12.1   Imitation in Animals: Function versus Mechanism and the Issue of Novelty
Thomas Zentall on Byrne

Much of the disagreement over what constitutes imitation comes from different origins of the question, Can animals imitate? More specifically, developmental and comparative psychologists typically ask, Can animals imitate as children do, and if they can, by what mechanism do they do it? whereas those who study animal behavior and behavioral ecologists (biologists) more typically ask, Can animals imitate and if they do, of what use is it to them?

In an attempt to integrate these two perspectives, Byrne ventures courageously beyond the narrow confines of the imitation of simple motor behavior typically studied by psychologists, into the complex actions involved in naturally occurring problems that animals encounter. However, he does so without adequately dealing with two important theoretical issues that have dominated the field of animal imitation.

First, Byrne admits that his behavioral parsing model does not speak to the issue of how an observer is able to translate the observed response of another into its own felt, but not seen, behavior (the opaque correspondence problem). At a functional level, it does not matter how an animal that is in the presence of other responding animals comes to respond in a similar way. Thus, for the behavioral parsing model, it does not matter if the mere presence of others increases the likelihood of the target response (social facilitation), or if the response of others draws attention to the objects involved in making the target response (stimulus enhancement), or even if manipulation of the object (e.g., breaking open nuts) shows the observer that nuts can be opened (affordance learning). It only matters that the response has adaptive value. However, to psychologists interested in

how animals are able to imitate, these are important alternative mechanisms that should be distinguished from imitation.

Second, Byrne distinguishes between *contextual learning*, in which behaviors already in the animal's repertoire are produced in novel contexts, and *production learning*, in which the behavior or behavioral sequence is not already in the animal's repertoire. The distinction between imitation of familiar and novel responses can be traced to Thorpe (1963). It may not be a useful distinction, however, because in a sense all behavior is an extension of, or generalization from, behavior already in the animal's repertoire. Byrne does not disagree, but he argues that in the case of more complex *sequences* of responses, it can be assumed that the particular sequence has probably never been performed. This assumption is questionable for two reasons. First, one must allow for the fact that the start and end points of the sequence (and perhaps even points in between) are likely to be determined by the nature of the task. More specifically, in the case of Byrne's example, processing of nettle leaves by mountain gorillas, the task must start with picking the leaves and end with placing the leaves in the mouth and chewing them. Furthermore, although the sequence of the remaining responses appears to be arbitrary (e.g., twisting off the stems that have nettles and folding the leaves so that the nettles are not on the outside), some responses may be more easily made before others and thus the order of those responses would also be influenced by trial-and-error learning, rather than imitation. If the probability of the target sequence of responses in the absence of observation of the sequence of responses made by others is not zero, then the distinction between novel and improbable is no longer useful because what Byrne calls contextual imitation could account for these results.

A more useful distinction in defining imitation involves the probability of the occurrence of the demonstrated behavior relative to that of an appropriate control condition. Although some have argued for the use of a predemonstration baseline control (see Anisfeld, vol. 2, ch. 4), typically such a control neglects many of the nonimitative factors mentioned earlier. Instead, a more appropriate control condition is one in which the effects of the response on the environment are the same and only the topography of the response is different (e.g., depressing a lever with a different part of the body). Ideally, the two responses would be selected so that observation of either would greatly increase the probability that the observer would then make a similar response. For example, Akins and Zentall (1996) found that Japanese quail that observed a demonstrator stepping on a treadle to obtain food learned to obtain food themselves by stepping on the treadle, whereas those that observed a demonstrator pecking at a treadle to obtain food

learned to obtain food themselves by pecking. Byrne (and others) would argue that both stepping (walking) and pecking were in the observers' repertoire prior to observation, but it is also unlikely that any of the birds had ever stepped on or pecked at anything like the treadle (a small, flat, elevated, platformlike device) used in this experiment. Furthermore, given the anatomy of the birds (they could not see their foot while stepping or their beak while pecking), it can be argued that both responses were perceptually opaque (Heyes & Ray, 2000).

Byrne proposes that this form of imitation should be treated as *response facilitation*. The presumption is that because both behaviors are familiar, observation results in the priming of the brain records of those motor behaviors. Evidence for the existence of mirror neurons that respond similarly to behavior performed and behavior observed supports the possibility of such priming (see Rizzolatti, vol. 1, ch. 1 and Gallese, vol. 1, ch. 3). However, in the case of mirror neurons, the perceptual features of the behavior observed and the behavior performed are generally quite similar (e.g., a grasping response with the hand). Therefore the problem of correspondence is greatly reduced. Furthermore, Byrne suggests that priming as a source of contextual imitation would be unlikely if testing were delayed, and as he notes, we have reported such deferred imitation in Japanese quail when observation and performance are separated by as much as 30 minutes (Dorrance & Zentall, 2001; see also Heyes, vol. 1, ch. 6). Thus we propose that experiments that have used the two-action method provide the best means that we currently have for studying imitation in animals. Finally, if, as Byrne proposes, it is the imitation of a sequence of actions that bestows a special cognitive status on an organism, we have recently found that pigeons show some ability to copy a two-response sequence (stepping on or pecking at a treadle, followed by pushing a screen, either to the right or to the left, that is blocking access to food; Nguyen, Klein, & Zentall, in press).

12.2 Novelty and Complexity: Two Problems in Animal (and Human) Imitation
Birgit Elsner on Byrne

Byrne highlights some aspects of imitation that have been neglected by researchers thus far: the observational learning of novel, complex, action sequences. These behaviors are especially relevant in animals, but also in everyday actions by humans. If individuals were able to acquire complex skills by observation, they could learn from merely watching

others' behaviors without needing explicit modeling. In the experimental investigation of imitation, novelty and complexity are difficult to define and to control, and this is probably the reason why evidence for production learning is weak at present. However, I doubt that Byrne's proposal to turn to observational data would overcome the problems of novelty and complexity. Moreover, although Byrne's examples of plant feeding techniques show that great apes perform standardized, remarkably complex actions, it remains unclear whether these behaviors actually represent observational learning of novel skills, and whether the acquisition of complex behaviors is actually driven by different processes than the acquisition of simple actions.

12.2.1 The Problem of Novelty

To Byrne, novelty will prove to be a cardinal requirement of imitation. However, the range of behaviors he accepts as being "novel" seems to be quite narrow. For example, he claims that nothing new is being learned in two-action experiments because the copied movements are in the individual's repertoire and are just applied to new situations. The question is, however, how individuals could imitate behaviors that are not in their repertoire. This becomes especially obvious in imitation by infants. A 9-month-old who has difficulty in releasing a small object from her hand will not imitate putting a ball in a cup (Elsner & Aschersleben, 2003). Nevertheless, deferred imitation of novel behaviors is a milestone in cognitive development (Piaget, 1951/1962). So the problem is how to define "novelty."

According to Meltzoff (1988a), an act can be novel in six senses: (1) It has never been seen, (2) performed, or (3) imitated by the infant before. (4) It is not a well-practiced game. (5) It has not been performed with a particular object before. (6) It occurs with near-zero probability in spontaneous play. The experimental evidence for "true imitation" fulfills at least criterion (6). Nevertheless, Byrne proposes to turn to observational data for evidence of imitation of novel, complex actions. But what is "novel" in the plant feeding techniques? Because the components of the action should be highly familiar to apes (e.g., stripping, folding), the novel part is the *organization* of the components, and this adds a seventh sense to Meltzoff's definition.

Byrne's second concern about the two-action method is that "true imitation" involves only contextual learning, whereas plant feeding requires the acquisition of a new behavioral complex, and thus production learning. Yet, according to Janik and Slater (2000), rearranging familiar, pre-existing signals into new combinations is *contextual* learning: "Production

learning refers to instances where the signals themselves are modified in form as a result of experience with those of other individuals" (2000, p. 2). If Byrne wants to transfer these definitions to plant feeding, it should not only include rearranging existing actions into novel sequences but also the addition of structurally different elements. However, the problem with the observational data thus far is that we know only the outcome of the learning process. If Byrne could present observations of juvenile gorillas who watched skilled models and added acts to their plant processing that they had never done before, this would be stronger evidence for production learning. But as long as we do not know how the action sequences are acquired, it is hard to accept the observational data as evidence for production imitation of novel behaviors.

12.2.2 The Problem of Complexity

Byrne's model provides an important theoretical contribution on how apes and humans could segment the fluid streams of movements they are confronted with while observing other agents. Although I highly value the model, I am skeptical about the idea that mirror neurons are the neuronal basis of behavior parsing (BP). Mirror neurons become active when a monkey observes a given action and when it performs that action (Gallese et al., 1996). Thus, all that mirror neurons can do is match an observed action to an action that is in the individual's repertoire. This is problematic in two senses. First, how could an individual parse an action that is *not* in its repertoire? To understand an action, an individual needs additional neurons that can process novel actions at least perceptually. Second, mirror neurons allow only action-level imitation; the observer performs the action he has seen. However, gorillas imitate behavioral organization on a program level, and this cannot rely on kinesthetic-visual matching, but has to take into account other aspects of the action sequence.

Byrne proposes that a string comparison enables the ape to detect the underlying structure of the observed behavior. However, a string comparison requires high cognitive capacity. The ape has to store the parsed sequences, decide which sequences lead to the same end, and then has to compute the statistical regularities within and between the components of these sequences. The question is whether and how great apes can actually do all that. An alternative idea would be that individuals parse behaviors, not in terms of movements, but in terms of subgoals.

By speculating that apes should be sensitive to the distribution of pauses in execution, Byrne highlights the role of functional modules within the sequence. D. Baldwin and Baird (2001) have shown that 10- to 11-month-

old human infants parse continuous everyday actions along "intention boundaries." They are surprised if an action is interrupted before the subgoal has been achieved. Similarly, Bauer and Mandler (1989) found that 19- to 31-month-old infants process the causal structure of action sequences. Thus, human infants process observed sequences of actions in terms of steps that lead to a final goal.

Concentration on subgoals reduces the complexity of behavior and would explain both the consistent ordering of the plant feeding sequences (which is guided by the succession of subgoals) and the idiosyncratic variation at lower hierarchical levels. Each animal may perform the parsed action in a slightly different fashion, but all animals obtain the same subgoal. However, this does not imply that apes have to understand the causality of the actions. They only have to memorize that action x leads to environmental state y, no matter why. Parsing behaviors along their subgoal structure would help the ape to determine whether its own action will lead to the observed consequence.

Taken together, the acquisition of complex behaviors by observational learning is an important issue in imitation research. The question is, however, whether we should assume separate processes for the acquisition of simple and complex behaviors. It is hard to imagine that an ape copies an observed novel sequence as a whole after extensive behavior parsing and string comparison. It seems more likely that the individual would begin with copying subgoals of the sequence by performing single actions, and finally arrive at performing the whole sequence by combining observational and trial-and-error learning. Taken this way, learning a complex skill may not be that different from the imitation of single actions investigated with the two-action method. The same processes of subgoal-oriented observational learning may help the individual to either achieve one goal by applying a single action from its repertoire in novel circumstances, or to achieve a hierarchical goal structure by rearranging multiple actions.

12.3 Do Parrots (and Children) Emulate Speech Sounds?
Richard W. Byrne on Pepperberg

Almost single-handedly, Irene Pepperberg has changed our view of vocal copying by parrots from a relatively trivial curiosity to an important window into the cognition of another species and a valuable foil for interpreting quasi-linguistic data from nonhuman apes. Whereas the average pet parrot blindly copies speech sounds, Alex and the other African Grey par-

rots exposed to the model/rival method use English words to answer questions and to make requests and comments. It seems that several aspects of the model/rival technique are important, including speaker-hearer role reversal, corrective feedback and demonstration, and constant social interaction; when *any* of these aspects are altered, the parrots fail to learn instrumental, referential use of human speech. This should serve as a reminder of how likely it is that our methods of examining animal cognitive abilities are not yet optimal, and that we are probably underestimating their abilities. In her chapter, Pepperberg has for the first time evaluated these data for what they tell us specifically about imitation.

She makes a telling comparison with learning birdsong, which is often regarded as the canonical form of vocal imitation. Pepperberg notes that the biological constraints that guide song learning, the limitations on what can be learned, and the fact that what is acquired is seldom quite the same as the model, make it difficult to claim most song learning as imitation at all. In contrast, when the vocalizations of *another* species are learned, evolved predispositions do not muddy the water, and we can safely attribute any close resemblance of the vocalization to imitative copying. The same has been found in motor learning. While we may strongly suspect that wild great apes learn many of their everyday skills by imitation, when those skills are natural ones for the species, it is very hard to be sure. In contrast, when captive great apes copy human actions, we can be sure that the actions were not somehow latent parts of the animal's natural repertoire, nor was their acquisition guided by evolved constraints (e.g., Russon & Galdikas, 1993, 1995).

Pepperberg makes a sharp distinction between Alex's imitation, which is referential and purposeful, and the "mere mimicry" of the pet parrot— following Tomasello in this dismissal (e.g., see Tomasello et al., 1993a). How such a classification would apply to birdsong is unclear, since song is clearly functional in many ways yet is not referential in its detailed content, and it is sometimes acquired by mimicry of more-or-less random environmental sounds, including ringing telephones, chainsaws, and car alarms. Pepperberg and Tomasello agree in treating mimicry as less cognitively demanding than imitation, but this seems to confuse the copying process (which is surely no less difficult in mimicry, or indeed different from it) with any subsequent use of the signal in cognitively sophisticated communication.

Pepperberg summarizes detailed acoustic analyses, which convincingly show that Alex and his kind produce human speech much the same way as we do: filtering (by cavity resonance) source vibrations (of the syrinx, in his

case). This lays to rest the more exotic idea, previously current, that independent sounds from the two sides of the syrinx interfere to produce a composite that resembles human speech. It also brings to the fore the central question: Are these parrots *imitating* speech, or copying it in some other way? Tomasello and his colleagues, following D. Wood (1989), have introduced a distinction between imitation *sensu stricto*, in which movement patterns are copied by observation, and emulation, in which what is copied are the results of actions, not the way in which they are produced (Tomasello, 1990; Tomasello et al., 1993a). Since the means of production are limited in any given species, when a result is attained by emulation, the method chosen will very often coincidentally be the same as that used by the model, and human observers may mistake the process for one of imitation. Alex, of course, cannot observe the movements of the human vocal chords in the supralaryngeal tract, and what he is copying with such fidelity is the result of those movements. By Tomasello's definition, his copying therefore reflects emulation, not imitation. (Note that Pepperberg uses the term "emulation" for "reorganization of sounds to create new labels," which would seem rather different.)

But surely, children can no more see the movements of adults' vocal tracts than can parrots. Should what they do also be described as emulation? In the sense of imitation beloved of comparative psychologists (e.g., Heyes, 1993) and sometimes called true imitation—characterized by rather detailed, element-by-element matching between model and mimic—the answer is probably "yes." However, there is more to it than that.

A child's copy of an adult's word is by no means an exact match; it differs in a distinctive way. All vowels are shifted upward because children have smaller vocal tracts. Hearing a child speak, adults automatically correct for this shift, in effect modeling the child's supralaryngeal tract, and correctly hear the vowel phonemes intended. Moreover, if the "real" adult vowel sounds were artificially inserted into the child's speech, we would hear them as different vowels or even just noises. This shows that when a child copies an adult's word, the process is not a simple, echoic one; the spoken word is analyzed or parsed into components, and the child then generates its own, synthetic version. Children's phonemes are produced the same way as those of adults, but they do not sound the same because of the difference in vocal tract size. Thus, when children copy adult words, the process is clearly not emulation, in the sense of copying a result, but is better described as a sort of imitation. Specifically, it is *program-level imitation* (R. Byrne, 1994, 1998b; R. Byrne & Russon, 1998), a synthetic process in which a novel organizational structure of behavior is learned by perceiving the

organization within another individual's behavior, and then copying the relevant section—building the same structure out of component elements that are already within the repertoire.

Evidently Alex can parse human speech at the words-within-sentences level, since he answers questions in ways that show he understands them. (Probably, then, he could imitate the sentences by resynthesizing the structure he perceives, out of words.) But I am not sure whether Alex's copying of human speech shows any evidence that the words themselves have been acquired by program-level imitation, as in the case of a young child. Careful examination of the details of his vocalizations for any comparable evidence of program-level imitation will be needed, and the result will throw new light on how Alex perceives the words he uses.

12.4   Some Reflections on Imitation in Human Language
Martin J. Pickering on Pepperberg

Human language makes use of a great deal of largely automatic imitation and is an ideal example of an application of current theories of imitation. I start by reviewing imitation-related work in human dialogue, and then relate this to Pepperberg's fascinating chapter. Given the current realization of the central importance of imitation to theories of mind and behavior, it is striking how little crosstalk there has been between theories of imitation on the one hand and cognitive psychology and psycholinguistics on the other. By drawing on some issues raised by Pepperberg, I hope to show how this connection can be made.

Most psycholinguistics has concentrated on isolated acts of production or comprehension, where there is little room for imitation. However, the most natural and basic form of language is dialogue, and it is quite clear that it involves a great deal of imitation. Studies using corpora show that it is highly repetitive in many ways (e.g., Tannen, 1989). In controlled experiments, interlocutors tend to "entrain" on referring expressions, so that if one refers to an object as a "sofa" or a "couch," the other will normally use the same expression (Brennan & Clark, 1996). They also entrain on particular "situation models," so that if one interlocutor refers to her position in a maze as "I'm in E4," the other will tend to say "I'm at A5," and will count in the same way (e.g., letters = columns, starting from the left); but if the first speaker says "I'm at the T-shape," the other might say "I'm at the L on its side" (Garrod & Anderson, 1987). Explicit negotiation is extremely rare, instead, interlocutors tend to imitate each other directly.

There are also strong tendencies to imitate grammatical form. Branigan et al. (2000) had interlocutors take turns describing pictures to each other (and in finding the appropriate picture in an array). One speaker was actually a confederate of the experimenter and produced scripted responses, such as "the cowboy offering the banana to the robber" or "the cowboy offering the robber the banana." The syntactic structure of the confederate's description strongly influenced the syntactic structure of the experimental subject's description, even when lexical items were not repeated. Interlocutors are of course unaware that they are imitating each other's grammar.

All this provides clear evidence that an automatic process of imitation occurs, and that it takes place at a range of different linguistic levels. This is particularly useful for theories of imitation because psycholinguistics draws upon very precisely defined levels of representation (e.g., syntax, semantics, the lexicon), so that we can say exactly what is being imitated. It also shows how important imitation is to real interactive language, simply because the effects are so strong (e.g., people find it quite hard not to refer to objects in the way that their interlocutor has just done). Pickering and Garrod (in press) argue that dialogue "succeeds" if interlocutors converge on the same understanding of a situation, and that imitation (which they call alignment) is the fundamental mechanism leading to such success. Dialogue therefore seems to involve a "perception-behavior expressway" at a range of linguistic levels (Dijksterhuis & Bargh, 2001).

Participants in a dialogue differ in their "status," so that a speaker seems to share a closer relationship with his or her addressee than with someone who is not currently being addressed, and indeed addressees seem to imitate speakers more than other people do. But it is interesting that experimental participants imitate speakers even when the speakers have not addressed them (Branigan et al., unpublished). Such a situation appears to occur in Pepperberg's model/rival training where the parrot watches an interaction between people. It is interesting that imitation appears to require that the trainers exchange roles, because this is clearly in keeping with forms of natural dialogue where a nonaddressee is "listening in" on a conversation or where two interlocutors are speaking to each other, but for the benefit of an audience (as in television interviews). We do not know precisely how forms of multiparty interaction differentially affect linguistic imitation; Pepperberg's studies may provide some insights into such effects.

It may also be of interest that intrinsic reinforcers are so necessary for learning. It is reasonable to argue that the reward that occurs during most dialogue involves successful understanding (either personal understanding

or realization that one's interlocutor has understood one). If so, understanding can perhaps be regarded as an intrinsic reinforcer, for the obvious reason that what is understood is the content of the dialogue. If an interlocutor received a reward for successfully participating in a dialogue, but successfully communicating a message was not really the interlocutor's goal (e.g., in an oral examination of linguistic competence), then imitation might be less likely.

Pepperberg's classification of levels of imitation is particularly interesting because, as she says, language involves considerable reference, functionality, and social interaction, and must involve higher-order imitation. She therefore suggests that it must be very different from the unconscious replication of others' motions in social settings (Chartrand & Bargh, 1999). It is not, however, exactly clear whether imitation needs to be improbable. I think that this depends on precisely what is being imitated and what is regarded as improbable. As I have said, interlocutors tend to refer to objects in the same way. If it is 60% likely that I will refer to an object as a "sofa" on first mention, but 90% likely after my interlocutor has called it a sofa, then this is surely imitation, even though it is not improbable. In contrast, it is of course extremely unlikely that I will utter "sofa" at a particular point, or that I will utter "sofa" with a particular set of acoustic characteristics. That said, the suggestion that most forms of linguistic imitation are higher order is almost certainly correct.

Overall, the connection between vocal imitation in Grey parrots and imitation in human dialogue may seem a distant one, but I suspect that some common principles relating to levels of imitation, type of social interaction, and perhaps nature of reinforcement can be found. I also hope to have demonstrated that studies of imitation, whether in humans or nonhumans, need to address the question of exactly what is being imitated. In this respect, linguistic imitation is particularly helpful because the levels of representation involved are (fairly) well defined.

12.5 Breathing New Life into the Study of Imitation by Animals: What and When Do Chimpanzees Imitate?
Bennett Galef on Whiten, Horner, and Marshall-Pescini

It took a hundred years, more or less, for behavioral scientists to come even close to a generally accepted demonstration of learning by imitation in any nonhuman animal (Galef, 1998). Whiten et al. now take it for granted that he, and others, have provided compelling examples of imitation in chimpanzees. Consequently, Whiten has moved on to a new stage in the study

of imitation. He asks, not whether chimpanzees can imitate, but what chimpanzees do imitate (acts, portions of acts, sequential structures of acts, hierarchical structures of complex sequences of acts), under what conditions chimpanzees imitate, and in what ways, if any, imitation differs in chimps and children.

Well, have they done it? Have Whiten et al. demonstrated imitation in chimpanzees? Personally, I am convinced that if the present evidence of imitation by chimpanzees in two-action procedures is replicated in other laboratories (and there is every reason to believe that it will be), the answer is "yes." Imitation in our great ape cousins will have been demonstrated.

We skeptics have thrown challenge after challenge to those claiming to demonstrate learning by imitation in animals. Happily, one after another those challenges have been overcome. It would be churlish to continue to demur, without solid grounds for demurral. The view that evidence of imitation is unacceptable unless an "imitated" act is novel (see Pepperberg, vol. 1, ch. 10) seems to me to foreclose the possibility of demonstrating imitation in species other than those that, like African Grey parrots, can produce an effectively infinite number of distinct outputs. We have no way of knowing whether an act "imitated" by an animal with a restricted behavioral repertoire is truly novel or a modification of a familiar act.

Why were we skeptics so hard to convince? It is not, as some have implied, that those who refused to accept early evidence of imitation in apes had a philosophical commitment to an unbridgeable gap in intellect between humans and apes. Nor are we unreconstructed radical behaviorists, unable to accept evidence of cognition in animals. Rather, until recently, the evidence for imitation in apes was not compelling (Galef, 1988), and if the scientific community were to accept weak evidence of imitation in animals, there would be no motivation to seek stronger evidence. In my view, if the field of social learning is to continue to move forward, as it has so remarkably for the past 30 years (Galef, 1998), it will do so only by parsimonious interpretation of strong evidence.

Whiten et al.'s chapter makes a convincing and substantial contribution to that forward momentum. It celebrates the opening of a new era in studies of imitation, the importance of which can best be understood in historical context.

Nineteenth-century naturalists considered imitation to be characteristic of women, children, savages, the mentally impaired, and animals, all believed to have little ability to reason for themselves (Darwin, 1871; Romanes, 1884; C. Morgan, 1896). Creative problem solving, what we today call individual learning, was considered the hallmark of rational minds

and was believed to be more or less restricted to mature European males who, because they could reason, only infrequently needed to imitate.

In the late 1890s, Edward Thorndike (1898), among others, started to see things the other way round. Thorndike felt, as most do today, that imitation required cognitive abilities beyond those needed to learn for oneself about environmental contingencies.

As is well known, Thorndike (1898) was unable to find evidence of imitation in the chickens, cats, dogs, and monkeys that he brought into his laboratory, though all learned by trial and error. He interpreted this failure of animals to imitate and their ability to learn by trial and error as revealing animals' inability to manipulate representations to solve problems. His views formed the basis of the behaviorist revolution.

The continued search for evidence of imitation in animals for the first 70 years of the twentieth century reflected an implicit questioning of the behaviorist *Zeitgeist*. For, in the Thorndikian view, if animals could imitate, then they must be able to manipulate representations.

The antibehaviorist revolution (Baars, 1986) of the past 30 years led to broad acceptance of the view that the behavior of animals as lowly as pigeons and rats is supported by cognitive activity. Consequently, the quest for evidence of imitation in animals lost its theoretical rationale in the search for evidence of animal cognition. The quest became a somewhat intellectually hollow, self-perpetuating enterprise.

That is why it is particularly important that Whiten and his colleagues have taken the next step. They have moved study of imitation in animals beyond an atheoretical, autonomously motivated search for evidence of a phenomenon to ask what is imitated, who imitates, and under what conditions is imitation most likely to occur. Such investigations have already breathed new life into a classic problem area in animal psychology that had been stagnant for decades.

Acknowledgment

12.6   Why Don't Apes Ape More?
Susan Jones on Whiten, Horner, and Marshall-Pescini

The larger context of the research by Whiten, Horner, and Marshall-Pescini is the question of the phylogenetic origins of imitation. A cross-species

comparative approach to this question can't help but be interesting to developmentalists like myself. I want therefore to comment on this broader aspect of the work instead of on the specific issue of whether chimpanzees are selective in their imitating. This is a bit hard on the authors, since the way they address the latter question is also interesting and perhaps more successful. However, it is what chimpanzee imitation can tell us about the origins of our own imitative behavior that I think may be of primary concern to many readers.

Actually, I do not think that much can be learned about the similarities and differences between the mechanisms underlying chimpanzee and human imitation in the kinds of studies reported here. Although in all of their comparative work the researchers model the same behaviors for the two species and then measure the same imitative responses, the chimpanzees and children are never really facing the same tasks. The two species come into the experiments with very different histories (including, for example, different amounts of experience with Plexiglas food boxes with complex fasteners) and ways of construing events in those histories that are likely to differ in unspecified but significant ways. The chimpanzees and children are on different cognitive developmental trajectories, have been on them for different lengths of time, and are moving along them at different rates. For at least these reasons, the tasks cannot be comparable for the two species, and thus it is not clear how similarities and differences in their rates and types of behavioral copying are to be interpreted.

What is clear and very interesting in this work is the revelation that imitation plays such different roles in chimpanzee and human lives. I am not referring here to the (unexplained) rigidity of the human children's copying versus the less faithful but perhaps more efficiently functional copying of the chimpanzees. Instead, I refer to differences in how common and how important imitation seems to be in the everyday behavior of each species.

I was surprised to learn how difficult it has been to demonstrate that chimpanzees do in fact imitate at all. And although that point seems to be settled, it is still striking that the copying that chimpanzees do do (at least in captivity) seems so unnatural to and effortful for the animals. Spontaneous imitation is apparently not observed among captive chimpanzees. And the copying elicited in experiments always seems to be extrinsically motivated, for example, by the prospect of special food rewards. Even then, the imitation that results is typically partial, which might be because it is selective, as the authors argue. However, it may also be because chimpanzees are not only not inclined to imitate, but are also just not very good at it.

In contrast, spontaneous imitation is easy to find in human behavior. It's everywhere, across ages and across cultures. And it does not seem to be tied at all (or at least not at all tightly) to any particular biological functions. Humans from an early age may learn clearly functional things through imitation: how to cross a busy street, pronounce a word, use a spear-thrower, or grind corn. But imitation can also occur in the service of just about any of the motivations that humans experience. It often looks intrinsically rewarding; toddlers touch their foreheads to a table just because it's their turn to do so. It can be done for a wide range of social purposes; think of how you learned to dance or to extend or accept an invitation to dance. Human imitation is often playful. And any parent with more than one child knows that imitation can be used as a method of torture. So, imitation is a very flexible and effortless multipurpose kind of behavior in humans.

The big comparative questions, then, would seem to be, first, how to account for the qualitative differences in the nature of chimpanzee and human imitation, and second, what that account might tell us about the nature of imitation as observed in each species. There are, of course, a variety of approaches to these questions. Until recently, one entirely respectable approach would have been to look for commonalities and differences in imitation by chimpanzees and humans. The commonalities might be attributed to a common hominoid ancestor, or perhaps to parallel evolution of similar mechanisms for dealing with similar environmental challenges. The differences might be attributed to selective pressures encountered since the two species diverged. The expectation would be that imitation in the nonhumans would be more limited, fragile, and harder to find than in humans (as it doubtless is) because the human imitative capacity, like so many other human capacities, is more highly evolved.

The trouble with this kind of approach is that it is associated with a highly questionable sort of folk psychology that posits the existence of modular cognitive and behavioral capacities that can be inherited as units. Historically, attributing behavior to innate modules has been a convenient way for psychologists to beg the explanatory questions. Now, however, neuroscientists have developed the tools to begin looking for these modules in human brains and they are not finding them. And while the evidence for innate behaviors remains poor, evidence for alternative ways of explaining our observations (for example, from connectionist models) becomes more and more compelling (e.g., Elman et al., 1999).

I would therefore like to argue for a different question and a different approach to cross-species comparisons of imitative behavior. The question

is, What common processes are at work in different instances of imitation? The approach is developmental. Unfortunately, I cannot point to a model in human developmental psychology. We who study humans have a very long way to go to account for the instances of imitation we see in human children. Contrary to widespread belief, however, I would argue that no inborn imitative capacity has been shown or will be shown to exist in humans (Jones, 1996, 2001). Therefore, I think there is a developmental story to tell.

Imitation seems just as unlikely as any other complex, flexible behavior to turn out to be a unitary or modular capacity. That means that imitation is unlikely to be something that is inherited. It seems much more likely that specific instances of imitation are produced by, or "emergent from," some combination of subunits of different skills and different kinds of knowledge, each with its own developmental history. A number of those components may well be learned in the typical human caregiver– infant relationship. And one product of some subset of those acquired components may be a general-purpose ability to copy the behavior of others.

Two apparently unrelated bits of knowledge seem relevant here. First is the fact that mothers across different cultures imitate their babies and in particular that they imitate the babies' vocalizations (e.g., Parton, 1976; Papousek & Papousek, 1989; Kokkinaki & Kugiumutzakis, 2000). So babies have months of experience of face-to-face interaction in which imitation is demonstrated to them in a turn-taking situation. And during those months, they begin to imitate the sounds their caregivers make. But the caregiver's imitation comes first.

Second is a bit of information about those uncommon humans who do not follow the normative developmental course (whatever that is) to free and flexible imitative abilities. I refer to the subset of autistic children who do not imitate. The failure to imitate, along with other cognitive deficits in autism, is often studied as one among many symptoms of the lack of a "theory of mind" and this lack is often made to sound like the result of a missing module.

However, many years ago Ovar Lovaas at the University of California at Los Angeles worked out a way to teach autistic children a wealth of different things using discrete trial training (e.g., Lovaas, 1987; Lovaas & Buch, 1997). And his very effective intervention starts with teaching the autistic child to imitate. Through Lovaas-inspired discrete trial training, many, many autistic children have learned to imitate first one specific behavior and then another. After learning to imitate some number of individual behaviors, many autistic children have shown a more general copying capacity. Some of these children have then gone on to master language, false-belief tests, and all kinds of other things.

The relevant point for the present discussion is that imitation can be learned. This suggests that imitation may in fact usually be learned, in the usual course of development. Or put another way, some combination of behavioral experiences, expectations, awareness, and knowledge of self– other physical and cognitive and motivational similarities, as well as other kinds of knowledge may occur or emerge in development, and may combine to enable and motivate behavioral copying. So now, what if human children actually do begin to learn to imitate in the context of thousands of social exchanges with their caregivers? Well then, the poor chimpanzee is at a distinct disadvantage unless someone imitates thousands of his actions from an early age. In short, I wonder if what the chimpanzee needs to develop a much stronger and more obvious imitative ability is not several million more years of evolution, but instead a large dose, administered early, of appropriate (though not typical for the species) experience.

Perhaps then, someone ought to use discrete trial training to teach young chimpanzees to imitate first one behavior and then another and another. Whoever does it should teach imitation for its own sake rather than as a means to another experimental end. Then they should go on to test for generalization of the learned imitation, and they should look for the emergence of an imitative motive in instances of spontaneous imitation. I think the results of this kind of enterprise are potentially fascinating, and the contemplation of such an enterprise is potentially important to anyone interested in how behavior works in any species.