

cidence of unwanted pregnancy is completely irrelevant to the argumentation.

Forgive my deep concern over the prospect of "large-scale, scientifically conducted, longer term empirical studies of fertility response to various incentives or disincentives" (David, 1986, p. 312). While our departmental review boards are busily straining at gnats, a pseudo-ethics of incentives and disincentives, of "priority interest" (David, 1986, p. 309) and David-determined "individual, family, and community well-being and an improved quality of life" (p. 309), is being presented in the place of accurate reporting, as if it were a legitimate means of "widening psychology's sphere of influence" (Kennedy & David, 1986, p. 296).

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Population and Health Psychology: A Response

Henry P. David

*Transnational Family Research Institute
Bethesda, MD*

In his comments on my March 1986 *AP* article, O'Connell (this issue, pp. 269-270)

demonstrated for me, as I had learned earlier at Notre Dame (David, 1980), that well-meaning people can have very differing perceptions on the sensitive issues of population, reproductive behavior, and fertility regulation. On rereading my essay, in which I offered my personal perspectives (David, 1986, p. 309), and the article by Holden (1986) to which O'Connell refers repeatedly, I am struck by what appears to me as sincere but selective perceptions. Let me respond only to what I believe to be the major documentable points of reference in O'Connell's comments.

O'Connell faults my perspectives because "they do not take into account the more recent revisionism reflected in a National Research Council (NRC) report that downplays the role of population" (p. 269). Aside from the fact that the NRC report was issued several months after my essay had been prepared, O'Connell's source, Holden (1986), wrote that the report, "says that rapid population growth, while not the main cause of all problems in the Third World, is more likely to impede progress than promote it" (p. 1493). Moreover, added Holden, "the extent to which the report is 'revisionist' is a matter of debate" (p. 1493). In addition to the NRC conclusions summarized by Holden and cited by O'Connell, other conclusions of Holden's not cited by O'Connell suggest that "the absolute number of uneducated people (also) rises with rapid population growth" (p. 1494), that "slower population growth will increase the rate of return to labor and reduce income inequality" (p. 1494), and that "sexual inequality will be reduced by programs to improve contraception" (p.

1494). The quotation O'Connell uses that population growth is no longer cast in the role of "one of the chief villains behind every major social, environmental, and economic problem plaguing developing nations" (in Holden, 1986, p. 1493) derives neither from the NRC report nor from Holden but from observations made by Kelley at the National Academy of Sciences symposium convened in conjunction with the NRC report's release.

As noted by Holden (1986), the NAS speakers "reported that family planning programs and economic development reinforce each other" (p. 1494); and "the general message was that, even if economic grounds for family planning are not as compelling as some maintain, they are amply justified on the basis of individual family health and welfare" (p. 1494). I can only repeat what I wrote in my essay that, in my opinion, individual, family, and community well-being, an improved quality of life, and freedom of choice in reproductive behavior are becoming areas of priority interest for health psychologists.

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Language in Child, Chimp, and Gorilla

F. G. Patterson

University of Santa Clara and San Jose State University

C. H. Patterson

University of North Carolina at Greensboro

D. K. Brentari

Gallaudet College

Terrace (September 1985) presented thought-provoking ideas about the development of naming behavior in children and apes. However, his argument suggested a qualitative or categorical difference in the naming behavior of the two, not a difference of degree. He saw not a continuum

of communicative behavior between humans and their closest biological relatives, but a discontinuity for which there is little precedent in the history of evolution (Wiener, 1984). Three major points will be considered here.

On Naming

Terrace (1985) made the following statement:

In emphasizing the ability to generate sentences as a uniquely human skill, psycholinguists have overlooked an equally important and perhaps more fundamental skill—the ability to refer with names. The same oversight can be attributed to the first generation of projects devoted to teaching an ape to use a language (p. 1011)

It may be rational to suggest, as Terrace did, that ape-language researchers

should not yet attempt to write grammars for apes' linguistic behavior. The route from infant pragmatic intentions to adult grammar is complex, and researchers studying the development of human language are still unsure of when children's communication becomes language. The issue becomes more complex with American Sign Language (ASL). Early conclusions on ASL grammar and syntax (Liben, 1978) are being reconsidered in light of recent phonological and morphological studies (e.g., Liddell, 1984). Using ASL with primates allows more spontaneity and flexibility in communication but adds to this problem. Nevertheless, preliminary reports (Chown, 1974; Fouts & Mellgren, 1976; B. Gardner & R. Gardner, 1971; Patterson, 1978b, 1978c, 1979, 1984; Ter-

race, 1979) suggest regularities in structure that are unexplained by imitation and that may be based on the apes' own encoding and reformulation processes. Terrace discounted this evidence and disregarded important data from projects that have not had grammatical analysis yet. A more fruitful approach may be to study the *function* of communication for apes, which would shed light on language evolution.

Terrence (1985) argued that chimpanzee referential behavior requires concrete rewards from teachers and that, although non-language-using chimpanzees will communicate with one another about food locations or about objects of prey, "such communication is in the service of some concrete end and is not intended simply to inform a companion that some feature of the environment has been noticed" (p. 1022).

Videotapes of a group of five chimpanzees (including one who learned signs exclusively from other chimpanzees) communicating in sign without human intervention clearly demonstrate that teacher incentives or rewards are unnecessary for chimpanzees' symbolic communication (Fouts, Fouts, & Schoenfeld, 1984). Reassurance, social interaction, and play accounted for 88.2% of the chimp-to-chimp utterances; feeding and four other categories accounted for the remaining 11.8%—an unexpected result if Terrace's line of reasoning is correct.

Noting that children refer to objects spontaneously, Terrace (1985) contended that "there is reason to doubt whether the most intensive training program imaginable could produce an ape that would approximate a child's natural ability to refer to objects as an end in itself" (p. 1017). On the contrary, apes do sign to themselves about their activities and surroundings, as documented in filmed records of the gorilla Koko (Harrar, 1983; Jampel, 1981; Schroeder, 1978) and the chimpanzee Washoe (Gardner & Gardner, 1973), and in published accounts (Patterson, 1978a, 1979, 1980a; Patterson & Linden, 1981). Contrary to Terrace's contention, apes often share information contained in such "noticing responses" as "that soft," referring to a velvet hat (Patterson, 1980a).

Terrence (1985) stated that apes' vocabularies consist mainly of names used in the presence of particular stimuli to earn rewards, and he asserted that after "projections of human meanings were stripped away, . . . it became clear that the ape's use of symbols amounted to a means of expressing demands for various incentives" (p. 1023). A breakdown of Koko's vocabulary at 6½ years of age by word type (Patterson, 1980a) revealed that

over 35% of the lexical items were not nominals. Furthermore, Koko seeks confirmation of her understanding of words and new information about her environment by using facial expression to change signed statements into questions (e.g., "That ink?"), pointing to a red flower on her smock (Patterson, 1979) or by using question signs (e.g., "For-for that?" on her first exposure to a woman in curlers). Meanings at more than one level have been reported: When asked, "What can you think of that's hard?" Koko answered, "Rock . . . work."

The Language Acquisition Support System

Terrence found the following:

For an ape to learn a new symbol, [it] had to be paired repeatedly with the relevant exemplar and a potent primary reinforcer had to be furnished for the correct selection of the symbol.

[U]nlike children, who are able readily to add new items to their vocabularies in response to casual instruction (or without any instruction at all), apes are able to do so only in narrowly structured situations and with extensive drill. What appears to be lacking in the case of the apes is an understanding of the fact that one can refer to an object by name. (p. 1021)

This is simply not true. The chimpanzee Louls learned 47 signs with no human intervention (Fouts, Fouts, & Schoenfeld, 1984). Like children, apes add new vocabulary items uninstructed (Gardner & Gardner, 1969; Patterson, 1978c, 1979, 1980b, 1986; Patterson & Linden, 1981). Koko frequently invents new signs for unfamiliar concepts, a process related to mental semantic cataloguing (Patterson, 1980b). Examples like Koko's sign for thermometer (tucking index finger under arm where temperature is taken) show that Koko is an active participant in the mental structuring of her world.

Apes have also composed new names of two or more independent signs. Mellgren, Fouts, and Lemmon (1973) and Fouts (1974) reported spontaneous novel sign combinations in Lucy, a chimpanzee, when she was presented with 24 fruits and vegetables over a period of 12 days (e.g., "cry hurt food" for an old radish). Similar novel combinations have been reported by Patterson (1979, 1980a, 1986). During a randomly chosen two-month period, Koko produced 15 such novel multisign combinations to label particular concepts (Patterson, 1979).

Referring to the Language Acquisition Support System (LASS), from which the production and comprehension of words emerge in children, Terrace (1985) stated, "There is no compelling evidence of analogs of LASS in interactions between

an infant ape and its natural or surrogate (human) parent" (p. 1019).

A LASS in any individual—ape or child—involves a complex, culturally influenced interchange between caregiver and child in the very early stages of development. Plooijs's (1978) observations of wild chimpanzees revealed parallels between preverbal pragmatic behaviors in children and such behaviors in chimpanzees. When an ape is taught a human language, the issue becomes more complex, and researchers have just begun to investigate this intriguing area. Chevalier-Skolnikoff (1981) presented strong similarities between apes engaged in language research and human children in Piagetian stages of sensorimotor development.

Unedited Transcripts as Evidence

Although Terrace criticized authors who do not publish "unedited transcripts," he himself has not done this (Terrence, 1979; Terrace, Pettito, Sanders, & Bever, 1979). In his reports, Terrace strips utterances of their context and divorces communicative acts from their settings, providing an unbalanced and often inaccurate picture of language performance. Unedited transcripts are useful in certain contexts, such as theses and dissertations, and have been presented by Patterson (1979).

Terrence leaned heavily on his own analysis of videotapes from Project Nim, the transcripts of which have been withheld from peer evaluation. These tapes reveal a serious flaw in Terrace's approach, as they are not records of conversations but records of repetitious training sessions. As Terrace noted, his research has been severely criticized (e.g., Gardner & Gardner, 1985; Patterson, 1981a, 1981b; Patterson & Linden, 1981; Yaeger, O'Sullivan, Autry, & Ingersoll, 1981). Terrace is strangely mute on the issues raised and blind to data contradicting his own ideas. He does not consider a subsequent study of his subject, Nim (Yaeger et al., 1981) revealing that Nim performed much better when videotaped in a more natural setting. Nim's level of spontaneity was 44% in conversational settings, but 14% in training sessions that were structured similarly to Terrace's videotapes (in which Terrace reported 13% spontaneous utterances). When evaluating capacity or potential, one should consider the entire range of performance in all settings.

Conclusion

Terrence concluded that he had identified a "nonsyntactic difference between animal and human consciousness," that of naming both external and internal states. Evidence of naming external states by apes

is readily available in the literature, and evidence that apes can refer to internal states is accumulating (Patterson, 1978c, 1979, 1980a, 1986; Patterson & Linden, 1981). A thorough examination of such abilities should yield differences of degree, not kind. Terrace's conclusion hinges on selective citation of the literature, and his judgments are made on the basis of the poorest performances in the most restrictive environments. This approach is neither objective nor productive.

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A more extended version of this article with additional references is available from F. Patterson, The Gorilla Foundation, Box 620-530, Woodside, CA 94062.

Response to Terrace

Emmanuel Bernstein
Adirondack Counseling

Terrace's article (September 1985) raised fascinating questions and directions. I was especially intrigued and excited about his idea of a potential study in which a primate might be allowed to enjoy an outdoor, complex environment so that the scientist could study more natural capabilities in nonhuman primates. I am sure I would perform better if I were not in a cage.

Terrace seemed to have again taken his famous position that nonhuman primates have rarely, if ever, approached anything close to human thinking. He seemed to emphasize that they do not use language spontaneously, nor do they use grammar in a complex enough way, nor do they culturally transmit as humans do. However, there are data showing otherwise.

The Fouts' studies have shown that chimpanzees talk spontaneously with one another all the time, especially in the absence of humans. This finding seems to contradict the notion that talking with signs occurs only from reward by the experimenter and in response to human cues. The Fouts' carefully controlled studies use remote videotape in the absence of humans. With 93% interobserver reliability, the Fouts' laboratory has recently tabulated conversations between three chimpanzees (Washoe, Dar, and Tutu) and discovered that in 88% of the conversations, 39% of the time concerned social matters, 29% asking for reassurance, 20% about play, and only 5% about feeding (Fouts & Fouts, 1985).

Rather than looking for complexities in verbal human language, a more productive and meaningful direction might well be through making a comparison between nonverbal humans using sign language and nonhuman primates using sign language. Wouldn't a study that compared children who have used only signs to verbalize with nonhuman primates who have used only signs to verbalize, for example, be especially relevant? Also, it seems that a simple, non-English language might show even more promise.

As far as sharing events goes, in the Gardners' laboratory (Gardner & Gardner, 1975) as well as in the Fouts' lab, the chimps often use signing to tell observers about things the observers are unaware of. The Fouts have used double-blind procedures to test these communications. For example, just recently, Dar was observed signing "dog" while looking out of a window, and the observer confirmed that a dog was in view outside