(1993). In S. T. Boysen & E. J. Capaldi (Eds.), The Development of Numerical Competence: Animal and Human Models. Hillsdale, NJ: Lawrence Erlbaum.

CHAPTER SIX

Numerical Competence in Animals: A Conservative View

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> About fifty years of work . . . have, in fact, produced a rich crop of several hundred heuristic concepts, but, alas, scarcely a single principle worthy of a place in the list of fundamentals. It is all too clear that the vast majority of the concepts of contemporary psychology, psychiatry, anthropology, sociology, and economics are totally detached from the network of scientific fundamentals. (Bateson, 1972, p. xix)

We assert the conservative views expressed here, because we are concerned that Bateson may have been more right than wrong about psychology and, if so, the reason for the detachment has been the too frequent willingness of psychologists to compromise basic scientific principles. We believe that the integration of the conceptual strengths of cognitive psychology with the methodological strengths of behaviorism is realistic, reasonable, and best for psychological science. It will not help to continue to compromise on behavioral methodology in the interest of preserving "interesting" interpretations of behavior that are not confirmed by the reported observations. In many cases, methodologically superior observations are feasible, and we believe that questionable studies should be redone accordingly. If methodologically appropriate observations are not feasible, then the subject of investigation is not scientific.

Early research on animal cognition was the target of much methodological criticism. Romanes (1883, 1891) was the frequent target of such criticism, and scholars have often cited his works as examples of what not to do when studying the mental abilities of animals. Washburn's

(1926, pp. 4–5) list of objections to the use of anecdotes was directed to Romanes's use of them. Morgan's (1914, p. 53) famous canon against the attribution of higher-order processes to animals when lower-order ones will do is sometimes cited as having been a reaction to Romanes (e.g., Mitchell & Thompson, 1986, p. 6). Such criticism put the study of animal cognition into disrepute and led to its decline as a reputable scientific endeavor. Although Romanes was often the target of methodological criticism, his theoretical views were progressive and we believe he will be increasingly appreciated.

In recent years, there has been a renewal of interest in animal cognition due in part to the rise of "cognitive behaviorism" (Thomas, 1984) or "liberal behaviorism" (Mahoney, 1989) as a reaction against radical behaviorism. Whereas it may have been necessary for such a reaction to occur to save scientific psychology from the stultifying rigidity of radical behaviorism, there are occasional lapses into a liberalization that is too extreme.

For example, Mitchell and Thompson (1986) recently wrote:

> When we use such terms as "think" or "feel" or "want" or "believe" with respect to animals (including humans), we are not necessarily explaining their behavior, but providing names for higher-order descriptive attributes of their behavior. . . . As editors, we admit to being a bit soft on mentalism. (p. xxiv)

In principle, this attitude may be justifiable, but merely to assert it without doing the hard work of establishing the proper use of "think," "feel," and so forth will impede more than help the development of scientific psychology. Some discussion of the difficulties associated with the use of mentalistic terms, especially in the functional sense implied by Mitchell and Thompson, may be found in Thomas and Lorden (1989).

Another example of excessive liberalization is Griffin's (1981) book, *The Question of Animal Awareness,* which Davis (1989) humorously described as "*The Satanic Verses* of animal cognition." Thomas and Lorden (1989) also cited objections to Griffin's book. A third example is Whiten and Byrne's (1988) attempt to justify the use of anecdotal evidence in the study of "deception" by animals. Washburn's (1926) objections to anecdotal evidence are just as applicable today, as Thomas (1988a) and others demonstrated in their commentaries on Whiten and Byrne's article.

THE RETURN OF CLEVER HANS?

Clever Hans was a counting horse whose abilities proved to be fraudulent (see Dewsbury, 1984, p. 189; Rilling, chap. 1, this vol.). Although the fraud may have been inadvertent (as suggested by Pfungst, the psychologist who discovered that Hans was cued by his master), it occurred in an era when reports concerning the mental abilities of animals were already receiving strong scientific opposition. Thus, Clever Hans and animals' use of number became symbolic of the need for rigorous control and theoretical caution before attributing higher-order cognitive processes to animals.

Hence, in important ways, some of the research on animals' use of number, including recent examples, reflects the insidious influence of relaxing methodological standards on the study of animal cognition in general. This chapter addresses some of those methodological compromises and discusses ways more likely to lead to conclusive determinations of numerical competence in animals. Although there are enough methodologically sound studies to conclude that some animals are capable of a conceptual use of number, we show that some recent reviewers have been too uncritical and a more conservative and cautious view of animals' use of number is warranted.

Before proceeding, we should specify that when we use phrases such as "a conceptual use of number," we usually mean the ability to affirm or discriminate the numerousness property of discriminanda on a conceptual basis. The principal exception is counting and, generally, the criteria to demonstrate counting have not been met with animals. What we mean by a "conceptual basis" and the "criteria to demonstrate counting" is discussed later.

REVIEWS OF NUMERICAL COMPETENCE IN ANIMALS

Memories of Clever Hans and early concerns about higher-order processes in animals must have influenced early reviewers of the literature on animals' use of number (e.g., Honigmann, 1942; Salman, 1943; Wesley, 1961) because they were more conservative about what constituted acceptable evidence for an animals' use of number than some recent reviewers. For example, Wesley concluded that only Hicks's (1956) study had been sufficiently free of confounding cues to support the conclusion that an animal had used number (in Hicks's case, the animals were monkeys, *Macaca mulatta,* and the reference number was 3).

Later reviewers, Davis and Memmott (1982), did not cite Wesley (1961) and they failed to explain, for example, why they found acceptable Koehler's research on birds' use of number when Wesley had not. Although Davis and Memmott raised questions concerning the published descriptions of Koehler's work, they also wrote, "The work of Koehler reflects the new rigor of research on higher mental abilities in animals" (p. 551).

Davis and Memmott (1982) also cited favorably and without negative regard an article by Ferster describing chimpanzees *(Pan troglodytes)* use of number (1964; based apparently on research by Ferster & Hammer, 1966). Thomas, Fowlkes, and Vickery (1980) reported that it could not be determined based on the published accounts whether Ferster and Hammer had confounded area with number cues (see related discussion later). Thomas and associates also noted that the number of trials involved (hundreds of thousands!) was consistent with the possibility that the chimpanzees had memorized the specific discriminanda rather than responding to number per se.

More recently, Davis and Pérusse's article (1988a), among its other stated purposes, can be viewed as an updated review that also reexamined much of the older research. They omitted reference to the aforementioned studies by Ferster, cited Wesley (1961) but not in the context of Koehler's work, and reviewed Koehler's work in the same relatively noncritical way as Davis and Memmott (1982).

It will be useful to iterate that Wesley (1961) objected to Koehler's work on the grounds of (a) possible experimenter cues—shades of Clever Hans?—from the manual manipulation of the discriminanda and reinforcers, and (b) the likelihood that odor cues from the food reinforcers guided the subjects' selection of the correct numerical discriminanda.

We do not take the view that Koehler's and Ferster's animals nor the many animals in other studies that could be similarly criticized did not use number cues. According to our view, solutions based on the use of number cues were too often confounded with nonnumerical solutions. The confounded nonnumerical solutions can be eliminated via proper experimental control, so we believe it is in the best long-term interest of the study of animal cognition to disregard confounded studies and replace them with appropriately controlled studies.

Some other possibly confounding cues in animal-number research that have long been recognized but too frequently overlooked or ignored are (a) using items that are uniform in size to construct the numerical discriminanda and, therefore, that confound cumulative area or volume cues with number cues; (b) using uniform backgrounds together with sets of equal area/volume items, permitting differential brightness cues to be used; and (c) using too few patterns of stimuli, especially when the items are figures drawn on cards, permitting the animal to memorize the specific patterns.

There are two recent examples where number and volumetric cues were confounded: Dooley and Gill (1977) used Froot Loops (a proprietary cereal product), which are approximately uniform in size as the numerical items. Rumbaugh, Savage-Rumbaugh, and Hegel (1987) used semisweet chocolate bits of approximately uniform size. We hasten to note that the investigators in both cases acknowledged the confounded cues, and Rumbaugh and colleagues were careful to describe their discriminanda in terms of "quantity" and "amount" rather than number. On the other hand, Rumbaugh and colleagues' stimulus manipulations were described in terms of number, and they used subitizing, a numerical process, to explain their chimpanzees' performances.

In addition to (a) inadvertent experimenter cues, (b) odor cues, (c) area/volume cues, (d) brightness cues, and (e) pattern cues, the possibility of (f) stimulus generalization based on failure to discriminate a new exemplar from a memorized exemplar, and (g) confounding class-concept-use solutions and learning-set-formation solutions faces those who wish to study the use of number concepts by animals. Explications of the latter two problems are summarized later. Before those explications are given and rather than continue citing methodologically inconclusive studies (which considerably outnumber the conclusive ones), it is proposed that any study that claims to have shown the use of number by animals should be examined and found to be free from the aforementioned seven confounded solutions (see Table 6.1).

THE CONCEPTUAL USE OF NUMBER

As noted before, there is no standard definition of concept (Heath, 1967; Kendler & Kendler, 1975; Premack, 1983; Thomas & Crosby, 1977) and, therefore, of using a concept or, in this context, of conceptual behavior.

TABLE 6.1
The Necessary Conditions to Show Animals' Use of Number

I. *Confounds to Be Avoided:*
  1. Inadvertent experimenter cueing.
  2. Odor of reinforcers as discriminative stimulus.
  3. Cumulative area, volume, etc. cues as discriminative stimuli.
  4. Brightness cues based cumulative area etc. cues and uniform background.
  5. Specific pattern memorization.
  6. Stimulus generalization interpretations.
  7. Learning set solutions separable from concept use.

II. *Conditions to Show Conceptual Use of Number:*
  1. Avoid confounds above.
  2. Use trial-unique exemplars.
     OR
  3. Use first-trial data when multitrial problems are used.
     OR
  4. Use text-exemplars without reinforcing responses to them.

Note: See text for explication.

However, it is understood generally that responses to discriminanda based on using a concept must be distinguished from responses based on memorizing specific properties of the discriminanda. Using a concept implies responses *Beyond the Information Given* (so well reflected in the title of a volume of Bruner's works; Anglin, 1973); whereas, responses based on memorizing specific properties implies responses that are limited to the information given.

The conceptual use of information, then, requires that it be free of the possibility of memorizing specific properties. Three ways to preclude responses to exemplars being based on memorizing specific properties of the exemplars are: (a) to present exemplars of the concept one time only; that is, to use trial-unique exemplars; (b) to restrict the critical evidence to the first presentations of exemplars when exemplars are presented more than once, and (c) to present test exemplars without reinforcing the responses to them, so the subject cannot memorize the association between specific exemplars and reinforcement. The latter, an infrequently used procedure, is exemplified in a study by Lombardi, Fachinelli, and Delius (1984), who investigated use of the oddity concept by pigeons. However, their test exemplars were such that a conceptual solution may have been confounded with a stimulus generalization solution based on physical similarities between test and training exemplars (this was suggested by M. R. D'Amato in a personal communication to R. K. Thomas, October 18, 1988).

Conceptualization Versus Stimulus Generalization

Control for stimulus generalization based on physical similarity should be observed in any study of concept use. Although some have equated stimulus generalization with concept formation (e.g., Keller & Schoenfeld, 1950, p. 155; Nevin, 1973, p. 141), we prefer views of stimulus generalization that suggest it is a failure to discriminate (Prokasy & Hall, 1963) or define it in terms of generalization gradients, such as "the degree of generalization will vary inversely with the distance of the stimuli from each other along this dimension. The function expressing this relationship is called the generalization gradient" (Kimble, 1961, p. 484).

Stimulus generalization, as we use the term, implies memorizing a specific reference stimulus or set of reference stimuli followed by responses to new stimuli based on a failure to discriminate between the reference stimulus and the new stimuli. The best way to avoid stimulus generalization in a study that purports to investigate the use of concepts is to preclude the possibility of memorizing a reference stimulus or set of reference stimuli.

Stimulus generalization should be characterized by a curvilinear gradient of affirming responses to exemplars whose physical properties depart systematically from those of some reference stimulus; whereas, concept-use results in a distribution of affirming responses to exemplars of the concept that is essentially rectilinear. The emphasis in both cases is on affirmation of exemplars as opposed to distributions of response times, and so forth. Obviously, the difference between stimulus generalization and conceptualization is not as clear as implied here and more could be said about them. However, we must move on.

Using Class Concepts Versus Learning Set Solutions

Many reports of animals' use of concepts have been based on transfer of training without using trial-unique exemplars, first-trial evidence, or nonreinforced test exemplars. Perhaps, the ideal example of transfer of training based on memorizing is represented in object quality learning set formation where better-than-chance performances on the second trial of new problem presentations constitute the best evidence of learning set formation (e.g., Harlow, 1949, 1959; Hodos, 1970). We do not suggest that learning set per se is not conceptual (see Thomas, 1989) but that responses based on using class concepts and responses based on object quality learning set formation are empirically and logically distinguishable.

Thomas and Noble (1988) showed that using the oddity concept (choosing the discriminandum that had the odd odor among three discriminanda, two of which had the same odor) was distinguishable from learning set formation. Following pretraining that was closely related to the oddity learning set training, their rats performed much better than chance on Trial 2 early within a series of 300, five-trial problems, but the rats never performed better than chance on Trial 1. The odd odor is evident on Trial 1, thus it was possible to use the oddity concept and respond better than chance on Trial 1. That they did not respond better than chance on Trial 1 but did on Trial 2 indicated they learned to identify the correct stimulus on Trial 1 (presumably via the well-known "win-stay/lose-shift" strategy; e.g., Levine, 1965) and respond to it based on rote memory on Trials 2–5.

Aversion to new stimuli likely did not account for the rats' poor performances on Trial 1, because (a) they responded unhesitatingly on Trial 1 when both the odd and nonodd stimuli were new, (b) only 16 odors were used and two were used per problem; thus, early in training the uniqueness of the problems was based on the combinations of odors rather than new odors, and (c) the odors were food flavorings to which one might expect rats to be attracted. Thomas and Noble's study shows that any report of an animal's use of a concept that fails to distinguish between Trial 1 and Trial 2 performances has the potential of confounding responses based on use of the concept with responses based on learning-set-formation in conjunction with rote-memory solutions.

THE PROCESSES FOR NUMERICAL DETERMINATIONS

In the present context, perhaps the most important paper in terms of describing processes of visual number judgments was that of Kaufman, Lord, Reese, and Volkmann (1949). They discussed three processes: (a) *subitizing*, a term they coined, (b) *estimating*, and (c) *counting*. Subitizing and estimating were distinguished by four findings that emerged from their study of humans' abilities to determine the number of dots in random arrays presented for 200 ms. They found that arrays containing from 1–6 dots were determined more (a) accurately, (b) confidently, and (c) rapidly than arrays of more than 6 dots. (d) A "breakpoint" in the data that occurred between 6 and 7 dots was also used to distinguish between subitizing and estimation. Counting, the third process acknowledged by Kaufman and associates, was presumed to have been precluded by the 200 ms presentation times.

Animal studies to date have not addressed confidence nor rapidity of response, and accuracy and the "breakpoint" between 6 and 7 have not been addressed in the sense that Kaufman and associates (1949) did. For that matter, accuracy has been assessed only in the sense of animals being able to discriminate between successive arrays of discriminanda (e.g., 3 vs. 4, 7 vs. 8, etc.; hereafter, such discriminations are abbreviated as 3:4, 7:8, etc.) and the "breakpoint" between 6 and 7 has not been addressed systematically in any way.

In fact, in the best controlled animal studies to date that examined successive arrays with 6 or more items, the "breakpoint" appears to occur between 8 and 9 or between 9 and 10. For example, both of the squirrel monkeys used by Thomas and colleagues (1980; *Saimiri sciureus sciureus*) met a stringent criterion (45 correct in a 50-trial session) discriminating between two simultaneously present arrays of 7:8 "dots" (using trial-unique problems controlled for area, brightness, and specific pattern cues). One of the two monkeys met criterion on 8:9 dots in the 500 trials allowed but failed to reach criterion on the 9:10 problem.

Terrell and Thomas (1990) found similar limits using the *number of sides* (or angles) of quasi-randomly constructed polygons as discriminanda; they also used trial-unique problems and controlled for area, brightness, and specific pattern cues. Two of four monkeys (one was *Saimiri scuireus sciureus;* the other, *S. boliviensus boliviensus*) met rigorous criteria (36 correct in a 40-trial session) distinguishing between heptagons and octagons but failed to reach criterion on the octagon versus nonagon problem in the 1,000 trials allowed.

Subsequent to Kaufman and associates (1949), investigations of "subitizing" in humans have raised questions about its definition and distinguishing criteria, including whether counting had been precluded by the 200 millisecond presentation times (e.g., Folk, Egeth, & Kwak, 1988; Mandler & Shebo, 1982). For reasons such as these, Terrell and Thomas (1990) suggested that subitizing as a term and hypothesized process had outlived its usefulness. It should be noted also that subitizing has never been an acceptable explanatory term, rather it is a descriptive term that was meant to reflect the direct apprehension of number without explaining how that might have occurred.

Davis and Pérusse (1988a) offered the most recent account of possible processes of numerical competence in animals. Table 6.2 includes a summary of their list of processes of numerical competence. Table 6.2 also includes our shorter but equally comprehensive list of processes (in terms of what animals are likely to use). Justifications for our disagreement with Davis and Pérusse's list follow.

Davis and Pérusse's list retained Kaufman and associates' (1949) subitizing, estimating, and counting and added "relative numerousness judgments" as the major processes. Subsumed under counting, Davis and Pérusse added "protocounting," which was defined as:

> Instances in which counting has been identified as the most likely numerical process (e.g., in situations where relative numerousness judgments and subitizing have been precluded), although control tests (e.g., for transfer) have not revealed evidence of true counting. (p. 562)

Davis and Pérusse also subsumed "Concept of Number" under counting, described it as "an attribute of true counting," and defined it "in terms of abstract or modality-free numerical ability and revealed in the capacity to transfer numerical discriminations across sense modalities or procedures" (p. 562).

In his commentary on Davis and Pérusse (1988a), Thomas (1988b, p. 600) argued that protocounting was "unjustified and unjustifiable" by (a) asking how one could ever know that other processes had been precluded and (b) noting that the last clause in the previous quotation, which defined protocounting, made no sense methodologically (one is required to prove the absence of "true counting," which is tantamount to proving the null hypothesis).

We also disagree with the necessity to use cross-modal or cross-procedural transfer to demonstrate "Concept of Number," as Davis and Pérusse (1988a) suggested. If an animal can demonstrate use of number

TABLE 6.2
A New Glossary of Processes of Numerical Competence:
Supersedes Davis and Pérusse's (D&P) Glossary (1988a).

The New Glossary:	
1. *Prototype matching (PM).*	
	Analogous to Rosch's (1975) use of prototype, and applied to numerousness discriminanda.
A. *Absolute PM*	May be precise, e.g., "sevenness" or imprecise, e.g., "manyness."
B. *Relative PM*	May be precise, e.g., "fewer" when applied to successive sets of entities (e.g., 7 vs. 8) or imprecise, e.g., "fewer" when applied to nonsuccessive sets (e.g., 25 vs. 50).
2. *Counting*	We agree with D&P on using at least the first three principles of counting presented by Gelman and Gallistel (1978; G&G) as the defining, operational criteria for counting. However, unlike D&P and G&G, we deem it to be necessary to show evidence for acquisition of a symbol system and the use of "tagging."
Davis and Pérusse's Glossary:	
1. *Relative numerousness judgments*	
	Subsumed by relative prototype matching.
2. *Subitizing*	Has outlived usefulness due to (a) loss of original meaning, (b) being descriptive but not explanatory, and (c) being replaceable by descriptive and explanatory process, prototype matching.
3. *Estimation*	Retains limited use but not in sense used by D&P. Their use is replaceable with prototype matching as previously indicated.
4. *Counting*	See *Counting* in *The New Glossary* in this Table.
A. *Protocounting*	
	Unnecessary, unjustified, and as D&P defined it, unjustifiable.
B. *Concept of number*	
	D&P's use is both too restrictive—need for cross-modal and cross-procedural transfer tests—and too unrestrictive—conditions set forth in Table 6.1 here.

Note: The new glossary is believed to be exhaustive in terms of the basic processes of numerical competence that animals are likely to use.

when the seven confounding conditions described earlier have been excluded and, especially, when trial-unique discriminanda are used, we believe it is appropriate to say that the animal is using number conceptually. However, we acknowledge this may enter the realm of discussion on the question, "What is the difference between a concept and a percept?", which is best left for another time. Intramodal and intraprocedural demonstrations of numerical determinations with trial-unique discriminanda are consistent with the larger body of literature on human and nonhuman animals' use of class concepts, whether they are based on color, shape, size, number, or combinations thereof. We do agree that a stronger case is made for more abstract conceptual numerical determinations when transfer is demonstrated with cross-modal or cross-procedural designs.

To summarize, Davis and Pérusse (1988a) identified four major processes of numerical competence: relative numerousness judgments, subitizing, estimation, and counting; protocounting, a process subsumed under counting was a fifth process. Our position, as discussed earlier, mandates that *subitizing* as a term and process should be abandoned and *protocounting* is unjustified and, as they defined it, unjustifiable. We agree with Davis and Pérusse that the first three of Gelman and Gallistel's (1978) five principles of counting may provide the best definition of and criteria for counting. Remaining to be considered is the usefulness of the processes implied by relative numerousness judgments and estimation.

Before we can give proper consideration to relative numerousness judgments and estimation, we need to say a bit more about counting and to introduce in the context of number judgments the process of prototype matching. We then suggest that prototype matching is both the underlying process for estimation and a prerequisite for relative numerousness judgments.

Counting

As noted, Gelman and Gallistel's (1978, pp. 77–82) principles of counting, especially the first three, provide the best definition and criteria for counting. Briefly, these are: (a) the one-to-one principle, refers to "tagging" each item uniquely; (b) the stable-order principle, which means the tags must correspond to the items in a "stable, repeatable order"; note that this refers to the tags and not to the items, for example, you may count the stars in the "Big Dipper" from any starting and ending position but you must apply the tags, one per star, in the same order; (c) the cardinal principle, which means the last tag applied to the last item describes the number of items in the set; (d) the abstraction principle, which means that if one can count, one can count any set of items; and (e) the order-irrelevance principle as applied to the items to be counted; see note in (b). That the fourth and fifth principles are not essential to the evidence for counting has been discussed by Gallistel (1990, pp. 339–340) and Thomas (in press), and Davis and Pérusse (1988a) also questioned their necessity.

We note, however, as did Davis and Pérusse (1988a, p. 562) that other definitions of counting have been more lenient in terms of the evidence implied in Gelman and Gallistel's principles. We disagree with these more lenient definitions but do not take time to argue the case here other than to agree with Davis and Pérusse that data obtained in accordance with such definitions might be explained by noncounting processes.

We disagree strongly with Davis and Pérusse (1988a) and with Gelman and Gallistel (1978) that one need not show observable evidence of tagging. For example, Gelman and Gallistel wrote: "We hold open the possibility that animals . . . may tick off items in an array, one by one, with distinct mental tags employed in a fixed order, and use the final mental tag as a representation of numerosity" (p. 77). Davis and Pérusse wrote, "verbal number tags need not be replaced by overt physical markers" (p. 566).

Our disagreement with Gelman and Gallistel (1978) and Davis and Pérusse (1988a) concerning the need for observable evidence of tagging and, more importantly, evidence of its underlying symbol system is based on the following three points:

1. Many instances of alleged counting by animals might be explained by noncounting processes; see Davis and Pérusse (1988a), who cited several examples with which we agree. Thus, in the absence of evidence for tagging and its underlying symbol system, counting may be mistakenly inferred when a noncounting process was used.

2. The use of tags implies the use of an underlying symbol system and the acceptance of "mental tags" or the absence of "overt physical markers" omits the need to show the evidence that the animal had an opportunity or ability to acquire the symbol system. We believe that after experience, the symbols might become internalized and used in the absence of overt physical markers, but one should be required at some point to show evidence for the acquisition of the symbol system. We believe that most claims of animal counting are questionable, because there was no opportunity in the animal's reported experiences to have acquired the necessary symbol system.

3. Ifrah (1985) reported that some numerically underdeveloped human cultures that apparently lacked the ability to count, nevertheless had precise number usage up to 4. Beyond 4, number usage in such cultures was reflected in terms analogous to "many" or "countless." Generally, number use greater than 4 involved a need for explicit, physical substitution (symbolic) of tags (e.g., body parts, notched sticks, pebbles or beads, etc.). It is our view that if humans who lack a linguistic system for counting must use physical tags in order to count numbers greater than 4, it is likely that nonhuman animals must also.

In this regard, we note that in the best study of animal counting to date, Boysen and Berntson's (1989) chimpanzee *(Pan troglodytes)*, Sheba, was trained at the time of their report to count up to 4. There was strong evidence to show that Sheba had acquired a symbol system, she had a knowledge of ordinality, and she gave some signs of partitioning and tagging. However, in view of evidence that noncounting humans can use numbers precisely up to 4 without counting, it will be important to show that animals such as Sheba can count numbers greater than 4.

Prototype Matching

For the remainder of this chapter, it is assumed that the processes discussed apply to the discrimination of number when counting has not been or cannot be demonstrated.

Rumbaugh and associates (1987) seemed to express a perceptual view of certain number discriminations when, in reference to Hicks's study (1956) showing rhesus monkeys use of "threeness," they wrote:

> We suggest that a perceptual norm, rather than a number concept, has been acquired and that such a norm is based on the limited perceptual configurations inherent in the simultaneous presentation of three items or objects. (p. 108)

It is clear from the text following the aforementioned quotation that "perceptual norm" applied to the relatively few patterns that a "small number of things" affords. However, the chapter by Rumbaugh in this volume suggests a broader implication comparable to that of prototype matching as used in this chapter.

Davis and Pérusse (1988b) also provided a strong hint of a perceptual process (without naming it, although they spoke in terms of "identify[ing] against a template," p. 604) in their response to Gallistel's (1988) commentary on Davis and Pérusse (1988a). Gallistel cited reaction time data from Mandler and Shebo's (1982) study of "subitizing" in humans to make the following argument:

> Thus, when dot arrays are presented in fixed *Gestalten* (like those on the faces of a die), then the reaction time for judging their numerosity is indeed what one would expect if twoness and threeness were directly per ceptible attributes like cowness and treeness, but when the arrays do not have a fixed pattern, then the reaction time function is what you would expect from a counting process. (p. 586)

Davis and Pérusse (1988b) opposed Gallistel's argument as one for counting with the following:

> We also believe that different representations of cows might well occasion a continuum of reaction times to correctly identify them as "cows." A conventional (canonical) photograph of a Holstein cow might yield a reaction time that is considerably shorter than, perhaps, a pen-and-ink caricature or, more tellingly, a cubist rendering. . . . Does it suggest that the receiver had to resort to enumeration to label the more extreme cows correctly? We believe not. (p. 604)

We agree with their opposition to Gallistel's argument in support of a counting interpretation. However, Davis and Pérusse used the previous quotation to argue for a subitizing interpretation, and our objections to subitizing have been noted.

Von Glasersfeld (1982) viewed subitizing as a perceptual process involving empirical abstraction to attend

> not to the specific content of experience, but to the operations that combine perceptual and proprioceptive elements into more or less stable patterns. These patterns are constituted by motion, either physical or attentional, forming "scan paths" that link particles of sensory experience. To be actualized in perception or representation, the patterns need sensory material of some kind, but it is the motion, not the specific sensory material used, that determines the pattern's character. Because of the dependence on some (unspecified) sensory material and motion, they are called figural patterns. (p. 196)

The formation of such figural patterns may involve spatial or temporal configurations of perceptual items. In young children, these figural patterns (e.g., a specific pattern of dots on a die) are associated with number words by a semantic connection and not because of the number of perceptual units of which they are composed. In animals, such figural patterns may be associated with reinforcement. The ability to form an association between a figural pattern on a die and a number word does not require a concept of number, nor does the ability to associate certain figural patterns with reinforcement mean an animal is counting. Such figural patterns have numerosity, but a subject's discriminative response to them may or may not reflect a concept of number depending on whether the conditions in Table 6.1 have been met.

What is the nature of these figural patterns and how may they be used in number discrimination tasks? Davis and Pérusse (1988b) suggested the possibility of comparing to a template. Presumably, with such a model subjects would form a template for each figural pattern involving a different number of perceptual elements. A stimulus, then, would be compared against a set of templates that have been stored in memory. The stimulus is compared to these templates in memory until a match or near-match is found. Such a hypothesized process has been found to be inadequate as a basis for pattern recognition because of its inflexibility, the infinity of templates required to recognize all possible figural patterns, and the temporal inefficiency of such a process (Matlin, 1989).

Terrell and Thomas (1990) noted the relationship between Gallistel's (1988) "cowness" versus numerousness example, Davis and Pérusse's (1988b) counterargument quoted earlier, and prototype matching interpretations of using concepts as exemplified in the work of Rosch and her colleagues. Prototypes are abstract, idealized patterns that are stored

in memory. When we see a new stimulus, we compare it to a stored prototype. The match does not have to be exact. In this sense, a prototype is a construction stored in memory of an average or representative pattern. The construction of a prototype is based on experience with many exemplars of a concept, and the prototype becomes a representation of the concept.

Rosch (1975) scaled a number of conceptual categories, including "bird" (but not "cow"), in terms of the closeness of exemplars to a prototype, and reaction time measures were included among others to scale the exemplars. The American robin was deemed to be an ideal exemplar of bird and was given a scale index of 1; other exemplars among the 54 birds listed and their scaled values were raven (2.01), pelican (2.98), and chicken (4.02). A stimulus that is similar to its prototype will usually require less time to match and result in a faster response.

Prototype matching is a well-established process to explain the acquisition and use of class concepts in general, and we suggest that numerousness concepts are not an exception. Although our emphasis is on simultaneously present sets of items, within limits, the prototype matching process should be applicable to temporally spaced sets of entities or events as well.

Terrell and Thomas (1990) suggested that for simultaneously present arrays of items, animals may acquire and use prototypes such as "twoness," "sevenness," and so forth. They proposed that prototype matching could account for data previously subsumed under subitizing because: (a) Prototype matching, unlike subitizing, is not defined in terms of measures such as accuracy, rapidity, and confidence, which are usually not assessed; and (b) prototype matching is both descriptive and explanatory whereas subitizing is only descriptive, so it was suggested that prototype matching replace subitizing.

Prototype Matching and Estimation

As noted earlier, we suggest that estimation is based on prototype matching. *Estimation* is a general term usually associated with approximate determinations of quantitative values, such as: How many? How heavy? How tall? How much time?. Accurate estimates in categories such as these rely on prior experience estimating number, weight, length, and time, and such experience is the basis for the formation of prototypes. The amount and kind of meaningful experience determines the strength of a prototype and, therefore, the reliability and validity of an estimate.

Estimates can in some instances be reliable and precise and in other instances reliable and imprecise. Precision is reflected in animals' abilities to discriminate between arrays constructed of successive numbers of items. The highly experienced monkeys in the Thomas and associates (1980) and Thomas and Terrell (1990) studies distinguished between sevenness and eightness reliably and precisely. However, the general failure of the monkeys to distinguish eightness from nineness suggests that there is a limit to precise numerousness estimates and it may be impossible to form precise prototypes that enable one to discriminate between successive random arrays of items numbering eight or more. If the latter proves to be the case, it may be one more instance of the limits of information capacity as discussed in Miller's (1956) well-known article, "The Magical Number Seven, Plus or Minus Two: Some Limits on Our Capacity to Process Information."

On the other hand, it is likely that monkeys could form discriminable but imprecise prototypes for nonsuccessive arrays involving more than eight items. For example, one can readily imagine that monkeys could learn, for example, to discriminate between random arrays of 25 and 50 dots. One can also imagine that monkeys could learn to recognize "twenty-fiveness" reliably using a discrimination procedure as long as the contrasting arrays differed sufficiently from 25. No doubt, there are systematic psychophysical difference thresholds associated with the discrimination of numerical arrays, and it will be of interest to investigate such in animals.

Prototype Matching and Absolute and Relative Numerousness Judgments

It is first useful to distinguish between absolute and relative numerousness judgments. This distinction is related directly to the distinction between absolute and relative class concepts proposed by Thomas and Crosby (1977) and discussed in the context of number by Thomas and associates (1980). In fact, numerousness concepts are examples of class concepts. Exemplars associated with absolute numerousness concepts, like exemplars of all absolute class concepts, have their concept-affirming properties among the inherent properties of each exemplar. For example, threeness is an inherent property of three simultaneously present items. Exemplars of relative numerousness concepts, such as "more" and "fewer" are not inherent in the exemplar; for example, 7 items are "more" when contrasted with 6 items but 7 items are "fewer" when contrasted with 8 items. A simple operational criterion distinguishes between absolute and relative numerousness (or other class) concepts; namely, whether it is necessary to compare exemplars in order to affirm the one that manifests the concept. It is necessary to compare exemplars of relative numerousness concepts but not of absolute numerousness concepts.

In the absence of counting, both absolute and relative numerousness judgments likely depend on prototype matching. A strong hint of this likelihood was seen in the Thomas and associates (1980) study. One of the two monkeys showed immediate transfer (responding correctly on 46 of 50 trials in the first session) to a 6:7 discrimination in the session following his having met criterion on a 5:6 discrimination. Responses to "fewer" had been reinforced throughout, which meant that the reinforcement contingency associated with sixness was reversed between the 5:6 and 6:7 problems. Prior to the 6:7 problem, "sixness" and "sevenness" had each been associated with nonreinforcement on a total of four previous problems (viz., 2:6, 3:6, 4:6, 5:6, 2:7, 3:7, 4:7, and 5:7), but responses to sixness or sevenness per se had never been reinforced prior to the 6:7 problem. Presumably the extensive experience with sixness and sevenness in problems prior to the 6:7 problem had resulted in the monkeys' acquiring discriminable prototypes for sixness and sevenness, which enabled the immediate discrimination of the 6:7 dot arrays. Consistent reinforcement for choosing "fewer" in several prior problems enabled the immediate selection of sixness on the 6:7 problem.

It may be noted that Thomas and associates (1980) deliberately confounded the absolute and relative class concept solutions in order to maximize the possibility of determining the monkeys best discriminative performances. Examples of studies that controlled against an absolute class concept solution and, therefore, showed a relative class concept solution are those of Dooley and Gill (1977) and Thomas and Chase (1980). Dooley and Gill demonstrated a chimpanzee's *(Pan troglodytes)* use of "more," and "less," and Thomas and Chase investigated squirrel monkeys' *(Saimiri sciureus sciureus)* use of "more," "less," and "intermediate" numerousness. The statistically reliable responses of the monkeys to "intermediate" also shows that they were capable of *ordinal judgments* of numerousness.

CONCLUSIONS

In conclusion, if our arguments are accepted (a) that subitizing and protocounting as number judgment processes should be abandoned and (b) that prototype matching is the basis for absolute and relative numerousness judgments, then prototype matching and counting are the only processes needed to account for numerical competence by animals.

A demonstration of the use of number necessarily implies a conceptual use of number. Otherwise, the results can be explained by the use of nonconceptual cues or nonnumerical solutions, especially those based on memorizing specific properties or patterns associated with numerousness discriminanda. In order to eliminate interpretations based on nonconceptual cues and solutions, it is necessary to control against the use of (a) inadvertent experimenter cues, (b) odor from the reinforcers, (c) confounded area or volume, (d) differential brightness cues, and (e) specific pattern cues, (f) stimulus generalization as opposed to conceptualization, and (g) rote-memory/learning-set-based solutions. In order to demonstrate a conceptual use of number, it is necessary to use (a) trial-unique exemplars, (b) only first-trial data when multitrial problems are used, or (c) data obtained from responses to test-exemplars that have not been associated with reinforcement; in all three cases, a stimulus generalization interpretation as summarized here must be excluded.

Regarding the processes by which number is used, counting is of the highest order, and we believe that Gelman and Gallistel's (1978) first three principles of counting provide the best definition and criteria of the evidence necessary to show counting. However, we disagree strongly with their assertion that it is not necessary to demonstrate a subject's use of tags. It is necessary at some point to demonstrate the use of tagging, because it must be shown that the subject has acquired the prerequisite symbol system that is the basis for tagging and counting.

Regarding noncounting processes, the most important numerical process is prototype matching. Prototype matching is a general process that describes and explains how humans and other animals use class concepts in general and, for present purposes, numerousness concepts in particular. Prototype matching is the basis for absolute numerousness judgments (e.g., affirming the "sevenness" of a set of seven items) and is a prerequisite for relative numerousness judgment (e.g., "more" and "fewer"). Prototype matching can be precise, such as, discriminating between arrays defined by successive numbers or imprecise, such as, discriminating between nonsuccessive arrays. The term *estimation* remains useful, because it is a general term related to quantitative judgments including but not limited to numerousness. Estimation is a descriptive, not an explanatory, term, and the basis for estimation is prototype matching, whether the estimated quantity involves length, area, volume, mass, time, number, and so forth.

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