

Facilitated Communication Since 1995: A Review of Published Studies

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Previous reviews of Facilitated Communication (FC) studies have clearly established that proponents' claims are largely unsubstantiated and that using FC as an intervention for communicatively impaired or noncommunicative individuals is not recommended. However, while FC is less prominent than in the recent past, investigations of the technique's efficacy continue. This review examines published FC studies since the previous major reviews by Jacobson, Mulick, and Schwartz (1995) and Simpson and Myles (1995a). Findings support the conclusions of previous reviews. Furthermore, this review critiques and discounts the claims of two studies purporting to offer empirical evidence of FC efficacy using control procedures.

KEY WORDS: facilitated communication; autism; literature review.

Autism, like other complex disabilities, has historically elicited considerable attention not only to aspects of etiology, characteristics, and classification, but also to effective medical, behavioral, and educational interventions (Harris, 1995; Waterhouse, Morris, Allen, Dunn, Fein, Feinstein, Rapin, & Wing, 1996). Educationally, interventions for people with autism have consistently relied on well-established and increasingly effective forms of augmentative and alternative communication (AAC).

However, intervention complexity related to autistic populations, particularly at more severe disability levels, creates greater weight for the attractiveness of some kind of "silver bullet" cure that may have been overlooked in decades of strenuous research and significant practical frustration among teachers and parents. Attractively intuitive interventions, almost always implemented without necessary attention to rigorous experimental control and replication, are problematic due to their persistence in the face of scientific and log-

ical challenge. Furthermore, these untested interventions may negatively compete with and possibly supplant previously validated interventions and services to children and youth with autism (Jacobson *et al.*, 1995). The most recent and prominent example of the widespread adoption of a suspect intervention in autism has been Facilitated Communication (FC), first proposed by Crossley in Australia (see Biklen, 1990; Crossley, 1992; Gorman, 1998; Green, 1992; Hudson, 1995) and brought to the United States by Biklen (e.g., 1990, 1993; Biklen & Cardinal, 1997; Biklen & Duchan, 1994; Biklen & Schubert, 1991).

THE CONCEPTUAL UNDERPINNINGS OF FC

Biklen's contributions were not only significant in terms of bringing FC before the American public, but also because he used his explanations of FC to challenge long-held assumptions and theories of autism. Biklen acknowledged that the etiology of autism reflected a controversial range of causal hypothesis in spite of specific autistic behaviors themselves (e.g., echolalia, mutism, perseveration, social interactional problems, stereotyped activity) being more accurately defined (Biklen, 1990). Biklen further asserted that

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identifying the implications of autistic behavior, especially as related to language, was generally considered meaningless among researchers, resulting in their assumption that these children were “. . . not smart” and that, for example, stereotyped utterances and incorrect semantics traditionally “. . . were presumed to be the trademarks of incompetence” (Biklen, 1990, p. 302). Biklen countered these traditional notions after observing Crossley’s students, maintaining that: “In light of the natural language produced by Crossley’s students through typing, we are compelled to search for an alternative explanation for their mutism and unusual speech” (Biklen, 1990, p. 303). Biklen’s “. . . obvious interpretation . . . [was] that autistic children have a neurologically based problem of expression” (p. 303), and not, as generally supposed, a problem of language.

Thus, in a significant departure from most accepted aspects of language issues in autism, Biklen believed that people without communicative ability, generally regarded as lower functioning than those having some use of language for communication, could not be assumed to be lower functioning because of their obvious expressive deficits. Biklen’s argument asserted that people with autism simply had problems expressing themselves rather than a cognitive deficit, which prohibited appropriate use of language. Simply put, people with autism possessed and were able to understand and utilize language, but were unable to express it (Crossley & Remington-Gurney, 1992). Biklen claimed that a major strength of FC was that it did not assume any cognitive deficit among noncommunicative or communicatively impaired individuals (Biklen, Morton, Gold, Berrigan, & Sweminathan, 1992) and that disorders of output might not necessarily be correlated with levels of intellectual functioning (Crossley, 1992).

Biklen argued that the linguistically expressive problems of people with autism could not only be circumvented by FC, but that doing so uncovered previously unrevealed cognitive abilities. Using FC, therefore, bypassed the expression problem, allowing “natural” language (accurate, true communicative intent formulated in the client’s cognition) to emerge (Biklen, 1990). Thus, Biklen claimed that clients were able to type “natural language,” even while producing echolalic or unintelligible vocalizations (Biklen *et al.*, 1992).

Biklen further bolstered the notion of autistic individuals’ untapped intellectual potential by claiming that they were able to learn at much more sophisticated cognitive levels than had previously been thought and in spite of having never received formal education. He

claimed that these sophisticated cognitive abilities resulted from the individual’s considerable exposure to written and spoken language, although not to formal teaching (Biklen *et al.*, 1992). Indeed, Biklen extended the “unexpected literacy” aspect of his position by raising the possibility that many noncommunicative, supposedly retarded children with autism were hyperlexic, given that many characteristics of hyperlexia were also characteristics of individuals with autism (e.g., the development of extraordinary superior word recognition skills; an excellent passive vocabulary; Biklen & Schubert, 1991; Donnellan, Sabin, & Majure, 1992). Biklen claimed that if autistic individuals showed characteristics common to autism and hyperlexia, then these autistic individuals were hyperlexic and able to learn without the benefit of formal teaching. For Biklen it followed that FC was the key that could finally reveal noncommunicative individuals’ previously unknown intellectual potential.

The empirical literature related to FC and its effectiveness has received a great deal of scholarly attention resulting in numerous critical commentaries and several previous reviews. For example, there are multiple shorter commentaries on various aspects of the FC and the FC literature (e.g., Ackerson, 1994; Biklen & Duchan, 1994; Danforth, 1997; Ferguson & Horner, 1994; Goode, 1994; Green & Shane, 1994; Halle, 1994; Hitzing, 1994; Horner, 1994; Jacobson & Mulick, 1994; Levine, Shane, & Wharton, 1994; Mostert, 1994; Shane, 1994; Silliman, 1992; Williams, 1994; Wolfensberger, 1992, 1994; Zirkel, 1995). A legal review of issues related to FC can be found in Margolin, (1994), while Gorman (1999) discusses several legal decisions related to FC.

The assumptions of FC proponents, while not well formed and severely challenged (e.g., Hudson, 1995; Jacobson *et al.*, 1995; Shane, 1994), have precipitated several empirical reviews of the effectiveness of FC.

PREVIOUS REVIEWS OF FACILITATED COMMUNICATION EFFICACY

There have been several previous reviews of FC studies (consecutively: Cummins & Prior, 1992; Green, 1992, 1994; Hudson, 1995; Jacobson *et al.*, 1995; Simpson & Myles, 1995a), as well as several critiques and commentaries (e.g., Jacobson, Eberlin, Mulick, Schwartz, Szempruch, & Wheeler, 1994; Spitz, 1997) which have accumulated evidence both for and against FC. A brief summary of these major reviews follows.

Cummins and Prior

Cummins and Prior (1992) addressed early concerns related to FC that would be repeated in later reviews. Their review essentially sketched the definition of FC, briefly explained the role of language in autism, and evaluated the then-extant FC data. An important aspect of their review is a description and analysis of the data presented to the Intellectual Disability Review Panel (IDRP) by the Interdisciplinary Working Party (IWP) in Australia, precipitated by FC's original claims espoused by Crossley (1992). (Supplementary details related to the FC movement and early issues in FC in Australia are presented in Hudson, 1995). Cummins and Prior argued in detail that Biklen's (1992) claims that the IDRP and IWP support the use of FC are weak, at best. A response to Cummins and Prior appears in Biklen (1992).

Green

Green undertook two separate reviews of the FC literature, in 1992 and 1994. Green's (1992) review, part of a larger paper addressing the scientific and ethical issues of FC, presented results of 12 studies with control procedures (CP), which overwhelmingly demonstrated the ineffectiveness of FC and supported the previous observations of Cummins and Prior (1992). Green classified the studies according to a limited set of criteria: the source of the study, subject classification and N (number of subjects per study), control procedure descriptions, and the success rate of FC. In sum, Green found that of 146 possible opportunities for subjects to communicate via FC across a wide variety of communication tasks, only three responses could possibly be attributed to FC.

Green (1992) noted the markedly different findings among studies with no control procedures (NCP) included in her review, all of which were offered by FC proponents as evidence of FC efficacy. These three studies reported that among 98 subjects using FC, 41 subjects produced sentences, 4 produced single words, 41 subjects demonstrated reading skills, 1 subject was able to indicate yes/no, and 1 subject was able to point at pictures. Furthermore, the authors of these studies noted considerable and heretofore unexpected gains in many subjects' communication abilities including the claim that many subjects were, via FC, performing close to academic grade level.

Green's (1994) review updated descriptions of the many studies that had appeared subsequent to her 1992 review with results once again disconfirming FC claims.

Her review classified the FC studies according to the study source, N , subject classification, and n (number of subjects within a study under different label or diagnostic classifications), the communicative context, the study setting, control procedure descriptions, and the success of subjects who apparently provided valid FC responses.

Green's summary of 25 published CP studies and technical reports showed that only 12 out of 226 possible subject responses could be considered unexpected demonstrations of skill above chance, although even these responses could not rule out other causes than FC. In contrast, among the 6 NCP studies cited by Green, FC proponents reported that 109 of 112 subjects demonstrated unexpected communication or literacy skills.

Hudson

Hudson (1995) provided a detailed history of the emergence of FC in Australia through Crossley's work. He also described several early experimental FC studies which collectively refuted the claims made by FC proponents. The most significant aspect of Hudson's review, however, related to a critique of FC as having no theoretical base. Hudson argued that the few theoretical inferences available in proponents' literature that might lead to a coherent theory of FC (objectives, theoretical bases of FC practice, and evaluation of whether FC practice met its objectives) could easily be achieved by other more effective interventions.

Jacobson, Mulick, and Schwartz

Jacobson *et al.*, (1995) provided a history of FC and descriptions of a representative set of controlled studies noting the number of subjects, the number of FC successes in these studies, and the methods used for obtaining results. They reported that among 126 subjects, there were only 4 possible instances of FC success. However, even among the four possible successes, they noted significant problems with replication and methodology that could have produced false positives.

Simpson and Myles

Simpson and Myles (1995a) confined their review to extant published studies using a similar study classification to Green (1994). They added other study characteristics of age range, study site, facilitator experience, and study duration. Simpson and Myles included most of the studies reviewed by Green (1994)

and also provided a useful overview of the history and issues surrounding FC. They reported that across 14 CP studies involving 43 FC elicitation tasks, only 2 tasks showed any possible FC effect. Their conclusions concurred with the previous reviews by Cummins and Prior (1992) and Green (1992, 1994).

PURPOSE OF THIS REVIEW

The purpose of this review is to synthesize FC studies appearing in the empirical literature since the last major reviews by Jacobson *et al.*, (1995) and Simpson and Myles (1995a). This review describes, analyzes, and summarizes primary study characteristics, provides summative findings supporting and opposing claims of FC efficacy, and examines findings of two studies using control procedures to claim empirical evidence of FC efficacy. While the majority of the studies in this review are later than 1995, several studies published slightly earlier than 1995 but not included in earlier reviews are included here.

REVIEW OF FC STUDIES SINCE 1995

Given the nature of the studies and their data, the evidence from the studies is presented in the form of a narrative review.

Narrative Reviews

Most research syntheses can be arranged in one of four ways: (a) through identifying or discussing new developments in a field, (b) by illustrating, assessing, or proposing theory, (c) by organizing knowledge from divergent lines of research, or, more pertinent to this discussion, (d) through integrative review methods (Bangert-Drowns, 1986; Cooper, 1982). Integrative reviews attempt to (a) make sense of divergent research findings around a similar research hypothesis and (b) to provide a summary of what is already known (Cooper, 1982).

This review describes published FC studies since 1995 in a narrative review to answer the following questions:

1. What are the characteristics of the FC studies published since 1995?
2. Overall, what is the nature of the evidence in these studies related to FC efficacy?
3. How legitimate are the claims of two studies using control procedures and claiming substantial evidence of FC efficacy?

SAMPLE STUDIES

Descriptions of the selection criteria for the studies in this review appear below describing how the studies were located, the criteria which resulted in their inclusion in the review, and the fundamental bimodal distribution of studies as either CP or NCP.

Locating Studies

Initially, computerized searches of education and psychology databases were executed from 1993 onward, using, in combination, several key descriptors (including education, severe, handicaps, Facilitated Communication, autism, communication, assistive devices and methods, speech/language, etc.). These searches revealed numerous documents, including books, special journal issues devoted to FC, peer-reviewed published papers, research reports, conference presentations, and a variety of other written records. Subsequently, author searches (e.g., using descriptors such as Biklen, Crossley, Donnellan, Green, Shane), manual searches of well known special education journals (e.g., *Journal of the Association for People with Severe Handicaps*, *Journal of Autism and Developmental Disorders*, *Remedial and Special Education*, *Mental Retardation*), and ancestry searches revealed several other studies. Several other studies were kindly noted by one of the anonymous reviewers.

Inclusion and Exclusion Criteria

The relatively wide range of documentation necessitated a primary criterion of published studies for inclusion in this review. Published studies are most useful for broad summative reviews because they have successfully negotiated peer review and appear in scholarly journals where they are more likely to be familiar to readers, are most widely circulated, and are most easily accessible. While literature reviews often exclude published studies which are methodologically unsound, such studies were included here because a central issue in evaluating the efficacy of FC revolves around proponents' use of methodologically suspect means to claim FC as a successful intervention (Biklen & Duchan, 1994; Goode, 1994; Green, 1992, 1994; Green & Shane, 1994; Shane, 1994).

Unpublished studies were excluded because, while possibly significant, they are less accessible and have not been subjected to peer review in the literature (Glass, McGaw, & Smith, 1981; Mullen, 1989). Furthermore, studies focusing only on facilitators but not

clients as study subjects (e.g., Perry, Bryson, & Bebko, 1998) were omitted.

Control Procedures (CP) Versus No Control Procedures (NCP)

The *sine qua non* of empirical research is the controlled experiment (Campbell & Stanley, 1963) allowing objectivity, generalizability, and replicability secured by internal and external validity (Gall, Gall, & Borg, 1999). True experimental research is often problematic in education given that random assignment of subjects, the composition of comparison groups, and accurate isolation of the dependent variable can be difficult. Rather, most educational research relies on quasiexperimental design (no random subject group assignments and often no comparison groups) which attempt to incorporate procedures to minimize bias; that is, the procedures attempt to control for plausible rival explanations of the phenomenon under investigation (Campbell & Stanley, 1963). While the use of any study control procedure does not necessarily rule out all rival explanations, it is more likely that one or more control procedures will rule out at least some rival explanations over studies that do not employ control procedures.

In previous FC reviews, it is at the level of control procedures (CP) vs. no control procedures (NCP) that studies have been evaluated. Similarly, the 29 FC studies reviewed below, following Green (1992, 1994), are divided into three groups: (a) 19 studies which provide one or more control procedures and which refute FC claims (CP–, Beck & Pirovano, 1996; Bomba, O'Donnell, Markowitz, & Holmes, 1996; Braman, Brady, Linehan, & Williams, 1995; Calculator & Hatch, 1995; Crews, Sanders, Hensley, Johnson, Bonaventura, Rhodes, & Garren, 1995; Edelson, Rimland, Berger, & Billings, 1998; Hirshoren & Gregory, 1995; Kerrin, Murdock, Sharpton, & Jones, 1998; Kezuka, 1997; Montee, Miltenberger, & Wittrock, 1995; Myles & Simpson, 1994; Myles, Simpson, & Smith, 1996a, b; Oswald, 1994; Regal, Rooney, & Wandas, 1994; Shane & Kearns, 1994; Simon, Whitehair, & Toll, 1996; Simpson & Myles, 1995b; Smith, Haas, & Belcher, 1994), (b) six studies which provide one or more control procedures supporting FC claims (CP+, Bebko, Perry, & Bryson, 1996; Cardinal, Hanson, & Wakeham, 1996; Heckler, 1994; Sheehan & Matuozzi, 1996; Vazquez, 1995; Weiss, Wagner, & Bauman, 1996; and (c) four studies with no control procedures and supporting FC (NCP+, Biklen, Saha, & Kliwer, 1995; Clarkson, 1994; Janzen-Wilde, Duchan, & Higginbotham, 1995; Olney, 1995;). No NCP– studies were found.

REVIEW RESULTS

Delineating key study features of the data set, in this case published FC studies since Simpson and Myles (1995a), is important because these features define the parameters of the data set and provide information for replication by other researchers. In the previous reviews of the FC literature noted above, only Cummins and Prior (1992) eschewed detailed information of reviewed primary study features. In contrast, Green (1992, 1994) and Simpson and Myles (1995a) provide similar study features, which formed the basis for how the studies in this review were analyzed. This review classified the FC studies under the following primary study characteristics: CP (CP– and CP+) versus NCP studies, study source (i.e., authorship and date), the study purpose or description, *N* (total number of subjects per study), label/*n* (designated disability category/number of subjects per category), sex (age range, years), subject characteristics, FC communication medium used, study settings, the FC experience of the subjects and the facilitators, the duration of the study, control procedures, and the results and conclusions of the study. The characteristics of the CP– studies appear in Table I, the CP+ studies in Table II, and the NCP+ studies in Table III. A summary of study features across studies appears in Table IV.

Study Characteristics

The results of the study characteristics for this review appear below.

Study Source

This feature identified the authors of the study and publication date and were obviously unique to each study.

Study Purpose or Description

A feature common to all the studies was a more or less detailed stated purpose or a description of what was to be reported. The purposes of the CP studies revolved around the effectiveness of FC both directly (e.g., academic tasks and communication) and indirectly (e.g., the impact of FC on behavior and social interactions). Several of these studies also investigated, to a greater or lesser degree, the central issue of facilitator influence (e.g., Bebko *et al.*, 1996; Braman *et al.*, 1995; Montee, *et al.*, 1995). For the NCP+ studies, study purposes revealed similar degrees of direct and indirect investigation.

Table I. Characteristics of CP Studies Refuting FC (CP—)^a

Study	Purpose	N	Label/n	Sex (age range, years)	Subject characteristics	Communication medium	Setting	FC experience subjects/facilitators	Duration	Control procedures	Results/conclusions
Beck and Pirovano (1996)	Validity of FC as a receptive language task across input variables	12	Autism/6 Severe-profound impairment/6	M: 7 F: 5 (7-36)	No functional independent communication Normal hearing	Supported typing	Various settings familiar to subjects	Yes/yes	n/a	1. Subjects paired with most successful facilitators 2. Facilitated & independent responses 3. Auditory & visual conditions randomized 4. Naive facilitator	1. Subjects' PPVT scores unobtainable 2. Effect of FC on receptive language unsupported 3. Individuals' independent responses higher than FC responses in autistic group 1. Instruction did not produce functional, typed communication 2. Subjects unable to individually communicate with FC
Bomba, <i>et al.</i> (1996)	Impact of FC as communication device on communicative competence	14	Autism	M: 11 F: 3 (8-14)	Significant adaptive behavior deficits	Supported typing	Separate room	Trained for study/yes	10 Weeks	1. Subjects paired with familiar facilitators 2. Set work-open ended conversation; pre-post test	1. Response systematically influenced by facilitator 2. Response latency much faster under facilitator-known condition
Braman <i>et al.</i> (1995)	Face validity of FC	3	Autism	M: 2 F: 1 (9-13)	Severe MR Severe language impairment	Supported typing	Familiar settings to subjects	Yes/yes	Eight sessions over 3-4 weeks	1. FC with preferred facilitators 2. Naive informed facilitators 3. Random statement presentations, flexibly presented 4. Spaced sessions to reduce variability 5. Multiple treatments 6. Subjects used preferred typing device 7. Familiar facilitating environment 8. Nonconfrontational testing format	1. Response systematically influenced by facilitator 2. Response latency much faster under facilitator-known condition

Calculator and Hatch (1995)	Assess the likelihood of FC allegations of sexual abuse	1	Moderate/severe MR	M: 1 (17)	Echolalie	Supported typing	n/a	Yes/yes	Single session	1. Validation protocol (formal & informal procedures; standardized & nonstandardized procedures across different input & output modalities) 2. Masked (naive) and unmasked (informed) facilitator	1. 100% correct subject responses in unmasked condition 2. Subject's FC ability depended on facilitator's knowledge of task 3. Subject did not possess communication skills to make sexual abuse allegations 1. 6 of 8 subjects unable to pass any messages 2. 2 subjects appeared to pass some messages, other explanations not ruled out 3. No evidence of efficacy of FC
Crews <i>et al.</i> (1995)	FC as a message passing paradigm	8	MR	M: 7 F: 1 (28-51)	Nonverbal CP Down Syndrome Bipolar disorder	Visual, auditory, & tactile communication Supported typing	Home institution	Yes/yes	3 consecutive days	1. Sound attenuated room 2. Auditory & visual stimuli 3. 3-way mirror 4. Identical settings at different institutions 5. Sessions video/audio taped 6. Paired with facilitator subjects worked with best	1. All subjects performed above chance when facilitated. 2. 4 of 6 subjects unable to type/point independently; 2 subjects able to type/point independently, but were able to do so prior to study 3. No subjects able to type via mechanical device 4. Informal follow-up revealed no positive gains
Edelson <i>et al.</i> (1998)	Evaluate use of mechanical device replacing human	6	Autism	M: 3 F: 3 (5-31)	FC Proficient	Facilitator support Independent typing Mechanical device support	Classroom	Yes/yes	8 Weeks	1. Conditions: facilitator support; independent typing by mechanical FC device 2. Videotape 3. Independent interrater agreement 4. Task Pretraining 5. Task execution 6. Task extension (posttraining)	1. All subjects performed above chance when facilitated. 2. 4 of 6 subjects unable to type/point independently; 2 subjects able to type/point independently, but were able to do so prior to study 3. No subjects able to type via mechanical device 4. Informal follow-up revealed no positive gains

continues

Table I. Continued

Study	Purpose	N	Label/n	Sex (age range, years)	Subject characteristics	Communication medium	Setting	FC experience subjects/facilitators	Duration	Control procedures	Results/conclusions
Hirshoren and Gregory (1995)	Communication value of FC and facilitator influence	22	Multiple Handicaps/16 Autism/4 OHI/1 MR/1	M: 19 F: 3 (6-21)	None	Supported typing	School	Yes/yes	n/a	1. Randomized conditions 2. Familiar facilitators 3. Headphones 4. Blind response raters	1. Only 2 of 192 responses possibly via FC across 3 conditions
Kerrin <i>et al.</i> (1998)	Facilitator effectiveness	2	Autism	M: 2 (9-10)	Nonverbal	Supported typing	Classroom	Yes/yes	10 Sessions over a month	1. Naive (unsighted) and informed (sighted) facilitator 2. Alternating treatments with sighted/non-sighted conditions counterbalanced 3. Verbal commands to subjects & subject responses independently evaluated by 2 observers	1. Subjects' responses more accurate in sighted condition 2. Facilitator influence likely unintentional
Kezuka (1997) Experiment 1	Evaluate role of facilitator	1	Autism	F: 1 (11)	Nonverbal, perseverative	Direct supported typing Indirect support via loose ribbon	n/a	Yes/yes	31 successive trials	1. "Telepathy" game via direct support and indirect support conditions 2. Videotaped trials 3. Electromyography confirmation of keyboard strokes 4. Interobserver evaluation of subject's responses	1. Successive correct responses in direct condition 2. Errors appeared more often, time trial slower as length of ribbon increased in indirect condition 3. More response errors than (2) with facilitator contact of nonfacilitating subject hand 4. Most accurate responses when facilitator, in indirect condition,

had very short
lether to subjects
facilitating hand

5. Facilitator sup-
plied force cues
to subject,
thereby control-
ling the typing

1. Force changes
greatest in the
vicinity of the
correct key

2. Facilitator pro-
vided force cuing
around the cor-
rect response key

1. Facilitation by
touch likely (*con-
tact condition*)
2. Correct
responses appear
more likely with
practice

1. Strain gauge
2. Dynamic strain
amplifier

n/a

Indirect con-
tact for
pointing via
rod with at-
tached strain
gauge

Nonverbal,
perseverative

F: 1
(11)

Autism

1

Verification of
facilitator
control of
subjects fa-
cilitating
movements

Kezuka
(1997)
Experiment
2

Trained for
study/
Trained for
study

n/a

Contact rod
No contact rod

Nondisabled

n/a

Nondisabled
pairs:
Parent-child/7
Friends/17
Sisters/1
Colleagues/5
Married cou-
ples/2

32

Verification of
facilitator
influence
Influence of
motor con-
trol in facili-
tator
influence
Whether motor
control was
visually
cued or by
physical
pressure

Kezuka
(1997)
Experiment
3

5 contact
sessions
5 Non con-
tact ses-
sions

n/a

Contact rod
direct
No contact rod
(indirect)

Nondisabled

n/a

Nondisabled
pairs:
Friends/4
Sisters/1
Colleagues/2

7

How facilitat-
ors control
subjects'
movement
through vi-
sual or touch
cues

Kezuka
(1997)
Experiment
4

1. Contact rod
blind (direct)
2. Contact ribbon
(indirect)

Yes/yes

n/a

Contact rod
direct
No contact rod
(indirect)

Nondisabled

n/a

Nondisabled
pairs:
Friends/4
Sisters/1
Colleagues/2

7

How facilitat-
ors control
subjects'
movement
through vi-
sual or touch
cues

Kezuka
(1997)
Experiment
4

1. Contact rod
blind responses
more accurate
than sighted con-
tact rod trials

2. Visual cuing not
necessary for fa-
cilitator influ-
ence, touch
sufficient

3. Contact ribbon
condition indi-
cated that facili-
tator pressure
was necessary
for subjects' ac-
curate responses

continues

Table I. Continued

Study	Purpose	N	Label/n	Sex (age range, years)	Subject characteristics	Communication medium	Setting	FC experience subjects/facilitators	Duration	Control procedures	Results/ conclusions
Kezaka (1997) Experiment 5	Relative effectiveness of pressure cues vs. motor control in FC	5	Nondisabled pairs: Friends/3 Sisters/1 Colleagues/1	n/a	Nondisabled	Direct support via control rod	n/a	Yes/yes	10 Trials	1. Contact rod, immobile condition 2. Contact rod, facilitator blind condition 3. Contact spring	1. Facilitator force influenced speed of subjects' response 2. Facilitators unconsciously applied sufficient force to move subjects' hand toward correct response
Montee <i>et al.</i> (1995)	Facilitator influence	7	MR	M: 4 F: 3 (20-52)	CP Epilepsy Autism ADHD	Supported typing	Residential center	Yes/yes	Weekly (# weeks not specified)	1. Familiar client/facilitator pairs 2. Varied response formats & conditions 3. Baseline data 4. Terminate session when subject appears anxious 5. Naturalistic setting	1. No communication via FC 2. Facilitator controlled typing 3. Anxiety/avoidance behaviors not a factor in FC failure 4. No difference in subjects' response to formats
Myles and Simpson (1994)	Effective use of FC to increase academic skills	10	Autism	M: 7 F: 3 (6-12)	Nonverbal/highly limited verbal Moderate/severe MR Various DD Severely socially impaired	Supported typing	Classroom	Trained for study/trained for study	15 Weeks, daily	1. Set work questions 2. Informed & naive facilitators 3. Affective questions 4. Spontaneous communication opportunities	1. Subjects generally unable to answer questions with naive facilitators 2. No appearance of cognitive, social, & communicative abilities above previous levels
Myles <i>et al.</i> (1996a)	Impact of FC on behaviors and social interactions	12	Autism	M: 9 F: 3 (12-28)	Severely language impaired Moderate/Severe MR Severely impaired social skills Aberrant/self stimulatory behaviors	Supported typing & pointing	School	Yes/trained for study	4 Days per week for 14 consecutive weeks	1. Varying conditions 2. Conditions counterbalanced 3. Targeted range of social behaviors	1. No differences in targeted behaviors after FC 2. FC did not make subjects more socially adept

Myles <i>et al.</i> (1996b)	Educational utility of FC	12	Autism	M: 9 F: 3 (12–28)	Severely language impaired Moderate/ Severe MR Severely impaired social skills Aberrant/self stimulatory behaviors	Supported typing & pointing	School	No/trained for study	4 Days per week for 14 consecutive weeks	1. Academic variables under different conditions 2. DI	1. No differences among subjects on any task after FC 2. FC not an effective educational tool 3. Subjects unable to demonstrate academic skills via FC 4. Novelty of facilitator's headphones might have been influential
Oswald (1994)	Assessment & measure FC influence	7	Autism	n/a (12–21)	Moderate/ severe MR	Supported typing	Classroom Residence	Yes/yes	Single session	1. Distractor picture paired with stimulus picture 2. Randomized presentation of distractor pictures to subjects and facilitators 3. Verbal stimuli prerecorded, delivered simultaneously via headphones to subject & facilitator 4. Interrater agreement for all responses	1. Clear indications of facilitator influence 2. Facilitators unable to detect their influence 3. Facilitators provided different levels of influence to different subjects 4. No unexpected subject linguistic competence demonstrated via FC
Regal <i>et al.</i> (1994)	Assets presence of rudimentary FC	19	All developmentally disabled Profound MR/15 Severe MR/4 Autism/5 Cerebral palsy/4	M: 9 F: 10 (23–50)	No previous communication before using FC	Supported typing and pointing	“Experimental room”	Yes/yes	Single session	1. Naive and informed facilitators 2. Pretraining in experimental tasks 3. Response variables (shape, number, color) randomly presented 4. Experimenter monitoring of “experimental room” 5. No verbal cues from facilitator	1. Total responses at chance levels 2. Individual subjects and facilitators' response at chance levels 3. No evidence of fatigue influencing lack of positive responses 4. Facilitators confidence in the effectiveness not supported by subject responses

continues

Table I. Continued

Study	Purpose	N	Label/n	Sex (age range, years)	Subject characteristics	Communication medium	Setting	FC experience subjects/facilitators	Duration	Control procedures	Results/conclusions
Shane and Kearns (1994)	Origin of FC authorship & facilitator influence	1	Severe MR	M: 1 (38)	Nonverbal	Supported typing	Workplace	Yes/yes	n/a	<ol style="list-style-type: none"> 1. Naive (unshared information) & informed facilitator (shared information) showed 100% subject accuracy 2. Uninformed facilitated condition (unshared information) showed 0 subject accuracy 3. FC authorship conclusively from the facilitator 	
Simon, <i>et al.</i> (1996)	Follow-up of apparent FC effectiveness from Simon <i>et al.</i> (1994) Comparison of accuracy of FC to Picture Exchange Communication System (PECS)	1	Moderate MR	M: 1 (14)	ADHD	Supported typing	Classroom	Yes/yes	22 Days (2 trials per day)	<ol style="list-style-type: none"> 1. Randomized presentation of stimulus known to subject 2. Subject description of stimulus to naive facilitator (via FC) or naive teacher (via PECS) 3. Subject description of stimulus with cuing 4. Follow-up at 10 weeks 	

Simpson & Myles (1995b)	Temporal effectiveness of FC	18	Autism	M: 13 F: 5 (3-20)	Nonverbal Severely impaired social skills	Supported typing	Classroom	No/no	5 Days/ week for 15 weeks	5. Video interrater agreement on subject responses
										<ol style="list-style-type: none"> 1. 50% of subjects only able to perform at prestudy levels 2. Subject responses increased over time, especially in facilitator-known condition 3. No evidence of FC as claimed by Bikien 4. Subjects uniformly inaccurate in passing information in naive facilitator condition 5. FC may have some small, highly specific application to some autistic individuals
										<ol style="list-style-type: none"> 1. 5 response conditions (set work, questions, informed/naive teacher questions, subject affective questions, spontaneous communication events) 2. No FC outside of study trials 3. No use of previous communication mediums 4. Consistent FC hold 5. Same facilitator throughout study 6. Facilitators control purposeless subject movements 7. Redirection of subject to focus on FC 8. No restriction of what was typed via FC 9. Response conditions conducted in same order across subjects 10. Sessions conducted in setting where subjects was most productive 11. Redirect subject responses where necessary 12. Model responses for subjects in set work and teacher-known condition

continues

Table 1. Continued

Study	Purpose	N	Label/n	Sex (age range, years)	Subject characteristics	Communication medium	Setting	FC experience subjects/facilitators	Duration	Control procedures	Results/conclusions
Simpson & Myles (1995b) Cont.											<p>13. All responses recorded & submitted for analysis</p> <p>14. Facilitators to consciously try to not direct subjects' hand movements</p> <p>15. Facilitators monitored for faithful execution of controlled conditions</p> <p>16. Interobserver reliability for facilitator compliance</p>
Smith <i>et al.</i> (1994)	Facilitated knowledge affecting assistance of output	10	MR	M: 6 F: 4 (14-51)	Moderate/severe profound MR Limited communicative ability	Supported typing	n/a	Yes/yes	n/a	<p>1. Multiple sessions</p> <p>2. Alternate facilitators</p> <p>3. Naive and informed facilitators</p> <p>4. Random item selection</p> <p>5. Facilitator support levels (none, medium support, full support)</p>	<p>1. No correct responses at no help, 1 at medium help, substantial correct response with full support</p>

^a MR, mental retardation; OHI, other health impair; DD, developmentally disabled; CP, cerebral palsy.

Table II. Characteristics of CP Studies Supportive of FC (CP+)^a

Study	Purpose/ description	N	Label/ <i>n</i> ^a	Sex (age range, years)	Subject charac- teris- tics	Commu- nication medium	Setting	FC experience subjects/ facilitators	Duration	Control procedures	Results/ conclusions
Bebko, <i>et al.</i> (1996)	Multiple method evaluation of FC	20	Autism/14 Non Autism/5 Other/1	M: 15 F: 5 (6-21)	Moderate/ severe cognitive impairment No literacy skills	Supported typing & pointing	Classroom	Trained for study/ yes	6 Weeks Follow-up 5-7 months	1. Multiple facilitation methods 2. Auditory/visual input 3. Pointing or typing 4. Informed & naive facilitators 5. Ordered conditions counterbalanced over sessions	1. Minimal support for FC across methods 2. Significant facilitator influence, but less than other studies 3. Subjects may appear more positive when using FC 4. Subjects communicated more effectively without FC 5. Subjects' independent responses lessened after introduction of FC 6. No FC effect at follow-up
Heckler (1994)	Facilitator influence	1	Autism (7)	F: 1 (7)	Nonverbal Echolalic	Supported typing	n/a	Yes/yes	6 Weeks, once per week	1. Informed facilitator 2. Naive facilitator 3. Informed subject information communicated to naive facilitator	1. Subjects consistently chose incorrect responses for controls 1 & 3 2. For control 2, responses mixed. Follow-up showed some accuracy of events unknown to facilitator 3. No substantiation of sexual abuse

Table II. Continued

Study	Purpose/ description	N	Label/ <i>n</i> ^a	Sex (age range, years)	Subject character- istics	Commu- nication medium	Setting	FC experience/ subjects/ facilitators	Duration	Control procedures	Results/ conclusions
Sheehan and Matuozzi (1996)	Validity of FC in corroborating unknown information	3	Autism MR	M: 2 F: 1 (8-24)	Severely impaired verbal ability Nonverbal	Supported typing	Day treatment program	Yes/yes	14 Sessions over 3 months	1. Naïve facilitator	1. Subjects sometimes able to communicate information to naïve facilitator but possible facilitator influence
Vasquez (1995)	Effectiveness of FC to facilitate word retrieval	3	Autism	M: 2 F: 1 (9-11)	Nonverbal	Supported typing	Classroom	Yes/yes	5 Months	1. Blind and double blind conditions 2. Word retrieval allowing description 3. Observation allowing handling of objects blind trials 4. Evaluation conducted as a game	1. No correct response for control 1 2. No correct responses for control 2 3. No correct responses on blind trials 4. Some correct/pseudo responses on known trials
Cardinal <i>et al.</i> (1996)	Authorship of FC	43	Autism/17 MR/22 MR-CP/2 DD/2	M: 27 F: 16 (11-22)	Severely handicapped Severe communi- cation disorders	Supported typing	Subjects across 5 campuses	Yes/yes	6 Weeks	1. Recorder presents/ documents subjects' responses 2. Observers to monitor FC interactions for protocol accuracy 3. Auditory and visual stimuli 4. Random stimulus selection 1. Uninformed facilitator	1. Some subjects able to pass information via FC 2. Measuring FC may benefit from subjects' practice of the test protocol
Weiss, <i>et al.</i> (1996)	FC effectiveness	1	MR Autism Seizures	M: 1 (13)	Echolalia Perseveration Self-stimulating behaviors	Supported typing	School Facilitator's home	Yes/yes	n/a	1. Subject accurately answered questions. 2. I phase terminated when subject began to give incorrect answers	

^a See Table I for explanations of abbreviations.

Table III. Characteristics of NCP Studies (NCP+)^a

Study	Purpose/ description	N	Label/n	Sex (age range, years)	Subject character- istics	Commu- nication medium	Setting	FC experience subjects/ facilitators	Duration	Control procedures	Results/ conclusions
Biklen, <i>et al.</i> (1995)	Facilitator assessment of subjects messages	17	Autism/11 MR/3 Other/3	M: 12 F: 5 (DOB 1975 1988)	Some verbal Some nonverbal Some echolalic	Supported typing	Various	Yes/yes	6 Months	None	1. FC valid because of demonstrable differences in students' attention, typing, message content 2. FC validated by compiling cumulative "student communication portfolio" 1. Subjects communicated much more with FC 2. Collateral effects (able to vote; participation in team meeting; visiting university lecturer) 1. Increased language ability 2. FC helpful 3. FC allowed better communication than oral communication 4. Oral skills did not deteriorate while using FC
Clarkson (1994)	Music therapy via FC	1	Autism	M: 1 (20 ⁷)	Nonverbal	Supported typing	School	Yes/yes	Several months	None	1. Subjects communicated much more with FC 2. Collateral effects (able to vote; participation in team meeting; visiting university lecturer)
Janzen-Wilde, <i>et al.</i> (1995)	FC vs. oral responses	1	Language/ learning disabled	M: 1 (6)	Some verbal ability	Supported typing Oral utterances	Classroom	Trained for study/ Trained for study	3 Months	None	1. Increased language ability 2. FC helpful 3. FC allowed better communication than oral communication 4. Oral skills did not deteriorate while using FC
Ofney (1995)	Communi- cation between facilitator and subject	1	Autism	M: 1 (22)	Moderate MR Some reading ability	Supported typing	Various	Yes/yes	n/a	None	1. Nonverbal behavior generally matched typed messages 2. Unexpected FC characteristics show no facilitator influence

^aSee Table I for explanations of abbreviations.

Table IV. Summary of Review Study Features^{a,b}

Study characteristic	CP- studies	CP+ studies	NCP studies
Label			
Autistic	101	38	13
MR	54	25	3
Other	53	8	4
Sex			
M	111	46	15
F	49	24	5
Age/Range	5-5	26-24	6-22
Communication medium			
Supported typing	20	6	4
Supported pointing	3	1	0
Other:	3	0	1
Setting			
School	3	2	1
Classroom	6	2	1
Other	8	2	2
FC Experience			
Previous experience			
Subjects	18	5	3
Facilitators	18	6	1
Trained for study			
Subjects	3	1	1
Facilitators	3	0	1
Duration	Highly varied	Highly varied	Highly varied
Control procedures			
None	0	0	0
Single	1	2	0
Multiple	18	4	0
Results			
Refute FC	53	9	0
Support FC	2	8	10

^a Frequency counts are as accurate as possible given the wide variations or omissions reported in the primary studies.

^b See Table I for explanations of abbreviations.

N

This study feature referred to the overall number of subjects in the study, which varied widely: CP- (*N* range = 1-32); CP+ (*N* range = 1-43); NCP+ (*N* range = 1-17). Subjects within each individual study were also widely varied. CP- studies reported 13 of 19 studies (.68) with fewer than 10 subjects; CP+ studies reflected 4 of 6 studies (.67) with fewer than 10 subjects, and for NCP+ studies, 3 of the 4 studies (.75) reported fewer than 10 subjects.

Label/n

The label/*n* study feature described the subjects' disability label and the number of subjects under each label. In CP-, CP+, and NCP+ studies, two label sub-

groups, autism and mental retardation, were most often represented. A variety of other labels and diagnoses were included across a number of impairments and degrees of severity. The studies also varied widely in the definition of subjects in all label groups, even when secondary characteristics were noted (see below). Among 160 subjects in the CP- studies labels were autism (101), mental retardation (54), and a variety of other labels (53). Similarly, for the 70 subjects in the CP+ studies, labels were autism (38), mental retardation (25), and other (8). For NCP+ studies among 20 subjects, the labels were autism (13), mental retardation (3), and other (4). In all three groups of studies, labels sometimes exceed the number of subjects because of comorbid or double labeling of subjects. For all studies, therefore, raw frequency counts provide the

most accurate representation given the variation of reported detail in the studies.

Sex (Age Range, Years)

All studies with two exceptions (Kezuka, 1997; Oswald, 1994) provided a breakdown by sex (male, female) and overall age range for *N*. Where sex was specified, subjects were predominantly male in all three groups of studies: CP⁻ (M = 111; F = 49), CP⁺ (M = 46; F = 24), and NCP⁺ (M = 15; F = 5). It was not possible to match all subjects' sex to their age, as this information was not consistently provided. The subjects' overall age range was much larger in the CP⁻ studies (6–52 years) than CP⁺ studies (6–24 years), and the NCP⁺ studies (6–24 years).

Subject Characteristics

A helpful feature of many of the studies was the reporting of secondary descriptive characteristics (e.g., severe cognitive impairment, nonverbal, Down Syndrome, echolalic) complimentary to the subjects' primary disability label (e.g., autistic and mentally retarded). Only Hirshoren and Gregory (1995) omitted this information, while Oswald (1994) provided comorbid labels. These secondary descriptive characteristics were helpful because they provided more detailed profiles than the subjects' generic disability label. This feature revealed the wide range of subjects, abilities, behaviors, and other cogent variables that might have some bearing on the study outcomes.

Communication Medium

The communication medium described subjects' means of communication during the study. Consistent with FC technique, almost all the studies used typing by the subjects with direct or indirect physical support by their facilitators (CP⁻ = 19, CP⁺ = 6, NCP⁺ = 4). A few studies added other communication features such as supported pointing (CP⁻ = 3, CP⁺ = 1, NCP⁺ = 1).

Study Settings

This study feature referred to the geographical location where the study data were collected and were reported for most studies. For CP⁻ (– and +) studies, the locations were dominated by school or classroom settings (some studies specified classrooms, others were more general, such as that the setting was a school). For NCP⁺ studies, there was a much wider spectrum of settings. However, this characteristic was markedly unspecific across all studies.

FC Experience

Facilitator or client FC experience was reported for all studies. While all studies reported that at the time of the study both facilitators and clients were familiar with FC, the amount of FC experience prior to the study varied among both facilitators and clients. Some facilitators (CP⁻ = 3, CP⁺ = 0, NCP⁺ = 1) and subjects (CP⁻ = 3, CP⁺ = 1, NCP⁺ = 1) were trained in FC for the study, while in other studies subjects (CP⁻ = 18, CP⁺ = 3, NCP⁺ = 3) and facilitators (CP⁻ = 18, CP⁺ = 6, NCP⁺ = 1) had been using FC for varied extended periods of time prior to the study.

Duration

Duration was the length of time over which study data were generated. Of all the study characteristics, this feature varied most widely across all three groups of studies [e.g., 4 days per week for 14 consecutive weeks (Myles, Simpson, & Smith, 1996a, b), versus 3 consecutive days (Crews *et al.*, 1995)]. Several studies provided no temporal information (Beck & Pirovano, 1996; Hirshoren & Gregory, 1995; Olney, 1995; Smith, Haas, & Belcher, 1994; Weiss *et al.*, 1996) or only vague information in this regard (Bomba, *et al.*, 1996; Clarkson, 1994; Vasquez, 1995).

Control Procedures

This characteristic described attempts to control for internal validity and attempts to eliminate the most obvious rival explanations for FC, the influence of the facilitator. Perhaps the most informative information in examining the three study sets relates to this CP vs. NCP feature. (Note: total outcomes supporting or refuting FC do not include all observations in the Results/Conclusions column of Tables I–III. The CP⁻ studies, all of which reported one or more control procedures, reflected 53 outcomes refuting FC and only 2 outcomes supportive of FC. For CP⁺ studies reporting one or more control procedures, 9 outcomes refuted FC and 8 outcomes supported FC. All four NCP⁺ studies were anecdotal, with no control procedures and reported distinctly different findings, that is, no outcomes refuting FC (0) versus many more outcomes (10) supportive of FC. These findings are summarized in Table IV. Generally, it appears that there is a tendency for studies with several control procedures to refute FC claims, those with fewer control procedures to produce mixed results, and those with no control procedures to support FC claims.

Results and Conclusions

This final study feature usually provided a summary of results and authors' conclusions based on results. All studies included this characteristic.

REVIEW ANALYSIS

Generally, the study characteristics reported for all studies are quite variable. However, several observations related to the current study set and for future investigation are important. First, more detailed descriptions of label/*n* would be helpful. While subjects with autism and mental retardation were most often cited, various other labels indicated that FC effects were evaluated among several different populations, which may be characteristically more or less communicative. This was particularly problematic when comorbid labels were reported (e.g., Regal, Rooney, & Wandas, 1994). Further, more explanations of the level of severity of communication impairment would have been helpful in judging whether FC should indeed have been used rather than other assistive interventions. Similarly, the *subject characteristics* component would have been more informative if the characteristics had been more closely defined (e.g., "severe cognitive impairment" does not adequately describe impairment level).

Second, the *sex (age range, years)* characteristic was provided for all studies except Kezuka (1997) and Oswald (1994). More information in this regard may be important in establishing the potential and relationship of previous learning experiences to the study task. Also, the study characteristic of *duration* was only vaguely described. Across all studies, more temporal information delineating the frequency and duration of experimental sessions would provide an added dimension with which to judge FC efficacy.

Given the study characteristics exhibited by the CP-, CP+, and NCP studies, there is a marked contrast in their findings (see Table IV), similar to previous reviews. Among CP- studies, findings heavily refute the viability of FC. For CP+ studies, the results are more mixed. In contrast, NCP+ studies heavily favor FC. It is reasonable to conclude from these characteristics that, generally, studies including one or more control procedures discounted the efficacy claims of FC, while studies ignoring control procedures supported FC efficacy claims. Among the three groups of studies, the CP+ studies are especially noteworthy because they include control procedures and claim findings supportive of FC (Bebko *et al.*, 1996; Cardinal

et al., 1996; Heckler, 1994; Sheehan & Matuozi, 1996; Vasquez, 1995; Weiss *et al.*, 1996). These studies can be subdivided into two vectors (see Table II): those which assert unspecific and undefined findings supportive of FC (Bebko *et al.*, 1996; Heckler, 1994; Sheehan & Matuozi, 1996; Vasquez, 1995), and those reporting relatively more substantial positive findings supportive of FC (Cardinal *et al.*, 1996; Weiss *et al.*, 1996). In terms of the former group, such confounding variables as a finding of no difference between FC and other communication techniques (Bebko *et al.*, 1996), the assertion of unspecified accuracy at follow-up when evidence of such accuracy failed to appear in the initial study (Heckler, 1994), and that some positive responses could have been pseudo responses influenced by other or unknown factors (Vasquez, 1995) can be dismissed as verifiable evidence of FC efficacy. Indeed, the frailty of these emanating from studies apparently using at least some form of control procedure is more likely evidence of the inefficacy of FC than vice versa.

The assertions of the two other CP+ studies, however, appear more substantial (Cardinal *et al.*, 1996; Weiss *et al.*, 1996) and required further examination.

Cardinal Hanson and Wakeham

Cardinal *et al.*, (1996) ". . . examined whether facilitated communication users, under controlled conditions, could transmit rudimentary information to a naive facilitator" (p. 231). The study incorporated 43 subjects exhibiting a range of disabling conditions (autism, mental retardation, cerebral palsy, and developmental delay). All subjects (27 male, 16 female, 11–22 years old) were characterized as severely handicapped with communication disorders. The settings for data collection were ten classrooms across five different school campuses and four school districts over a 6-week period. Single, randomly selected words were shown to the subjects outside of the presence of the facilitator. The subjects then had to communicate the stimulus word through facilitation once the facilitator entered the room. All subjects had used facilitated communication prior to the study, as had the facilitators.

Three sets of study participants elicited and recorded the data via the following procedure:

Recorders

Twenty-seven recorders were responsible for recording what the subject attempted to communicate via facilitated communication. They were teachers or other school personnel (e.g., teaching assistants) who, prior to the study, were involved with the subjects in

“similar educational activities [to the study activities]” (Cardinal *et al.*, 1996, p. 233). The recorders (a) presented the random stimulus words to the subjects outside of the presence of the facilitator, (b) then directed the facilitator to enter the room, (c) recorded the letters typed by the subject via facilitation based on the facilitators’ saying what letters the subject was typing, and (d) stored the data securely.

Facilitators

The 31 naive facilitators asked the subjects to type the word just presented by the recorder before the facilitator entered the room. The facilitators provided standard facilitation support. The facilitators were either teachers or other school personnel (e.g., teacher assistants). Eighteen facilitators worked with a single subject, 9 facilitators with 2 students each, and 4 facilitators with 3 students each. Each facilitator was also “. . . one of the students’ typical facilitators in school [prior to the study]” (Cardinal *et al.*, 1996, p. 233).

Research Observers

The research observers, consisting of “. . . the [study] authors, a communication specialist, and a university professor” (Cardinal *et al.*, 1996, p. 233), were responsible for (a) monitoring each recorder–facilitator dyad until the study procedures had been mastered and thereafter (b) monitoring the dyads “. . . at least once a week after that (i.e., 33% of sessions)” (Cardinal *et al.*, 1996, p. 233) to ensure that the study procedure was accurately implemented. Over the course of the observed sessions, 75 trials were eliminated from “. . . over 3800 total trials” (Cardinal *et al.*, 1996, p. 233) as being inaccurate.

Test equipment consisted of laminated photocopies of actual keyboards, the same standard devices used by the subjects in their regular school activities. Subjects, via facilitation, were thereby able to point to letters on the laminated photocopies they wished to communicate. Test materials consisted of a 100-word set of stimulus words

. . . primarily of nouns, with some verbs, that contained no more than six letters . . . selected because they were considered to be familiar to the students in that they were part of the age-appropriate, community based, functional curriculum presented in an inclusive school program . . . words that would be encountered by even the youngest study participant within their daily curriculum . . . (Cardinal *et al.*, 1996, p. 234)

The stimulus word set had not previously been presented to the subjects, although “facilitators did have open access to the word set” (Cardinal *et al.*, 1996, p. 234).

Data were collected across three conditions in consecutive order with subject responses counted as accurate only if the stimulus word was produced accurately and with no other letters present as distractors:

Baseline 1 Condition

The baseline 1 condition consisted of 5 trials per subject for 2 days. The entire procedure as described above was performed, but without facilitation. That is, the naive facilitator was present but did not actively engage in facilitation.

Facilitated Condition

The facilitated condition consisted of 5 trials per subject, 3 days per week over 6 weeks. The entire procedure was performed with facilitation by the naive facilitator.

Baseline 2 Condition

Baseline 2 condition consisted of 5 trials per subject for 2 days. The entire procedure was performed, as for baseline 1, without facilitation.

Results indicated low levels of accuracy in the baseline 1 condition (no facilitation), significant high levels of accuracy in the facilitated condition, and higher levels of accuracy in the Baseline 2 condition (no facilitation) than in baseline 1.

Based on these results, Cardinal *et al.*, (1996) assert two clear findings from their study, “. . . that (a) under controlled conditions, some facilitated communication users can pass accurate information and (b) measurement of facilitated communication under test conditions may be significantly benefited by extensive practice of the test protocol” (p. 231). Several methodological problems within the study, however, indicate that the conclusions drawn by the authors are, at best, inconclusive.

First, the study does not control adequately for data collector bias, in this case that of the facilitator–recorder dyad. Data collectors can influence study outcomes because, absent experimental control, they can unconsciously distort what they are collecting to make certain outcomes more likely (Frankael & Wallen, 1999). The study used “independent” recorders, whose responsibility was to record the letters typed by each subject. The typed letters were relayed to the recorder by the facilitator, not by the recorder directly observing and recording what was being facilitated. This aspect of the study was also problematic, because both the facilitators and recorders had previously worked

with the subjects on academic tasks and were likely familiar with at least some of the stimulus words (see below). Furthermore, the laminated keyboard copies do not provide a hard record of what letters the subject selected and it is likely that potential for recording errors is greater when the recorder has to judge which letters are selected by the subjects. In addition, the authors supply no verification (a) that the letter called to the recorder by the facilitator was actually the letter selected by the subject, (b) whether the letter called by the facilitator was actually selected by the subject or whether the subject only pointed in the vicinity of the called letter, or (c) that what the facilitator called was what was written down accurately by the recorder. These aspects remain problematic in spite of the authors' claim of "research observers" who ". . . monitored each experimental dyad [facilitator/recorder]" (Cardinal, *et al.*, p. 233) because the specific actions related to how the "research observers" observed are not provided. In addition, the research observers were not present at every trial across the study, allowing for the increased possibility of inaccurate relaying by the facilitator or recording by the recorder to go undetected.

There is further evidence of collector bias. While the facilitators were supposedly blind to the words presented to the subjects by the recorders, the facilitators knew the pool of words prior to the study. "Facilitators did have open access to the word set" (Cardinal *et al.*, 1996, p. 234). Furthermore, the words were drawn from the subjects' school curriculum—the same curriculum with which the facilitators were intimately familiar from working with the subjects in school prior to the study. This raises the likelihood that while the facilitators could not have known which word would be presented by the recorder, subject accuracy may have been bolstered by facilitator influence once the first letter or two had been selected. A practice effect would have grown over time given that the facilitators facilitated 90 trials with each subject and that 13 of the 31 facilitators facilitated with more than one subject. Furthermore, the likelihood that facilitators could guess the word, seeing that they knew which words were in the pool, appears much higher than the 1 chance in 100 claimed by the study. Thus, there are only 19 different first letters in the pool words, a 1 in 19 chance (.052) that the facilitator could have guessed the correct initial letter. If the first letter had been guessed correctly, the probability of being able to guess consecutive letters, and, therefore, pool words, increases dramatically, although there is some variability given the frequency of similar first letters in the pool. For instance, the probability of guessing four of the pool

words (jump, key, leg, orange) after identifying the first letter was 1.0, because only one word in the pool began with those letters; the probability of guessing four of the pool words (apple, arm, girl, green) after guessing their first letter is 0.5, and so on. The probability range is 1.0–0.06, with a mean probability of the facilitator guessing the word at 0.25, much higher than the authors' proffered probability of 0.01. There is, therefore, no way of establishing whether spelling accuracy emanated entirely from the subject, from the facilitator, or was the result of a combined subject–facilitator effort.

Second, the materials used for the study may well have precipitated inaccurate results, in effect, providing a testing threat to internal validity. Testing threats to internal validity typically arise between a pretest and a posttest, where any number of rival explanations for the posttest results can be offered (Frankael & Wallen, 1999). In this case, however, no pretest was executed, making the design even weaker than had a pretest been used. The study task words were ". . . selected because they were considered to be familiar to the students in that they were part of the age-appropriate . . . curriculum presented in [their] inclusive school program" (Cardinal *et al.*, 1996, p. 234). "Considered familiarity" and actual knowledge of the stimulus words are not necessarily compatible. The study supplies no information as to whether, prior to the study, subject knowledge of the words was verified (i.e., a pretest). The possibility exists, therefore, that some subjects could not recognize some of the stimulus words, and, even assuming legitimate subject FC authorship, would, therefore, be unable to communicate these words to the facilitators. Thus, spelling these words correctly may have either been impossible or, equally, possible by chance or by facilitator influence rather than actual subject knowledge of the words. In any event, this lack of verification could have influenced what was reported by the facilitators and recorders.

The third problem with the study findings involves causal assumption, that is, that study results were potentially influenced by preconceived assumptions of what caused a desired study effect. None of the subjects, according to the authors, ". . . demonstrated the ability to generate written language prior to the use of facilitated communication." (Cardinal *et al.*, 1996, p. 232). This assertion seems to make the claim that *any* demonstrated written language ability resulted exclusively from FC both before and during the study. No attempt is made to consider or gauge the possibility of rival explanations, which might have threatened the internal validity of the study. For example, the subjects' history

and maturation both before and during the study might have resulted in improved written language function, as both prior to and during the study the subjects continued in their academic work via FC in their “. . . core curriculum classes” (p. 232). Furthermore, the authors offer that even before FC was introduced to these subjects as part of their academic curriculum, instructional attempts for increasing language function was already underway, that “. . . prior to using this technique [FC] the students had educational objectives that they were learning to “read” community sign vocabulary or to write their names” (p. 232). The possibility that other educational attempts both before and after the introduction of FC were at least partly responsible for the study result is not addressed in the study.

Fourth, the testing procedure itself may well have threatened the internal validity of the study. That is, attributing results to a particular intervention (here FC) might be inaccurate (Frankael & Wallen, 1999), with results simply being an artifact of the baseline 1 condition. The authors assert that the elevated performance levels in the facilitated condition over the baseline 1 condition, and the less elevated results in the baseline 2 condition over the baseline 1 condition, were only the result of FC. However, given that the stimulus words were deliberately selected to be familiar to the subjects and the facilitators, and that words for all three conditions came from the same limited pool, a distinct practice effect exists. This potential practice effect, unaddressed by the authors, is particularly troubling given their confidence that these subjects are much more intelligent and academically adept than what they are usually given credit for, a perception that would add to the likelihood of the testing effect.

Fifth, closer attention to the reported baseline 1 condition (unfacilitated) results vs. the facilitation and baseline 2 (unfacilitated) conditions reveals other problems with the study in terms of regression effects. That is, when study subjects, as is the case here, are selected at extremes of low performance, improvement over time often results in spite of any proffered intervention (Frankael & Wallen, 1999). Thus, the claim that “. . . when students were allowed to use facilitation under the protocol conditions . . . they did significantly better than when they were not allowed to use facilitation” (Cardinal *et al.*, 1996, p. 236) is unremarkable given regression effects which are not controlled in this study. The authors’ explanation for the increase of baseline 2 is that practice of the study protocol under the facilitated condition resulted in increased performance without facilitation. Regression is an equally likely effect unaddressed by the study design.

Given these inconsistencies, the results reported by Cardinal *et al.*, (1996) must, at best, be interpreted as equivocal.

Weiss, Wagner, and Bauman

In the second CP+ study, Weiss *et al.*, (1996) studied facilitation of a single subject whom they reported “. . . was able to demonstrate valid facilitated communication” to pass information, concluding that “. . . facilitated communication can sometimes be a valid method for at least some individuals with development disabilities” (p. 220). The 13 year-old male was characterized by autism, severe mental retardation, and a seizure disorder. He had used FC for an extended period of time prior to the study and had “. . . been integrated into regular education classes . . . [where] he was successfully completing Grade 6, and subsequently Grade 7 academics with the use of facilitated communication . . . maintaining an A to B grade average” (p. 221). Study participants were the subject, a naive experienced facilitator, the first author (serving as the “experimenter” and as a second facilitator), and a referee responsible for verifying “. . . procedures and results” (p. 221). Data collection occurred over three separate story trials, trials 1 and 3 occurring in a classroom and trial 2 at the subject’s home. Stories in trials 1 and 2 were written by a research assistant and the story in trial 3 by a television producer. Each story was “. . . chosen because they were relatively short, contained several specific facts, were cross-culturally meaningful, and had a ‘moral’ to the story which would allow for the possibility of inferential descriptions of content” (p. 222). Similar successive procedures were followed in each trial, as follows:

Story Phase

With the naive facilitator absent, a short story was read to the subject by the experimenter while being typed into a word processor by the experimenter. The story was then read aloud to the subject a second time. No referee was present.

Consolidation Phase

With the naive facilitator absent, the experimenter asked the subject questions about the story that had just been read. The subject answered via facilitation, with the experimenter serving as the facilitator. Referee was present.

Test Phase

With the naive facilitator present, the experimenter asked the subject questions about the story. The subject answered via facilitation with the naive facilitator. Referee was present.

The questions were transcribed and an exact record of what the subject typed via FC was preserved.

Weiss *et al.* (1996) reported that the subject “. . . was highly accurate in his response to questions” (p. 225) in story trials 1 and 3. Story trial 2, however, did not produce accurate responses. Following several inaccurate responses in story trial 2, the subject facilitated that he was nervous and questioned whether the researchers thought he had cheated in getting accurate results in story trial 1. Based on this information, story trial 2 was terminated. The authors conceded that the expressed sentiments in story trial 2 might have been those of the facilitator rather than the subject.

Based on these results, the authors claimed two findings, that (a) story information elicited by the questions emanated from the subject, not the naive facilitator, and (b) that the subject was unexpectedly able to demonstrate simple inferential and abstract reasoning.

The first problem in this study relates to subject responses facilitated by the “experimenter” in the consolidation phase. In spite of protestations to the contrary, it is highly likely that the “experimenter” was influencing the answers in this phase for several reasons. First, the experimenter was privy to the story content and, therefore, was predisposed to influence the subject’s answers to story questions. The literature documented elsewhere in this review has unequivocally established that it is the facilitator rather than the subject who is responsible for answers via FC, as is probable here. This study provides no explanation or control procedures for readers to think otherwise. Second, the claim that the experimenter was not adept at facilitating does not negate the probability of facilitator influence, even if the authors suggest that as long as facilitators cannot discern obvious influencing of subjects’ movements, that facilitator influence is not occurring, a claim that has been thoroughly discredited in controlled investigations of facilitator influence (e.g., Kerrin, *et al.*, 1998; Kezuka, 1997; Oswald, 1994). The likelihood of the experimenter’s influence is strengthened when comparing the results reported in story trial 2, where the answers obtained using the experimenter are easily as accurate as what he obtained in story phases 1 and 3, while the answers obtained in story trial 2 by the naive experi-

menter are all incorrect, which quickly lead to the termination of the trial.

The second problem arises because the purpose of the consolidation phase is not explained. While the consolidation phase mimics the test phase (the same questions are asked by the experimenter in both phases), the difference being the facilitator (consolidation phase: experimenter; test phase: naive facilitator), changing the facilitators from the consolidation to the test phase clearly allows for an implementation threat to internal validity via different data collectors (Frankael & Warren, 1999). That is, different facilitators will likely have different influences on the facilitated outcome. For example, not only are the two facilitators different people, but they have very different levels of FC experience. Furthermore, internal validity is also weakened in terms of instrumentation because the characteristics of the data collectors (experimenter, naive facilitator) are markedly different.

A third problem arises when the subject was successful in answering questions to the experimenter as facilitator in trial 2, but was unable to do so to the naive facilitator. The authors assert that this unsuccessful trial was terminated because of the subject’s “. . . apparent discomfort” (Weiss *et al.*, 1996, p.225). Aside from the validity threats noted above, the authors do not explain why, for only story trial 2, the questions asked of the subject were markedly different from the experimenter versus the naive facilitator. This instrumentation bias further invalidates the results of the study.

Fourth, the successful story trials (1 and 3) are also problematic. The authors report that the subject successfully answered story-related questions via his (naive) facilitators in both story trial phases. Closer examination of the results, however, is less convincing because only for the third trial was a “referee” present to “. . . independently verify that the [naive] facilitator was indeed uninformed of the story contents being presented” (Weiss *et al.*, 1996, p. 222). Claiming verification via a referee for only story trial 3 leaves open the distinct possibility that the facilitator was not naive to the stories in story trials 1 and 2.

Fifth, the authors’ claims of subject inference are worth noting. They claim that “. . . some of the response during testing with an uninformed facilitator implied logical inferences, conjectural extrapolations on a story, and an abstracted ordering in his memory of story events” (Weiss *et al.*, 1996, p. 226). However, such a result, even assuming the ability of the subject to actually pass information to the naive facilitator, is predictable given that the questions are asked by the

facilitators, who thereby guide the ordering and nature of the responses. In addition, the authors themselves concede that the inference examples might not be the subject's, but that ". . . the uninformed facilitator could have inferred these responses" (p. 227).

As with Cardinal *et al.* (1996), the efficacy claims made by Weiss *et al.* (1996) cannot stand as irrefutable evidence of FC efficacy.

SUMMARY AND CONCLUSIONS

The results of the review support and confirm the conclusions reached by previous reviewers of the empirical FC literature. The divide between the results of studies incorporating control procedures find very little to no support for the efficacy of FC, studies employing fewer control procedures produce mixed results, and studies ignoring control procedures almost universally find FC to be effective. In the cases of the few, tentative positive results emerging from studies reporting some form of control procedures, as in the cases of Cardinal *et al.* (1996) and Weiss *et al.* (1996), these results are much more likely the artifact of methodological problems than an accurate representation of persuasive evidence.

However, should researchers so choose, there is still much work to be done related to FC, both theoretically and experimentally. Theoretically, many questions remain related to FC proponents' stance on anecdotal versus scientific evidence, the critique of Green and Shane (1994) and Biklen and Duchan's (1994) response notwithstanding. As this review makes clear, the correspondence of evidence or lack thereof appears correlated to the methodological means of obtaining that evidence. Any hope of establishing credibility for FC, as unrealistic as that may be, can only come from increased experimental rigor. At a more philosophical, level, Hudson's (1995) challenge to the weak theoretical underpinnings of FC should be addressed by FC proponents given that current theoretical perspectives of FC are, at best, unformed.

Experimentally, if investigation into FC is likely to continue, several aspects remain unaddressed. Specifically, more attention in future research could be paid to subjects (*n* and disability label), their age, the physical act of facilitation itself, issues around the settings for experiments, the motivations and beliefs of the facilitators, the inefficacy of FC over time, and an even greater emphasis of quantitative over qualitative methods for verification.

First, this review showed that the number of sub-

jects per study was highly variable. Future studies using larger groups of subjects, perhaps even by developing a representative sample, could add more definitive conclusions around the issue of authorship. Larger samples would also allow the formation of control and experimental groups where FC could be compared, under controlled conditions, to other augmented communication methods. Furthermore, the studies in this review show a variety of primary disability categories among the subjects. While the characteristics of each category is generally known, it is equally obvious that within each category the level of disability severity can be highly variable. More refined definition of disability characteristics and levels of severity might prove useful in establishing the inefficacy of FC in all disabilities and at all levels of severity.

Second, similar to aspects of label and severity, populations more closely defined by age could provide added evidence of FC inefficacy. Careful investigation along these two dimensions would establish unequivocally that FC is ineffective with all disability categories and among all age groups.

Third, further attention to facilitation itself might prove useful. A long-standing claim of FC proponents is that the ultimate goal of FC is unsupported facilitation. That is, over time, support should be faded and, ideally, eventually removed altogether (Crossley & Remington-Gurney, 1992). Investigation into differing intensities of facilitator support (e.g., full support at elbow or wrist, support only by touching no support) and the potential for facilitator influence should be more closely investigated, especially in the light of Kezuka's (1997) work and in spite of claim by Haskew and Donnellan (1993) that exotic psychological communicative powers exist between facilitators and their clients.

Fourth, closer attention should be paid to the settings in which the experiments occur, especially in view of FC proponents' insistence that naturalistic settings are a contributing factor to subject comfort and, therefore, the likelihood of communication via FC (Biklen, 1990). For example, how should the naturalistic setting be defined and operationalized? What is the threshold level above which alleged subject cooperation for FC is lost? and so on.

Fifth, given that the literature has established the influence of the facilitator in FC, and that much of influence appears to be unconscious (e.g., Braman *et al.*, 1995), and assuming the likelihood that more experienced facilitators will be more influential than less experienced facilitators, there is a need to further

investigate differential effects of facilitator influence based on their experience and proclivity for wanting the subject to succeed, an area addressed in small measure by Perry *et al.* (1998). That is, the issue of facilitators' influence must be even more closely linked to their beliefs that FC is effective.

Sixth, most investigations of FC have occurred over fairly short periods of time (e.g. Crews *et al.*, 1995), leaving their findings, in temporal terms, open to question. More studies over extended periods of time (e.g., Myles *et al.*, 1996a, b; Simpson & Myles, 1995b) are, therefore, warranted.

In sum, FC proponents must be encouraged to subject their claims to further scientific verification, the claims of anecdotal evidence notwithstanding. If any small part of FC is to ever be found effective or even plausible, it is abundantly clear that only by careful use of controlled experimental methods will this be established.

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