

Letters to the Editor

Questions Concerning Facilitated Communication: Response to Duchan

This letter is intended to express our concern with the featured tutorial article on facilitated communication in the December 1993 issue of *JSHR* (Duchan, 1993). As representatives of the autism and augmentative and alternative communication (AAC) research communities, and as editorial consultants for *JSHR*, we feel an obligation to express this concern. The article states that we have much to learn from research on facilitated communication (FC) when, in fact, scientific evidence is overwhelming that FC users are not communicating anything independently.

Facilitated communication is a discredited technique that has failed to demonstrate its validity in numerous trials. As of February 1994, there were 16 published articles that established that written output produced by FC is dependent on the facilitator, not the person with a disability (see references). Every controlled study has shown that when the facilitator does not know the question or the correct answer, the disabled person cannot provide the target response. Allegations of abuse, usually of sexual abuse, made through FC have been disallowed from court proceedings in numerous states on the basis of these findings. The *JSHR* article discussed only three controlled validation studies, giving the false impression that the validation literature is small. In fact, the extant literature refuting the validity of FC is extensive.

The arguments made by proponents of FC questioning the truth of these findings are hollow. The tutorial states, for example, that in the early stages of facilitation, facilitators are trained to ask questions "requiring familiar, easy to access, single word answers." These are exactly the kinds of questions used in the validation procedures. Yet, claims are made that word-finding difficulties may prevent FC users from responding appropriately in the validation procedures. Why would FC users be able to respond in the earliest stages of facilitation to these types of questions and be unable to do so later? Proponents claim the problem lies in a lack of trust and a failure on the part of the validation experimenter to assume the FC user's competence. Yet even validation studies carried out with familiar facilitators yield the same results. Are we to believe that the mere presence of a skeptical examiner renders the FC user unable to communicate?

One of the article's most surprising arguments concerned an explanation for how FC users learn to read and spell so rapidly without any instruction. The claim is made that they have learned passively by watching from the sidelines. Both sets of literature cited in support of this argument, that of Love and colleagues and the emergent literacy literature, stress the essential nature of interaction and participation in the learning process. Although both lines of research emphasize the importance of immersion in events, neither would make the claim that mere passive exposure would be sufficient to promote learning.

Since the author herself has been one of the most articulate advocates of the central role of interaction in language learning, we find this argument especially mystifying.

The central premise of this tutorial concerns the collaborative nature of communication. We fully agree that communication with people who use augmentative systems *is* a collaborative process, as is all communication. People who communicate with spelling board users, for example, do, as the tutorial reports, often finish a word for a client before the client has indicated all the letters in the word. However, communicators using non-FC methods often protest that the word the listener guessed is wrong, or correct their partner when the message received is not the message sent. Communication boards for non-FC spellers are often designed to contain messages, such as "that's not what I mean" or "start over," to handle these not-infrequent situations in which the listener guesses what the speller means, but guesses wrong. We know of no report of FC users' protesting that the message spelled out in collaboration with the facilitator is not the message they intended.

The *JSHR* article raises a series of questions that are alleged to merit further research. Yet were we to use Occam's razor, giving the simplest explanation for each of the phenomena cited, the answers to these questions emerge quite clearly. The author asks why FC users' emotional expressions fail to match the content of their messages. The simplest answer: The communication is authored by the facilitator, not the client. Why do FC users sometimes produce echolalic speech unrelated to the FC message, or scream, or whine while engaged in FC? Because the facilitated communication comes from the facilitator. Why can FC users type coherent responses when they don't appear to be attending to the message or to the facilitator or are not looking at the keyboard? Because the facilitated communication is controlled by the facilitator. Why do FC users send messages that appear telepathic? Because the facilitated communication is under the control of the facilitator.

There are many negative costs of further FC use and research that were not discussed in the tutorial. Shane (1993), for example, pointed out that FC has resulted in many changes in educational placement for people with severe disabilities, so that they are given FC in mainstream classrooms but no other special instruction. Such placement can deprive students with disabilities of necessary special education services that would allow them to derive benefit from the inclusionary placement. Further, millions of dollars of public funds have been spent paying facilitators to engage in intervention of dubious value, at best. FC users are losing precious treatment time when they are put into FC interactions instead of learning communication strategies that will lead to more independent and adaptive expression. Clinicians have devoted a great deal of effort to learning to use an experimental, invalid technique with their clients instead of

teaching them how to be more independent functional communicators. Finally, valuable time and resources have been spent by researchers attempting to validate a failed method, when their efforts would be better expended by moving on to uncover scientific information that would be of value to individuals with autism and other severe disabilities.

The tutorial stated that it is "a call for research emanating from my view that many of those with autism are achieving success with the method, and that we need to investigate further the nature of that success and those for whom it is successful" (p. 1115). Although other articles published by this journal must be statistically based and driven by hypotheses, *JSHR* has published this tutorial recommending that "we carry out research that is not driven by well-formulated hypotheses and well-controlled experiments, but rather is sensitive to the new phenomena revealed by those who are successful with FC" (p. 1116). We, as editorial consultants of *JSHR*, protest any move in this direction. The real questions about the validity of communication through FC have already been answered. To advocate for people with severe disabilities and autism, we need to move past facilitated communication and to explore other techniques. Efforts spent on researching a discredited method are not forms of advocacy, but wasteful of our time and treasure.

Melanie Fried-Oken, PhD
Assistant Professor of Neurology
Oregon Health Sciences University
Director, Assistive Technology Program

Rhea Paul, PhD
Professor of Speech and Hearing Sciences
Portland State University
Director, Portland Language
Development Project

Warren Fay, PhD
Professor, Retired
Oregon Health Sciences University

References

- Apel, K., & Vandervelde, S. (1993). *Facilitated communication: One validation study*. Paper presented at the annual convention of the American Speech-Language-Hearing Association, Anaheim, CA.
- Beck, B., Warburg, M., Parving, A., Jansen, E., Arendt-Nielsen, A., Elbro, C., & Klewe, L. (1992). *The Copenhagen investigation of assisted communication between severely handicapped persons and their assistants*. Paper presented at the World Congress of the International Association for the Scientific Study of Mental Deficiency, Gold Coast, Australia.
- Bilgh, S., & Kupperman, P. (1993). Evaluation procedure for determining the source of the communication in facilitated communication accepted in a court case. *Journal of Autism and Developmental Disorders*, 23, 553–557.
- Cabay, M. (in press). A controlled evaluation of Facilitated Communication with four autistic children. *Journal of Autism and Developmental Disorders*.
- Crews, W., Sanders, E., Hensley, L., Johnson, Y., Bonaventura, S., & Rhodes, R. (in press). An evaluation of facilitated communication in a group of nonverbal individuals with mental retardation. *Journal of Autism and Developmental Disorders*.
- Duchan, J. F. (1993). Issues raised by facilitated communication for theorizing and research on autism. *Journal of Speech and Hearing Research*, 36, 1108–1119.
- Eberlin, M., McConnachie, G., Ibel, S., & Volpe, L. (1993). Facilitated communication: A failure to replicate the phenomenon. *Journal of Autism and Developmental Disorders*, 23, 507–530.
- Green, G., Chellquist, K., Krendel-Ames, S., Ross, S., & MacDonald, R. (1993). *Analyzing the source of "facilitated" communications*. Poster presented at the annual meeting of the Association for Behavior Analysis, Chicago.
- Hudson, A., Mellita, B., & Arnold, N. (1993). Assessing the validity of facilitated communication: A case study. *Journal of Autism and Developmental Disorders*, 23, 165–173.
- Kallstrom, S., Piazza, C., Hunt, L., & Owen, J. (1993). *Experimental analysis of facilitated communication training with developmentally disabled and autistic clients*. Poster presented at the annual meeting of the Association for Behavior Analysis, Chicago.
- Klewe, J. (1993). An empirical evaluation of spelling boards as a means of communication for the handicapped. *Journal of Autism and Developmental Disorders*, 23, 559–566.
- Marks, H., Conrad, T., & Hart, P. (1993). *Facilitated communication: An empirical demonstration*. Paper presented at the annual meeting of the American Association on Mental Retardation, Toronto.
- Meinhold, P. (1993). *A clinical method for testing claims of facilitated communication*. Paper presented at the annual meeting of the American Association on Mental Retardation, Toronto.
- Moore, S., Donovan, B., & Hudson, A. (1993). Facilitator-suggested conversational evaluation of facilitated communication. *Journal of Autism and Developmental Disorders*, 23, 541–551.
- Moore, S., Donovan, B., Hudson, A., Dykstra, J., & Lawrence, J. (1993). Evaluation of facilitated communication: Eight case studies. *Journal of Autism and Developmental Disorders*, 23, 531–539.
- Price, G., & Kirkpatrick, M. (1993). *Clinical and psychometric data on facilitated communication*. Paper presented at the annual meeting of the Association for Behavior Analysis, Chicago.
- Ragal, R., Rooney, J., & Wandas, T. (in press). Facilitated communication: An experimental evaluation. *Journal of Autism and Developmental Disorders*.
- Shane, H. (1993). The "dark" side of facilitated communication. *Topics in Language Disorders*, 13, ix–xv.
- Shane, H., & Kearns, K. (1994). An examination of the role of the facilitator in "Facilitated Communication." *American Journal of Speech-Language Pathology*, 3, 48–54.
- Siegel, B. (in press). Assessing allegations of sexual molestation made through facilitated communication. *Journal of Autism and Developmental Disorders*.
- Simon, E., Toll, D., & Whitehair, P. (in press). A naturalistic approach to the validation of facilitated communication. *Journal of Autism and Developmental Disorders*.
- Smith, M., & Belcher, R. (1993). Facilitated communication with adults with autism. *Journal of Autism and Developmental Disorders*, 23, 175–183.
- Szempruch, J., & Jacobson, J. (1993). Evaluating the facilitated communications of people with developmental disabilities. *Research in Developmental Disabilities*, 14, 253–264.
- Teodoro, J., Meinhold, P., & Koch, E. (1993). *Validating and invalidating claims of facilitated communication*. Paper presented at the annual meeting of the Association for Behavior Analysis, Chicago.
- Vazquez, C. (in press). A multi-task controlled evaluation of facilitated communication. *Journal of Autism and Developmental Disorders*.

Wheeler, D., Jacobson, J., Pagliere, R., & Schwartz, A. (1993). An experimental assessment of facilitated communication. *Mental Retardation, 31*, 49-60.

Received March 7, 1994

Accepted May 20, 1994

Validity of Facilitated Communication Intervention: Response to Duchan

I am writing to respond to some of the proposed areas of research that Duchan (1993) set forth in the December 1993 issue of *JSHR*. I am aware of the risk that Judy Duchan has taken by outlining her emerging theory of autism and proposed program of research. I am respectful of her courage and do not doubt her personal integrity. However, I think that several of her suggestions, if taken to heart, will mislead the field.

Admittedly, I am a skeptic about the validity of facilitated communication (FC) as an intervention. In a recent review (Green, in press), only 4 of the 25 controlled studies reviewed included subjects who communicated even one message that could not be explained by facilitator influence or pre-FC abilities. Several of these responses were very simple and short (e.g., identifying single numerals) and occurred on only a small proportion of the total number of opportunities (Green, in press). These few FC messages could have been facilitated via physical prompting, a teaching technique with a long history that is much simpler in its theory and application than FC.

Ten studies have used double-blind conditions to determine the extent to which facilitators are controlling the message content (Green, Chelquist, Krendel-Ames, Ross, & MacDonald, 1993; Hudson, Melita, & Arnold, 1993; Kallstrom, Piazza, Hunt, & Owen, 1993; Klewe, 1993; Moore, Donovan, & Hudson, 1993; Shane & Kearns, 1994; Simon, Toll, & Whitehair, in press; Smith, Haas, & Belcher, in press; Teodorao, Meinhold, & Koch, 1993; Wheeler, Jacobson, Paglieri, & Schwartz, 1993). In double-blind studies, facilitators and FC users are given different information on some trials, and the FC user is asked to communicate the information given to him or her. The proportion of trials on which responses corresponded to facilitator's information, not the FC user's information, ranged from 20% to 100% (Green, in press). When asked whether they were aware that they were controlling the message content, most facilitators indicated they were unaware of any influence over FC message content (Green, in press). The type of control over message content seen in these double-blind studies is quite different from the more subtle help that mothers give young communicators. In early mother-child communication, message content is negotiated, has a clear child contribution, and mothers are aware of their help. On double-blind trials in which it is clear the facilitator controlled the message content, there is no evidence of FC-user contribution and the facilitator is often not aware of her/his control.

As Duchan (1993) states, "If many or all messages are originating with facilitators rather than those facilitated, then the method should not be seen as a successful means of communication for FC users." According to Duchan (1993), the proportion of messages that originate from the facilitator is difficult to determine, in part because testing FC authorship under controlled circumstances may violate the supportive conditions FC users need to communicate.

It should be noted that the aforementioned double-blind studies contain a range of measurement conditions. Several of the studies used "naturalistic" measurement conditions in which the FC facilitator is asked to pass information to the examiner that the FC user is exposed to when the facilitator is out of the room (Crews et al., in press; Marks, Conrad, & Hart, 1993; Simon, Toll, & Whitehair, in press; Vazquez, in press). That is, not all of the controlled studies in the Green (in press) review can be criticized because of the use of artificial tasks, such as confrontational naming.

Not all of Duchan's (1993) points centered on the authorship issue. However, it cannot be taken for granted that the content of FC messages can be used to test hypotheses about the nature of autism. For example, we cannot follow Duchan's (1993) advice to prefer FC-typed messages over user-spoken messages simply because the typed message says to do so. Even if sound research shows that some FC users author FC messages some of the time, naturally occurring messages cannot be used to address research questions about the nature of autism because some of these could be written by the facilitator. In my opinion, the issue of FC-user authorship is of primary importance. Other research questions may or may not be appropriate to address depending on the resolution of the authorship issue.

The FC-user authorship issue has at least three aspects to it. First, we must show that particular messages are authored by the user. Second, we must show that the FC process helped the user communicate the message without controlling message content. And third, for FC messages to be used as informative of what the user thinks or feels about having autism, we must be confident that all messages used as data in such research are authored by the user.

To show that particular FC messages are authored by the user, we must prevent facilitator influence and control for observation bias in judgments of message appropriateness and relevance. Unfortunately, the observational procedures that Duchan (1993) suggests and the currently published qualitative studies do not adequately control these variables.

For example, syntactic or spelling errors, thought by some as evidence of FC-user authorship (e.g., Biklen, 1993), could be due to user interference and inaccurate typing from the facilitator. In fact, many of the messages that double-blind studies show are definitely produced by the facilitators contain such errors (Green, in press).

Revelation of information that is assumed to be unknown to the facilitator is another qualitative method that some have suggested might be used to detect FC-user authorship (e.g., Biklen, 1993). Wheeler (1993) found that almost all of the reports of such revelations could be explained by (a) facilitators' having access to the information from other sources, or (b) observers assuming the information in FC messages was correct, when it was not.

Measurement procedures and contexts vary along a continuum of control. The qualitative/controlled dichotomy is too vague a descriptor to be useful in discussing the needed measurement procedures to address the FC authorship issues. Green (in press) suggests several measurement contexts that exert some needed control without introducing much artificiality into the situation. Her suggested methods rule out all possible ways the facilitator could influence production and uses two or more independent observers to judge the appropriateness of the FC message to the topic at hand. One such method involves asking the FC user to communicate to the examiner information that the FC user is given when the facilitator is absent from the room (e.g., Cabay, in press). If the FC user communicates the information accurately as judged by two or more observers using a priori criteria, then FC authorship can confidently be assigned to the FC user.

To determine if the FC process facilitated a particular message that is clearly authored by the FC user, one must determine that the skills needed to convey that message were absent before the user engaged in the FC process. Duchan (1993) states that there are 30 FC users "who have typed without physical support." The question is whether using FC techniques helps the FC users to become independent typists? Can their messages be explained by pre-FC abilities?

To skeptics, the presence of independent communicators who once used physically assisted FC is very important. By independent communication, I mean no visual, physical, or auditory cuing from the facilitator is necessary for the FC user to communicate the message. Independent communication is the most persuasive demonstration of user authorship in the natural environment. The only messages observed in completely uncontrolled conditions that could be used to test research questions about the nature of autism are those that are communicated independently. Facilitated messages in uncontrolled situations could be influenced by the facilitator. If such independent communicators are located, and doing so is evidently difficult (Green, in press), the primary question is whether the FC process helped them to become independent communicators.

In the final analysis, we do not need new methods or new rules of evidence developed for deciding whether FC aids the development of independent communication. The efficacy of FC can be tested using existing research methods. Accurate, independent communication would be the ideal dependent variable. In the ideal study, one would need to control one of the primary alternative explanations to independent communication: pre-FC ability. In other words, the usual controls against the threats to internal validity are needed to test whether FC helped the user develop the ability to communicate independently. If such studies supported the use of FC as a means for facilitating independent communication, I think the case for FC would be advanced far more than those studies suggested by Duchan (1993).

Much of Duchan's (1993) message was that qualitative methods are better suited to addressing the research questions most important to the FC process than are controlled observations. Certainly, qualitative methodology is an important part of the scientific enterprise. Certainly, the context of

measurement affects performance and learning. Good qualitative methods may very well be better suited to addressing certain aspects of the FC process than are quantitative methods. However, qualitative methods that do not rule out alternative, trivial explanations for FC messages that we observe in the natural environment will not further our understanding of the validity of FC as a means to facilitate communication development. Such trivial explanations are (a) the facilitator's influence on FC message content, (b) the observer's subjective interpretations of FC messages as accurate or relevant, and (c) the user's pre-FC literacy skills.

Admittedly, one cannot prove the null hypothesis that FC does not facilitate independent communication. Any one study that fails to validate FC can be criticized on the basis of subject selection, measurement conditions, and FC process. However, at some point, we must stop pursuing an unproductive line of inquiry.

Paul J. Yoder
Vanderbilt University
Peabody Box 328
Nashville, TN 37203

References

- Biklen, D.** (1993). *Communication unbound: How facilitated communication is challenging traditional views of autism and ability/disability*. New York: Teachers College Press.
- Cabay, M.** (in press). A controlled evaluation of facilitated communication with four autistic children. *Journal of Autism and Developmental Disorders*.
- Crews, W. D., Sanders, E. C., Hensley, L. G., Johnson, Y. M., Bonaventura, S., & Rhodes, R. D.** (in press). An evaluation of facilitated communication in a group of nonverbal individuals with mental retardation. *Journal of Autism and Developmental Disorders*.
- Duchan, J. F.** (1993). Issues raised by facilitated communication for theorizing and research on autism. *Journal of Speech and Hearing Research, 36*, 1108-1119.
- Green, G.** (in press). The quality of the evidence. In H. C. Shane (Ed.), *Facilitated communication: The clinical and social phenomenon*. San Diego: Singular Press.
- Green, G., Chellquist, K., Krendel-Ames, S., Ross, S., & MacDonald, R.** (1993, May). *Analyzing the source of "facilitated" communications*. Poster presented at the annual meeting of the Association of Behavior Analysis, Chicago.
- Hudson, A., Mellita, B., & Arnold, N.** (1993). Assessing the validity of facilitated communication: A case study. *Journal of Autism and Developmental Disorders, 1*, 165-173.
- Kallstrom, S., Piazza, C., Hunt, L., & Owen, J.** (1993, May). *Experimental analysis of facilitated communication training with developmentally disabled and autistic clients*. Poster presented at the annual meeting of the Association for Behavior Analysis, Chicago.
- Klewe, L.** (1993). An empirical evaluation of spelling boards as a means of communication for the multihandicapped. *Journal of Autism and Developmental Disorders, 23*, 559-566.
- Marks, H. E., Conrad, T., & Hart, P.** (1993, June). *Facilitated communication: An empirical demonstration*. Paper presented at the annual meeting of the American Association of Mental Retardation.
- Moore, S., Donovan, B., & Hudson, A.** (1993). Facilitator-suggested conversational evaluation of facilitated communication. *Journal of Autism and Developmental Disorders, 23*, 541-551.

- Shane, H. C., & Kearns, K.** (1994). An examination of the role of the facilitator in "facilitated communication." *American Journal of Speech-Language Pathology*, 3, 48–54.
- Simon, E. W., Toll, D. M., & Whitehair, P. M.** (in press). A naturalistic approach to the validation of facilitated communication. *Journal of Autism and Developmental Disorders*.
- Smith, M. D., Haas, P. J., & Belcher, R. G.** (in press). Facilitated communication: The effects of facilitator knowledge and level of assistance on output. *Journal of Autism and Developmental Disorders*.
- Teodorao, J., Meinhold, P., & Koch, E.** (1993, May). *Validating and invalidating claims of facilitated communication*. Paper presented at the annual meeting of the Association for Behavior Analysis, Chicago.
- Vazquez, C.** (in press). A multi-task controlled evaluation of facilitated communication. *Journal of Autism and Developmental Disorders*.
- Wheeler, D. L.** (1993, March). *Facilitated communication: Can we sort the sense from the nonsense?* Seminar sponsored by Alternatives for People with Autism, Brooklyn Park, NJ.
- Wheeler, D., Jacobson, J., Paglieri, R., & Schwartz, A.** (1993). An experimental assessment of facilitated communication. *Mental Retardation*, 31, 49–60.

Received January 11, 1994
Accepted May 13, 1994

Issues Raised by Facilitated Communication for Theorizing and Research on Autism: Comments on Duchan's (1993) Tutorial

The validity of facilitated communication authorship is an issue provoking extreme responses from supporters and opponents alike. The ideology of both camps, as reflected in the public and academic rhetoric on authorship, obscures the possibility of a middle-ground. These comments on Duchan's (1993) thoughtful position paper are intended as a case for a middle-ground. On the one hand, much of the supportive evidence published to date (Biklen, 1990, 1992, 1993; Biklen, Morton, Gold, Berrigan, & Swaminathan, 1992) lacks the rigor expected of qualitative research premised on a contextualist philosophy of science. On the other hand, much of the published research denying the validity of FC messages is equally questionable.

A number of studies published to date reflect a neobehaviorist view of language and communication and a strong bias towards the experimental design as the only means of so-called "scientific" evaluation (e.g., Shane, 1993; Wheeler, Jacobson, Paglieri, & Schwartz, 1993). The inference drawn from these analyses is that the experimentalist world view motivates criticism of the challenges to theory and practice that Duchan (1993) raises (see also, Duchan, 1994). If this is the case, then dogmatism is replacing dialogue between contextualist and experimentalist perspectives.

To resolve the many controversies surrounding facilitated communication, two aspects of the debate seem most relevant for clarification. One concerns the conceptual frame-

works underlying "negative" studies on the independence of facilitated communication authorship. For example, the research of Wheeler et al. (1993) is increasingly cited in the media and in scientific publications as the basis for denying message validity. However, some critique of the study appears in order because many of its components are offered as strategies for assessing independent authorship in efficacy research. The facilitated communication debate brings to the foreground a broader issue: whether the contextualist/qualitative paradigm is incompatible with the experimentalist paradigm. If not, how might they become more compatible in order to address some of the issues?

Wheeler et al. (1993) Study

The design of this blind experiment is clever. Facilitator-participant pairs did not see each others' stimulus cards (pictures) in any of three assessment conditions: (a) a facilitated condition where the participant, but not the facilitator, was presented with a stimulus card; (b) a non-facilitated condition in which, again, only the participant was given a card but, this time, the facilitator could use no physical contact, only verbal prompts; and (c) a distractor condition where both the facilitator and the participant were shown cards and the normal use of facilitated communication was permitted. However, in this last condition on 50% of trials, stimulus cards were the same (distractor-same), and for the remaining 50%, cards were different (distractor-different). In sum, 180 trials were available to document naming authenticity: 10 facilitated condition trials for the 12 participants (120 trials) and 60 trials of the distractor-different condition.

The assumption behind the distractor-different condition was a simple one. If authorship was real, participants would not identify names of pictures shown (and known) only to the facilitator. Findings were consistent. Across the 180 total trials, participants as a group were unable to produce correct responses, including correct responses in the facilitated and the distractor-different conditions (Wheeler et al., 1993, Table 4, p. 55). For example, in the facilitated condition, of 120 total responses, none were accurate names, only 1% were partially correct, whereas 66% were recognizable but inaccurate (e.g., the typed response *boat* to a pair of shoes), and 31% were "incorrect nonsense." Across the 60 total responses in the distractor-different condition, no response was clearly correct, 20% were judged as facilitator-stimulus correct (that is, the participant response was identical to the card shown to the facilitator even though the participant could not see that card), 43% were conventional but inaccurate names, and 36% of responses were classified as incorrect nonsense. Similar patterns emerged for the 60 total trials of the distractor-same condition.

On the surface, these are powerful findings. Contrary to initial hypotheses, the authors could only conclude that the evidence supports "very clear, uncontested proof of what we can only call (unintentional) facilitator control" (Wheeler et al., 1993, p. 56). In reviewing this study, however, three questions emerge about its methodology. These concerns focus on the criteria for participant selection, the concepts of

language and communication motivating the study, and the rationale for choosing a naming task.

Who is a candidate for facilitated communication?

Based on age distributions provided by Wheeler et al. (1993, p. 51), the mean age of the 12 participants was determined to be 23.6 years (range: 16:4 years to 30:4 years). It appears, then, that the participants were primarily a young adult sample, all of whom were listed as having severe to profound retardation either as a condition of autism or of the way in which they were assessed initially.

All 12 had been using facilitated communication from a minimum of 5 months to a maximum of 1 year. Before the validation study, "All could produce simple sentences or word combinations in facilitated communication, and several had consistently engaged in interactive conversations using facilitated communication" (Wheeler et al., 1993, p. 58). But no further information is furnished about why these individuals (along with 26 others) were initially selected to be users of facilitated communication. Descriptions about levels of "expressive language competence" are reported in telegraphic form only (e.g., "Nonverbal; some sounds and gestures," Wheeler et al., 1993, Table 1, p. 51). Furthermore, particulars are missing on individual variation in the duration of direct experience with facilitated communication. Also, details are omitted on how many of the 12 participants were residential members of the program (and how long they had been residential members) versus those who attended on a day basis (did they live at home? in a sheltered community? etc.). The acceptance of results as "uncontestable" proof of facilitator control means that the candidacy issue has been adequately addressed. In this case, standards of adequacy remain in serious doubt.

Is the study's methodology consistent with a functional view of language and communication? The nature of the participants' "learning" in their day program activities is only mentioned indirectly. The study used familiar picture cards that the participants had been exposed to during "lessons in vocabulary skills [and as] often used in clinical assessments, during facilitated communication network sessions, and to assess language competence through the use of facilitated communication" (Wheeler et al., 1993, p. 51). In the absence of further information, one infers surfaces. Learning and linguistic/communicative competence were defined primarily as naming, suggesting a nonfunctional approach to the meaning of communication.

Were task demands cognitively and pragmatically appropriate? The experimental activity was a confrontational naming task and, therefore, a retrieval task, although not designated as one. Two erroneous assumptions seem to be operating in task selection.

The first premise is that naming a series of pictures is a simple production activity, when, in fact, it is not (Snyder & Godley, 1992). Moreover, in examining the distribution of the 180 responses in the facilitated and distractor-different conditions, 59% ($n = 106$) fell into the category of a conventional but inaccurate name, such as *bird* for *basketball* (Wheeler et al., 1993, p. 54). This suggests that, for some of the participants, their errors may have been comparable to miscues, a strategy for enhancing comprehension (Weaver, 1994). For example, the substitution of one isolated word for another

may be a constructive attempt to use existing phonological and lexical knowledge to make a best-fit prediction about meaning, in this case to spell that meaning. Unfortunately, error analysis in the Wheeler et al. (1993) study was not conducted beyond a superficial level. As a result, it is impossible to discern more independent strategies that certain participants may have used to make sense of the task.

The second questionable assumption concerns definition of the social context of the activity. No matter how "supportive" the pretest desensitization measures were (Wheeler et al., 1993, p. 53), the activity setting could be interpreted as none other than a test of independent performance. Duchan (1993) speculates appropriately that some users of facilitated communication, like children with a language learning disability or adults with aphasia, may encounter more difficulty in the retrieval of isolated single words when placed in an independent performance situation. A reasonable conclusion is that cognitive and social requirements for independent performance cannot readily be separated from how efficiently users deploy existing resources to attend to the multiple dimensions of 30 retrieval trials.

Given these questions, it might still be the case that the results of the Wheeler et al. (1993) study have external validity. However, findings might be valid for all of the wrong reasons: (a) the participants were poor candidates for facilitated communication; (b) they had been taught a schema that communication was a nonfunctional activity consisting primarily of naming "things"; and (c) task demands caused breakdowns in the more efficient use of retrieval strategies for the recall of single lexical items, which may not have been encoded yet in other than unanalyzable wholes (Walley, 1993).

Paradigm Compatibility

The major grounds for disputing the authenticity of independent authorship resides in the doubts cast about the scientific appropriateness of qualitative research for addressing validation. One explanation often cited for the inadequacy of the qualitative design refers to the hidden psychological agenda of some facilitators. Here, the issue seems to be that qualitative designs cannot control individuals' intentions, including those that may be founded in personal agendas (Shane, 1993). Certainly, false charges of sexual abuse are unacceptable breaches of professional conduct and should be condemned. However, abuse of the research or clinical relationship does not seem to be the major basis for paradigm conflict, although it is the red herring most frequently offered in recent media and scholarly criticism.

Even those whose philosophies of science differ can certainly agree on one issue. Unusual claims require unique evidence, rather than new rules of evidence, for determining whether or not facilitated communication supports the ultimate development of independent communication. At the same time, the argument can be made that existing "experimental" research designs are inadequate to address learning that is the result of complex social and cognitive interventions (Brown, 1992). In her article, Duchan (1993) outlines a number of areas to explore, including the kinds and

levels of information necessary to consider, in systematically investigating the efficacy of facilitated communication. The theoretical and empirical issues being raised do seem to demand novel investigative tools.

Duchan's recommendations are all consistent with an emerging qualitative research design called a situated experiment (Brown, 1992). This type of contextualist intervention research is designed to inform practice by studying naturally occurring communication as a holistic phenomenon. The researcher "engineers" an innovative learning context, such as a classroom, that facilitates intentional learning-how-to-learn strategies through the integration of reading and writing, while simultaneously engaging in experimental studies of how process is connected to outcomes. The goal is to understand whether a theoretical model is a reliable and valid description of how thinking and learning are actively supported over time through collaboration with others in real instructional situations. Here, the meaning of the experimental enterprise is grounded in real communication contexts. However, its definition is potentially compatible with the traditional definition of "experimental" as a laboratory context. For example, Brown (1992) elaborates that laboratory studies can be used to assess whether a developmental trend, originating in the communicative context of the instructional setting, can be replicated under more controlled conditions.

Duchan's (1993) recommendations also fit with other contextualist views that consider activity settings as the central social structure through which guided participation or assisted performance occurs (Rogoff, Mistry, Goncu, & Mosier, 1993; Tharp, 1993). In this perspective, interpersonal processes are the mechanisms that support (or do not support) individual development in language learning, whether oral or written. Because all learning originates through collaboration, analysis of the activity structure becomes crucial for understanding the efficacy of any intervention approach to communication, not just facilitated communication. An example of the need for such microanalyses would be the activity structures for facilitating communication in the studies conducted by Wheeler et al. (1993) and others (e.g., Biklen, 1993).

The issues and possible solutions that Duchan (1993) advances can be consistent with a middle-ground position. One hopes that this kind of voice will be heard as meaningful for the more dispassionate and systematic investigation of facilitated communication as a collaborative process.

Elaine R. Silliman
Department of Communication
Sciences and Disorders
University of South Florida
Tampa, FL 33620-8150

References

- Biklen, D.** (1990). *Communication unbound: Autism and praxis*. *Harvard Educational Review, 60*, 291-314.
- Biklen, D.** (1992). Typing to talk: Facilitated communication. *American Journal of Speech-Language Pathology, 1*, (2), 15-17.

- Biklen, D.** (1993). *Communication unbound: How facilitated communication is challenging traditional views of autism and ability/disability*. New York: Teachers College Press.
- Biklen, D., Morton, M. W., Gold, D., Berrigan, C. & Swaminathan, S.** (1992). Facilitated communication: Implications for individuals with autism. *Topics in Language Disorders, 12* (4), 1-28.
- Brown, A. L.** (1992). Design experiments: Theoretical and methodological challenges in creating complex interventions in classroom settings. *Journal of the Learning Sciences, 2*, 141-178.
- Duchan, J. F.** (1993). Issues raised by facilitated communication for theorizing and research on autism. *Journal of Speech and Hearing Research, 36*, 1108-1119.
- Duchan, J. F.** (1994). Two approaches to research in child language. *Language, Speech, and Hearing Services in Schools, 25*, 48-51.
- Rogoff, B., Mistry, J., Goncu, A., & Mosier, C.** (1993). Guided participation in cultural activity by toddlers and caregivers. *Monographs of the Society for Research in Child Development* (Serial No. 236, Vol. 58, No. 8).
- Shane, H.** (1993). The dark side of facilitated communication. *Topics in Language Disorders, 13* (4), ix-xiv.
- Snyder, L. S., & Godley, D.** (1992). Assessment of word-finding disorders in children and adolescents. *Topics in Language Disorders, 13* (1), 15-32.
- Tharp, R.** (1993). Institutional and social contexts of educational practice and reform. In E. A. Forman, N. Minick, & C. Addison Stone (Eds.), *Contexts for learning: Sociocultural dynamics in children's development* (pp. 269-282). New York: Oxford University Press.
- Walley, A. C.** (1993). The role of vocabulary development in children's spoken word recognition and segmentation ability. *Developmental Review, 13*, 286-350.
- Weaver, C.** (1994). *Reading process and practice: From sociopsycholinguistics to whole language* (2nd ed.). Portsmouth, NH: Heinemann.
- Wheeler, D. L., Jacobson, J. W., Paglieri, R. A., & Schwartz, A. A.** (1993). An experimental assessment of facilitated communication. *Mental Retardation, 31*, 49-60.

Received March 28, 1994
 Accepted May 2, 1994

The Role of Experimental Research in Validating Facilitated Communication: A Reply

I am pleased to be given a chance to comment on the thought-provoking set of responses to my article on facilitated communication (Duchan, 1993). I find this opportunity appealing for several reasons. First, I appreciate having a place for debate in our academic journals, rather than the media or in the courts, which have been the primary forums for airing the issues related to this important topic. Second, I am glad for the chance to respond to the new series of published articles that have appeared since I completed my article in July of 1993. Third, I take delight in finding that all three responses target one issue from the many possible: the usefulness of experimental research for studying the validity of facilitated communication. Their unity makes my response much easier. Finally, I like having to address these three

respondents together. Their ideas are instructive as a set because they represent perspectives along a continuum of opinion. Their opinions range from a strict adherence to the highly controlled experimental approach (Fried-Oken, Paul, & Fay), to a less strict one (Yoder), to one of distrust (Silliman).

In my 1993 article I argued that results of controlled experiments do, indeed, show that those using facilitated communication are inexplicably inept in the context of a controlled experiment. At that time, the Wheeler, Jacobson, Paglieri, & Schwartz (1993) study had just been published in which it was reported that subjects were unable to perform under highly controlled experimental task conditions. Also available were the results of an Australian study (Intellectual Disability Review Panel, 1989) and a pilot study by Calculator and Singer (1992). The latter two studies reported some positive results under less controlled conditions. Since that time the *Journal of Autism and Developmental Disabilities*, a primary forum for reporting the negative findings, has published results of 10 controlled experimental studies—all showing that most of the subjects were unable to do the task most of the time (for references, see Fried-Oken et al. this issue, and for an overview from different perspectives see Green, 1994, and Sonnenmeier & Duchan, 1995). Other experimental studies of FC validation (with negative results) have been reported at conferences and in a few other journals. Fried-Oken et al. count a total of 16 published articles to date—with more coming.

My respondents and I regard these research results as conclusive, but of different things, all having to do with whether facilitators are authoring their own messages. I read the studies as violations of the communicative endeavor for those using facilitated communication, and therefore having little to do with authorship of messages created under more natural conditions. I will be no more or less convinced by more studies of this type unless they are done differently, analyzed differently, or produce new insights. Indeed, I have experienced the problem directly. Facilitated communicators whom I have seen express their own ideas through facilitated communication—ideas that contain information unknown to their facilitator—typically have considerable difficulty performing under conditions of a controlled experiment. What I do not conclude, however, is that these results can be related to authorship of messages outside the controlled context.

Fried-Oken and her colleagues are convinced by these studies that facilitated communicators are unable to author their messages, whatever the context. In their words: "Scientific evidence is overwhelming that FC users are not communicating anything independently." They recommend that we stop using the method, stop studying it, and stop wasting time on the debate because facilitated communication is "a discredited technique."

Yoder takes a more moderate stance. He concludes from these experimental studies that facilitated communicators are unable to author their own messages in tasks involving the strictest controls. He favors controlled studies because they have internal validity; but he appears unwilling to claim, on the basis of these controlled studies, that facilitated communicators will be unable to author messages in other, less controlled contexts. Yoder recommends creating new

validation studies that are single blind and that allow for emotional and communicative support throughout.

Yoder recommends that results from loosely controlled studies, once obtained, be taken as the final word on the validation question. Yoder's admitted bias as a skeptic reveals itself when he assumes that the results of these studies he proposes will be negative. He, like Fried-Oken et al., expresses impatience with the validation debate around FC and calls for its end: "At some point we must stop pursuing an unproductive line of inquiry."

Silliman, as well, does not doubt the results of the experiments. Yet she draws quite different conclusions from them. She turns the results around and instead of using them to question the authorship of the subjects, she uses them to question the validity of the experiments themselves. Whereas the other respondents praise the validation studies for their internal validity (use of proper controls), Silliman criticizes the same studies for their lack of external validity (generalizability). She analyzes the Wheeler et al. study as a prototype of this research and finds fault with its subject description and choice of task.

Silliman concludes from her analysis that even if the Wheeler et al. study and others like it are externally valid, they lack wide generalizability. She argues such results would be generalizable only to some individuals and for those individuals only when they are performing on a non-functional confrontational naming task. She then asks the important question whether the theory behind such studies is capable of handling the complexity of what may be going on during facilitated interactions. Her answer is an implicit "no" in that she recommends the creation of novel investigatory tools, such as those involved in studying naturally occurring communication.

How might a reader decide from among these three perspectives (or others that are not represented here)? I have argued elsewhere that one's choice may have something to do with one's upbringing (Biklen & Duchan, 1994; Duchan, 1994). Scholars and clinicians brought up as empiricists or behaviorists, constituting many of those who are engaged in carrying out experimental validation studies, will be prone to favor controlled experiments and to look to their results as being the final word about all FC interactions. Those brought up as rationalists and structuralists are more likely to rely on and appreciate the methods of naturalistic research for discovering truths about facilitated communication.

What the current polarized climate calls for is what Elaine Silliman has described as an atmosphere in which dialogue replaces dogmatism. I hope the comments that follow are expressed in a dialogic voice. I would like to list a few problems that are inherent in the controlled experiment when used to assess the validity of FC. I will then try to counter some of the remaining criticisms raised by my respondents.

Possible Biases of Experimental Research

Texts covering experimental research usually present a set of steps to follow. Typically included among them are the following: formation of hypotheses, design of experiments,

data collection, and analysis and interpretation of data. Authors often accompany their discussions for how the steps should be carried out with a set of cautions for how to escape common traps inherent in each. The cautions, many of which are also applicable to nonexperimental research, allow one a glimpse of where validation studies of FC might fall short. A critique of the validation literature using controlled experiments may thus be done from within the point of view of those sympathetic to the research.

Formation of hypotheses. Experimental scientists begin their research by raising hypotheses (sometimes cast in null form) about what they are about to study. The hypotheses arise from implicit or explicit theoretical perspectives and from past research—experimental, descriptive, or observational. Experimental researchers studying FC have a theoretical bias toward communication that allows them to frame their hypotheses in terms of single authorship, and to ask whether an outsider, the facilitator, has influenced the communicators' message. The theory revolves around a conduit view of communication that conceives of it as a message-passing enterprise (Duchan, 1993, 1995). My colleagues and I have proposed an alternative view of communication—that of collaboration in which partners work together to create conversations (Higginbotham, 1989; Higginbotham, Sonnenmeier, & Duchan, 1993). The conduit perspective of communication leads to hypotheses that treat communicative support as a form of cuing, that treat co-construction of messages as a form of influence, and that treat joint attention as a violable part of the communication process. The collaborative perspective sees collaboration as legal and even essential to communication. One can conclude from this view that conditions that violate the collaborative nature of communication are not valid contexts for measuring communicative competence.

Design of experiments. It is well-known among experimentalists that external validity is best achieved when experiments are designed to simulate conditions in everyday life. Experiments that fail to do this are seen as possibly lacking in external validity, and even worse they are seen as leading the subjects to a new reality frame—a reality peculiar to the experiment. This has been called *experimental realism* (Aronson & Carlsmith, 1968). The danger of experimental realism is that subjects will act differently when they feel they are part of an experiment rather than engaging in an everyday life event. One regularly cited example of experimental realism has been referred to as the "Hawthorne effect." People have been found to behave differently simply because they are participating in a study.

As pointed out by Silliman, facilitated communicators are likely to experience naming tasks as quite different from what takes place during ordinary communication. The subjects in experimental studies are not usually asked to respond as communicators, but rather are asked to perform as test takers. Specific responses are required and are judged as either correct or incorrect. The context of interaction is not a naturally occurring one, but one that is tampered with in a variety of ways. Boards are often set up between subjects and facilitators; experimenters sometimes hide behind walls so as not to cue subjects; unrelated pictures are presented to each of the partners separately. Indeed, for those few

reported times in which subjects have been successful, the experiments were closer to everyday reality—subjects were asked to report on something they were interested in, and the reports were made under conditions that were free from the physical or linguistic constraints of a controlled experiment (e.g., Intellectual Disability Review Panel, 1989, Sheehan, 1992, Vazquez, 1994).

Data collection. Since subtle influence from the interactant or the experimenter is known to be a persistent problem in experimental research, extra cautions are often taken to prevent it. Efforts are made to control influence by blinding the potential influencers to what is happening in the study. Single-blind studies are ones in which the interactants do not know which experimental manipulation they are being presented with. A double-blind study not only makes the participants blind to the conditions of the experiment but the officials in the experiment (experimenters, observers, scorers) blind to what is going on.

But controlled experiments that include single- and double-blind controls are likely to result in conditions that are unusual for the subjects. This may cause the subjects to shift their thinking away from their everyday reality to an experimental reality. Silliman comments on this shift as a possible violation of the pragmatics of typical communicative contexts. A shift in thinking jeopardizes one's ability to assume he or she is performing and thinking in the same way as in contexts of everyday life.

Most validation researchers of FC consider the double-blind treatment to be one that controls for influence from the subjects (facilitated communicator and facilitator). Few have controlled for the influence from the experimenter or scorer. Also, most of the published studies include scoring procedures that only allow for certain answers. A communicator's response of *vehicle* when shown a picture of a car is scored as incorrect (Wheeler et al., 1993) because "vehicle" may refer to a truck as well. Unfortunately, most studies do not show the subjects' responses (for an exception see Vazquez, 1994). The reader must therefore rely solely on the scoring judgments of the researcher.

Analysis and interpretation of data. Experimental scientists are required to develop objective measures of their variables. In order to be objective, the measures must be quantifiable and ambiguities eliminated. Methods designed to disambiguate answers may eliminate unscorable items or treat them as incorrect responses (Danziger, 1990). For example, in many of the FC validation studies subjects were reported to have typed words that were neither what the facilitator saw nor what the communicator saw. The words were regarded as incorrect, since those scoring the results were unable to see the relationship between the words and their expected responses.

Gene Marcus, a facilitated communicator, currently is conducting a study in which he comments on his experiences during and after engaging in a validation experiment (Marcus, Shevin, & Sabin, 1995). The study includes a commentary from Marcus about how and why he produced seemingly irrelevant responses on the controlled experimental task. In one instance Marcus reported that he typed *stove* because that was what he had been asked to respond to earlier; in

another he said he typed *violin* because that was what the picture looked like to him. Now that he saw it a second time he could see that it was a dentist and patient. There may be a number of responses that might be explained, were we to ask the person producing them.

The results from Marcus and his colleagues are instructive. I would not want to claim from them that all of the unusual responses produced by FC users on experimental tasks should be counted as correct. Rather, the claim I am making is that much of the data in these studies is not being analyzed because it does not fit with the strict approaches dictated by the experimental research paradigm. The responses judged irrelevant may not be irrelevant to the facilitated communicator. If the participants could be given the opportunity to talk about their experiences, the results—now taken as evidence against FC—might look different.

In sum, experimentalists know from their training that their approach, like all research approaches, has its deficiencies. It behooves the experimentalist doing validation research in FC to be skeptical not only of others' approaches but also of their own approach. They are aware of the issues, since the problems are covered in classic texts in their field. A discussion of these problems by those conducting the studies would help create the atmosphere of open dialogue that Silliman calls for.

Responses to Other Criticisms

Fried-Oken et al. ask that I follow the principle of most parsimonious explanation ("Occam's razor") when explaining the unusual interactive styles of some facilitated communicators. Why don't their emotions fit their faces? Why are their oral messages different from their written messages? Why don't they look at the keyboard? How might they be conveying what seem to be other's thoughts? Fried-Oken et al. explain these phenomena easily—the facilitator is authoring the messages, not the communicator.

But the "facilitator as author" explanation is parsimonious only if one ignores the other data that provide evidence for authorship in naturally occurring contexts. The questions were problematic for me because they were not consistent with other positive evidence of authorship that has been reported in the nonexperimental research literature (for a review, see Duchan, 1993, and for an example, see Janzen-Wilde, Duchan, & Higginbotham, in press) and that I have experienced directly. For example, information has been conveyed in typed messages that the facilitator could not have known—information that is verified later as accurate; misspellings have occurred in the same form across different facilitators; and the same unusual content has been conveyed to different facilitators.

The explanation that the facilitator is authoring the messages is, indeed, the simplest explanation for my posed questions, if those questions are taken out of context. But if the other qualitative data are added to what needs to be explained, then the conclusion about facilitator's authorship works against Occam's principle in that it complicates matters. It fails to explain how a message gets typed that contains information that only the communicator knew.

Similarly, the time-resource concerns raised by Fried-Oken and her colleagues stem from their view that FC is discredited. Any resources paid to support or investigate facilitated communication are thus seen as wasted. The picture is quite different for those of us who have seen startling improvement in the communication and behavior of those using facilitated communication. More resources are needed. Training methods need to be developed to help communicators protest against undue influence and to progress with greater speed toward communicative independence. More support needs to be provided children in classrooms so that they can be included in activities and the social life around them. Research needs to be carried out to determine why individuals are competent in one context and incompetent in another, why they can perform on setwork tasks and have such difficulty with controlled experiments, or how they could have developed language and literacy abilities from the interactional sidelines.

Yoder argues that skeptics will remain unconvinced by such evidence as idiosyncratic use of language or independent typing. They could argue that facilitators are misspelling words in a second code or that typing before independence was guided by facilitators. Even if subjects were able to pass the controlled experiment, Yoder comments that "there is still the possibility that facilitators author many messages in the natural environment." Despite his pessimism about the ability of proponents to convince skeptics, Yoder supports the use of FC as a way to work toward independent communication. He also sees the less-controlled experiment as a way to answer what he takes elsewhere to be the unanswerable question—who is authoring facilitated messages?

Finally, Yoder favors the idea that the facilitated communicator be trained to do his less-controlled experimental tasks. Cardinal and Hanson (1994), by the way, have worked with facilitated communicators to accomplish the experimental tasks such as that used in the Wheeler et al. (1992) study and found a notable improvement over a 6-week period in the performance of their 43 facilitated subjects on a controlled validation task (46% of them were able to name at least two pictures in a single-blind condition).

When all is said and done, I hope that we will be able to look back on this period of conflict as having positive outcomes. One such outcome would be that those who are committed to the controlled experiment consider the possible biases inherent in their theoretical and methodological approach. Decisions should not be based on the results of one research paradigm if it excludes important evidence that does not fit that paradigm.

What the debate has already shown is that the issues are very important, very emotional, and very complex. They are neither a "waste of time" nor an "unproductive line of inquiry." They deserve more scholarly debate—debate that should be carried out in our own journals, such as *JSHR*. We should continue to examine the many aspects of facilitated communication and to develop a variety of ways for studying it. In so doing we may be able to arrive at a broader and deeper evaluation of its worth. Like Rome, building an understanding of the complex phenomena surrounding facilitated communication will take more than a day.

Judith Felson Duchan
 State University of New York at Buffalo
 Department of Communication
 Disorders and Sciences
 110 Park Hall
 Buffalo, NY 14260

References

- Aronson, E., & Carlsmith, J.** (1968). Experimentation in social psychology. In G. Lindzey & E. Aronson (Eds.), *The handbook of social psychology* (2nd ed. Vol. 2, pp. 1–79). Reading, MA: Addison-Wesley.
- Biklen, D., & Duchan, J.** (1994). "I am intelligent": The social construction of mental retardation. *Journal of the Association for Persons with Severe Handicaps*, *19*, 173–184.
- Calculator, S., & Singer, K.** (1992). Preliminary validation of facilitated communication. *Topics in Language Disorders*, *13*, ix–xvi.
- Cardinal, D., & Hanson, D.** (1994, May). *New findings regarding the validity of facilitated communication*. Paper presented to the Facilitated Communication Conference, Syracuse University, Syracuse, NY.
- Danziger, K.** (1990). *Constructing the subject: Historical origins of psychological research*. New York: Cambridge University Press.
- Duchan, J.** (1993). Issues raised by facilitated communication for theorizing and research on autism. *Journal of Speech and Hearing Research*, *36*, 1108–1119.
- Duchan, J.** (1994). Two approaches to research in child language. *Language, Speech, and Hearing Services in Schools*, *25*, 48–51.
- Duchan, J.** (1995). *Dancing together: A new metaphor for understanding facilitated communication*. Manuscript in preparation.
- Fried-Oken, M., Paul, R., & Fay, W.** (1995). Questions concerning facilitated communication: Response to Duchan. *Journal of Speech and Hearing Research*, *38*, 200–201.
- Green, G.** (1994). The quality of the evidence. In H. Shane (Ed.), *Facilitated communication: The clinical and social phenomena*. (pp. 157–225). San Diego, CA: Singular Publishing Group.
- Higginbotham, D. J.** (1989). The interplay of communication device output mode and interaction style between nonspeaking persons and their speaking partners. *Journal of Speech and Hearing Disorders*, *54*, 320–333.
- Higginbotham, D. J., Sonnenmeyer, R., & Duchan, J.** (1993). The impact of technology on facilitated communication. *Technology and Disability*, *2*, 68–78.
- Intellectual Disability Review Panel.** (1989). *Investigation into the reliability and validity of the assisted communication technique*. Melbourne, Australia: Department of Community Services, Victoria.
- Janzen-Wilde, L., Duchan, J., & Higginbotham, D. J.** (in press). The successful use of facilitated communication with an oral child. *Journal of Speech and Hearing Research*.
- Marcus, G., Shevin, M., & Sabin, L.** (1995). *Self directed validation of facilitated communication by FC users*. Manuscript in preparation.
- Sheehan, C.** (1993). Facilitated communication validity: Alternative explorations. *Facilitated Communication Digest*, *1*, 6–9.
- Silliman, E.** (1995). Issues raised by facilitated communication for theorizing and research on autism: Comments on Duchan's (1993) tutorial. *Journal of Speech and Hearing Research*, *38*, 204–206.
- Sonnenmeyer, R., & Duchan, J.** (1995). *Issues of validation: A history and review of methods used to validate facilitated communication*. Manuscript in preparation.
- Vazquez, C.** (1994). Brief report: A multitask controlled evaluation of facilitated communication. *Journal of Autism and Developmental Disorders*, *24*, 369–379.
- Wheeler, D., Jacobson, J., Paglieri, R., & Schwartz, A.** (1993). An experimental assessment of facilitated communication. *Mental Retardation*, *31*, 49–60.
- Yoder, P.** (1995). Validity of facilitated communication intervention: Response to Duchan. *Journal of Speech and Hearing Research*, *38*, 202–204.

Received July 6, 1994
 Accepted August 4, 1994

Validity of Facilitated Communication Intervention: Response to Duchan

Paul J. Yoder

J Speech Hear Res 1995;38;202-204

This information is current as of December 10, 2013

This article, along with updated information and services, is
located on the World Wide Web at:

<http://jslhr.asha.org>



AMERICAN
SPEECH-LANGUAGE-
HEARING
ASSOCIATION