ON THE OTHER HAND

On the Columban simulations: A reply to Gallup

I object strenuously—as strenuously as academic print will allow—to the recent review (*CP*, 1984, 29, 593–594) of the Research Press film *Cognition, Creativity, and Behavior: The Columban Simulations*. The film describes studies I did with Skinner and others in which complex, humanlike behavior was produced in pigeons.

The review, by G. G. Gallup, Jr., is unscholarly and misleading. Gallup does not review the film per se; rather, he uses it as a device for commenting on and dismissing the simulation research. He does this not on the basis of the published literature but entirely on the basis of snippets of the film and its narration. Note, however, that Skinner and I did not make the film! In other words, Gallup has evaluated a body of serious research on the basis of what a film production company said about it—in a twenty-five-minute commercial film for undergraduates.

For the record, papers on this research have appeared or will soon appear in many scholarly journals, several books, and elsewhere (e.g., Epstein, 1981, 1983, 1984b, in press, and the references cited therein). Psychologists of several persuasions—Gestalt psychologists, comparative psychologists, cognitivists, and praxists alike—have reacted to the work enthusiastically. Gallup's title, "Will Reinforcement Subsume Cognition?," misses the point. Far from being an indictment of one point of view or another, the work has opened up productive and exciting lines of communication between various disciplines.

The review says nothing about the value of the film as a classroom tool, which is clearly what it was meant to be (it even comes with the proverbial discussion guide, prepared by Frederick Kanfer of the University of Illinois). It is beautifully edited, entertaining, and downright provocative, no matter what one's point of view. It contains superb color footage of pigeons doing what may be the most complicated things pigeons have ever done in the history of their species—solving the box-and-banana problem, making memoranda, engaging in simple languagelike behavior, and so on. It also raises provocative questions about the applicability of such studies to humans; the extrapolation problem is dealt with more rigorously elsewhere (Epstein, 1981, 1984b). In a document prepared by E. McGrath (1983), chair of the Films and Other Media Committee of the American Psychological Association, the film was singled out as one of the most outstanding films shown at the 1983 convention of the association. Audience ratings were in the "very good" to "outstanding" range.

In this department, CP *invites discussion of reviews and of books reviewed. Typically, contributions are limited to half the space of the text criticized.* CP *edits letters when it thinks they should be edited.*

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Gallup dismisses a demonstration of what appears to be spontaneous imitation as what Thorpe (1963) would call "local enhancement"—one pigeon has simply called attention to some location, and a second pigeon now pecks there. But, again, Gallup has fallen into the trap of commenting on substantive research entirely on the basis of a few seconds of footage. The series of studies on which these few seconds are based (e.g., Epstein, 1984c) shows unequivocally that local enhancement is the wrong label for the imitative behavior we observed. Pigeons imitate pulling just as readily as pecking, and we have observed deferred effects after twenty-four hours and longer. "True imitation" (Thorpe, 1963) also seems an inappropriate label, but the effect is far more significant than Gallup suggests.

Gallup is most concerned with the mirror-use behavior shown in the film, since this bears most directly on his own work. Gallup (1970) showed that chimpanzees who had extensive experience with mirrors could use a mirror to locate a spot on their bodies that they could not see directly, and Epstein, Lanza, and Skinner (1981) showed that pigeons who had been taught how mirrors work and who had also been taught to peck spots on their bodies could do the same. Gallup (e.g., 1979) has insisted that only the higher primates could pass this test, and so our result was no doubt disturbing.

Again, the trap. Gallup asserts, on the basis of a few seconds of footage in the film, that he has the "overwhelming impression" that the bird shown is merely "searching in vain" for the dot in mirrored space. His impression is beside the point. As we reported in *Science* (Epstein et al., 1981), we had three independent observers score six randomized videotape segments obtained during control and experimental periods with three birds. The observers recorded what *they* judged to be "dot-directed" responses, and no further instructions were given. Note that if the birds were merely searching in vain for dots, they would surely have turned around, since virtually all of the dots they had ever seen in training had appeared behind them; none turned. Gallup (1970) also reported "mark-directed" responses, not touches. Moreover, he reported seeing an average of only one such response every 4.5 minutes (the rate obtained from our observers was ten times as high), and he seems to have been the only observer.

Gallup cites what he calls a "failure to replicate" the Epstein et al. (1981) study—a convention talk by Gelhard, Wohlman, and Thompson (1982). How utterly misleading. Gelhard et al. reported having a heck of a time training two pigeons to use a mirror. They gave up on one bird after nearly a year. In contrast, one of our birds achieved a high level of proficiency after only fifty-nine minutes of training, and none required more than fifteen hours. I doubt that Harvard birds are smarter; it seems more likely that we were simply using different training techniques. Roger Thompson (personal communication, December 1983) himself has said that he has "no doubt" that we achieved the result we reported, and he recently told me that he has decided to withhold publication of his report until he has had a chance to replicate our procedure more closely. Furthermore, Carl Cheney (personal communication, May 1984) of Utah State University has conducted a systematic replication of our study and reports positive results with all three of

the birds he tested. Finally, as Gallup knows, one must be careful in interpreting what appear to be negative results. I. S. Russell (1978) of the University of London has reported repeated failures to replicate Gallup's (1970) results with chimpanzees. I, for one, do not doubt Gallup's (1970) report—after all, pigeons can do it.

The simulations have led to a formal theory of the emergence of novel behavior, and a model I have developed has proved reasonably successful in predicting the emergence of complex, novel behavior moment-to-moment in time in human subjects (Epstein, 1984a; cf. Epstein, 1985, and Epstein, Kirshnit, Lanza, & Rubin, 1984). It is a shame that the animal research that has made this possible should be dismissed summarily on the basis of a cursory, secondhand account. When I expressed my dismay over this review to a senior colleague, he commented, "It is better to be criticized unjustly than to be ignored." I'm not so sure.

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Superficial simulations: A reply to Epstein

Epstein implies that because some of my work was the subject of one simulation, my review was biased and self-serving. Rather than be party to an acrimonious and, in this context, unproductive debate about pigeons and mirrors, I would refer readers to detailed accounts of the problem published elsewhere (e.g., Gallup, 1982, 1983). Suffice it to say, however, that Roger K. R. Thompson (personal communication, August 26, 1984) has now failed a second time to replicate Epstein's work with pigeons and mirrors, in spite of making every attempt (including numerous telephone conversations with Epstein) to duplicate exactly Epstein's procedures.

For purposes of this rejoinder I will focus on two other Columban simulations depicted in the film. In an attempt to show that the languagelike performance of the chimpanzees Sherman and Austin is nothing new, Epstein, Lanza, and Skinner (1980) taught two pigeons named Jack and Jill to engage in a quasi information exchange. Lest readers be misled to believe that I am alone in my view that this entire approach is suspect, consider the following quote from a critique of the Jack-and-Jill study that recently appeared in (none other than) the *Journal of the Experimental Analysis of Behavior.*

> The study did not deal with the whole of the complex behavior that was reported for chimpanzees, yet it implied that it did and that nothing remained to be explained. The present paper argues that a good deal remains to be explained and that any apparent similarity between the verbal behavior achieved by Sherman and Austin and the behavior exhibited by Jack and Jill is superficial and reveals little about the nature of mands, tacts, or the process of communication itself. The Jack-and-Jill study, if undertaken from a conscientious comparative perspective, could lead to a better understanding of the phenomenon of communication from an evolutionary perspective. Pigeons surely do have something to tell us about the emergence of language processes, but until the work with them moves beyond satirical simulation, we are not likely to find out what it is that the pigeon can say. (Savage-Rumbaugh, 1984, p. 248)

As for Epstein's simulation of Köhler's insight experiments, Pastore (1984) points out that twenty years ago he had done with canaries what in essence Epstein, Kirshnit, Lanza, and Rubin (1984) claim to have accomplished with pigeons. In spite of the fact that the canary results appeared in a total of five different sources, Pastore's work went completely unacknowledged both in the film and in Epstein's published report.

Alas, the simulations are not only superficial, satirical, and difficult to replicate, but they lack scholarship and in some instances the data on which they are based are not even new.

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Further comments on the Columban simulations

Pastore's experiment has about as much in common with the Epstein et al. (1984)

study as pigeons have with chimps (which isn't much). *The single canary* that Pastore confronted with a variant of the box-and-banana problem required *twenty-five reinforced trials* before it could move its box (a cardboard "prism") smoothly to the correct position on the floor of the chamber (Pastore, 1954, p. 288; 1955, p. 73). In other words, the behavior that superficially resembled the behavior of Köhler's chimps was, as Pastore reported, *learned,* like the escapes of Thorndike's cats. By no reasonable criteria could the canary's performance have been considered "insightful." The field long ago passed judgment on this modest demonstration (Epstein, 1984c).

In contrast, Epstein et al. (1984) reported a systematic study with eleven pigeons in which three pigeons with relevant training histories solved the box-and-banana problem in a dramatic, humanlike fashion *the first time they were ever confronted with it.* The performances satisfied all of the traditional criteria of "insight": Periods of apparent confusion were followed by sudden, rapid, and entirely appropriate performances. By systematically varying the training histories of other birds, we also determined the probable contributions that a variety of different experiences had made to success in the problem. Finally, we offered a running account of the novel performances in terms of empirically validated principles.

Regarding Thompson's failures, I can only offer a quote from a manuscript I received recently from C. D. Cheney, which reports a replication of my self-awareness experiment (Epstein, Lanza, & Skinner, 1981) with four pigeons: "Given the relatively modest level of sophistication and experience of the trainers in this study, the results indicate a rather robust phenomenon" (Cheney, 1984, p. 6).

I have written at length about what simulations do and do not reveal (e.g., Epstein, 1984d) and have made only modest claims: Properly constructed simulations in science provide "plausibility proofs" of a hypothesis; they prove merely that a conjecture is plausible. In evolutionary biology, meteorology, praxics, and other fields in which direct experimentation is often impossible, the simulation is a useful tool for exploring ideas.

The Columban simulations have proved useful in two ways: First, they have allowed us to generate increasingly complex, novel performances in simple organisms. Several weeks ago, for example, we achieved the spontaneous interconnection of four repertoires in a pigeon (cf. Epstein, 1985). Second, they have led to the development of equations that have proved effective in predicting complex ongoing behavior in humans (Epstein, 1984b).

The history of psychology convinced me long ago that the kind of debate in which I am engaged with Gallup is entirely pointless. He is interested in mind; I am interested in behavior. We are in separate fields (cf. Epstein, 1984a).

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Columban inconsistencies: The debate goes on

I was surprised to learn that Epstein now feels that pigeons and chimpanzees do not have much in common. Isn't this antithetical to the whole point of the so-called Columban simulations?

It was also revealing to note how quick Epstein is to dismiss the performance of Pastore's canary as having been "learned," only to then describe his own pigeons as having had "relevant training histories."

Epstein argues that because he is interested in behavior, he and I are in separate fields. I disagree. Operant psychologists do not have a monopoly on behavior. The issue is whether you choose to use behavior to pose interesting questions, or whether you want to dismiss or even preclude such questions with superficial simulations.

Whereas Epstein concludes that the debate is pointless, I have found it to be very worthwhile, if for no other reason than to expose some of the weaknesses of the operant approach to cognitive phenomena.

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