closed economy," wherein all the subject's rewards are delivered in a live-in environment (cf. Hursh 1980), or myriad other fashions.

The model does raise interesting questions, however, both about its adequacy and about the interpretations on which it is based. In terms of its adequacy, the model appears to miss one of the key characteristics of learning to discriminate private events. As suggested by Skinner (1945) and outlined by L&T, one can consider some of the "training" of such discriminations in everyday life to be based on what might be called "guesswork" [see also special issue on work of B. F. Skinner, *BBS* 7(4) 1984]. That is, the "teachers" in the verbal community have access only to public events that are associated with the private event (e.g., grimacing, crying, and grabbing a foot after stubbing a toe), not the private event itself (i.e., the pain in the toe). In the model, the private event is guaranteed to be present via injection of an active dose of drug. Having certain knowledge of the presence of cues makes the discrimination training of the model virtually isomorphic with discrimination training with public stimuli. I wonder if L&T consider this a strength or a weakness of the model. If the latter, can they offer modifications of the synthesis that make it correspond more closely to the "real-world" case?

The fact that public accompaniments serve as the base for "teaching" people to describe their private events presents an interesting problem if one gives credence to the phenomenon of "overshadowing." Overshadowing occurs in the development of stimulus control if two stimuli are present when reinforcement occurs. Frequently, one of the stimuli gains more control than the other. In fact, sometimes one of the two stimuli is completely overshadowed and gains no control at all (Mackintosh 1974). One of the factors that can lead one stimulus to overshadow another in the development of stimulus control is what has been called "stimulus validity" (Wagner 1969). If two stimuli are correlated with reinforcement, the one that is correlated better will overshadow the one that is less well correlated. This is exactly the situation that is hypothesized by L&T (following Skinner) to exist in learning to discriminate private events. The public accompaniments are the best correlates of social reinforcement because they are the stimuli that serve as the basis for the delivery of reinforcement, yet the private stimuli gain control. The question is, why are the private stimuli not overshadowed by the public ones? Can the model help here? Is this issue related to the view that when people label their own emotions they, too, are responding primarily to publicly available conditions (Schacter & Singer 1962)?

A couple of other minor points may deserve a reply from L&T. First, when introducing the possibility of studying animal communication, they state that learned communicative behavior in animals "is not language." This pronouncement is surprising, given that it follows a nice discussion of how communication can be thought of as illustrating a continuum. I am surprised that L&T adopt such an essentialist stance (cf. Palmer & Donahoe 1992). Second, there is an apparent contradiction near the end of the paper. L&T speculate that one of the factors that has led to the development of verbal behavior to communicate emotional states in humans, as contrasted with chimpanzees, is that human behavior is characterized by a lack of consistent correlates of affective states. If that is so, is not the entire interpretation, which is based on the verbal community having access to correlates, rendered suspect?

Animal communication of private states does not illuminate the human case

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Lubinski & Thompson (L&T) advance a bold and far-reaching thesis with respect to their pigeon model (PM), *viz.*,

> (*) PM illuminates, or helps us understand, human communication about private states, even when such states are novel.

Unfortunately, (*) is not supported by L&T's experimental instantiation of PM – nor is it supported by other parts of L&T's discussion. In fact, it seems relatively easy to provide a formidable argument – A_1 – for the view that (*) is not only unsupported, but *false.*

A_1 uses one of the many crucial locutions traditionally left imprecise in the spirited dialectic about whether apes (and the like) have linguistic capacities significantly like those of humans.[1] Here is a partial, compressed list of these locutions, all of which are at work in L&T's paper:

(L1) x communicates with y
(L2) x communicates with y in a (non)species-specific manner
(L3) x communicates a private state to y
(L4) x communicates a private state to y in a (non)species-specific manner
(L5) x talks to y (about x's private state)
(L6) x talks to y (about x's private state) via symbolic expressions.
(L7) x talks to y (about x's private state) via sentences in some grammar G

(L1)-(L7) are configured here so as to intimate an encapsulation of the continuum that L&T apparently affirm (sect. 2, para. 1) – a continuum that, put in picturesque terms, might run from "communication" between colliding single-cell organisms, through communication by pheromone release and reception, through the symbolic communication mastered by the likes of Washoe, to the give-and-take found in *BBS*. Now, it's rather doubtful that a study of animal communication that reaches only less restrictive parts (e.g., [L1]) of this continuum, however rigorous, however ingeniously regimented with help from keys and boxes and flashing lights, would illuminate the human exchange of information regarding novel private states among *Homo sapiens*. To put it bluntly, we all know that collies can communicate private states, but a novelist determined to capture the essence of what "first love" feels like "on the inside" wouldn't turn to Lassie for inspiration. With this, L&T would of course agree. But then this two-part question arises: Where do we put PM in the continuum, and is its place therein "high enough" to support (*)? It is not entirely clear where PM should be placed – but neither is it clear that it matters, because of the following chain of reasoning.

A_1

(1) If the animal communication of private states in model M is to illuminate the human communication of novel private states, then the animals involved in experimental instantiations of M must have the capacity to talk via sentences generated from some grammar.[2]

> (2) L&T's pigeons, involved in instantiations of PM, don't have the capacity to talk via sentences generated from some grammar.
>
> ∴ (3) The animal communication of private states in PM fails to illuminate the human communication of novel private states.

We don't claim that this is an outright proof, only that A_1 is formidable. Its reasoning is an instance of the ancient and unexceptionable rule of inference, *modus tollens*. The issue then becomes the truth of the premises. Premise (2) is one that L&T would obviously concede. What about (1)? Surely this would be one that L&T would resist.

We assume, then, that L&T would not concede (2), at least not intentionally. It seems, however, that they have conceded it, in short because when it comes time for them to talk about human private states they are compelled to follow others in the use of English sentences – and rather complicated sentences at that. For example, L&T tell us (sect. 6.1, para. 2, 3) that considering sentences such as the following is an integral part of the psychological study of human private states: "I feel like going for a walk," "My stomach feels funny," "I have a pleasant feeling in my stomach," and even "My thoughts come more easily than usual," "I feel as if I would be more popular with people today," "I feel like joking with someone."[3] That such (L7)-based communication is central in the human case is in part what recommends (1).[4]

L&T might retort that although such species-specific reports are often used by humans to communicate private states, this fact does not immediately imply that *all* communication with the power to explain the human case must be as linguistically sophisticated as these reports. Indeed, the belief that, at bottom, human communication of private states isn't really special is one we see expressed in statements from L&T such as:

> [Phase 2] of the synthesis was predicated on the idea that the reason humans can describe novel stimulation (e.g., unique "mixing of emotions") is because these states often share components with familiar states they have learned to report. (sect. 7.1.2)

But this idea – on which L&T say Phase 2 of their implementation of PM is based – may or may not be correct, and in fact rather simple thought experiments move *us*, anyway, to the negative answer: Suppose a father abandons Steve, a boy of five, leaving him to his mother; Steve only communicates with the father again once, one day when he visits his father at the age of 15. Steve's memories of the father are few and far between, but those he has are vivid and loving. One day Steve hears out of the blue that his father has died of an aneurysm; the day Steve gets news of the death, *ceteris paribus*, will be a day filled with (to use L&T's terminology) "novel interoceptive stimulation." Would it be plausible to hold that if Steve communicates his thoughts on that day they will be the product of what L&T call "stimulus generalization" (sect. 7.1.2)? What you might call the naïve "building block" view of such communication, stimulus generalization, would imply that Steve's communication would be the result of assembling (possibly sublinguistic) reports of familiar states experienced in the past. But again, is this plausible? Isn't it true that if Steve really wants to communicate his private states in this case, he will very likely have to struggle with which words and which sentences he ought to use? Apparently so. And that struggle does not accord with the view that humans just use the pigeon-mastered technique of stimulus generalization.

L&T would probably reply by pointing us to section 8 ("Implications and human parallels") of the target article, where, they would insist, an explicit connection *is* made between their implementation of PM and the human sharing of covert states. More specifically, they would draw our attention to their in-the-context-of-PM discussion of autism, personality disorder, mental retardation, and pharmacological prostheses. Unfortunately, a defense of the aggressive and fascinating (*) requires more than a mere parallel between the pigeon and human domains; here is one way this point can be put concretely: L&T inform us that "Controlled interoceptive prostheses directed at attenuating, increasing, or sharpening discriminative control of interoceptive stimuli have emerged over the past 35 years with the advent of modern psychiatric drugs" (sect. 8, para. 7). The phenomena associated with such prostheses are of course consistent with PM; the phenomena may even parallel results obtained in PM. But the issue is (*), that is, whether or not PM *illuminates* the development and use of these prostheses. And here the observation that research on and deployment of such pharmacological aids has proceeded in ignorance of the niceties of pigeon communication argues against L&T's thesis.

NOTES

1. When such locutions are left wholly intuitive there is perhaps reason enough to doubt the explanatory efficacy of models that adopt and conflate them.

2. Obviously, A_1 can be modified so as to use different (L_i) from the continuum. Proposition (1), given the accuracy of skepticism about the ability of even apes (and the like) to generate sentences from some grammar, would imply that even ape communication of private states fails to illuminate the human case. As a matter of fact, we share this skepticism – in short, because in order to reach (L7) (apes would seem to reach [L6]), it seems to us that an animal's talk-aspiring behavior must warrant formalization as a two-player finite state transition diagram (of which the game of checkers is one instantiation). For details, see Bringsjord (in preparation). For a nontechnical expression of this skepticism, see Boysen et al. 1980; Terrace et al. 1979.

3. For a study of human private states in which self-descriptions played a central and indispensable role, see Bringsjord et al. (1986).

4. For a look at how serious the implications of human private states might be for cognitive science, see Chapter 1 of Bringsjord (1992).

Plausible reconstruction? No!

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Lubinski & Thompson (L&T) describe a situation in which pigeons acquire and utilize tags. They consider this situation to be a plausible reconstruction of how people do the same. We disagree.

Consider a solitary individual viewing a television broadcast of the Westminster dog show. An unfamiliar breed of dog appears and is named by the announcer. The viewer learns what the breed is called, that is, the viewer attaches a label to a stimulus pattern. Some weeks later, an acquaintance sees the unfamiliar breed and asks the viewer, "Do you know the breed of that dog?" "Why, yes," replies the viewer, "That is a whippet." The viewer was able to respond appropriately to the acquaintance without having been reinforced either for acquiring the appropriate tag for the stimulus pattern or for responding "whippet" prior to being asked.

Now consider L&T's description of the situation. According to them, we would have to have a discriminative stimulus (S^D) followed by a verbal response (R_v) that is reinforced (s^r). Their formulation for learning the response "dog," applied to the case of "whippet," is:

$$S^{D(\text{WHIPPET})} \rightarrow R_v^{(\text{WHIPPET})} \rightarrow s^{r(\text{"Yes"})}.$$

We will note two inadequacies of this formulation. First, it fails to recognize the distinction between (1) applying labels to stimuli (Pavlovian situations) and (2) responding verbally to stimuli (instrumental response). Second, it assumes that reinforcement is necessary for performance changes to occur in either Pavlovian or instrumental situations, or both (the formulation is silent on that point), an assumption which is completely at variance with the example given for the solitary television viewer. The viewer was reinforced neither for the tagging operation nor for the verbal response. In fact, the viewer acquired both the tendency to apply the tag and to respond appropriately merely as a consequence of watching a television program.

Would anyone deny that performance changes can occur in the way we described for our human television viewer? Note that even pigeons have been shown to acquire appropriate performance changes merely as a function of observing a sign and what it signifies (e.g., Browne 1976). Moreover, rats have been shown to acquire instrumental responses in the absence of reinforcement (e.g., Tolman & Honzig 1930). In view of such findings, to suggest that humans are unable to apply tags or to utilize them in the absence of reinforcement (i.e., merely as the result of observation) is implausible. Given that tags can be acquired in the way we have described for our viewer, and given that our viewer could utilize the tag in the absence of prior reinforcement, L&T's analysis of how tags are acquired and utilized fails the plausibility test.

Consider our emphasis upon observation, rather than reinforcement, from another point of view. Some categories, such as dogs, have literally hundreds of exemplars. It seems unrealistic to suppose that someone would learn the correct tags for each of these hundreds of breeds of dogs at the hands of a tutor who reinforced every verbal response. This would be too time consuming, considering that humans can correctly apply thousands of tags. Thus, the insistence on reinforcement not only denies the power of observational learning, which is by now well established in animals as well as people, but it is implausible as well, considering the great number of tags people possess.

In sum, we consider L&T's "plausibility proof" to lack plausibility because it relies on reinforcement when there is a more efficient learning mechanism available, namely, simple observation. Of course, if reinforcement occurred coincident with observation, it would not necessarily reduce learning. However, it would not necessarily increase learning either. Consistent with our view, Ninio and Bruner (1977) concluded from an observational study that the likelihood of a child immediately repeating a label was independent of the nature of the mother's positive feedback. In their study, a child repeated labels about as often in the absence of positive feedback from the mother as in its presence.

Merely demonstrating that one can produce a pattern of behavior under a particular set of conditions (e.g., pigeons in operant chambers) does not necessarily mean that under other conditions (e.g., people in real life) the behavior arises for the same reasons. For example, computer chess programs play chess and play chess well, yet they are not regarded as plausible models of human chess playing because they rely on extensive search processes that far exceed human capabilities (Charness 1991). Thus, the demonstration of a similarity in behavior between two different organisms or between a computer program and an organism is not sufficient by itself to suggest that the two are similar, much less identical.

L&T suggest that tagging interoceptive events requires a more complex explanation than tagging exteroceptive ones, because in their view we have more direct access to exteroceptive events than to another person's interoceptive ones. This is incorrect. To see why, consider a mother teaching her child to discriminate between a cat and a dog. Does the mother intend the tag "dog" to refer to the larger of two animals? Or the furrier one? Or the darker one? The cat and the dog differ along many dimensions, and which of these dimensions is intended to differentiate the two can be inferred by the child only by observing the tags applied to cats and dogs in a wide variety of contexts. The child's task is to infer the mother's referent. Learning the referent of other exteroceptive events, for example, a just cause, may be more difficult than learning the referent of many or most internal events. It seems clear that learning to tag exteroceptive events requires making inferences about another's internal state as much as does learning to tag internal states themselves.

No report; no feeling

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Lubinski & Thompson (L&T) have shown that a mechanism described by Skinner really works: organisms can get other organisms to behave in arbitrary ("non-species-specific") ways corresponding to certain of their (the latter organisms) "inner" states. The behavior mimics the behavior of individuals reporting how they feel, following a request to do so. But the drug-cue bird in this study does not actually report anything, nor are we given any reason to think the bird is responding to how it feels.

In places L&T come close to acknowledging this. They carefully point out that the pigeons' behavior "is not language" (sect. 2, para. 1), and they note the complaint that no intention to communicate is involved (sect. 7.2). But elsewhere they seem unaware of the possible gulf between what happens in their "animal model" and what happens in what it is a model of. In section 6.1 (para. 6), for example, they speak of "exchanges in which one animal reports to another animal how it feels." So it would be well to rehearse the differences.

1. Even if the pigeons' behavior is "communicative," nothing – no information – is communicated in it. There is nothing that the drug-cue bird knows that the decoder bird comes to know because of the former's choice of key to peck. The former might know, for example, that it has pecked the key appropriate for the way it feels (but see below). The latter learns at best, however, that the former has indicated which specific-drug symbol it (the decoder bird) should peck to complete the routine and/or to obtain food. *We* of course can tell from the first bird's choice what "interoceptive state" it is in. But the bird is not "reporting" this or any other information, neither to us nor to its partner.

If any communication of information takes place, it is in the nonprogrammed behaviors that emerged. It is tempting to suppose that the decoder bird knows that it wants the other bird to "do its thing," and pecks at the Plexiglas and behaves aggressively intending to communicate precisely this information. It is also tempting to suppose that the other bird gets the intended message. Perhaps this behavior is innate or species-specific: pecking to get a conspecific's attention, aggressive displays to elicit a certain response. But *what* response? We can hardly suppose evolution to have equipped pigeons with the means for signaling "Do whatever it is you do to get a symbol to appear on my sample key." Perhaps: "Do that which I am most interested in having you do now [and you can realize what that is by considering what we have been doing, and what has been happening]." In nature, such a contextually indicated desired response might most often be "Retreat! Back off! [This is *my* mate/territory!]" If so, it is intriguing that these pigeons used this behavior to elicit a *cooperative* response. How could it have this result? If the behavior really has the suggested content of "Do that which you can tell from context I want you to do," it can work only if the recipient of the "message" is sensitive enough to context and has an adequate "theory of mind" of the other pigeon. (But this *need* not mean it has mental states like ours, or regards itself and other pigeons as being mentally alike in any way; neither might it have conscious mental states itself nor regard other pigeons as having them; see Davis 1978; 1989.) Notice, by the way, that in this analysis, the decoder bird is reporting how it feels to the drug-cue bird: it *wants* the latter to do something.

All this is speculation, however; I offer no argument for taking it seriously. I want only to emphasize the contrast. Whereas I find it tempting to understand the Plexiglas-pecking and aggressive displays as genuine communication, perhaps even of "inner" state, I have no such temptation regarding the key-pecking behavior of either bird.

2. In the preceding, we assumed that the drug-cue bird pecked the key it knew "was appropriate for the way it felt." But nothing supports this assumption. We know little more than that the choice of key came to be controlled by the type of drug administered. Well, we may reason that the influence of the drug has to get to the mechanism or structure in the CNS that mediates the choice of key; so we can speak of an interoceptive "stimulus" eliciting this "response." Perhaps we can even speak of the "output" of the "interoceptive transducers" as carrying the *information* that such and such a drug state obtains, and so determining appropriate key-choice. Nowhere in all this is it implied that the *pigeon* (as contrasted with the mechanism that mediates the choice) makes use of this information. Nowhere is it implied that the pigeon feels any particular way at all, still less that it knows (is aware of) how it feels, or behaves as it does because of this knowledge (awareness).

"What else?" someone might object. "Unless you deny altogether that the pigeon's consciousness has any impact on its behavior, or deny that the pigeon is conscious?"

But what evidence is there that the drug has entered the pigeon's presumed consciousness, or that it *consciously* modifies its behavior as a function of this aspect of its consciousness? On this last point, there is evidence that *humans* can learn to respond in certain ways without awareness that they are doing so (Conger & Killeen 1974; Hefferline et al. 1959). Perhaps the pigeon also remains unaware of what its response is *to* – even in the human case, awareness of the stimulus may have played no role in the process.

Even if we suppose that the drug produced something that can be called a "private stimulus," what happened is still very different from human awareness of and response to an emotional state, or to "inner" states and events of other sorts. Among the examples L&T cite from the Addiction Research Center Inventory, what happened is probably most like the "internal condition" leading to the responses "I have a pleasant feeling in my stomach" and "My nose itches," associated with morphine, and "My appetite is increased," associated with alcohol. But is "private stimulus" really a helpful term for what is reported by "My thoughts come more easily than usual," "I feel as if I would be more popular with people today," or "I feel like joking with someone"? Perhaps the amphetamine really does make the person's thoughts "come more easily [= more quickly?] than usual," and *that* is what the person is aware of. Is awareness of one's thoughts a matter of detecting a private stimulus? What about awareness of one's behavioral tendencies ("joking with someone") or their likely effect on others ("I would be more popular")? Genuine awareness of an emotional state such as anger involves all this and more. Anger, for example, is a matter of having certain kinds of thoughts ("He insulted me") plus certain behavioral tendencies ("I'd like to smash his face") – and the behavioral tendencies must have resulted from these thoughts. People can distinguish between anger and, say, mere drug-induced aggressiveness, so in some sense they must be aware of all this complexity. But this is not simply awareness of "private stimuli." (See Gordon 1987, especially pp. 95–109; and more generally, Alston 1967.)

A simpler kind of case, which already shows the inadequacy of treating all discrimination of "inner" phenomena as responses to "private stimuli," is awareness of acting. Humans asked to identify the moment when they begin an action like moving a finger regularly indicate a moment significantly prior to the onset of relevant muscular events (Davis 1987; Libet 1985; McCloskey et al. 1983). They are thus not responding to kinesthetic or proprioceptive stimuli, and probably not to any event outside the CNS at all. McCloskey et al. speak of a "command to move"; Libet calls it the "final motor cortical output." These phrases are probably less misleading than "interoceptive" or "private stimuli."

3. None of this is to deny that the mechanism first described by Skinner and now demonstrated by L&T might really figure in a complete story of how we become able to report our feelings and other "private" states – including our intentional actions – to one another. But that story remains to be told. The present case may involve the mechanism, but no report, and probably no feeling.

A human model for animal behavior

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Lubinski & Thompson (L&T) propose an animal model for human communication based on private stimulation. I believe that there are many interesting things to be learned from their experiment, but I do not believe that their experiment provides us with a good model of communication of the distinctively human sort. To show this, I begin with an analysis of what is communicated in the human case and then endeavor to show that nothing of the sort is communicated in the experiment L&T describe.

Suppose someone says to you, "I am in pain." What condition must hold, if we are to say they have successfully communicated something to you? Quite simply, it is this: prior to their statement you did not know they were in pain, but now as a result of their statement, you do. So what they have communicated to you is the knowledge that they are in pain. Let us call the person who is supposed to be sending such knowledge, the "sender" (the drug-cue bird in the L&T experiment) and the person who is supposed to be receiving such knowledge the "receiver" (the decoder in the experiment). My thesis, then, is that the L&T experiment is a poor model for distinctively human communication for the simple reason that no knowledge of the sender's internal state is communicated to the receiver.

There is a way this thesis can be clearly demonstrated, for anyone wishing to do so: run a similar experiment, only use humans instead of pigeons, and afterwards question the receivers to determine whether they have received any knowledge about the senders' private state. If the conjecture I am proposing above is correct, the results will be that humans who successfully perform the same basic tasks as the receiver pigeons will be unable to tell you anything about the senders' internal state. This would show quite conclusively, I think, that the receiver pigeons also performed their tasks without any knowledge of the senders' internal state.

Ultimately, the correctness of my conjecture rests upon someone actually performing the above-described experiment with humans. In the meantime, I can only describe in brief outline the reflections that led me to this conclusion: as a kind of thought experiment, imagine you are in the position of the receiver (decoder) in the L&T experiment and ask yourself what you know and, more important, what you would need to know in order to perform as the experimenters wanted you to.

First of all, you would know that you were reinforced for pressing certain letters (e.g., S, C, or P) when certain other letters (e.g., N, E, or D) were presented to you, but you would *not know (since the birds don't know) that the experimenters* regarded these letters as names: that S = saline, C = cocaine, P = pentobarbital, N = no drug, E = stimulant, and D = depressant. Nor would you know that the person in the adjoining chamber, the sender (or drug-cue person), had gone through the same conditioning process as you (of learning to correlate the letters S, C, and P with the letters N, E, and D). Not only that –

and here is the worst part – you would not know that the sender was supposed to be a sender and that you were supposed to be a receiver, nor that the sender had been specially trained to press N, E, or D when in certain internal or private states. Since the sender pigeons can't be assumed to know such things, neither can you.

In spite of all this ignorance, you would still be able to perform as the experimenters want you to. When the lights came on in the sender's chamber, for example, that would be your cue to press a certain button. The experimenters call it the "How do you feel" button, but if you are in the pigeon's position, you would not know this. You would know, however, that you are now supposed to press it all the same. You might or might not then observe that the letters N, D, and E light up in the sender's chamber and that the sender presses one of these. Now it is crucial to point out here that you *would have no way of knowing* that it was a private state that prompted the sender to press that letter at that time. Nor do you need to know this in order to do your part. For the next thing that happens is that a certain button lights up in your chamber. This is called the "Thank you" button by the experimenters, but you, like the receiver pigeons, would not know this either. All you know or need to know is that you are to press this button. Following this you may or may not note that a blue light starts flashing in the sender's chamber. But, most important of all, you do note a certain button in your chamber displaying the letter N, D, or E and you then match whatever letter is displayed with the corresponding letter (S, C, or P), as you were taught. The flashing blue light is a generalized reinforcer for the sender and the display button (showing N, D, or E) is called a sample button by the experimenters, but you (like the receiver pigeons) have no way of knowing this. No matter, however, for you can still perform just as well; all you have to do is match the displayed letter with the corresponding letter, as you were taught. If you do all of the above things correctly, you will make the experimenters happy and they (after having deprived you of food rather severely) will now reward you with a piece of bread or maybe even a bagel.

We have noted, then, that you were ignorant about quite a number of things and yet this in no way interfered with your ability to perform your assigned tasks. Since you are human and can reason (i.e., can string propositions together in a logical fashion), you might have made some good guesses about what was going on, particularly if you were familiar with reinforcement theory. Thus, you might have guessed that the flashing blue light was a generalized conditioned reinforcer for the sender. It would not, however, be reasonable to assume a pigeon could formulate such a hypothesis, for without an elaborate human type language (not to mention a considerable amount of background knowledge) it is hard to see how any organism could do so, human or otherwise. In any case, no matter how clever we might suppose you to be (even if you are Sherlock Holmes), there is simply no way (on the strength of the information you had about the situation you are in and about the sender) you could have made any inference about the sender's private state that prompted the sender to push the button (N, D, or E). So, there is no way you could have knowledge of the sender's relevant private state. We cannot say, therefore, that there is any sense in which the senders have communicated to you any knowledge of their private state. And yet this can in no way be imagined to have the least effect upon your performance.

What is true of a receiver human holds doubly for a receiver pigeon. For similar explanations apply to the receiver human's performance and the receiver pigeon's performance and in neither case need we at any point assume a knowledge of the sender's private states. In fact, we see that it is quite impossible for the receiver human to have received such knowledge, and this counts doubly for the receiver pigeon who, after all, has only a bird brain.

There is more. I believe that what is true of the L&T experiments applies to the other experiments mentioned by them as well (including Epstein et al. 1980; Lubinski & MacCorquodale 1984; Savage-Rumbaugh et al. 1980). I cannot develop parallel thought experiments here to support this contention, so I will leave it to readers to do so on their own. In those cases too, of course, actual experiments would be even more convincing. But those tasks too I leave for others to perform.

Communication versus discrimination

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In their target article, Lubinski & Thompson (L&T) mention two different items as the goals of their research program: (1) developing an animal model of communicative behavior to illustrate features of the relationship between speaker and listener, and (2) articulating the nature of the process by which we learn to identify feelings and report them to others. The results presented, however, accomplish neither of those goals: L&T fail to sketch a model of communication. Nevertheless, they do outline evidence for sophisticated discriminative abilities in pigeons. That is, they point out the (surprising) fact that what it is like to be a pigeon may be interestingly similar to what it is like to be human.

Savage-Rumbaugh et al. (1978) do give us an animal model of communication (at least, they present a model that I believe meets L&T's first goal). Two factors about their paradigm should lead us to conclude that they have captured relevant aspects of a meaningful symbolic exchange. First, both chimpanzees learned the same relationships among the geometric forms, the actual food and drink items, and photographs of those items. Hence, we could argue that both participants recognize what the arbitrary symbols stand for. Second, the success of their social interchange depended upon both chimps using this knowledge to get a reward. Hence, we might say that the interchange causes the "listener" to understand what the "speaker" perceived.

Both of these factors are missing in L&T's pigeon study. They do notice that, with the pigeons, the arbitrary symbols only have "unidirectional functional significance." However, they claim that this does not affect the structure of the putative interaction; instead, it only alters the "stimulus equivalence relationships." Here I believe they are just dead wrong. For actual communication to occur, both parties have to appreciate what the symbols that are being exchanged represent; otherwise, we cannot claim that any information has been transferred. Without imparting "news" to the decoder, the most that has happened is that the drug-cued pigeon has made and remarked upon some interoceptive discrimination. This model therefore fails to capture a true speaker-listener relationship, so the model falls short of goal (1).

If we assume (as L&T do) that a third mark of (human) communication is that the motivation for engaging in symbolic exchange comes from conspecifics, we must again conclude that their pigeon arrangement comes up short. They claim that an awareness of the other pigeon is "necessary for social facilitation" when one of the pigeons is satiated, similar to our need to be aware that we are talking to someone when we are not being coerced into communicating by some external contingency. However, L&T's claim is not supported by the evidence. The best they can say is that the presence of both pigeons is *sufficient* for the exchange under certain conditions. They give no reason to believe that the task of inducing the satiated pigeon to perform could not be accomplished by some other object, including something programmed into the experimental apparatus.

Moreover, since the deprived pigeon "displays" in order to frighten the other into performing his half of the exchange, we can understand this performance as a type of coercion – exactly the opposite of what we assume happens in paradigm cases of human communication. (I take it that the fact that some marginal cases of human interaction do in fact turn on coercion is irrelevant, simply because these are special cases. Indeed, in these cases, too, there is no reason to believe that the presence of the conspecific motivates the other to speak – rather, fear [or anger] does.) Since our motivation for learning to articulate facts about our inner life in a social setting presumably does not correspond to any specific emotional state, I must conclude that this model does not meet goal (2) either,.

Even if Catania (1990) is well justified in believing that language is primarily a means of changing others' behavior, a *linguistic* exchange only occurs if behavior is changed in virtue of successfully reporting a fact about the world. The pigeons' matching various symbols to other symbols cannot be linguistic since the decoder (at least) has no way of understanding what the symbols refer to. And aggressive displays cannot be linguistic either, since they report no fact.

Nevertheless, I still wish to conclude that Lubinski & Thompson's project is very important, though not for the reasons they emphasize. Even if we cannot say that the decoder and the drug-cue pigeons communicate in any important sense, L&T have still demonstrated something remarkable. Pigeons have an inner life of some fairly sophisticated sort! They can reliably differentiate the sensations of hunger from thirst from a stimulant from a depressant. Who would have thought that a mere bird could distinguish its internal states so well? Showing how much a pigeon can discriminate is no trivial feat, even if we do not yet have a way to test whether they can communicate this knowledge to other birds.

This fact cannot be overemphasized. Studies like these should provide plenty to speculate about for those who wonder what it is like to be a bat (or a pigeon), for what it is like to be a bird may not be fundamentally that different from what it is like to be me. Darwin scores again.

A promissory note is paid, but has this bought into an illusion?

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In 1945, B. F. Skinner proposed that one learns to discriminate one's private internal events only through verbal interactions with others. By this argument, it follows that one comes to know one's internal states only through indirect relationships between the internal events that comprise those states and the correlated external events that other people have access to. This has become a conventional view within behavior analysis, although the interpretive (as distinct from the experimental) nature of the account made it necessarily speculative (cf. Schnaitter 1978). Now, however, Lubinski & Thompson (L&T) buttress this account by bringing to bear the substantial literature on pharmacological manipulation and drug discrimination. The development of discriminations of internal states is verified through direct conditioning methods and these discriminations are combined in experimental syntheses of primitive "vocabularies" of transaction between experimental subjects, whereby one organism can report its internal events to another. Thus, for a particular case, using nonhuman organisms whose prior histories are not problematic as those of normal humans would be, interpretation has been replaced by direct analysis and synthesis. With this accomplishment, L&T's target article makes a substantial payment on the promissory note that was implicitly drawn years ago (Skinner 1945), and that has been renewed over the ensuing years (e.g., Skinner 1957; 1969; 1974).

L&T also elaborate a link with another biobehavioral line of work, which can be seen through comparison with an earlier *BBS* contribution. Engel (1986) showed that if one arranges, by means of special transducers, for an arbitrarily chosen physiological change (such as a change in blood pressure at a particular locus in the body) to interact with the external environment similarly to the ways overt behavior affects or is correlated with environmental events, that physiological change will come to display functional properties similar to those of overt behavior. Thus, Engel integrated the internal behavior patterns of an organism with the behavior patterns of the organism as a whole, using conventional conditioning paradigms to identify functional roles of environmental events. L&T's strategy is complementary, using direct manipulation of, rather than direct recording of internal events. Both in Engel's and in L&T's work, we find that conventional notions of "outside-in" and "inside-out" can readily be turned around, both interpretively and experimentally.

So far so good. There is, however, an implication of the behavior-analytic interpretation of awareness that advises caution in extrapolating from L&T's demonstration. As Skinner pointed out (1945; 1969), if this account of our normal repertoires of self-description is correct, we usually know our most intimately private functioning less well than we know our public actions. By definition, private events are inaccessible to the verbal community, handicapping its ability to teach us to discriminate them. Only in special situations like biofeedback procedures (or drug administration) do other people have the basis for teaching us accurate descriptions of private events comparable to that arranged in L&T's experiments.

Skinner noted this, and came close to suggesting the additional point I wish to make here, when discussing students' descriptions of a demonstration in which he had shaped a pigeon's behavior through contingent deliveries of food. The students' descriptions were phrased in terms of the pigeon's presumed hopes, observations, feelings, associations, and expectations, from which Skinner concluded:

> [The students were] describing what they would have expected, felt, and hoped for under similar circumstances. But they were able to do so only because a verbal community had brought relevant terms under the control of certain stimuli, and this was done *when the community had access only to the kinds of public information available to the students in the demonstration.* . . . Private stimuli may have entered into the control of their self-descriptive repertoires, but the readiness with which they applied them to the pigeon indicates that external stimuli had remained important. (Skinner 1969, pp. 237–38)

I would revise the first sentence of this quotation, as follows: The students wrote or spoke as they had learned to write or speak in situations analogous to the one they saw the pigeon as being in. That is, although I accept L&T's demonstration that one can learn to describe one's internal states, I question whether many of the statements we commonly accept as describing internal states are in fact doing that. The forms of the statements may be subtly misdirective to both speaker and listener.

Our descriptions are often gratuitously directional. For example, it is unremarkable for me to say that corn whiskey is potent and that I am allergic to tulips. In the former, I attribute behavioral effects to an environmental event, in the latter, to my own susceptibility. This shift is not justified by a change of causal locus, however, for in each case there is a clear environmental influence on behavior, as well as a known physiological mechanism. Furthermore, the potency of the whiskey *is* my susceptibility to it, and my allergy to tulips *is* their potency to make me sneeze. Attribution theorists (e.g., Jones & Nisbett 1971; Ross 1977) have observed and discussed these interpretative shifts, but have failed to appreciate their relevance to formal psychological theory (Hineline 1990; 1992).

More relevant to the present issues: in saying that I look at my computer screen, I suggest that this is something I do to the screen. It would be more accurate to say that I orient my eyes in such a way that the screen's light stimulates them. I also say that I see patterns on that screen. To say instead that I see only my sensations rather than external patterns would be to risk a trap of solipsism. But if one examines how we learn to speak of internal events, one finds a similar trap. I learn to speak of frustration in certain environmental configurations – analogous to the patterns on the screen. Yet I am taught to characterize the frustration as an internal state, analogous to the sensation of the screen patterns. If I were called upon to articulate what it is to be frustrated, it would be details of the surrounding circumstances rather than of my internal reactions that would provide meaning to the description. Thus, the internal character of our feelings, relentlessly implied in conventional language, may be illusory – mislocated through the misdirection of ordinary talk.

Difference without discontinuity

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As our authors view it, this essay represents another skirmish in the ongoing war between those, call them *Darwinians,* who believe that human beings are part of nature, and those, call them *Cartesians,* who believe otherwise. Being Darwinians, the authors hope to demonstrate the "biological continuity" of man with beast at the very juncture where Cartesians posit discontinuity, namely, the communication of information about conditions internal to the body.

In this target article, Lubinski & Thompson (L&T) claim to have taught pigeons to discern how they feel, just as human beings do, and to tell each other, just as human beings do. If they have succeeded, they have proved that pigeons possess not only the capacity for introspection but also the capacity for communication, two capacities long thought by Cartesians to be exclusively human.

Why pigeons? Why not orangutans, or chimpanzees, whose symbolic capacities have been the subject of recent studies? Is the motive one-upmanship, showing how to do with inferior creatures what other experimenters have been able to do only with more capable ones? Presumably not. Presumably, the reason is to demonstrate that the beasts who can introspect and communicate like human beings need not be next to them on the evolutionary scale. Instead, they can be as different biologically and behaviorally as pigeons.

If this were not so, L&T appear to fear, Cartesians might be able to postulate discontinuity once again, perhaps by locating a gap between lower-order beasts and higher-order anthropoids. In that case, faith in "biological continuity" might be undermined once more and, along with it, confidence in the long-standing practice of drawing conclusions about human beings from experiments with such primitive creatures as rats and pigeons.

For these reasons, which I glean from between the lines, our authors want to show that communicating information about internal states and conditions does not even presuppose language. Instead, it requires only arbitrarily selected and syntactically unrelated signs. Thus, L&T make do with lettered dots and lighted discs, one for each condition an experimental pigeon is to identify and tell its fellow about.

In their first experiment, one starved or dehydrated pigeon is first taught to identify its particular drug-induced condition by pecking the appropriate dot or disc. Then a second pigeon is taught to reward the first with a piece of grain or drink of water. The result: communication to the second by the first of introspectively discerned information about its internal condition. Or, anyhow, so the authors claim.

Is their claim warranted? This question divides into two. First, have the pigeons in question *introspected* their internal states? Or have they merely responded differentially because of them, without becoming aware of the causes of their responses – as a recently magnetized metal bar now attracts iron filings without knowing that it does so, much less why? Second, assuming that the pigeons have introspected, have they also *communicated?* Has their introspective knowledge been transferred to their companions?

The authors appear to think so; but it is not clear that their experiments justify their conclusion. The experiments certainly demonstrate differential *responses* by, and differential *interactions* between, pigeons whose internal states have been altered. But whether these responses and interactions amount to introspection and communication depends on how you define *introspection* and *communication.*

To see this, suppose that we count as introspection or communication only reports embodied in explicit, syntactically structured sentences in some recognizable language – or some rough approximation thereto. From this definition, which closely approximates ordinary usage, it will follow at once that our authors have failed to demonstrate either that pigeons introspect or that they communicate.

Notice, by contrast, that this negative conclusion does not appear to follow so readily or so surely when we come to the studies, conducted by Savage-Rumbaugh (1986) and others, of the linguistic capacities of chimpanzees and apes, who appear to learn not only arbitrarily chosen signs but also how to combine these signs in syntactically legitimate phrases.

To say this is, of course, to acknowledge that there is indeed a gap between pigeons and human beings. It is to admit that human beings can do some things that pigeons cannot, even given a lot of experimentally ingenious and patient "chaining" of simple acts into complex performances. It is also to admit that the behavior of apes makes a closer approximation to the behavior of human beings.

But so what? Why should such admissions disturb anybody? Does acknowledging that pigeons are unlike human beings mean giving up belief in "biological continuity"? Must we, in order to continue thinking of human beings as creatures of evolution, endorse the belief that they have no capacities that do not go *all the way down,* to the bottom of the evolutionary scale?

I do not see why we should concede anything so implausible to the Cartesians. Can we not, instead, think of man as a creature possessing some of the most distinctive capacities ever produced? And may we not suppose that, among these distinctive capacities, are the almost unique ability to reflect on one's condition and use language in describing it for the benefit of others?

If not, I confess that I, who think of myself as a good Darwinian, cannot see why. Perhaps our authors will explain it to me.

Behaviorism is alive and well

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Lubinski & Thompson (L&T) show clearly that there is power in Skinnerian behaviorism, and by implication in behaviorism generally. Several comparative investigators are cited by the authors as placing man in a class apart from other animals with respect to communication of inner states, but L&T demonstrate that the cited authors erred. It may be useful to discuss their error in a context that is rarely referred to today. Lloyd Morgan's

Canon of 1894 and an obvious corollary are highly relevant: In no case may we interpret an action (in animals other than man) as the outcome of the exercise of a higher psychological faculty, if it can be interpreted as the outcome of one which stands lower in the psychological scale. The corollary, for which I have no attribution, is as follows: In no case may we interpret an action (in human beings) as the outcome of a higher psychological faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale. That is, do not attribute something special to man that is not required to explain a seemingly identical phenomenon observed in an animal other than man.

One source of the error in placing man in a class apart was to go beyond the use of "conscious" as a descriptive term (reporting on inner states) to the reification of "consciousness." The latter is generally considered an attribute characteristic of human beings. It is not at all clear that cognitive psychologists are avoiding the error of reification. Mentalistic constructs may not be as useful as their users believe. To what extent are cognitive psychologists merely introducing new terms for familiar phenomena? If the same hypotheses can be derived without a mentalistic construct, the construct should be dropped until it can be shown to be essential in the accurate prediction of behavior. Psychology is a behavioral science. It is not the science of cognition per se.

Are pigeons conscious of inner states? The answer to that question depends on one's choice of definition, but I accept the L&T operations as constituting a reasonable definition. Thus my answer is "yes." I also accept their operations as providing a sufficient definition of conscious behavior in the human.

Intelligence is a related construct. Intelligent is a descriptive term which, as intelligence, is commonly reified as an entity. The development of intelligent behavior and its measurement may not be as remote from operant conditioning as commonly believed. Behavioral synthesis describes a phenomenon in which independently trained behaviors are combined to solve a problem not heretofore encountered. As L&T discuss, this represents the behavioral explanation for insight that for many years supposedly separated Gestalt psychology and behaviorism. Two behavioral components are the minimum needed for a synthesis. Presumably many components increase the problem-solving repertoire of the animal.

The similarity between the ingredients for behavioral synthesis and the definition of intelligence I have been using for many years (Humphreys 1971; 1985) is inescapable. Intelligence is a short-hand expression for the acquired repertoire of cognitive knowledge, information, and skills available to the organism at a particular point in time. "Cognitive" is defined by consensus among psychologists doing cognitive research. The acquisition and utilization of a cognitive repertoire have both environmental and genetic substrates, but the genetic contribution is to the structural-chemical attributes of the organism. Intelligence is not an entity.

Although my definition has not won popular acclaim, it is consistent with the kinds and varieties of items that appear on standard tests of intelligence such as the Wechsler tests and the Stanford-Binet (Humphreys 1992). It is not surprising, therefore, that the accumulated data concerning the correlates of those tests are also congruent with my definition. There are few if any items in a standard test that meet the criteria that many persons have for measures of "real" intelligence, but the standard items, when combined in a total score, are highly effective predictors of future performance in a large number of important roles in our society. A large cognitive repertoire allows for behavioral syntheses suitable for a wide range of problems and provides multiple components for a single complex problem. There are other definitions of intelligence, but serious measures of those definitions are few. The alternative measures available are substantially less effective than current standard tests. Furthermore, I see nothing on the horizon stemming from the cognitive revolution that is likely to supplant present predictors of future intelligent behavior.

Pigeons and the problem of other minds

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Lubinski & Thompson's (L&T's) target article demonstrates that pigeons can discriminate stimulations of some recently discovered sensory transducers. However, despite the authors' claims, it does not demonstrate that pigeons can communicate their "private mental states."

Recent discoveries of sensory receptors deep in the CNS (like the opioid and GABA receptors) are interesting, but the term "interoceptive" is unfortunate. L&T define the term loosely as meaning "under the skin," but this won't do. Tactile receptors are also under the skin, but nobody would suggest that this calls into question whether we can discriminate tactile stimuli. What is exciting about this research cannot merely be that pigeons have discriminated stimuli transduced by receptors under the skin.

A better definition of interoceptive might be that interoceptive transducers are those that are sensitive only to stimuli from inside the body. But there is nothing distinctive about this, either. Transducer-specific stimuli can be introduced into the body from outside in a number of ways, for example, by injecting drugs. Given sensory receptors of any kind and the ability to stimulate them, we should expect that subjects will be capable of discriminating them, no matter how "deep" they are in the body.

The target article does an admirable job of demonstrating that pigeons can discriminate drug stimuli. However, this does not justify the claim that pigeons can communicate their mental states any more than the fact that pigeons can discriminate visual stimuli does. L&T's argument that discriminating drug stimuli amounts to communicating mental states is based on a simple error: they identify mental states with the stimulation of "interoceptive" transducers, that is, with those recently discovered transducers deep in the CNS. But mental states cannot merely be the stimulation of interoceptive transducers any more than they could be equivalent to the stimulation of exteroceptive transducers like the photoreceptors of the retina.

In principle, the argument for this is simple: having a doglike pattern of irradiation on one's retina is not the same thing as thinking about a dog, because one might be attending to something else, or hallucinating, or blind. Likewise, having a characteristic pattern of stimulation at a particular interoceptive transducer cannot be the same thing as "feeling wired" or "feeling drunk," though the two may often coincide.

To make this clearer, it will be useful to be careful about the differences between stimuli, stimulations, and mental states. Stimulations and mental states are characteristically private: there is no such thing as a public stimulation or a public mental state, just as there is no such thing as a married bachelor. Stimuli, on the other hand, can be public or private, but the difference is theoretically insignificant.

It should not take any argument to show that all stimulations and all mental states are private. A stimulation is an event that happens at a particular sensory receptor in a particular organism. Such an event is only publicly accessible insofar as we understand the mechanics of the sensory receptor. If mental states exist at all, they are properties of individual organisms which are only objectively accessible through the mediation of third-person interpretations, for example, behavioral or neurophysiological data.

Stimuli, on the other hand, can be public or private. A private stimulus is one that is available to only one subject at a time. By contrast, a public stimulus is available to more than one subject at a time. Whether a particular stimulus is public or private depends only on how many subjects receive it: the distinction has nothing to do with the modality of the stimulus, the location of the sensory receptors, or the mental state of any organism.

Some stimulus modalities are more amenable to public cueing than others. Visual stimuli, for example, are available to any subject with a line-of-sight to a stimulus. Likewise, auditory stimuli are available to any subject within hearing distance. On the other hand, drug stimuli are more amenable to private cueing. We think of intoxication as a private event, but we can use drugs as public stimuli: suppose we fill the experimental chambers with opium smoke.

With the public/private distinction out of the way, we can see what we would really like to know – whether any subject can communicate its mental state to another. We knew already that subjects can communicate differential stimulations (which are always private whether or not the stimulus that produces them is public). That pigeons can communicate about recently discovered kinds of stimulations only shows us that they can discriminate more kinds of stimulations. Just because the receptors at which these stimulations occur are in the CNS does not make the stimulations mental states.

Another way of seeing this is to consider that the same problem exists for "public" events as exists for "private" events: we always have to infer the mental states of others, regardless of what the stimulus is and whether it is accessible to us. Even when a dog is publicly accessible (visible to both parties), one can never know that another is actually thinking of that dog. The only indications we have are based on just the kinds of things L&T attribute only to inferences to "private" mental states: public behavior and contextual factors. Just because I see a dog and you are in the same room doesn't automatically mean that you see the dog. I may, however, come to believe that you see the dog if you exhibit certain characteristic behaviors, such as looking at it, smiling, calling it, puckering your lips, or petting it. In just the same way, a pigeon's pressing one key rather than another when under the influence of a drug is a collateral behavior, not a direct report of a mental state.

The matter of how we could ever know the mental state of another is the age-old "problem of other minds." Asking another thing what it is thinking may give you another piece of behavior, but it will never give you direct access to its mental state.

Animal modeling in psychopharmacological contexts

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Lubinski & Thompson (L&T) seek to explain the communication of internal "private" states using an animal model of the following kind: a bird is administered one of three pharmacological agents (cocaine, pentobarbital, or saline) and is trained to communicate information about its ("drug-induced") internal states to another bird. As the authors put it, "Indeed, it is generally agreed that some pharmacological stimuli generate in laboratory animals interoceptive states that share components with affective states in humans; there is also evidence that comparable subcortical structures are involved in the mediation of these phylogenetically shared states" (sect. 6.1, para. 5).

We are worried about L&T's underlying methodology. They cite Darwin and Bernard as laying out the basic methodological presupposition: "Both Darwin and Bernard argued that anatomy, physiology, and behavior not only look similar in different animals but often share common evolutionary origins and current regulatory mechanisms" (sect. 1, para. 1). Unquestionable. But the authors follow Bernard, who goes much further: "All animals may be used for physiological investigations, because with the same properties and lesions in life and disease, the same results everywhere occur" (Bernard 1885/1949, p. 115). In other words, nonhuman animal systems may serve as causal analogs for human systems because of phylogenetic continuity. The assumption is that once quantitative differences in body weight, and so on, have been allowed for, effects produced by a compound in laboratory animals may be generalized to humans. The current study is an extended *causal analogical argument* relying on these additional assumptions.

Causal analogical arguments have the following general structure: X (*the model*) *is similar to* Y (*the subject being modeled*) *with respect to properties* $\{a, \ldots, e\}$. *X has additional property f. Although f has not yet been observed directly in Y, Y probably also has the property f.* There are three conditions that must be satisfied to make legitimate inferences from X to Y (in this case, from the animal model to human communication of private psychological states): (1) the common properties $\{a, \ldots, e\}$ must be causal properties which (2) are causally connected with the property $\{f\}$ we wish to project – specifically, $\{f\}$ should stand as the cause(s) or effect(s) of the features $\{a, \ldots, e\}$ in the model. Moreover, (3) there must be no causally relevant systemic disanalogies between the model and the thing modeled. Most organisms used in experiments are intact biological systems composed of a set of mutually interactive subsystems. The third condition specifies that there be no causally relevant differences between either the subsystems or their interactions.

In sections 3–5, L&T suggest that conditions (1) and (2) were satisfied, although their arguments are less than compelling. But even if they were compelling, condition (3) remains a substantial stumbling block for them – as it does for most experiments relying on animal models. If this condition is not satisfied, any inferences from the model to the thing modeled are suspect. This condition will always be problematic, however, because researchers can infer that there are no systemic disanalogies – or at least no statistically significant risk of them – only if the model and subject modeled are *causally isomorphic*. Two systems are causally isomorphic if they have all and only the same causal mechanisms. L&T indicate that they think there is such isomorphism when, in section 4, they suggest that they have eliminated any causal disanalogies by adjusting for differences in the subjects' body weight. But they offer no *argument* that the models and subject are isomorphic. It is difficult to see how they could.

This belief that they are isomorphic assumes, among other things, that animal models display relative insensitivity to variations in initial conditions so that (small) evolved morphological, metabolic and neurological differences between humans and nonhuman species (birds, in the present case) can safely be ignored. We argue (1993a; 1993b) that this assumption is highly questionable – especially in pharmacological contexts like those used here (sect. 7). There is a growing body of evidence that animal models may be highly sensitive to differences in initial conditions (small, evolved, biological differences between species), and hence that causal inferences between species are statistically much weaker than has been assumed. (Morphine, for example, sedates humans, but stimulates cats; see Brodie 1962). Moreover, given the widespread existence of cross-species variation in the way pharmacological agents are metabolized, it is difficult to ascertain how the responses in birds may be generalized to humans. Differences in metabolization will alter the gross physiological condition of the animal subjects which will probably modify the animals' internal psychological states. For example, Davis (1979) points out that aspirin (which causes birth defects in rats and mice) is an analgesic for humans, poisons cats, and has no discernible effects whatsoever in horses. Hence, any claim that this animal model explains the human communication of private states is, at best, doubtful.

Perhaps, however, that was not L&T's intent. Perhaps they are merely looking for a model of *animal* communication which might serve as a heuristic device to spur investigation of human communication (see LaFollette & Shanks 1993b). At times it sounds as if that were their intent. If so, perhaps this model moves in the right direction. If, however, they wish to assert that they have a model that gives us any insight into *human* communication, then their conclusions are dubious.

We can reliably report psychological states because they are neither internal nor private

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At the core of Lubinski & Thompson's (L&T's) target article are a number of confusions between different meanings for the same words. The two most important confusions concern the words "internal" and "receptor," especially as they are combined in interoceptor.

L&T describe reports of both drug effects and "feelings, thoughts, and perceptions" as arising from interoceptors in the same way as reports of states such as fatigue or thirst. They assume that when people (or pigeons) discuss their feelings or drug experiences, and so on, they are reporting on the activity of internal receptors. Their question is, then: How could we learn to accurately identify the activities of these internal receptors?

The confusion begins with two very different senses of "receptor." In the first meaning, receptor cells in all of the sensory systems function to transduce energies from the world into nerve activity. The function of sensory receptors is precisely to transform something that is not neural activity into something that is. In exteroceptors, the energy is something like light or sound, and in interoceptors it is tension or pressure. So, in every case we can see two clearly different things. The first is the energy or substance that excites a receptor cell and the second is the subsequent nerve activity.

L&T suggest opioid receptors in the brain as models for the CNS interoceptors which detect feelings, and so on. Unfortunately, this is a very different and systematically misleading sense of the term "receptor." Although we can loosely speak of cells as opioid receptors, when they are functioning normally these cells do not detect the presence of opioids and transduce that information into neural activity. Instead, these neurons fire in response to an opioid neurotransmitter originating in the presynaptic neuron. In fact, the receptors are not the cells, but the sites on the postsynaptic neuron to which the neurotransmitter attaches.

The problem arises when the opiate system is treated as if it were a sensory receptor system. If the opiate system and the visual system were parallel, then there would have to be something that excited these opiate receptor cells that was analogous to the light that excites the retina. The next slippery step is to think that something different from neural activity exists and is transduced in these "receptor" cells. Since the effect of activity in the opiate system is the experience of pleasure, it is all too tempting to think that these opiate receptors are transducers of pleasure. Thus we are led to the logical error that the brain contains both neural systems and things like feelings (of pleasure), thoughts, and perceptions, which are detected or transduced by special receptor cells.

Instead, an opiate injection is much more like a short-circuit. Normally, the system which includes opioid-using neurons is active when some pleasurable event occurs. We might crudely think of it as the system that keeps us eating the steak or attentive to a sexual partner. When neurons of this system are excited by an injection of an opiate, they are induced to fire by this foreign chemical, rather than by the activity of their presynaptic neurons. The sequence of events is like what occurs when you press gently on your closed eye and induce what seem to be flashes of light. These are actually produced by the mechanical distortion of cells in the retina, which fire in this short-circuited way. In both of these cases, the short-circuit produces an erroneous message – there are flashes of light in this room, or this is a great way to spend my time.

The issue is further confused by mixing the two senses of "internal." L&T adopt the most literal sense of internal – inside the skin. A second, more ambiguous sense of internal has traditionally been ascribed to aspects of mind. Since mental events and objects seem real, but not directly observable, they must be hidden, "inside" somewhere.

Long ago, people could assume that the mind was hidden in a geographically interior region of the body, like the brain. But now we know that we will not find a feeling, a thought, or a percept inside there. All we will see are neurons, neurons, and more neurons. Mental objects and events are not, then, "inside" in the geographical sense, and certainly there could not be receptors inside the brain which detect and respond to these mental things.

If this is true, then reports of feelings cannot be reports on the activity of internal brain receptors. At most, an experimentally induced, chemical short-circuit might produce an illusion that one is happy or tired or whatever. This point is clearer if we imagine human beings as subjects in the pigeon study. These people would have to be unaware that they had received a drug or of its effects. If asked how they felt, they would certainly not say they felt the effects of pentobarbital or cocaine, the only events that actually occur geographically inside the brain. Instead, these people would probably say something like "Very mellow, man, like I don't want to do anything," or "Ambitious, ready for action."

If feelings, thoughts and perceptions are not reports of geographically internal events, what are they? The answer proposed by philosophers such as Ryle (1949) is that instead of feelings and so forth being hidden internally, they are abstract patterns of action and behavior. In other words, they are hidden before our eyes, in the doings of people and pigeons. This idea has been developed as self-perception theory (Bem 1972). The premise of self-perception theory is that when people talk about their feelings, they are describing the essentially public patterns of their actions and the contexts in which they act. A great deal of empirical evidence supports this interpretation (e.g., Laird & Bresler 1992). And whereas these patterns of action and context exist in the public world, there is no mystery about how we learn to talk sensibly about them: adults teach us, as they teach us all our talk. In short, to learn to talk sensibly about our feelings, thoughts, and perceptions, we do not need anything more than ordinary childrearing practices.

What's *biological* about the continuity?

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There is a cloying and annoying ambiguity to Lubinski & Thompson's (L&T's) target article that might be expressed by the following sentence (the first half of which is to be read softly and the second emphatically): There really aren't any such things as "minds," "private feeling states," "self-awareness," "intentions," "images," or "combinatory productive language [as a] species-specific form of behavior," *and furthermore, our pigeons can do it all!*

The ambiguity is foisted on other researchers as well. We are told that Epstein and his colleagues "have also used pigeons to synthesize . . . 'self-awareness'" (Epstein et al. 1981b). But the cited paper concludes straightforwardly, colors clearly nailed to the mast: "We have shown that at least one instance of behavior attributed to self-awareness can be accounted for in terms of an environmental history. We submit that other instances, including those exhibited by humans, can be dealt with in a similar way" (p. 695). Epstein et al. do not claim to have synthesized self-awareness: rather they purport to show that the whole notion of self-awareness is myth-ridden and wholly dispensable in favor of "an environmental history."

L&T also ambiguate foes. After quoting Terrace's (1985) claim that naming, and naming inner states in particular, is specific to humans, they immediately state their basic experimental result as if it refuted Terrace, namely, that "arbitrary, non-species-specific communication between organisms based on private events may extend beyond *Homo sapiens* and does not require language." Indeed, in reporting the more extravagant claims of ape researchers (and ignoring the criticism and remarkable retraction of such claims by Terrace and others over the last decade), L&T latterly dismiss their own caveat about language when they conclude "These findings oppose the idea that 'man is the only animal to have combinatory productive language . . . a species-specific form of behavior' (Miller 1967)" and summarize Savage-Rumbaugh et al. (1978) as demonstrating that "organisms other than humans learn to interact communicatively by exchanging arbitrary symbols with one another." So their final position seems to be that apes and pigeons can have the essentials of language.

On the contrary, Miller's generalization seems much more firmly supported today than when he first issued it. Surely it is now wholly bizarre to think of human linguistic communication as an instance of "exchanging arbitrary [non-species-specific] symbols" or one to be wholly accounted for by "an environmental history." Both these points come out clearly in L&T's own example of autism. Although it was common in the 1950s and 1960s to attribute autism to a malign home environment (with Mom the inevitable suspect), current research has clearly shown genetic and physiological causes with characteristic brain abnormalities (Bolton & Rutter 1990; Dawson & Levy 1989; Schopler & Nesibov 1987); specifically, though he may be generally intelligent, the child with autism lacks the native cognitive equipment to develop a "theory of mind" (Frith et al. 1991). [See also Premack: "Does the Chimpanzee Have a Theory of Mind" *BBS* 4, 1978; Gopnik: "How We Know Our Minds" *BBS* 16(1) 1993; and Goldman: "The Psychology of Folk Psychology" *BBS* 16(1) 1993.] Both Down's syndrome and Williams syndrome children, on the other hand, despite having moderate to profound general intellectual deficits, are quite good at attributing feelings, beliefs, and intentions to others (Karmiloff-Smith 1992). Of course it would in a sense be more parsimonious to explain each of these syndromes through a just-so story of shaping through a supposed "environmental history," but such explanations seem false, and where not false, empty or impoverished. After all, if you want your cognitive/affective framework to be equally appropriate to all species (which is the other side of the "arbitrary" and "non-species-specific" stipulation), you are naturally going to impoverish your explanatory possibilities for the sterile and often unfalsifiably vague jargon of behaviorism.

But this brings a further point in its wake. Is there any justification for the restriction to living organisms? L&T begin by noting that Leonardo da Vinci and René Descartes thought mechanical devices could be used to model aspects of human physiology. Imagine a device, "beta," with a crude internal temperature gauge that "monitors private states" to determine whether the experimenter has injected a below normal, normal, or high amount of energy; when turned on by the experimenter, another device, "alpha," sends an electric current through a wire (helpfully labeled "How are you?") to device "beta," which sends back "a reply" by one of three wires, depending on which "private state" it is in; this reply signal turns on an electromagnet, which raises a flag (helpfully labeled "Thank you") and also, depending on which wire carries the current, displays the letters L ("low"), N ("normal"), or H ("high"). The beta device is also capable of stimulus generalization because its internal temperature may be raised by applying a butane lighter or lowered with ice cubes.

Pigeons as communicators and thinkers: *Mon oncle d'Amerique deux?*

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Animal models are often useful in looking for causes of human behavior, although sometimes they simply help researchers to notice things they might otherwise overlook. The target article at first seems to fall into this latter category, but, on reflection, the extrapolation from pigeons to humans seems gratuitous. Nothing new seems to have been gained by the pigeon model, at least in understanding humans, because we already knew everything discussed in section 8.

The opposite extrapolation, from humans to pigeons, seems to do a lot of undercover work, however, particularly in supporting the labeling of the interaction between the pigeons as "communication." In fact, no "communication" of internal states occurs. Communication *of* or *about* private states may depend, as Lubinski & Thompson (L&T) write, upon "interanimal exchange of discriminative stimuli based on the interoceptive state of one of the participants" (sect. 7.1.1, para. 3), but it requires in addition that the receiver of the exchange end up with information about the private state of the sender, which certainly has not happened with the pigeons. The pigeon that "requested" the drug-induced bird to act never finds out anything about the internal state of the drug-induced bird. (Indeed, it is unclear even that anything has been "exchanged.") Whether the drug-induced bird itself knows something about its own internal state is unexamined, as other factors may cause the bird to act as it does. Thus, the "communication" may not even be "based on" internal states. All we know for sure is that the "requesting" pigeon knows what to press when.

Anthropomorphic implication is also present in the term "request." The "requesting" pigeon's attacks on the glass dividing it from the sluggish bird do not necessarily indicate even an intention to get the other bird to do something, let alone a request to "hurry the other along" – it may be that the attacker was simply redirecting its aggression toward the other bird as a result of being unsatisfied. If anything in particular was requested by the bird, L&T remain quiet about exactly *what* it was. The other bird, excited by the attacks, reacts as it usually does, and pecks the appropriate key. No evidence is provided that this pigeon wants to satisfy the "request" of the other pigeon. The fact that the sated bird failed to respond when not seeing the other bird, but did respond when the other bird "requested" whatever it is supposed to have requested, does not indicate that the pigeon's acts are either free from a "mechanistic" interpretation or based on recognizing the intention of the "requesting" bird (as implicitly suggested in sect. 7.2). Rather, it is likely that any means of instigating the bird's movements would have stimulated it to go through its programmed ritual – for example, if the experimenter had tapped aggressively on the window, the pigeon would probably have started pecking the appropriate key (if the other key initiating the sequence were lighted). Indeed, when not sated the pigeon enacted the entire sequence without the other bird's presence; why expect the bird to act when it is sated? The interpretation of both pigeons' behavior is based largely on anthropomorphic inference, such that in actuality a human model of pigeon behavior is provided, and an unsatisfactory one at that.

A pigeon model of chimpanzee behavior and a chimpanzee model of pigeon behavior are also presented, though again they are inadequate. For example, Köhler's (1925/1976) study showed that chimpanzees, without any explicit reward, combined previously uncombined activities, which were learned by exploration initiated by the chimpanzees, in order to achieve a reward. The components of Köhler's task were not shaped by the experimenter, as they were with the pigeons. If Köhler's methods with the chimpanzees were replicated with pigeons, I doubt they would perform comparably. An organism's experience with component parts of a task supports combination of these parts, but chimpanzees seem to need limited scaffolding by an experimenter to solve complex problems, whereas pigeons require extensive scaffolding. Insight, in Köhler's (1925/1976, p. 190) terms, is present when an animal produces a "solution with reference to the whole layout of the field," which pigeons fail to provide. Unlike pigeons, chimpanzees did not need to be reinforced to move the box closer and closer to the banana (indeed, some chimps seemed never to figure it out), nor to contact the banana. In addition, replication is clearly in order: a study purporting to show "self-awareness" in pigeons (Epstein et al. 1981b) akin to that in chimpanzees was itself not only conceptually inadequate but has consistently failed to replicate (Thompson & Contie, in press).

As L&T are aware, extrapolation from ostensively similar behavior to identical causality can be misleading (see discussion in Mitchell 1992; 1993). However, their awareness wanes at times, so that they specifically note and then ignore salient differences between behaviors of different species. They continually point to some minimal level of identity in the behavior of chimpanzees, humans, and pigeons and use this identity to imply identity in the processes leading to these behaviors. Their argument that continuity between species should ensure this identity of process is specious: Should we assume that identical processes are present in the migrations of birds and humans? in deceptions by dogs and people? in imitations by neonates and adult humans? Clearly in some cases there are identical processes in both humans and animals, but just as clearly nonidentical processes can lead to similar behavior. Both the power of restricted inference from animals to humans and the absurdity of unrestricted inference are illustrated in the film *Mon oncle d'Amérique* (1980), when a man leaving his wife is depicted as a clothes-wearing white lab rat while a scientist describes the similarities between rat and human behavior under stress. Perhaps next time the filmmaker should use pigeons.

The role of convention in the communication of private events

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Lubinski & Thompson (L&T) show how it is possible to construct an animal model that at first glance looks like communication about private events in humans. They demonstrate fairly convincingly with pigeons that internal states can act as discriminative stimuli which can be "translated" into arbitrary symbols in order that those symbols can be exchanged with conspecifics. However, the similarity to human communication about internal states is entirely superficial. In this commentary, I argue, first, that L&T's account of communication, whether of private or public events, misses a critical aspect of language that enters into its acquisition and use in humans – its conventional or shared nature. I then go on to suggest that as a result, L&T's account, in its present form, will never be able to capture the full range of human communication about internal states.

L&T put much store in the arbitrary (non-species-specific) nature of human communication and hence of the validity of their model. But arbitrariness is only one, relatively insignificant, aspect of human language. Of vastly more importance in the present context is the fact that human language is conventional. By conventional, I mean that linguistic symbols are understood by the users to carry the same meaning for different users. When I say "cat," I am referring to the same object or kind of object as you are when you say "cat." Furthermore, this sharing of reference is not just fortuitous, I use the symbol "cat" because I *know* that you will understand its reference, that is we share its meaning. This feature is absolutely central to the nature of human language, as can be seen from a consideration of the acquisition of linguistic symbols. It is not the case, as one might believe from reading L&T, that words are simply learned in an operant fashion – mother reinforces child's production of the correct word in the presence of the appropriate referent. Rather, words are initially learned by young children in the context of joint attention, that is, when child and adult are attending to the same object or event. Children from around 18 months will acquire words which they hear others produce when it is obvious to the child that the other is focused on the same object (Baldwin 1991; Tomasello 1992). In other words, sharing reference through joint attention provides the basis upon which such symbols are acquired.

If such is the case for the acquisition of labels for objects that are publicly observable, labels for internal events might appear to present something of a problem. How can one person's internal state be the focus of the joint attention necessary to establish shared meaning? The solution here is to recognize that many internal states have observable, behavioral manifestations. Emotional states provide perhaps the clearest demonstrations. For example, feeling sad manifests itself in expressions of sadness – characteristic vocalizations, facial expression, and posture. In this way joint attention to an emotion can be established, even though in actuality the two persons are attending to different things – the child attends to the internal stimulation of the emotion, the adult to the external characteristics of the emotion (Beckwith 1991). If the emotion label, "sad," is introduced at this time, the child could conceivably acquire the label to refer to the distinctive feelings of sadness.

So far, we have an account of how children might acquire an emotion label for their own internal feelings. However, the extension of such labels includes others' emotions, to which the language learner has no direct access. But the acquisition of the emotion label to refer to the external manifestations of the emotion in others presents no more of a problem than the acquisition of any label for external objects. The emotion label may be introduced when both speaker and hearer are attending to the external manifestations of the emotion in a third person. Thus, the use of an emotion label in these contexts will lead to the symbolic identification of that emotion, including its internal characteristics and external manifestations. Through the acquisition of the label, the child can learn that "this feeling that I have" is the same thing as "what is happening to my brother when he makes that face" (see also Barresi & Moore 1992).

At this point, children are in a position to recognize the equivalence of their own internal states and others' expressions of those states through the use of symbols. The reason this conventional understanding is important for the communication of internal states is that certain kinds of internal states (those that are "about" something or "intentional," in the classic sense of that term) are impossible to appreciate without it. Take the case of belief. To be able to discriminate and then express one's belief about something, as opposed to simply revealing one's knowledge, requires a sensitivity to the possibility for similarity and diversity in knowledge states, for example, "he thinks X but I think Y" (see, e.g., Perner 1991). Without the understanding that both self and other can experience the same kinds of knowledge states, there is no possibility even for the expression of belief, let alone true communication about one's beliefs. [See also Gopnik: "How We Know Our Minds" *BBS* 16(1) 1993; and Goldman: "The Psychology of Folk Psychology" *BBS* 16(1) 1993.]

In sum, although ingenious application of operant principles might allow one to mimic communication based on simple internal states, there is little resemblance to human linguistic communication about internal states. One bird in L&T's model is using symbols based on the internal discriminative stimuli, but there is no way the other bird could recognize them as such. Communication, in the sense of a reversible exchange of symbols, has not taken place – there is no shared meaning. Without an appreciation of the conventional nature of these symbols and the shared nature of the events to which they refer, the use of symbols to express more complex internal states, such as beliefs, will remain forever beyond the scope of such modeling.

Behaviorism, introspection and the mind's I

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Lubinski & Thompson (L&T) have addressed many provocative issues in their animal model of communication based on internal states, and the methodology they developed could have profound applications. I would like to explore a related topic in this commentary: What implications does the behavioristic approach have for the way we conceive of the psychological and physiological processes underlying consciousness and introspection?

Despite Watson's (1913) polemics, consciousness and such related phenomena as introspection and awareness have long been a source of concern to behaviorists. Skinner (1974) posed the essential question in the following way: "What is inside the skin, and how do we know about it? The answer is, I believe, the heart of radical behaviorism" (p. 218).

Skinner's particular point of attack was the (largely Cartesian) assumption that we have privileged, incorrigible knowledge of internal phenomena. That assumption is comprehensively mentalistic, insofar as it assumes a superordinate "mind" that has the requisite sort of knowledge and initiates action, including speaking about the phenomena. Indeed, being against mentalism is a major part of what Skinner's radical behaviorism is all about. Hence, Skinner (1974) suggested that "behaviorism calls for probably the most drastic change ever proposed in our way of thinking about man. It is almost literally a matter of turning the explanation of behavior inside out" (p. 256).

Nevertheless, Skinner (1974) stated that "it would be foolish to rule out the knowledge a person has of his current condition or the uses to which it may be put" (p. 215). The fundamental question is what kind of an organism is a human, such that it can give introspective reports of private states? Skinner (1945) proposed that "being conscious, as a form of reacting to one's own behavior, is a social product" (p. 277; see also Catania & Harnad 1988, pp. 150–217). Section 5.2 of L&T's target article provides a brief but excellent outline of Skinner's version of the ontogenic events that must take place for an organism to report on its own internal state. These events involve the verbal community's basing differential reinforcement on collateral behavior or attendant contextual factors. Thus, the verbal community teaches us certain responses to our internal states. Contrary to Descartes, we need considerable shaping to "know ourselves," at least to the extent verbal responses are involved.

The present summary of how analogous reports could be generated in nonhuman animals suggests a certain continuity in the processes. The experimenters are the animals' verbal community. The various manipulations (e.g., drug injections) constitute the attendant contextual factors upon which the experiments base differential reinforcement. Hence, the situation is extremely complex, particularly in humans. We may "know" we are in pain, but such self-knowledge is not inevitable when the individual faces an emergency, or is in the heat of battle, and does not realize the extent of a painful injury until after the emergency has passed.

Natsoulas (e.g., 1983; 1985) has raised a number of incisive questions concerning the behavioristic approach. For example, consider the following passage from Natsoulas (1983):

> The behaviorist account holds that all awareness of anything *requires* that whatever it is stimulate one or more of our sense receptors. In addition to a form of responding's being necessary for awareness of anything, *the activation of sense receptors is also necessary.* Otherwise, we cannot do what is supposed to be necessary to all awareness of anything, namely, that which is called by radical behaviorism "respond to it." (p. 21, italics in original)

Natsoulas's point is characteristically well-taken: for differential responses to develop to private states, there must presumably be some sensory contact with them. So far as we know, this sensory contact is provided by the interoceptive and proprioceptive nervous systems (e.g., Skinner 1974, pp. 20–21). How peripheral or central this contact should prove to be is an empirical question. Ordinarily, bodily states or activities of the intact organism are the objects of introspection. The interoceptive and proprioceptive nerves that detect these bodily states and activities are the medium of contact. What we call the stimulus is the bodily condition, not necessarily the activity in the sensory nerves produced by the bodily condition. More complex cases such as "phantom limb" pain or "referred" pain suggest that the physiological mechanism is not restricted to responding to stimulation from the sensed area, but rather will respond given peripheral stimulation of appropriate nerve fibers.

Of course, even with a plausible account of introspective reports, terms describing private events tend to be inexact for two reasons. First, the verbal community that teaches us to apply terms to our private events works under the handicap of privacy: the verbal community does not have precise access to the described conditions, and there may be some inconsistencies in the actual antecedent conditions under which reinforcement is administered. Thus, there is some variation as the usage develops. Second, and more in keeping with the physiological question, Skinner suggested cryptically that we may not have nerves going to the right places (Skinner 1974, pp. 221–23).

Natsoulas (1985, p. 93) has questioned Skinner's statement that we do not have nerves going to the "right places." Presumably, Skinner was addressing some of the more extravagant claims about our nervous system. For example, the structuralists claimed that with proper training, one might introspect as many as 42,415 different sensations (e.g., Lundin 1991, p. 88). The sensations were presumably different activities of the CNS. Skinner argued that the problem of privacy prevented the verbal community from establishing such a fine-tuned repertoire in us. Moreover, we don't have sensory nerves going to places that make it possible for us to discern that many sensations, even if our interactions with the verbal community were favorable. We reveal a lack of sensitivity in a two-point limen test in the lower back because of low representation of sensory information. Similarly, we have a lack of sensitivity to many activities going on inside our skins because of low representation of interoceptive and proprioceptive information. People who make various sophisticated claims about their "sensations," "feelings," or "states of mind" are simply making fanciful statements that presumably have little validity.

Watson was of course biased against including any central phenomena. He originally thought the only possibility was for peripheral processes to influence behavior. Skinner (1957) declined to "make guesses about the muscular or neural substratum of [covert] verbal events . . . we can talk about both [covert and overt] forms of response . . . without identifying physiological mediators" (p. 435). Nevertheless, Skinner (1957) repeatedly appealed to "subaudible," "incipient," and "inchoate" forms of verbal behavior (e.g., pp. 143, 400). Skinner (1957) similarly noted "difficulties in assuming that covert behavior is always executed by the muscular apparatus responsible for the overt form" (p. 435). Perhaps the point of contact between sensory and motor systems, or between different aspects of sensory systems, will prove to be fairly central after all. Natsoulas (1985, p. 89) has raised this important matter by asking, "Why cannot brain processes be objects of introspection?" but it has not been resolved. Standard texts such as Shepherd (1979, Ch. 16, e.g., p. 365) point out that perhaps as many as one-third of the motor neurons in the cortex are located outside the traditional motor cortex and are actually in classical somatosensory areas. Perhaps the overlapping pathways with the collaterals and various other projections provide a relatively central point of contact between "motor" functions and "sensory" functions, given the traditional dichotomy. Thus, there may well be relatively central points of contact that plausibly account for the development of some kinds of introspective reports (cf. Skinner 1974, p. 223). However, even if talk of this kind of sensory contact is valid, the sensory contact is probably insufficient to support the many extravagant claims made by contemporary psychologists about the relation between brain processes and behavior as it involves introspective reports.

Private states and animal communication

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Wittgenstein, master philosopher, had a challenging but ultimately flawed view of mental language and mental life. Expressed with characteristic obscurity, his view (1953) seems to have been reasonably close to the following. First, a private language is impossible. By a private language, he seems to have meant one that has a referential or naming function with respect to private events, characterized as events to which only one individual has privileged or direct access. Since he clearly took his view as applicable to our own mental language, he was thus denying that our own mental language is private in this sense. Private criteria or tests, such as memory fixation, could not serve to fix reference, especially repeated reference, to the same type of event, he argued. Hence, linguistic rules governing mental concepts must be grounded in publicly accessible tests. So there are logical relations connecting mental concepts with public input/output in public contexts, and no logical relations with private, uniquely accessible, introspectively gained information. It would further seem to follow that reference is the wrong semantical model for mental language.

Lubinski & Thompson's (L&T's) position opposes this at a number of points. I have little critical to say about their fine study. Rather I shall attempt to show how thoroughly they dispose of Wittgenstein's distinctive and sophisticated brand of behaviorism.

It is clear that L&T take their subject matter to be states inside the skin, about which the individual has a distinctive direct source of information (sect. 5). The model for directness is stimulation of internal chemoreceptors, or interoceptors; no one else is connected to my interoceptors. A peripheral issue here is that Wittgenstein took himself to be opposing dualist, nonphysical states; but, like behaviorists in general, he used a cannon to shoot a mouse, since the blanket denial of a referential role for mental language applies equally to physicalist reconstruals such as L&T's.

L&T's main argument is that behaviorist reinforcement theory finds the successful systematic learning of mental symbolic productions (language) hard to explain (sect. 5.2), while adding "internal stimuli" and receptors for them allows reinforcement to fix regularities in the behavior easily. Since overt stimuli correlate poorly with symbolic mental behavior, the relevant concept formation would remain mysterious if these determined the reinforcers.

Here it is crucial that L&T's model involves the use of signs that are conventionally associated with the states they represent. What is to be explained is learned language, not natural behavior, since the latter presumably has a more ready explanation in terms of environmental reinforcement or genetics.

One can also see why nonhumans such as pigeons are used. Innocent of all conventional symbolic language, they provide an ideal tabula rasa for language learning; the outcome, statistics show, is a simple and highly regular form of learning. Furthermore, the importance of a communicative situation is to model reference more persuasively. How otherwise could reference be modeled if but one animal were involved? Induce a type of inner state and train a type of symbolic response. This is well short of representation, however. For that, one wants at least a symbolic communication, since reference should have both causal input and causal output to the referent. Here there is some cross-purpose with Wittgenstein, who, as we saw, focused on opposing a language for one. But the meeting-point is our understanding of our language, which Wittgenstein clearly aimed to be advancing.

Similarly, reference must be fixed, that is, symbol types must correlate with inner-state-types. Weak reference, that is, poor discrimination, especially the fixing of irrelevant aspects of types, then, explains autism. Here, Wittgenstein found trouble with memory as the discriminator of inner-state-types, but he was obviously operating with a restrictive view. Rather, it is a simple neuroanatomical argument. We can ensure gross neuroanatomical state-types by the interoception of types of neurologically active chemicals. These interoceptor states are then sufficient explainers of the observed symbolic regularities. Thus, the philosophers' "reidentification" of "state-types" is ensured by cause, not logic.

This methodology demonstrates plausibly the internality/privacy of the communicated referents: there were no obvious overt differences between stimuli initiating the communicative sequences, simply injections (including a placebo). Any differences discriminated must therefore be of interoceptor states. This also increases the significance of observed similarities of verbal behavior as correlated with similarities of injected drugs, since it must be similarities in the interoceptor state complexes that explain the symbolic similarities here. The generalization of reliable correlations to similarities and differences between unlearned and learned drugs demonstrates that what we have here is not merely an artifact of the experimental learning situation but is best explained by similarities and differences in the interoceptor referents.

L&T stress the importance of the emergence of novel behavior, namely, aggressive prompting by the decoder bird when food satiation makes the drug-cue bird fail to respond in the communicative sequence, the extinction of the satiated drug-cue bird's response when the aggressive behavior is concealed though not when the drug-cue bird is not satiated, and subsequent reappearance of response when concealment was removed. This again shows that the behavioral sequence is not merely a function of the learning situation but rather of the novel aggressive prompting which produces an otherwise improbable response as outcome. This models communication and thus reference more successfully in that the birds' awareness of each other is an essential ingredient which is absent when an unsatiated bird continues the sequence, irrespective of overt or concealed aggressive prompting by the other. It also models something Wittgenstein was not addressing, namely, human confrontational psychotherapy. Here satiation models the target of the psychotherapy, the defective human capacity to discriminate feelings or report them.

Finally, the connection with specifically human concepts is reinforced by the use of drug-types that have a pattern of similarities and dissimilarities for human interoceptors, as revealed in highly correlated reports on standardized tests. When similar striking classificatory correlations are manifested in animals, it is reasonable to conclude that the animals are modeling the semantical abilities of humans simply but successfully.

Communicative acts and drug-induced feelings

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I will leave criticism of Lubinski & Thompson's (L&T's) basic experimental design to others (e.g., Savage-Rumbaugh 1986), and focus on two terms that are central to L&T's thesis: (1) communication and (2) internal states. My disagreement with L&T's use of these terms is the basis for my disagreement with much of what they say.

Use by L&T – and others – of a standard, limited definition of communication causes incessant controversy about such research. Of the many existing definitions of communication (see Beer 1982), none expresses what (I believe) separates communicative competence from other behavioral interactions. Most definitions of communication provide some variation on a single theme: that communication is a means by which one organism transfers information to another for the purpose of influencing the response of the recipient (Catania 1990). These definitions describe, at best, a *communicative act*. Only Smith (1991) comes near to describing the critical, additional capacity that I believe defines *communicative competence:* full competence requires that both the sender and the receiver have the capacity to choose, from among various possible sets of rules that have been acquired or taught, the set that appropriately governs the current processing of the information to be transferred. According to such criteria, a subject that is limited to sending or responding on the basis of a single set of rules, such as pressing a few different keys in a single context, does not have the occasion to demonstrate complex communicative behavior.

The types of research tasks used by L&T do not require their subjects to evaluate the context that governs both the choice of information to be transferred and the information itself. L&T's protocol does not even require understanding any questions. For subjects to have achieved true communicative competence, and not just the ability to engage in a single type of communicative act, they must be able to take into account the type of question that is being asked and the context in which it is being asked. Whether subjects can report that they are either sedated or energized is irrelevant to communicative competence if they cannot comprehend and respond equally well to a question about an entirely different state (anger) or realize that the same physical question in a different context requires a different answer. Thus the subject must be able to respond appropriately when presented with a choice of several different possible contexts ("Am I being asked about my level of hunger?" "Am I being asked about the clarity of objects I see?" "Am I being challenged for occupancy of this territory?" "Is the challenge coming from a stranger or a neighbor [i.e., how upset should I be]?" and so forth) prior to choosing among several different appropriate within-category responses ("I'm ravenous," "I'd like a snack"). In contrast, responses based on a single type of question do not require understanding the question or its context: such responses require merely the association of a few learned responses with a few internal or external situations.

L&T may argue that my distinction is one of degree, rather than kind. I think not, but such a discussion would encompass my remaining space. Whatever the basis, the distinction is not trivial but critical for separating full communicative competence from simple associative learning.

My second point involves L&T's use of the terms internal states and "feelings." First, I question L&T's assumption that their pigeons must necessarily be reporting internal states or "feelings." Second, I question L&T's insistence that animals express their internal states in operant (verbal) terms.

The drug-induced "feelings" that are reported by L&T's pigeons may not be about internal states. A bird could be reporting that stimuli are blurred, hyperclear, or normal; that is, rather than reporting "feelings" it could be reporting its perception of stimuli under different drugs. Reporting perceptual information is not the same as reporting information about sadness, happiness, anger, or fear. L&T's design thus has the potential to conflate these different types of information.

Moreover, internal states can be reported in great detail without operant constraints. Ethologists can quantify symbolic actions in terms of how particular species express internal states and levels within such states. Auditory playback studies, for example, detail the extent to which a bird demonstrates aggression by providing careful gradations of its vocal responses to various permutations of broadcast songs in specific contexts (see Dabelsteen & Pederson 1990). Normally, these hierarchically organized, learned symbolic expressions (sect. 2, para. 1,2) are communicated to an interloper. Researchers test stimulus generalization by examining similarities in the bird's response to songs of various intruders or by quantifying how much a test song must differ from that of a neighbor to be interpreted as coming from a stranger. Other animals often maintain status by expressing internal states (aggression) symbolically, especially after becoming dominant (see de Waal 1986).

So why do L&T (sect. 8, para. 1) insist on non-species-specific, operant responding for communicating internal states? Is it because operant responses to report purported internal states can be seen as labeling, a higher-order process than direct expression of the state? But such responses are not isomorphic with labeling and may cloud, not clarify, the basis for a response. A bird's use, for example, of one key to report hearing an intruder's song versus another key for a mate's call cannot be uniquely interpreted as reporting internal states of aggression versus affiliation; responses could be based on ability to discriminate between the physical form of the two vocalizations. And any child, autistic or not, given an extrinsic reinforcer (e.g., M&Ms) for saying "I'm angry" (sect. 8, para. 1), would probably be confused by the entire exchange. Little is gained by the addition of operant constraints.

What L&T apparently wish, but fail, to do is provide their subjects with a referential, symbolic means of expression. Such symbolism may exist in nature; for example, L&T might interpret a bird's initial singing to an intruder (a symbolic threat) as "I'm angry." Might an autistic child who observes a session with models, a punching bag, and the label "angry" learn to use the label expressively? Might a bird that is taught a repertoire similar to that of a young child learn to express a *number* of internal states appropriately and conclusively?

In sum, I agree with L&T that an animal can communicate internal states to another individual, but I disagree that such behavior is shown by their subjects. My primary subject, an African grey parrot (*Psittacus erithacus*), uses English speech to ask for appropriate items when hungry, thirsty, or in need of stimulation, and to refuse items that differ from his specific requests (Pepperberg 1988). Whether he and my other birds can learn to express less easily quantified feelings (of love, anger, fear) via symbols rather than natural actions (allopreening, biting, shrieking, respectively) is yet to be determined. My disagreement with L&T's conclusions and claims for their subject's abilities is merely based on their definitions of communicative competence and internal states.

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Animal models of human communication

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My guess is that most readers will react to Lubinski & Thompson's (L&T's) article in keeping with their own previous views concerning the cognitive capacities of nonhuman animals (in this case, pigeons). Because my own view is that the capacities of nonhuman animals often differ from human capacities in degree rather than kind, I find L&T's main thesis (i.e., that nonhuman animals can be used to model human communication about private states) to be perfectly reasonable.

If nonhuman animals can learn to communicate about public events, I see no reason why they should not be able to communicate about private events. After all, despite the fact that stimulus control tends to be more slippery with private events than public events, private events are no less "real" or discriminable than public events. As Skinner (1953, p. 257) wrote 40 years ago: "We need not suppose that events which take place within an organism's skin have special properties for that reason. A private event may be distinguished by its limited accessibility but not, so far as we know, by any special structure or nature."

Among human adults, verbal reports of heartburn (based on interoceptive stimulation) are not considered more difficult to make than verbal reports of sunburn or verbal reports about the burning sensation from a candle (based on exteroceptive simulation). Likewise, we do not generally regard the identification of anxiety (a private state) as more impressive than the identification of anxiety-producing stimuli (public events). Why, then, should we be surprised to learn that nonhuman animals can report on their private states? [See also *BBS* special issue on the work of B. F. Skinner, *BBS* 7(4) 1984.]

Perhaps introspection seems uncomfortably close to consciousness, an endowment historically reserved only for humans. Or perhaps nonhuman introspection is surprising because verbal reports concerning private events seem to require greater cognitive development than verbal reports concerning public events. Children usually learn to identify public stimuli ("Daddy," "ball," "dog," etc.) long before private stimuli ("sad," "hungry," "headache," etc.). Hence, from an ontological perspective, verbal reports of private events may appear more sophisticated than verbal reports of public events. Base rates may also partly explain why introspective reports seem beyond the ability of a pigeon. In the wild, introspective reports among pigeons are as rare as ice skating among bears or ball balancing among seals. Whatever the reason, however, L&T's account makes one thing clear: it is not pigeons' introspective abilities that are limited, but rather, human estimates of pigeons' abilities.

In 1985, Terrace (p. 1026) stated that "human beings are able to name their inner states; animals are not." Yet this categorical assertion of unique human ability, like many before it, has now been conclusively refuted. Indeed, the work of L&T takes its place among several other pathbreaking reports of "human" abilities among nonhuman animals. For example, Epstein et al. (1981b) showed that pigeons could use mirrors to locate body markings which were not directly visible. Pepperberg (1990) taught an African gray parrot to verbally identify the name, shape, and color of objects chosen from a set of 100 possible combinations. Beninger et al. (1974) taught rats to discriminate among their own behaviors and press levers corresponding to whether they were face-washing, walking, rearing, or immobile. Huffman and Seifu (1989) discovered that wild chimpanzees, when ill, sought out bitter-tasting medicinal plants and medicated themselves in appropriate dosages. And Griffin (1976; 1978; 1984; 1992) has written three books and a *BBS* target article enumerating many other animal abilities that were once thought to be the sole province of human beings.

If I have any reservation about the model proposed by L&T, it is not that the model is unreasonable, but that its clinical value is limited. For example, although L&T discuss a number of parallels between their pigeon subjects and autistic children, I see no straightforward way of using their model to help alleviate autism. I say this as someone who has conducted behavioral research with pigeons and who has been a behavior therapist for autistic children.

My reservation is based largely on three considerations. First, because "tacting" (to use Skinner's term) is so foreign to pigeons and stimulus control is so daunting when private states are concerned, the process of preparing test subjects is extremely labor intensive. L&T (both highly accomplished researchers) spent 10 months, six days per week, just to establish reliable interoceptive discrimination in their pigeons. This amount of preparation would be prohibitively expensive for most clinical uses (e.g., testing compounds for use as "pharmacological prostheses").

Second, although animal models are often used to test whether psychopharmacological agents impair functioning in well subjects or ameliorate symptoms in unwell subjects, in the model proposed by L&T researchers would be searching for ways to improve the performance of well subjects (i.e., healthy pigeons). That is, unlike the clinical population being modeled, the pigeons themselves are not autistic, and there is no reason to suppose that, for example, a pharmacological agent which improved the performance of healthy pigeons would reduce the communication difficulties of autistic people.

Finally, to my knowledge, there is no empirical evidence that the communication deficits of autistic or retarded people are mainly a function of interoceptive impairment. Because autistic and retarded people have difficulty communicating about public events as well as private states, a more parsimonious explanation would be that their communication skills are faulty *in general*, and that their debility arises from mechanisms subserving both publicly based and privately based communication.

Of course, this reservation concerning the clinical value of the model does not detract from the theoretical importance of Lubinski & Thompson's work, and I do not mean to suggest that the authors advocated a specific pharmacological model for curing autism. If they believe their model can be adapted for such a purpose, however, it would be worthwhile hearing their thoughts on how it might be done.

How do we know when private events control behavior?

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Lubinski & Thompson's (L&T's) review of experiments in which private events come to control behavior through conditioning is significant because it shows us an effect without the usual obfuscation caused by the verbal instructions so intimately tied to human experiments. If a pigeon can be conditioned to discriminate its state (private event) as induced by one drug from that of another, then it becomes more believable that human responses are equally controllable in this manner. But if private events' control over verbal behavior is conditionable, then it also follows that people will be conditioned to varying degrees and to varying stimuli. Furthermore, if we do not know that conditioning history, it follows that we cannot know exactly what private event (if any) is controlling a verbal response. In addition, if we do not know the degree to which any particular private event is controlling a particular response, we also do not know, especially if the conditioning of the verbal response to the private event is not strong, to what extent external stimuli might be controlling the very response that we are using to inform us concerning the private stimuli.

On the one hand, we have all (or almost all) suffered from some pain which we were unable to share. That kind of stimulation has been quite real for us; indeed, to use Skinner's (1945) examples, we have altogether too often found our toothaches to be more salient than our typewriters. Almost all of us, on the other hand, have found it convenient to say we are in pain when we are not, allowing our verbal behavior to serve as an avoidance response, such as not having to do something aversive. Furthermore, when the doctor is palpating us and asking, "Does that hurt?" while moving across an area we complained about, our psychophysical judgments are not always so very accurate or at least consistent as either the doctor or we would have preferred. Faint pains are not very much more easily discriminable than faint lights or sounds.

It becomes clear then that given the condition that our behavior is controlled by a stimulus to which no others can be exposed, we need to be able to gauge the effect of external stimuli present at the time of the verbal response. Fortunately, we have some relevant experiments. Some years ago, Salzinger and Pisoni (1958; 1960; 1961) and Salzinger et al. (1964) showed that one can increase reports of private events such as "I am sad"; "I hate"; "I love" or decrease them by the simple expedient of verbal reinforcement in the course of an interview or even in the course of a monologue. These studies were inspired in part by the oft-reported phenomenon of flatness of affect in schizophrenic patients. Beginning with the fact that flatness of affect is ascertained by interview, these studies examined the interviewer's reinforcing behavior and found that one can increase or reduce affect, thus showing that "self-report" may not reflect private events but rather how much the interviewer reinforces the patient. In addition, normal individuals hospitalized for physical ailments did not differ from schizophrenic patients in number of affect statements during the operant level; that is, when no reinforcement was administered, the number of statements reflecting private events of affect did not differ.

Further pursuit of the comparisons showed that a difference arose only when comparing rates of extinction of the response class of affect. Schizophrenics emitted fewer statements about their private events after reinforcement had been discontinued, that is, during extinction, than did a comparable group of normal individuals (Salzinger & Pisoni 1960). Schizophrenic patients tend to respond to reinforcers as much as normals, but when the reinforcement is left out, they tend to extinguish more rapidly in their private event statements. Thus we see that the extent to which their statements reflect private events is at least partially a function of the external reinforcement. Shallowness of affect may reflect a combination of resistance to extinction and the response class of affect rather than affect alone.

A comparison was also made between those schizophrenic patients who were released from a state hospital and those who remained in it after a period of six months (Salzinger & Portnoy 1964). Those who left the hospital showed a greater sensitivity to changes in reinforcement than those who remained in it. Thus, speaking about private events, or, if you prefer, responding to their private event of affect, differentiated patients from one another not in terms of number of such responses but rather in the degree of change that they showed when reinforced or not. Patients with a better outcome (those who left the hospital in six months) tended to increase the number of affect statements when reinforcement was contingent on their emission and tended to decrease those statements when reinforcement was discontinued. How shallow in affect was any given schizophrenic patient? The answer has to be given in terms of the condition of reinforcement.

Other experimenters have examined private events, for example, Schachter and Singer (1962). Subjects given injections of epinephrine were asked to fill out forms about how they felt after witnessing different behaviors under different instructions. When they were informed about what physiological effects to expect, they tended not to be influenced by a confederate of the experimenter who acted either angry or happy; on the other hand, when they were not informed about the effect they would experience (the private event), they tended to report the emotion (private event) displayed for them by the confederate. This is clearly another example of how the tenuousness of our conditioning histories with respect to our private events makes us sensitive to external stimuli.

Now that we are confident about the conditionability of responses to private events in animals, our next move should be to study that process in human subjects, perhaps even conditioning their behavior in a similar way.

Cross-fertilization between research on interpersonal communication and drug discrimination

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This commentary concentrates on the advantages and limitations of using drugs to produce private states, and notes some possible extensions to the work that Lubinski & Thompson (L&T) have carried out to date. L&T have exploited discriminative stimuli produced by drugs in order to attack a broader issue in behavioural analysis. The special advantage of drugs for this purpose is the control they afford over interoceptive stimuli. The wide range of qualitatively different stimuli associated with distinctive drugs and the ease of presenting stimuli reproducibly and at precisely specified times must all facilitate implementation of the complex schedules that L&T use. In addition, the very robust nature of conditioning with drugs as stimuli, as compared with other interoceptive conditions, is an enormous practical advantage. The assumption is made that principles learned from studies with drugs apply also to other types of interoceptive stimuli; while it seems that such a view is generally correct, the present line of work could usefully be extended to include communication of some private states produced by other means.

For behavioural pharmacologists, it is interesting that at several points L&T emphasize neuropharmacological mechanisms as sources of stimuli, referring to opioid and GABA receptors for example. They are wise to interpret drug cues in this way because there are numerous well-documented examples of drug-produced stimuli that have been strongly linked to drug actions at particular receptor sites. In contrast, there are relatively few instances where specific subjective phenomena have been related to drug-produced cues; most often, it is the stimulus complex, the combined effect of several components of drug action as experienced subjectively, that probably forms the basis of a cue. In one of the few exceptions, it has been argued that the discriminative stimulus effect of pentylenetetrazol is due to an anxiety-like state; nevertheless, this position remains controversial. Preston and Bigelow (1991) have written the most detailed review to date of the relations between subjective state and discriminative effects based on studies where they were assessed simultaneously in human subjects, and a number of discrepancies emerged. Some of these instances may perhaps be resolved when fuller data are available, but perfect correspondence seems as unlikely here as with any other correlation between different psychopharmacological systems. In most cases, it seems to me that drug-produced stimuli have rarely been related to any one subjective effect of the substances concerned.

I doubt that it is the intention of L&T to imply that the communication of private states is nothing more than the communication of neural correlates of behaviour such as activation of receptors for drug action, and in this respect there may be definite limitations to what can be achieved by chemical means. The pharmacological approach to private states could therefore be complemented by other methods of manipulation, perhaps including deprivation conditions, electrical stimulation (several studies document discriminative stimulus effects of intracranial electrical stimulation), and consequences of recent behavioural events.

A small number of very interesting drug discrimination studies test for similarities between stimuli produced by drugs and consequences of more naturalistic social interactions. Thus, Vellucci et al. (1988) described partial generalization from the pentylenetetrazol discriminative stimulus in rats to stimuli associated with defeat in agonistic social encounters. Leidenheimer and Schechter (1988) studied discrimination of the related β-carboline drug FG 7142. Like pentylenetetrazol, FG 7142 is thought to produce an anxiety-like state, and it was reported that there was partial generalization from it to the states produced both by footshock and by exposure to a novel apparatus. Miczek (1991) reported that social defeat in rats produced tolerance to the analgesic but not the discriminative stimulus effects of morphine. All these experiments need replication and extension; nevertheless, they suggest additional routes for extending the generality of findings of L&T to a wider range of stimulus conditions.

In conclusion, the target article marks a quantum leap towards solving the very difficult problem of the relationship between the discriminative stimulus effects of drugs and subjective state. One of the biggest difficulties here is that, operationally, we know what we mean by a discriminative stimulus but we are less sure what we mean by a subjective effect. Analyses of the type described introduce new concepts as well as new technology into this area; they help in the development of operational parallels to subjective effects; they may also enable behaviourists to contribute more effectively to domains of psychology and psychiatry where their influence has been limited by sometimes incorrect perceptions of their theoretical orientation towards studies of private events.

Are some mental states public events?

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The target article relies on a distinction between private and public mental events (sect. 5, para. 1), a distinction that is intuitively appealing but does not withstand critical examination. Lubinski & Thompson's (L&T's) definition of a private event uses a first-person criterion and an internality criterion. Their first-person criterion defines private mental events as those about which only the subject speaks with unchallengeable authority. But L&T also believe that private events are part of the causal stream – that is, that they are caused by observable external events and may in turn cause observable external events. The notion of mental causality does not sit well with the notion of mental privacy because any event that is in the causal stream is providing constant public notice of itself.

L&T seem to be aware of this problem and so include within their definition of the first-person criterion the requirement that private knowledge be gathered by direct rather than indirect means. They seem to mean a distinction between a direct neural connection with the action and an indirect connection via behavioral observation. This distinction has an ephemeral appeal. Some neurons are relatively directly connected to activity in some other neurons. For instance, there are neurons in my spinal chord that clearly have direct access to information coming in from the "pain" sensors in my big toe. But this is a long way from saying that "I" have direct access to pain in my toe. First, activity in afferent pain fibers is not identical to pain. Second, "I" am not my spinal nuclei. So the part of my nervous system that decides whether my big toe hurts is not directly connected to the toe in this sense and is thus making a fallible inference on the basis of information from a variety of sources.

Still, a direct connection might be one that was inherently more reliable than an indirect one. In this case, to demonstrate that I have direct access to my mental states I need only show that I am better able to predict the causal consequences of these mental states than others. But this is not always the case. Sometimes, others predict my own reactions better than I. So, if L&T hope to validate the privacy of some mental states by the superior ability of the subject to predict that state's causal consequences, then they must concede that some mental states are public rather than private. Such an approach would suggest, among other things, an experiment in which their observer pigeons were given the opportunity to assess the drug state of their comrades without any communicative help from the drugged animal itself. I would be amazed if observer pigeons took as long as six months to learn to distinguish between a pigeon on cocaine and one on opium. Does this mean that drug states are public mental events?

The criterion of internal origin is as problematic as the first-person criterion. All the discriminated events in this study, whether they were injections or lights, began outside the body, were mediated within the body, and were realized in discriminated operants. The decision to focus on some events as internal and others as external thus requires some additional justification.

One obvious suggestion is that internal stimuli are those that *appear* internal to the subject and external stimuli are those that *appear* external. But this suggestion also fails. Consider, for instance, the irritating manner in which other people often behave when I have had too much coffee to drink. Is this a private mental event? In deciding whether an event is internal or external on the basis of the subject's experience, we must remember the Gelb disk. A Gelb disk may appear luminous because we assume that the light coming from it arises "internally" rather than "externally." But in fact, the disk is cleverly illuminated from a concealed light source. When this fact is demonstrated by placing a piece of paper in the path of the hidden light that illuminates the disk, the disk is seen as illuminated rather than as glowing. Thus, luminosity is not a characteristic internal to the disk but a characteristic of its relationship to other things in the visual field. Similarly, irritability is not a characteristic internal to individual persons but a characteristic of individuals' relationship to persons and objects in their surroundings. If the irritating aspects of my surroundings are cleverly "illuminated" with caffeine, I will perceive myself as good-humored and my environment as irritating. Only when my excessive coffee drinking is pointed out to me do I perceive that the world is normal and I myself am drugged.

The experimenters present no evidence that the pigeons are experiencing their drug states as internal. Opium and cocaine may alter the pigeon's perception of its world in any of a variety of ways that may serve as a cue. Opium may make the cage feel warm. Cocaine may make it feel small and confining. And so on. For every hypothesized "internal event" there is a hypothesized "external" event that will produce effects that are indiscriminable to the pigeon.

The distinction between private and public events is a conceptual rather than a physical one. I have written elsewhere that psychology is the field that deals with the contrast between first- and third-person accounts (Thompson 1987). This rational reconstruction of the field of psychology fits nicely with the majority of its concepts, most of which seem to be concerned with explaining discrepancies between first- and third-person accounts. One of the most widespread of such explanatory psychological constructs – in fact, so widespread that we are hard pressed to think of it as such – is the distinction between private and public events. But like all such constructs, it is a cognitive achievement, not a fact of nature. As such, it is something to be explained by our cognitive theories, not a foundation on which these theories may be built.

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The assessment of intentionality in animals

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The ability to communicate to others the nature of one's private states must play an important role in the survival of animals (especially those that live in complex social groups, such as humans and many primates). For young animals to receive proper care, the communication of certain private emotional states (e.g., distress) must be present at birth in a wide variety of species. The question then is not whether private states *can* be communicated, but whether an animal is capable of *learning* to communicate either an arbitrary private state (i.e., one that it would not communicate naturally) or an emotional state by means of an arbitrary response (e.g., learning to press a bar or use language when angry, rather than striking out).

Lubinski & Thompson's (L&T's) question of whether animals can learn to communicate about their private states needs to be separated into two subquestions. The first is whether an animal has the ability to use an internal state as the basis for arbitrary (i.e., learned) behavior. The second is whether the animal can communicate that state to another animal.

The answer to the first subquestion is clearly yes. Once a rat has learned where food, water, and its nest are located in a complex (maze) environment, it surely can run directly to food or water depending on its (private) state of deprivation (i.e., hunger or thirst). Furthermore, as L&T note, the ability of an animal to discriminate between drug states (i.e., make one response in the presence of one drug and a different response in the presence of a second drug) is a well-established method of asking whether two drugs create different internal states (see, e.g., Overton 1987).

Thus, the important aspect of L&T's overall question involves the second subquestion: whether these private states can be *communicated* to another animal. The issue of communication is a much more complex one. If, for example, an animal cries out when attacked by a predator and, as the result of those cries, others of the prey species take evasive action, one would not necessarily conclude that the attacked animal was *communicating* to the others. For the concept of communication to have meaning, one must be able to demonstrate that a *purpose* or *intention* of that behavior is for it to be detected and responded to by others. Unfortunately, there is no easy way to assess intentionality.

This problem is not unique to animals. In humans, for example, one of the most important and difficult judgments that must be made by jurors in a criminal trial is the intentionality of the action that caused an injury. In such a case, the individual best able to provide that information, the defendant, may not be a credible source because of the aversive consequences of an incriminating response. In other words, just as with animals, the response given to the question "What was your intention?" will be strongly influenced by the consequence of the response. Thus, in L&T's experiment, one's ability to infer intentionality is obscured by the consequences (food) of the pigeon's response.

L&T's experiment was based on a similar experiment by Epstein et al. (1980). It should be mentioned that Epstein et al. published their results to highlight weaknesses in a similar experiment involving chimpanzees (Savage-Rumbaugh et al. 1978). To underscore that it was inappropriate to conclude that purposive communication took place between the chimpanzees, Epstein et al. used response keys with English words written on them (e.g., "what color?" and "thank you"). Clearly, the authors were not presenting evidence that the pigeons were reading. Rather, they were suggesting that similar evidence cannot be used to conclude that the chimpanzees were reading. Furthermore, in L&T's experiment, the fact that the second pigeon was able to use the behavior of the first as the basis of its own response (i.e., as a discriminative stimulus) does not allow one to conclude that the *purpose* of the first pigeon's behavior was to communicate.

L&T apparently recognized that the motivation provided by rewards of food or water was sufficient to account for the behavior of both birds because the pigeons were later tested when not deprived of food and water. Because the birds continued to supply information concerning their drug state when there was no apparent external reward for doing so, L&T suggest that the pigeons were motivated to communicate.

Do L&T mean to imply that the sated pigeons were motivated by altruism ("I don't need food or water but maybe my partner does.")? Or do they mean to imply that the pigeons were motivated instead by the desire to communicate? Is it not more parsimonious to conclude that there was residual primary motivation present (i.e., the pigeons were not completely sated) or that food in the chamber was subjectively better than food in the home cage?

L&T's anecdotal suggestion that the drugged pigeon appeared to solicit a trial-initiating response to the "How do you feel?" key from the other pigeon does not clarify the issue. It is likely that the drugged pigeon was merely trying to peck the key in the other compartment. Any notion that this behavior represents an intent to communicate goes well beyond the data.

More to the point of intentionality is L&T's observation that the key-pecking behavior ceased in the absence of the other pigeon. This finding, however, merely leads to the hypothesis that the other pigeon served as a discriminative stimulus for the first pigeon's responding.

My purpose here is not to deny that animals might be capable of intentionality. I only suggest that the procedures presented here do not provide an adequate context in which to test that hypothesis. To provide convincing evidence of an animal's cognitive capacity one must elicit behavior that cannot readily be accounted for by some noncognitive learning process (for a discussion see, e.g., Galef 1988; Zentall 1988; Zentall et al. 1984). Given the difficulty in assessing intentionality in humans, I suspect that convincing evidence of such a process in animals will be very difficult to provide.

What's the stimulus?

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I have long argued (Zuriff 1984; 1985, pp. 86–87) that private events, as conceptualized by Skinner, are theoretical entities. My reasoning is that these hypothesized events, along with their associated processes, are inferred rather than observed. They therefore qualify as hypothetical constructs rather than as empirically observed stimuli and responses. My position has been criticized by radical behaviorists (e.g., Moore 1987) who reject theoretical terms but accept the concept of private events. The work reviewed by Lubinski & Thompson (L&T), including their own studies, is an important step in bringing this debate to a close. Their model and the evidence for it are a powerful contribution toward transforming the concept of private events from theoretical entity to empirical observation. As I applaud their excellent work, I want to address three questions to them concerning their conclusion that the discriminative stimulus in their study is interoceptive.

First, as I have discussed elsewhere (Zuriff 1985, pp. 229–31), many behaviorists have interpreted reports about feelings to be discriminations of behavior rather than of internal events. For example, in saying "I feel angry," people may be discriminating their own overt hostile behavior rather than internal states. Thus, I wonder if the drug-cue bird's behavior can be understood as a discrimination of its own behavior: depressed, stimulated, or normal. In this case, the decoder's initial response should be "translated" as "How are you behaving?" rather than "How are you feeling?" I am not sure it is possible to distinguish between a discrimination of behavior and a discrimination of internal states other than by somehow dissociating the internal effects of a drug from its effects on behavior.

My second question concerns how we should best talk about the stimulus in the L&T study. Consider how we talk about a typical exteroceptive discrimination, say, between a red and a blue light. Although we know that the discrimination is possible only because the light energy stimulates the photoreceptors of the retina, we do not conceptualize this as a discrimination of photoreceptor activity or any subsequent physiological events. We speak of the external initiating stimulus as the discriminative stimulus. Similarly, if different food stimuli are placed on the tongue inside the mouth of a subject who discriminates between sweet and sour, we speak of the external objects on the tongue as the discriminative stimuli, not the chemical-sensitive receptor activity. Indeed, we do so even if a long temporal delay occurs between the removal of the object from the tongue and the discriminative report. Although we assume that the delay is mediated by events within the nervous system, we still say that the external object is the discriminative stimulus rather than those neural events. Turning now to the L&T study, we can ask whether the discriminative stimulus is better identified as the injected drug rather than the subsequent interoceptive events. Like the visual and gustatory stimuli, the drug is an external stimulus that initiates a series of internal events, including the stimulation of neural receptors, and may properly be considered the discriminative stimulus even when the discriminative response occurs after a substantial delay. If so, the decoder's initial response should be "translated" as "What drug were you injected with today?"

There are two advantages to choosing the drug rather than an interoceptive event as the discriminative stimulus: (a) the drug is subject to better control and measurement, and (b) it is not clear which of the many and continuous internal events initiated by the drug should be designated as the discriminative stimulus that stimulates the interoceptive receptors.

Third, I wonder whether L&T consider a non discrimination interpretation of first-person reports relevant to their model. I have suggested (Zuriff 1985, pp. 240–42) that not all reports of internal states need be considered as discriminations of those states. Consider, for example, a rat conditioned to press a lever for food reinforcement. The lever-pressing behavior occurs only when the rat has been deprived of food, but not when it is satiated. Nevertheless, we do not consider the behavior to be under the discriminative control of hours of deprivation or the internal states it causes. Instead, we say that the state of hunger increases the strength of a class of responses that has been reinforced by food. Similarly, we may perhaps want to say that when Jones is angry, a set of responses, including, for example, pounding the table and yelling at others, becomes reinforcing and is strengthened. Saying "I am angry" may be a member of this set. Just as pounding the table is a function of his anger state but is not a discrimination of it, so his verbal response "I am angry" is functionally related to his anger but is not a discrimination of his internal state.

Before concluding, I want to emphasize that my requests for conceptual clarifications in no way detract from the importance or quality of Lubinski & Thompson's studies and model.

Authors' Response

Animal models: Nature made us, but was the mold broken?

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The target article provided an account of communication based on private states (henceforth CPS) by combining (1) elements from the three-term contingency (Skinner 1938; 1945; 1957), (2) findings from the study of species and individual differences (Blakemore & Greenfield 1987; Griffin 1992; Savage-Rumbaugh 1986; Wasserman 1993), and (3) principles drawn from neurobehavioral pharmacology (Thompson & Schuster 1968; Thompson et al. 1993). Several commentators accepted our analysis of the way children learn affective interoceptive discriminations and report them to others. Our model and synthesis unfolds without referring to intentionality, language, mental representation, or a theory of mind. Although this caused concern among some commentators, it is possible (as **Humphreys** points out) to explain complex psychological phenomena by focusing on observable behavior and its environmental and biological context. Novel behavioral combinations emerge when unique configurations of stimuli impinge on an organism that has been taught certain prerequisite skills (relevant to each constituent stimulus event); no nonmeasurable mental entities need be postulated to explain this scientifically, as Skinner (1957) suggested in his treatise on verbal behavior. Behavioral syntheses are not artificial products of laboratory jury-rigging, as some commentators suggest; they are ubiquitous in nature, across an array of species, including our own.

An understanding of CPS requires attention to biological predispositions within (Bouchard et al. 1990) as well as between species (Blakemore & Greenfield 1987) and to the way the three-term contingency procedures are implemented (Lubinski & Thompson 1986; 1987; Thompson & Lubinski 1986). The fact that large individual differences arise from constitutional-genetic as well as environmental-historical factors in hearing, vision, and perceptual-motor performances is widely accepted. But CPS similarities between species and individual differences within species seem to arouse opposition to what would otherwise be obvious. Comparative/differential psychology, the experimental analysis of behavior, and neurobehavioral pharmacology have been polarized for too long. We are pleased that some commentators found useful our treatment of constitutional differences in internal experiences and the capacity to communicate on their basis.

The notion that "nature made him, and then broke the mold" (Aristo 1532) may have provided a misleading metaphor for human uniqueness. Throughout *scala naturae* (Hodos & Campbell 1969) there is much to be learned from appreciating the common behavioral principles acting on each organism's biological substrate to shape what it can communicate. This is the guiding tenet of our work and the responses to the commentaries that follow.

R1. How isomorphic to the human condition must an animal model of CPS be to achieve plausibility? Although most commentators found value in our approach, several criticized it for lacking critical elements. Our definition of communication was especially troublesome to some. **Pepperberg** insists that information must be exchanged between participants; **Hardcastle** requires that shared symbolic meaning be present in the exchange, and **Zentall** requires intentionality and purpose. A number of commentators mentioned that our pigeon model is not language. Ideas based on human/nonhuman discontinuities stemming from concept formation and language underlie the commentaries of **Bringsjord & Bringsjord, Garrett, Hardcastle, Leiber, C. Moore, Pepperberg,** and **Zentall.** Yet models of human communication do not require, and indeed many forms of human communication do not involve, language. It was not our intention to synthesize language, and we explicitly stated this.

The commentators who raised questions about the nature of language seem to have limited familiarity with the communicative abilities of the broad range of humanity. Approximately 3% of people are born with mental retardation and a significant percentage more have other disabilities that severely limit their acquisition of typical adult language (e.g., autism, pervasive developmental disabilities); many others experience damage to their developing nervous systems after birth from infections, toxin exposure, or other insults that alter communicative ability (Thompson & Grabowski 1977). Depending on their range of social skills and their ability to respond to abstract symbols, many such children learn to communicate vocally, but their verbal repertoires are limited. Others use alternative communication systems (e.g., manual signs, gestures, or iconic symbols) instead of speech. Moreover, the early language development of children with mental retardation and autism is often similar to that of normal children except that their symbol set is limited, the length of symbol-combinations is smaller, and the rate of concept learning is slower. Youngsters with retardation and autism usually display intentionality, sharing abstract symbols (e.g., gestures, icons) with the listener (or observer), but they often fail to display other features of communication that several commentators indicated are prerequisites for language. Perhaps some commentators would be reluctant to conclude that such communicative exchanges are truly linguistic, or if they are, they still do not qualify for comparison with the behavior of our birds or Savage-Rumbaugh's chimpanzees and benobos (a species in the same genus as the common chimpanzee; Savage-Rumbaugh et al., in press).

We believe the language of children with developmental disabilities, rather than being impoverished, is prototypic of communication in transition to adult language. Chimpanzees and benobos can be taught to display such skills (cf. Savage-Rumbaugh 1984a; 1984b; 1986; Savage-Rumbaugh et al. 1990); our pigeons learned some aspects of them too (and have the potential for even more complex exchanges; see Herrnstein 1990; Herrnstein & de Villiers 1980; Wasserman 1993).

Children, chimpanzees, and other great apes seem predisposed to learn by observing what other members of their species do, including the use of symbols, whereas pigeons do not, hence there are obvious predispositional differences. Joint attention, in which the developing preschool child and parent visually fixate on a concrete object at the same time is important for later language learning in sighted children. The parent names the object the child is looking at – "Ball . . . that's right, Ann . . . you want the ball!" – while handing the child the object. Later, children with less severe disabilities will learn to reverse roles, handing their parent the ball when they are both looking at it. If they do not do so spontaneously, the parent may say, "Give daddy the ball . . . that's right, good girl," often followed by hugs and praise. Eventually, the word "ball" will come to "stand for" the object, and in the more severely disabled child, a manual gesture will similarly stand for "juice."

Some children with mental retardation have great difficulty learning to look at and point to the desired object at the same time as their parents. Others with less severe disabilities begin to reproduce the name of the desired object vocally, "Buh, buh. . . ." The pleased parent spontaneously reinforces the child verbally and with other signs of approval ("Ball, that's right, that's a ball"). Many children with severe retardation develop a limited repertoire of one- and two-symbol names for objects ("blanket," "juice") and simple actions ("go out," "drink," "up" [asking to be picked up and held]), some by speaking, and others with gestures or pictorial icons; this is usually referred to as "language" by people working on mental retardation. The difficulty that children with autism or more severe retardation have acquiring language through joint attention (and observational learning) shares features with the learning processes seen in our pigeons as well as most other organisms.

For example, people with autism learn social interactions especially atypically. They often do not appear to find social stimuli inherently reinforcing. This social component of human communication is simply not manifested by many individuals with autism, who often have particular trouble learning pronouns, especially personal pronouns. Some appear to lack a "sense of self," as conceived in some experimental animal work (see Beninger et al. 1974, on rats; Epstein et al. 1981b, on pigeons; Gallup

1970; 1977; 1983, on primates). Perhaps this explains why some early psychoanalysts described them as "lacking ego." It is extremely difficult to achieve even basic exchanges with these subjects about their feelings.

Bringsjord & Bringsjord, Hardcastle, Leiber, C. Moore, Pepperberg, and **Zentall** provide a series of increasingly stringent criteria for both communication and language (see **Baer**'s analysis of the ever increasing height to which the crossbar has been elevated on the language hurdle). Many children with developmental disabilities could not possibly satisfy such criteria, so our commentators would have to conclude that they do not display "human language" or "communication" (in the sense we have used the latter term). They may have inadvertently created criteria that refer only to human communication of the most complex and fully elaborated type, not realizing that this excludes much of the early communication of normally developing children or the communication of many people with developmental disabilities and other forms of central nervous system disorder. Whether those with such severe disabling conditions have a "theory of mind," we will probably never know; however, that they reliably manipulate abstract symbols under appropriate circumstances and use them for social purposes is clear enough and seems sufficient to conclude they are communicating. Perhaps even some of our more skeptical commentators would acknowledge that such exchanges are instances of language.

In addition, cognitive capabilities across species must be calibrated in a multidimensional framework; intraspecies individual differences on all behavioral dimensions display interspecies overlap. In a recent paper, Wasserman (1993) has reviewed the conceptual/symbolic as well as the syntactical capabilities of nonhuman species. We are not alone among animals in being able to form and rearrange symbol classes to refer to and manipulate complex concepts. Creative syntax is neither necessary nor sufficient for demarcating human/nonhuman discontinuity. Articles like Wasserman's deepen the appreciation of the overlap between *Homo sapiens* and other species, especially on behavioral dimensions that some believe distinguish humans from other animals.

R2. Is our model too good? During training, the internal stimulation was controlled with as much precision as that typically seen in exteroceptive laboratory discriminations (i.e., our model did not use less predictable indicators in establishing interoceptive stimulus control: attendant contextual factors and collateral behaviors). This led **Baer** and **Branch** to wonder whether our model was too good, but this is exactly what one aims for in the laboratory, the isolation of causal influences uncontaminated by the countless forms of happenstance encountered in natural settings. We consider this a strength not a weakness.

R3. Can a more sophisticated ape model better handle language and shared symbolic meaning? How would we know that a nonhuman animal truly shares a symbolic understanding with us about the character of its (or our) internal state? It seems likely that Savage-Rumbaugh's (1986; Savage-Rumbaugh et al. 1990) chimpanzees or bonobos or the Gardners' (Gardner et al. 1989) chimpanzees could be taught to indicate that: (1) when they have received amphetamine, they want to go for a walk or climb a tree, rather than rest on the mat in the tree house, and (2) when they have received a benzodiazepine such as Valium, they want to rest, rather than go for a walk. Given that two apes were trained to make such discriminations, the possibility exists that the two animals could learn to respond differentially to how the other is feeling based on their own communicative signs (i.e., I feel like going for a walk, I feel like climbing, I feel like bouncing on the trampoline). It is also likely that bonobos or chimpanzees would be able to learn to discriminate, from one another's motor behavior, which drug they received on a given day; other animals have been trained to make such discriminations. If chimpanzee *A* observed chimpanzee *B* and made the discriminative response that chimpanzee *B* had received amphetamine that day, and then also indicated that chimpanzee *B* "wanted to go for a walk," we would probably be more convinced that the chimpanzees understood one another's private states. This scenario seems plausible, based on what we know about the abilities of the bonobos and chimpanzees and drug discrimination technology. This may adequately address Gray's (1985) remark: "For an adequate study of anxiety in all its aspects, then, we shall need to use animals. But to be able to do this, we must first find some way in which to define 'anxiety' in terms appropriate to animals. It is no use simply to look at a rat, a chimpanzee, or a pigeon and ask yourself whether it looks anxious; and you certainly can't ask the beast." Perhaps we can![1]

R4. Why do humans teach one another interoceptive discriminations and why do other organisms fail to teach them? Savage-Rumbaugh (1986) and Gardner et al. (1989) have shown that their chimpanzees are able to communicate with humans and with one another based on external stimuli in their physical surroundings. We have attempted to show that pigeons are capable of learning discriminative responses based on internal cues and that such internal discriminations can be the basis for primitive communicative exchanges of arbitrary cues (between pigeons) based on those internal discriminations. Chimpanzees should accordingly be able to acquire such a repertoire, containing symbolic stimulus equivalency relationships, though we are not aware that this has been demonstrated empirically. As **Baer** points out, if our nonhuman primate cousins can communicate with one another using some system of shared abstract symbols, based on their internal stimulation, why have they failed to develop that skill? Conversely, why does *Homo sapiens* do so with such facility?

Human relationships are built on coordinated behavioral patterns that reciprocally satisfy, enrich, and fulfill needs and assist in preventing or warding off unpleasant, harmful, or otherwise intolerable events. In infancy, relationships are largely one-sided (though parents of course obtain important sources of reinforcement from infants too). Mature adult relationships are more balanced in rather complex ways. Mowrer (1950) observed that a very special relationship develops between two organisms when highly valued reinforcers for one are mediated by the other, and vice versa:

Here we seem to have an excellent example of what

> Allport (1937, 1940) has termed "functional autonomy." It is not, as the writer [Allport] seemed at first to imply, that a given type of performance, once stably acquired, continues indefinitely, without benefit of motivation or reward. Instead, the "autonomy" is only apparent in that what looks like a case of performance without motivation turns out to involve simply a shift in motivation, in which the new, substitute motive is so subtle as to be difficult to identify. (p. 693)

Autonomous reciprocal relationships are built on providing important reinforcing events; the stimuli associated with those events are generalized conditioned reinforcers (Skinner 1953; 1957) and hence largely independent of momentary primary motivational states. Maintaining those relationships requires facility in meeting such reciprocal needs: assisting one another in achieving access to powerfully satisfying events (presenting positive reinforcers) and avoiding or removing unpleasant events (negative reinforcers) through manipulations of abstract symbols. When unforeseen problems arise or attempts to attain goals fail, then sources of satisfaction and joy are lost, and (negative punishment or removal of positive reinforcers) or unpleasant circumstances (an aversive stimulus, or positive punishment) occur.

The ability to look at our partner and ask, "Can I help you with that?" or "You really look down, what happened today?" facilitates the listener's reporting a private feeling (e.g., frustration or discouragement). This in turn makes it possible for the speaker to assist in solving the problem or at least reduce the unpleasant feeling by providing "tangible" and "emotional" social support (cf. Caplan 1974; Cassel 1974; Cobb 1976). With the help of a few suggestions, a person may successfully repair a broken faucet and achieve a sense of satisfaction (tangible support). The feeling of dejection arising from the loss of a fellow worker's approval earlier in the day may be diminished and the partner feels less despondent (emotional support). The ability to reliably report one's internal feelings is an essential component in maintaining human relationships; it gives loved ones the opportunity to be supportive. Human communication serves this essential function, which is central to intimate partnerships (e.g., married couples), family relationships (e.g., parent-child relationships), and friendships.

The skills involved in communicating through pointing at symbols on a symbol board (e.g., Savage-Rumbaugh et al. 1990), signing (Gardner et al. 1989), or pecking at keys (Lubinski & Thompson 1987) are very different from the skills required to induce a group of conspecifics to adopt a system of communication by using shared symbols. A human child born into a speaking community does not face the task of inducing the members of that community to begin communicating by spoken or other symbolic means. They need only acquire the use of the symbol system. Even the more capable benobos seldom spontaneously construct sentences longer than three symbols, and they typically do so only when prompted (personal communication, Savage-Rumbaugh 1993). They find it easy to discriminate spoken human communication but have limited ability to manipulate others' behavior *in ways that would be necessary to adopt a shared symbol system*. It is not surprising, therefore, that even the most capable apes lack the necessary communicative skills to induce other members of their community to adopt an abstract symbol system for coordinating their own and others' actions.

Even at the level of intimately bonded pairs of animals one might wonder why occasional pairs of bonobos or chimpanzees did not develop the ability to communicate with shared symbolic systems, the way deaf children develop gestural communication systems with properties of typical early child language even without being taught them by their parents or other adults (Goldin-Meadow & Mylander 1984). Two possibilities suggest themselves. There may be little adaptive advantage to another bonobo in knowing that a fellow bonobo is feeling frustrated or that its neighbor is feeling unhappy about being scolded (Savage-Rumbaugh, in press). Maintaining reciprocal relations between pairs and among members of family constellations of bonobos or chimpanzees may not be facilitated by communicative exchanges based on private sources of stimulation. A second reason the apes may not have developed skill in reporting to one another about their internal sources of stimulation is that little is reinforcing about that process. It is noteworthy that the bonobos and chimpanzees that have been studied most extensively rarely seek out those opportunities. One suspects that humans are considerably more competent at reinforcing bonobos' communicative attempts than are fellow bonobos. Moreover, the facility with which people are able to communicate, combined with the richness of human communicative repertoires, makes it far easier for people to have something of interest to say to a bonobo than a bonobo would to another ape. Thus, it is not surprising that bonobos and chimpanzees appear to prefer to communicate with humans rather than fellow conspecifics.

R5. What is private and what is the causal status of private stimuli? It is noted by **Baer, Davis, J. Moore, Mortensen, N. Thompson,** and **Zuriff** that different people mean different things by "private" event, experience, and stimulation. Distinguishing among these terms raises some of the most basic unresolved issues in philosophy: the mind/body problem, what Schopenhauer called ("*Weltknoten*") the "world-knot." The problem arises because *experiential* phenomena do not appear to be intersubjectively confirmable, for humans as well as other species (cf. Nagel 1974). Some behavioral scientists have dismissed this topic or treated it superficially (Griffin 1992). Some of this disinterest is probably attributable to the realization that a comprehensive behavioral science does not require investigators to address this philosophical issue (cf. Meehl 1950). What follows is intended to take a step toward greater clarity.

In traditional philosophical discourse, *private* has two meanings (Feigl 1967; Meehl 1966). Gunderson (1984) distinguishes Leibnizian from Skinnerian privacy. Both refer to public inaccessibility, but the latter refers to a form that is intersubjectively inaccessible (for technical empirical reasons), whereas the former is inaccessible to more than one individual in principle. Leibnizian privacy is not illuminated by neurochemical or environmental events contributing to the prediction and control of behavior. It pertains to subjective experience, as opposed to stimulus events and their behavioral accompaniments. This distinction pertains to all sensory modalities, including experiences engendered by exteroceptive stimuli.

In a Utopian world of behavior analysis and neurochemistry, what is currently considered Skinnerian privacy would be public, because nearly infallible indicators of private events would be available. In such a world, these neurochemical indicators could be used to predict verbal and nonverbal behaviors with near certainty. Given this state of affairs, the goal of behavioral analysis would be completely achieved, vis-à-vis the question of privacy. The boundary between public and private ("that changes following every new discovery making private events public," Skinner 1953, p. 292) would be nonexistent scientifically, because empirical measures of private stimulation would be readily available. What, if anything, would the foregoing circumstances reveal about Leibnizian privacy? According to Gunderson (1984), nothing at all: Skinnerian privacy concerns the physical nature of the spatial/temporal properties of the variables controlling behavior. It does not address the experience of the individual who is being acted upon by either exteroceptive or interoceptive stimuli.

Feigl (1967) posed the example of a colorblind physicist/neurophysiologist who has complete knowledge of the effects of all forms of stimulation on brain chemistry and behavior. Such an individual would never *experience* the sensation engendered by 520 nanometers of light applied to the retina, the sensation of "green" in a peer with the same knowledge *and* normal vision. It is in this sense that the latter scientist "knows" something by acquaintance that the former does not. That *experience* is not intersubjectively confirmable does not necessitate the conclusion that one's personal experiences are nonphysical events, but rather that experiential effects are only *non*inferential in first-person accounts (Feigl 1956).[2]

When one portion of a person's brain interprets a neural pattern (e.g., a network of prefrontal cortical neurons) taking place in another portion in response to external or internal stimulation (e.g., activation of occipital cortical cells by an impulse routed through the geniculate body) and the person is capable (at least in principle) of responding verbally or by means of a nonverbal motor response, indicating that the stimulus has occurred, one can say that the person is having an experience. Detecting one activity of the brain by processes in other brain areas is neither mysterious nor metaphysical. However, the detection process can only be carried out in the nervous system of the person or other organism doing the experiencing.

It might seem that a computer connected through a complex network of microscopic fiber-like wires to every nerve cell in a person's brain (what Feigl [1958] called an "autocerebroscope") would share experiences with the brain of the person having the experience. It may turn out to be possible in the future for the computer to share many of the experiences with the organism whose brain is being "tapped," but they would not be identical. There is no way of being certain that as we are scanning the brain of the person doing the experiencing, the brain tissue is not undergoing continual structural changes from moment to moment, with those changes, making it impossible to be certain we are detecting each and every neural event underway in the brain in question. This may even involve the Heisenberg uncertainty principle (whereby the process of measurement changes what is being measured), but that is not essential to the main difficulty. The main difficulty is that experiential phenomena are directly accessible via one road, a road on which only one person travels.

Laakso is right that all stimulation involved in imparting exteroceptive discriminations is typically private in the Skinnerian sense; it is the stimuli, not the stimulation, that is public to parents and teachers. The stimulation is observable in principle, however (i.e., we can measure events in the retina and deeper CNS channels, but we seldom do so in training exteroceptive discriminations, even in the animal laboratory). What teachers and learners share is the stimuli, but the stimulation is inferred with high degrees of reliability in exteroceptive discriminative training. Private experiences stemming from interoceptive channels are not shared by teachers and learners; what they do share are fallible indicators (viz., observable behaviors and attendant contextual factors) indicative of interoceptive stimulation. Like exteroceptive stimulation, this stimulation too is measurable in principle, but, again, experimentalists seldom do this when training interoceptive discriminations. Our target article suggested that both kinds of discriminative learning share similar processes, but because interoceptive discriminations are imparted by teachers much more inferentially, they lack the precision found in exteroceptive discriminations. In conventional Skinnerian analyses of private events, external as opposed to internal stimulation denotes public access to the stimulus rather than the process of transduction of which Laakso is speaking. Experiences (or, as Laakso calls them, "mental states") engendered by both kinds of stimulation are, again, private in the Leibnizian sense. This underscores the reason we stressed that ours is a model of communication *based on* private stimulation, not communicative exchanges *of* private stimulation (a number of commentators appear to conflate these two usages, see **Mitchell, C. Moore, Bringsjord & Bringsjord, Pepperberg, Zentall**). We don't exchange our experiences with others; if we could the mind/body problem would never have been posed.

Finally, with respect to the causal status of private events about which **Baum** asks, we assign the same causal status to private and public events. The concept of stimulus control is helpful here. And yes, private events do exercise control over behavior. We do not, however, establish stimulus control based on experiences; rather, we establish it through public stimuli (using fallible and infallible indicators) and differential reinforcement.

In conclusion, although all of the variables responsible for one's behavior are, in principle, intersubjectively confirmable and directly observable, one's experiences are only *indirectly inferable by others and directly experienced by oneself*. This constraint, however, does not diminish the likelihood of discovering variables for predicting behavior, nor does it limit the scope of behavior-analytic methods. It also does not mean that, because experiences are not intersubjectively confirmable, they are nonphysical. It simply means that the *direct experiences* engendered by stimulation of any kind (exteroceptive or interoceptive) will always be in the exclusive purview of the subject experiencing their effects. They are by nature exclusively restricted to first-person accounts.

Subjective concepts are inferable and can be treated scientifically when placed in a theoretical network that is anchored to other concepts that *are* empirically verifiable. This is done when we tie verbal reports of green-hue experiences to intersubjectively confirmable anatomical and electrochemical events in the retina that covary with photons of a given frequency and can be traced to deeper processing in the visual cortex. Evidence that these relationships hold across species makes interspecies similarities in (at least components of) the *experience* of green-hue more plausible. The concept of experience, however, will always remain an *inferred* entity.

In his treatment of introspection, E. G. Boring (1953) distilled the essence of Leibnizian privacy:

> There have been in the history of science two important dichotomies that have been made with respect to introspection. (a) The first is animal psychology vs. human psychology: human beings are supposed to be able to introspect, and animals are not. (b) The second is the unconscious mind vs. the conscious mind, with introspection the means of observing consciousness. These two dichotomies reduce, however, to one: inference vs. direct experience. (pp. 186–87)

R6. Analogy is not the same as homology. We share **Bringsjord & Bringsjord's**, **Capaldi & Proctor's**, and **Mitchell's** concern that forms of behavior that appear similar do not necessarily imply that the underlying processes or mechanisms are similar. The presence of wings (e.g., in birds and bats) does not indicate homologous origins of anatomical features; one of us (Thompson 1987) has asked, "Is the Capuchin monkey wearing a waistcoat and trousers and cranking the handle of the hurdy-gurdy a believable model of Gustav Leonhardt at the keyboard? Is the chimpanzee trained to puff on cigarettes to gain access to flavored fruit, a valid model of tobacco dependence?" Obviously not. "The behavior of one organism is a model of that of another, not when the behaviors merely look alike, but rather when the conditions giving rise to and maintaining them are the same and they share common processes." We believe we have shown in the present manuscript that there are shared but not isomorphic processes in the pigeon laboratory model and the human (especially children with certain developmental disabilities). To that extent, we believe the pigeon CPS is a believable model of some aspects of human communication.

One of the more compelling examples of animal models of human behavioral phenomena can be found in the realm of drug dependence. When it was proposed in the early 1960s that it may be possible to model important aspects of human drug dependence in the nonhuman animal laboratory (cf. Thompson & Schuster 1964; Weeks 1962), the initial reaction among researchers as well as drug abuse clinicians was skepticism. By the 1970s, however, the animal laboratory self-administration model (using rats and monkeys) was found to be highly effective in predicting the likelihood that specific drugs would be abused by people (cf. Thompson & Unna 1977), and by the mid-1980s, the animal self-administration model had been adopted throughout much of the world as one of the most effective ways of evaluating the likelihood that a new drug developed by a pharmaceutical manufacturer would be abused by people. A rhesus monkey living in a cage connected by flexible tubing to a remote infusion pump, activated by the animal's pressing a lever inside its cage and thereby infusing a small amount of the addictive drug, seemed an unlikely model of a Wall Street broker addicted to cocaine, an inner-city teenager smoking crack, or young adults robbing innocent victims on the street to obtain money for their next dose of heroin. Yet, these laboratory models predict with precision not only which drugs will have addictive potential but, as in the case of stimulant drugs, they create an ordinal ranking of their relative abuse potential (Griffiths et al. 1985).

We argued for basic common processes across species, but we did not suggest that all the characteristics of all animals were interchangeable; yet this is apparently what **LaFollette & Shanks** think we claimed. They fault our analysis, in which we cited Claude Bernard's *Experimental Medicine* (see also Thompson 1984). To make their case that we and Bernard were misguided, they quote Bernard as follows: "All animals may be used for physiological investigations, because with the same properties and lesions in life and disease, the same results everywhere occur (1885/1949, p. 115)." Had LaFollette & Shanks read the remainder of that sentence and a later sentence in the same paragraph, they would have come away with a different impression of Bernard's intent, as well as our own: "the same results everywhere occur, though in mechanism the vital manifestations vary greatly." Bernard concluded, "for without knowledge or appreciation of their individual characteristics [in different species], we can have neither biological exactness nor precision in experimentation" (Bernard 1885/1949, p. 115).

R7. In maintaining that their pigeons do not have language, do L&T adopt an essentialist position? Throughout our target article, we took pains to affirm biological continuity as opposed to an essentialism (Mayr 1982). **Branch** wonders whether our distinction between pigeon communication and pigeon "language" is essentialist; we think not. The joint display of all the individual elements of what several of the commentators have called "human language" may be limited to a subset of people. Language appears to differ quantitatively, not qualitatively, from other forms of communicative behavior. It is intimately tied to the phenomena of relational learning (e.g., transitivity of abstract symbols, of which one instance is stimulus equivalence; see, for example, McIlvane et al. 1984; Saunders & Green 1992; Sidman 1986; Soracci et al. 1990; Spradlin et al. 1973). Although the acquisition of such complex semantic relations is usually limited to children older than 2 and is seldom displayed by adults with severe or profound mental retardation (Sidman 1986), limited equivalence relations can be acquired by rhesus monkeys (McIntire et al. 1987) and chimpanzees (Gardner & Gardner 1984; Gardner et al. 1989). These findings suggest that relational learning, which appears to involve elements of semantic aspects of language, is not uniquely human (cf. Wasserman 1993).

Julian Huxley (1943) remarked that "the miracle of mind is that it can transmute quantity into quality." Some might claim that certain quantitative differences in abilities are so great that they may as well be qualitative differences. That reasoning poses significant problems. There are numerous instances of extraordinary abilities which remain on the same continuum as those of other people. For example, Albert Einstein's (1923) ability to manipulate abstract symbolic relationships led to a rethinking of the basic laws and theories of the physical universe, and Stephen Wiltshire's (1991) extraordinary pen and ink drawings of a Venetian lagoon defy the artistic imagination, despite his autism. The fact that both Einstein's and Wiltshire's abilities differ quantitatively from those of their fellow *Homo sapiens* does not lead us to question whether their abilities belong on the same continuum as those of other humans, though this is essentially the argument of those who say that the quantitative difference between the bonobo's communication and that of a human child is so great as to be qualitative.

R8. Of what clinical relevance is the drug discrimination study to working with people with autism? The relevance of our drug discrimination work to children with autism was questioned by one commentator (**Plous**). To be sure, there is no empirical evidence that the communicative deficits of people with autism are mainly a function of interoceptive impairment. We did not intend to overstate the case, merely to point out the possibility that some of the elusive, disturbed behavior of children with autism may occur in response to interoceptive stimuli that are not accessible to parents or teachers. It is difficult to determine which stimuli are commanding the attention of a child with autism under the best of circumstances, even when the stimulus is part of the external physical environment. Autistic children are at times exquisitely sensitive to faint sounds that are imperceptible to others and yet on other occasions they are oblivious to loud noise that disturbs everyone else in the room. One occasionally encounters a child with autism who displays rage outbursts at what appear to be random times. On careful scrutiny, however, it may turn out that the outbursts only occur when the child is seated immediately below a fluorescent light fixture which emits a constant buzzing sound. When the child's seat is moved across the room away from the fluorescent light, the outbursts stop.

Selective responsiveness to stimuli that may not be apparent to others is typical of children with autism and there is no reason to believe that such stimulus overselectivity is limited to exteroceptive stimuli. Internal cues associated with anxiety may play a role in some of their behavior. Whether we are able to use that information therapeutically or educationally is another matter. Serotonergic reuptake inhibitors, such as clomipramine, may attenuate such disturbing internal stimuli. Moderating the intensity of internal stimulation experienced in obsessive-compulsive disorder (OCD) is often associated with reduction in OCD behavioral rituals; it may produce similar effects in autism (Cook 1990; Gordon et al. 1992; McDougle et al. 1992).

Children with limited communicative abilities, especially those with autism and other pervasive developmental disorders, often develop alternative motor behavior patterns that serve social purposes. All too often, the behavior developed is harmful to themselves. Head banging, self-biting, and other self-inflicted harm are disturbingly common ways, such children indicate to those around them that what they want is to be left alone, that demands upon them should stop, the noises going on around them should cease, or the person seated in their preferred chair should leave. Such self-injurious actions are interpreted in the research literature in special education and developmental psychology as indicating communicative intent; they are consistent with the principles of operant learning (Carr 1977; Carr et al. 1991). In this case, the self-injury is often negatively reinforced by the parent's or teacher's discontinuing the demand. At other times children with severe disabilities are left alone and scream or strike their head against their chair until an adult caregiver begins talking to them. Some have questioned whether such primitive communicative behavior is really any more language-like than an infant's screaming when it is hungry. Nevertheless, many people working in the field refer to these self-injurious actions as communicative (cf. Horner et al. 1990), though growing evidence suggests they may also involve endogenous opioid self-administration (Thompson et al., in press).

R9. Perhaps failure to communicate based on affective states is more a function of reinforcement history than of substrate or interoceptive system differences. The landmark work of **Salzinger** on schizophrenic verbal behavior (Salzinger & Pisoni 1958) attests to the range of communicative and language variability in the human condition, addressing a major source of that variability: reinforcement history. We agree that history must be carefully considered in accounting for individual differences in children's tendency to express affect associated with our interoceptive states, a point we addressed in our paper. At the same time, it is difficult to overlook the neurochemical and neuroanatomical substrates on which those histories act in expressing themselves. There is an increase in dopamine-2 (D2) receptors in schizophrenia, and there appears to be a relationship between the number of D2 receptors and the positive symptoms in schizophrenia (hallucinations, delusional thinking; Owen et al. 1985). The dopamine system appears to be intimately involved in affective experiences described as "euphoria," "pleasure," "elevated mood," and so on, which are often defective in people with advanced schizophrenia. Although it is possible that deficient reinforcement histories contribute to the change in number of dopaminergic receptors, heritability estimates for schizophrenia (Gottesman et al. 1982; McGue & Gottesman 1989) suggest powerful biological predispositions. It seems more parsimonious to suppose that adults with schizophrenia were born with a genetic liability that manifests itself in midlife, possibly causing cell death through failure to regulate excitatory transmitters. Given sufficient dopaminergic cell loss, reinforcement mechanisms would begin to fail and normal exteroceptive control over the way the world looks, sounds, and feels would deteriorate.

Individual differences in underlying neurochemical receptor processes are also relevant to nonpathological individual differences associated with the experience of affect (Eysenck 1961; Gray 1982). In a series of studies, Chait et al. (1984; 1985; 1986) and Chait and Johanson (1988) examined individual differences in the discriminative stimulus effects and subjective effects of d-amphetamine in normal (i.e., non-drug dependent) human volunteers. In one study (Chait et al. 1984), subjects were trained to discriminate 10 mg of (oral) d-amphetamine from placebo, then tested with 2.5, 5.0 and 10.0 mg of d-amphetamine as well as 10 mg of diazepam for stimulus generalization. All the subjects responded correctly to the 10 mg dose of amphetamine, but correct responding dropped off in a dose-dependent fashion at lower amphetamine dosages. Those who reliably discriminated amphetamine did not differ in their predrug mood ratings, but were more sensitive to some of the subjective changes associated with amphetamine. Subjects trained to discriminate d-amphetamine from placebo generalized minimally to caffeine; however, they did generalize to the weaker amphetamine-like stimulant, benzphetamine (Chait & Johanson 1988). Approximately half of the subjects responded correctly across studies during amphetamine discrimination training.

These individual differences in the ability to learn to discriminate d-amphetamine might arise from several sources. Some subjects may have less central or peripherally mediated physiological and neurochemical effects in response to a given amphetamine dose (i.e., less intrinsic activity), thereby producing fewer or less intense cues to discriminate. Differences in threshold may be related to metabolic differences or differences in receptor numbers. Schuster and Johanson (1988) suggested that it was possible to test this idea by administering a higher amphetamine dose to nondiscriminators to determine whether correct responding emerges. Alternatively, nondiscriminators may respond neurochemically or physiologically to the same degree as responders, but have not previously learned to make endogenously mediated discriminations similar to those produced by amphetamine.

To the degree that an individual responds in nonsymbolic ways to such internal events (e.g., squirming, being distracted), they are said to display *signs*. A person's report of how they feel (e.g., a sharp pain, feeling euphoric) is said to be describing a *symptom*. Conventionally, humans are said to report symptoms, and nonhuman animals display signs. The present research shows that having been taught the requisite skills, nonhuman animals thought capable of displaying signs only may also report symptoms.

Some people may have certain private experiences, but never having been taught the necessary vocabulary and communicative skills with which to report their symptoms, they display only signs of excitement, pain, or euphoria. Others appear oblivious to their internal milieu, the counterpart to the tone deaf person to whom distinctions among musical notes and temporal patterns of notes are largely irrelevant. Such individual differences in reporting affective states may be due, at least in part, to variations in physical properties of their interoceptive receptor systems.

Some people are able to report on refined distinctions among interoceptive events (e.g., people with hypochondria). Although such individuals may complain about nonexistent discomforts, many of their somatic self-reports are probably accurate but exaggerated (Dahlstrom et al. 1972, p. 181). Perhaps one reason hypochondriacs are inordinately responsive to internal state fluctuations is a hypersensitivity of the neurochemical receptors involved in producing internal stimulation, or the excessive production of the neurochemical transmitters responsible for those sources of stimulation.[3] It is also possible that their ability to discriminate subtle differences in internal milieu reflects the combined influence of a hypersensitive neurochemical substrate and a history of differential reinforcement by parents for reporting small variations in their internal environment.

R10. Pharmacology is a means of manipulating events occurring at neurochemical receptors regulating natural affective states. Do the pharmacological agents administered in drug discrimination studies, **Laird** asks, such as the ones described in the target article, overwhelm the normal operation of the pigeon (or human) nervous system, causing a "short circuit"? Although a sufficiently high dose of any drug will cause toxic side effects, this colorful metaphor is inconsistent with a substantial research literature on the specificity of the effects of many such pharmacological agents. The drugs used in the studies described in this manuscript bind to the same neurochemical receptors to which naturally occurring ligands bind in producing affective states experienced in everyday life. They have been shown to be involved in human affective experiences of euphoria and elevated mood (cocaine), on the one hand, or anxiety or the converse of anxiety (barbiturates and benzodiazepines), on the other. These pharmacological interventions provide a way of beginning to understand how these receptors and neural pathways are involved in normal affective events.

Laird also questioned our use of the term "receptor." Along similar lines, **J. Moore** paraphrases Skinner in claiming that organisms have to "have nerves going to the right places" to account for interneuronal stimulation as a source of discriminative control. We use the term "receptor" to refer to a sensory transducer for a drug as well as a binding site on the cell surface that sets off a cascade of electrochemical events. According to Laird's reasoning, although rods and cone cells in the retina project to specific occipital cerebral cortical areas, there is a problem, because no comparable cortical projection maps have been identified for events occurring when cocaine binds to the dopamine receptor. This raises broader questions about the CNS representation of an array of affective experiences. Modern anatomical and physiological techniques reveal a high degree of compartmentalization in subdivisions of the parietal and frontal cortical association areas. These are divided into smaller, specialized information centers that retain a large measure of specificity. For example, the posterior parietal cortex is subdivided, with unique sets of connections to both sensory and limbic system structures (Goldman-Rakic 1988). Although there are no known counterparts of the projection areas for vision, hearing, or somasthesis, the connectivity of these prefrontal and frontal "association areas" with limbic structures suggests the possibility of nonspatial sensory projection for diffuse interoceptive stimuli. It may be that a variety of discriminable CNS events are not represented by a specific cortical homunculus; rather, several subcortical and other cortical structures may be involved in performing such affective discriminative responding.

That there is no known cortical map does not detract from the orderly relationships between specific neurochemical receptor binding processes (as demonstrated by independent binding studies) and psychophysical effects, which are operationally indistinguishable from those of more conventional exteroceptive stimulation (e.g., sensory thresholds, stimulus generalization, specificity to receptor types, lack of generalization to other drug classes) and have been well worked out over the past two decades (cf. Balster 1990; Colpaert & Balster 1988; Thompson & Pickens 1971). These processes have been extensively studied and the robustness and specificity of these drug discrimination phenomena are widely accepted. The absence of an identified cortical projection map poses an investigative challenge but does not rule out the neurochemically mediated stimulus control processes.

R11. How do we know what stimuli are being discriminated? Several commentators inquired about what is being discriminated and how we go about specifying the relevant stimulation. Are verbal reports of "affective experiences" responses based on interoceptive cues, or are they conditional discriminations based on external environmental events (e.g., winning a race, being scolded by a parent) as well as internal stimulation? When one says "I feel frustrated," is the verbal statement arising from the internal stimulation when one's efforts to build a tower out of blocks has repeatedly failed, or in response to the tumbling pile of blocks on the table? (**Hineline**) Moreover, **Baer** and **N. Thompson** wonder whether the bird's discriminative responding following drug administration is based on internal stimulation produced by the drug or perhaps correlated changes in the bird's own behavior. **Laird** and **Pepperberg** raise a related issue, suggesting that what is being observed may be a nonspecific side effect (akin to pressing on the eyeball and seeing flashes of light or blurring of objects in the visual field). And finally, **Zuriff** asks whether it makes any more sense to talk of discriminating interoceptive effects of a drug in terms of its receptor binding than to talk of the discriminative stimulus effects of 24 hours of food deprivation versus 4 hours of food deprivation. In other words, the operation that is the basis for the discrimination is the level of food deprivation; we do not conventionally speak of the discrimination in terms of the differential physiological and neurochemical events associated with deprivation level.

In humans, discriminations of internal states are usually associated with certain types of external events and hence probably become conditional discriminations. Since the early work on drug discrimination by Overton (1971) it has been known that discriminations based on powerful interoceptive stimulation can overshadow effects of exteroceptive stimuli in discriminative settings even when the two types of stimulation are perfectly correlated. This is not to say that external cues are irrelevant, but that a strong interoceptive stimulus might "hold sway" when pitted against correlated external stimuli. Being told that one's daughter scored 16 points at the high school basketball game is not likely to exercise more control over one's verbal behavior than the internal stimuli associated with the grief of having learned earlier in the day of the death of a good friend. When the internal stimulation is less intense and the external and internal stimulation are compatible, a degree of combined stimulus control is likely and **Hineline**'s suggestion regarding what is being discriminated seems more pertinent.

The specificity of drug effects in discrimination studies reduces the possibility that animals are responding only to side effects. As **Stolerman** points out, some drugs induce complexes of affective stimulation (e.g., relaxation, euphoria); **Bamberg** wonders whether our model is apt because people frequently report having two emotions at the same time. People often report "mixed emotions," so the combination of affective states is not limited to pharmacologically induced internal stimulation. Humans exposed to the drugs used in animal drug discrimination studies do not experience side effects comparable to flashes of light when the eyeball is pressed or blurring of vision until very high doses are reached (e.g., ataxia, dizziness, diplopia).

We also agree with **Stolerman** that other approaches, such as training animals to discriminate other internal states, can be informative about the specificity of internal discriminations. Corwin et al. (1990) trained rats to discriminate 22 hours of food deprivation from 3 hours of food deprivation. Once the discrimination was well established, they administered medications that reduce food intake and found that when rats were food deprived for 22 hours when injected with cholecystokinin, they selected the lever corresponding to 3 hours of food deprivation (i.e., as if more food satiated). Schuh et al. (1991) trained rats to discriminate the effects of insulin injections from saline, 2-deoxyglucose from saline (both producing hypoglycemia and eating, but by different mechanisms), 23 hours of food deprivation from 2 hours of food deprivation, and neuropeptide Y (which also induces eating) from vehicle injection. Each animal was then tested with the drugs that had not been used in training to determine whether they produced similar internal cues. Animals trained to discriminate insulin and 2-DG from saline generalized to each other and to 48 hours of food deprivation but they did not reliably generalize to neuropeptide Y injections. This suggests that although there may be overlaps in mechanisms inducing eating, not all internal stimulus effects are isomorphic. These findings address **Zuriff**'s question about the rationale for specifying an interoceptive drug stimulus in terms of neurochemical or physiological correlates when we seldom do that in discriminations based on hours of food deprivation. It may be useful to do so when neurochemical or physiological variables covary uniquely with the behavioral data obtained (i.e., when blood glucose level covaries with the selection of one lever or the other in a discrimination task).

Davis points out a second kind of discriminative behavior our pigeons did not display, namely, reporting on the strength of response tendencies that covary with drug-induced interoceptive states. There is certainly a fundamental difference between discriminating "I have received drug *A* versus drug *B*," and naming various internal events that covary with that discrimination. Davis expressed concern about the links between the former discrimination trained in our pigeons and a response to the Addiction Center Research Inventory (ACRI) items (e.g., "I feel like going for a walk," true/false). We share this concern, and would be more confident that one animal understands another's internal states if they were able to "say" things about them over and above the fact that they "have them." Skinner's (1957) discussion of how people came to respond differentially to aspects of their own behavior (i.e., the state of strength or the likelihood of their behavior) is one of his more interesting contributions. Autoclitics are verbal responses in which some aspect of speakers' behavior controls other aspects of their behavior (Skinner 1957, p. 313). If someone asks you, "Do you know how to ride a bicycle?" your response is an autoclitic; it is a discriminative response based upon your experience with respect to your behavior. Responses based on the strength of one's own behavior, (e.g., "I'm very enthusiastic about art") are a subset of autoclitics. What is being discriminated is the strength of the speaker's own behavior. Davis asks whether it is useful to refer to cocaine as simply inducing a private stimulus state, when what people are actually discriminating are states of strength of their own dispositions to respond (i.e., autoclitics). The understanding of autoclitics is at the root of what Davis and other commentators referred to as "self-awareness" or "consciousness." When we ask people with Alzheimer's disease whether they can ride a bicycle, they may say "no"; but when confronted with a bike, they may mount it and begin to pedal like an experienced bicycle rider. In short, there is a breakdown of self-knowledge or, as Skinner would have put it, autoclitic stimulus control. We do not claim that our pigeons made such discriminations, although other investigators have shown that animals can learn to discriminate aspects of their own behavior (Wasserman 1993). We do not doubt that with the proper arrangement of experimental conditions, our pigeons could learn to discriminate some of their own response tendencies (Killeen 1978; Shimp 1982; 1983) and to peck keys accordingly. We doubt that it is necessary to specify the kind of "awareness" Davis mentions in order to begin to understand how the pigeon, chimpanzee, or young child comes to respond discriminatively to private stimulation.

R12. Is it more difficult to learn drug-induced interoceptive discriminations than those based on external cues, and is this learning based on Pavlovian conditioning? It is suggested by **Capaldi & Proctor** that children learn to apply labels (i.e., "tacts," see Note 2 of target article) to concrete stimuli in the world around them by Pavlovian conditioning. We know of no evidence for this. They also propose that language learning must be accomplished observationally. It may often happen this way, but not necessarily always and certainly not in the case of many children with disabilities. In the pigeon studies we conducted, the birds were required to learn the discrimination under two states of deprivation (food and water) and under three internal stimulus conditions (cocaine, pentobarbital, and saline). That would probably be a difficult discrimination for most normally developing people to learn, let alone a pigeon. Abstracting a single attribute from a complex exteroceptive stimulus is accomplished by most young children around 2–3 years of age through differential reinforcement of responding to one aspect (e.g., color, texture, size). Surely Capaldi & Proctor do not believe that praise and corrective feedback are irrelevant in learning the names of objects or their attributes ("No, the *red* ball"; "That's right, pet the *kitty*"). (Perhaps it would be illuminating to note that reinforcement does not require a concrete exchange of commodities. Generalized conditioned reinforcers are much more typical, like the many kinds of praise [e.g., yes, good, right, thank you]; these examples of verbal reinforcement support behaviors of all kinds. Observing others can also serve as a maintaining or reinforcing event for complex behavior. There is nothing incompatible about reinforcement theory – as discussed by conscientious learning theorists – and observational learning; cf. MacCorquodale 1969; 1970.)

Capaldi & Proctor suggest that it is as difficult to learn exteroceptive discriminations as interoceptive ones, if not more so the referent is often difficult to identify because the stimulus being labeled in natural environments is multidimensional. As **Hineline** and **J. Moore** note, imparting interoceptive discriminations is more difficult than training exteroceptive discriminations in nature (but not in the laboratory, as we have shown) because teachers have direct access to the source of stimulation in the latter, but not the former.

R13. Some technical issues. Concern was expressed by **Plous, N. Thompson,** and **Zuriff** about the training time involved in establishing the constituent behaviors of our synthesis (viz., 10 months). Three-key interoceptive discriminations can be trained in eight weeks to pigeons (France & Woods 1985). Our training regimen took much longer because, for theoretical reasons, we trained our subjects under two states of deprivation: food and water. We did not want the drug-cue birds' performance to covary with only one motivational condition; the performance was to be established under multiple motivational conditions; our subjects were to *tact* (not merely to *mand* or *mand* impurely, cf. Lubinski & MacCorquodale 1984; Lubinski & Thompson 1987).[4] Water deprivation added several complexities to the drug-cue birds' training, not the least of which was the difficulty of ascertaining the optimal postinjection interval for the communicative exchange, inasmuch as pharmacological agents are not absorbed and distributed as readily under water deprivation as food deprivation. **Zentall**'s point about the grain used in typical experimental chambers compared to the chow that pigeons are usually fed in their home cages is important. Pigeons do tend to prefer the grain to the chow; this is why we fed our birds grain in their home cages as well as in their experimental chambers.

Hocutt's colloquial use of the terms "starved" and "dehydrated" in referring to our pigeons' preparation to peck transilluminated keys, which led to access to mixed grain or drinking water on alternate days, should not be confused with our procedures and their consequences. In our study, pigeons had unlimited access to food or water on alternating days; they experienced 24 hours of deprivation for each commodity, and free access to the other, prior to each experimental session. "Starvation," according to *Stedman's Medical Dictionary* (1990) refers to "Suffering from long-continued deprivation to food," which does not conform to the procedure used in our study. Technically, "dehydration" (usually called relative dehydration) refers to a deficit relative to the content of solutes contributing to osmotic pressures. Dehydration in birds occurs when there is prolonged water deprivation along with access to only a dry diet in a hot, dry environment (e.g., 3–4 days at 31–36° C). Many avian species drink rarely under natural conditions, obtaining much of their liquids from vegetation (Bartholomew & Cade 1963; Fisher 1972). Some birds go for extended periods without drinking at all when ambient temperature is moderate, with little apparent dehydration, a pattern that is similar to that encountered under natural conditions. The birds in our study went without water *or* food every other day and were given free access to both for approximately 24 hours one day per week. Perhaps Hocutt meant our birds were either thirsty or hungry.

Branch asks why exteroceptive events (those that covary with distinct interoceptive states) do not accrue stimulus control properties. The answer, we believe, is that many external circumstances engender familiar affective states (joy, anxiety, sadness); stimuli evoking interoceptive states are features of a heterogeneous collection of complex stimuli. In the teaching of color discriminations to children, colors are always attached to some object, but because there are so many different objects with similar colors (green is found on books, cars, grass, etc.), other physical features of these objects accrue little, if any, stimulus control properties.

Zentall wonders whether we believe our birds behave altruistically in response to their conspecifics' Plexiglas pecking to "hurry the other along," when one of the participants is satiated. We suspect this behavior operates more like an aversive stimulus whose termination is found negatively reinforcing. This is akin to the difficulty experimenters encounter when trying to food-deprive a subset of caged primates in a large animal colony. Often the food-deprived apes will emit species-specific cries and nearby conspecifics with free access to food will hand chow or biscuits to their food-deprived peers. We suspect that these episodes are motivated more by the termination of a species-characteristic aversive stimulus (much like a child's cry) rather than a pure form of altruistic behavior.

R14. Behavioral syntheses and human intelligence. The commentary of **Humphreys** highlights the underappreciated complementarity of behavior-analytic methods and his own analytic strategy in differential ("trait") psychology. Both psychological viewpoints stress the scientific legitimacy of abstracting psychological phenomena at the behavioral level (the complementarity of these two approaches has been noted by others; Lubinski & Thompson 1986; Meehl 1986; Skinner [1987] himself thought behavioral units could be meaningfully abstracted at the conventional trait level). Operants and traits are basically dispositional response classes, but abstracted at different levels of molarity. Both are abstractions of behavioral classes. Analyzing behavior at the behavioral level is important for building a powerful psychology, as Humphreys and Skinner have done with intelligence and learning, respectively. In doing so, interestingly enough, they have also provided systems of powerful behavioral laws that enabled scientists from other disciplines to build interdisciplinary connections, although this was never their intention (inasmuch as both were content with abstracting psychological phenomena at the behavioral level). Skinner (1938; see also Thompson 1988) has enabled behavioral pharmacologists (Dews 1986) and neurophysiologists (cf. Mogenson & Cioe 1977) to find scientifically significant order in lower-level biological phenomena, while Humphreys's work has provided a clearer picture of the content and communality of intelligent behaviors. Intelligent behaviors abstracted at this level have provided the most interesting cross-disciplinary link (Vernon 1993). [See also Schull: "Are Species Intelligent?" *BBS* 13(1)1990.]

R15. Conclusion. Our target article offers a natural science account of one way humans acquire the ability to identify and communicate based on their feelings. In our treatment, we reviewed several definitions of communication from comparative psychology, ethology, and linguistics and found them too restrictive to accommodate recent advances in behavioral synthesis. The conceptual capabilities of great apes (Greenfield & Savage-Rumbaugh, in press; Savage-Rumbaugh & Rubert 1992) as well as pigeons (Herrnstein 1990) far exceed initial expectations; and Wasserman (1993) has provided evidence for other nonhuman manifestations of language, including syntax. Taken as a whole, the evidence reviewed here might lead some to reconsider what is meant by communication, language, private psychological events, and what behavioral attributes are distinctively human. Perhaps one reason for Neal Miller's (1962) call for "a 20-year moratorium on consciousness" was to allow experimentation to provide a more solid empirical basis for understanding what consciousness means. Much as the development of split-brain preparations caused a rethinking of philosophy of mind, research on neurochemistry and plasticity of microstructure in the nervous system (Ebner & Armstrong-James 1990) calls for a rethinking of our notions of the accessibility of thought and affect for empirical inquiry. Research on species and individual differences in the emergence of components of what has conventionally been called communication and language will continue to convert what were once thought to be qualitatively unique phenomena into basic components we share with our fellow animals. As Montaigne (1923) remarked, "Miracles exist from our ignorance of nature, not in nature itself."

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NOTES

1. It seems possible to build animal models with language components that meet the requirements proposed by most (but not all) scholars. For example, Chomsky (1965) conceives of language as an abstract system of rules which are distinguished from the behavior that underlies language (cf. Lee 1981). According to Chomsky (1968), language "has no existence apart from its mental representation" (p. 81). This view of language is similar to views of disciplines like logic and mathematics. Lee (1981) has pointed out that views of language such as Chomsky's place the subject matter outside the domain of natural science.

2. Herbert Feigl (1967, p. 138) discussed the mind/body problem with Albert Einstein: "I had the privilege of discussing the problem (along with many more 'tangible' matters in the philosophy of physics) with Albert Einstein one afternoon in his home in Princeton. I asked Einstein whether in an ideal perfect (of course utopian) four-dimensional, physical representation (à la Minkowski) of the universe the qualities of immediate experience (we called them metaphorically the 'internal illumination' of the 'knotty clusters of world lines' representing living-awake brains) were left out. He replied in his characteristic, humorous manner (I translate from the German in which he used a rather uncouth word): 'Why, if it weren't for this 'internal illumination' [i.e., sentience] the world would be nothing but a pile of dirt!' This reply suggests that the (ultimate-utopian) physicalistic account, though complete in one way, is incomplete in another."

3. This could be examined empirically. Would hypochondriacs be more adept at discriminating and distinguishing

minuscule levels of pharmacological agents from placebo, compared with the less hypochondriacal individual?

4. As **Pepperberg** suggests, Savage-Rumbaugh (1986) and Terrace (1985) were critical of the Epstein et al. (1980) synthesis for this reason (and our experiments were designed to forestall this concern); Epstein et al. (1980) synthesized mands (verbal operants controlled by aversive stimulation or deprivation) or impure mands (controlled by the latter plus discrminative stimuli), but not tacts (pure discriminative stimulus control).

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Letters *a* and *r* appearing before authors' initials refer to target article and response respectively.

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