

A Case Study in the Misrepresentation of Applied Behavior Analysis in Autism: The Gernsbacher Lectures

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I know that most men, including those at ease with problems of the greatest complexity, can seldom accept the simplest and most obvious truth if it be such as would oblige them to admit the falsity of conclusions which they have proudly taught to others, and which they have woven, thread by thread, into the fabrics of their life. (Tolstoy, 1894)

This article presents a case study in the misrepresentation of applied behavior analysis for autism based on Morton Ann Gernsbacher's presentation of a lecture titled "The Science of Autism: Beyond the Myths and Misconceptions." Her misrepresentations involve the characterization of applied behavior analysis, descriptions of practice guidelines, reviews of the treatment literature, presentations of the clinical trials research, and conclusions about those trials (e.g., children's improvements are due to development, not applied behavior analysis). The article also reviews applied behavior analysis' professional endorsements and research support, and addresses issues in professional conduct. It ends by noting the deleterious effects that misrepresenting any research on autism (e.g., biological, developmental, behavioral) have on our understanding and treating it in a transdisciplinary context.

Key words: autism, applied behavior analysis, misrepresentation, research methodology, ethics

This manuscript is unconventional. I did not write it for publication, but for students at the University of Kansas (KU), colleagues and acquaintances on and off campus, families of children with autism,¹

I thank many colleagues for indulging my many questions about autism and its treatment and for their constructive comments on the manuscript's earlier drafts. I acknowledge them by including their fine work in my reference section.

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¹ According to the American Psychiatric Association's (2000) *Diagnostic and Statistical Manual of Mental Disorders*, autism is a neurodevelopmental disorder whose core features are impairments in communication (e.g., lack of spoken language) and social interactions (e.g., lack of social or emotional reciprocity) and restricted, repetitive, and stereotyped patterns of behavior, interests, or activities (e.g., rituals, self-stimulation) (p. 75). These features are often associated with other conditions that vary from severe to mild within and across individuals (e.g., mental retardation, chronic aberrant behavior). Autism also falls within

and ultimately for those children. I also wrote it for myself, both as a professional and as a person. Professionally, I was obliged to respond to recent misrepresentations of applied behavior analysis in autism. Personally, I was aggravated enough that I thought that writing the manuscript might prove cathartic. In the end, though, the catharsis was more intellectual than emotional. I learned a great deal about autism research and treatment, and am now better able to address their misrepresentation. This sense of intellectual satisfaction, however, did not fully overcome my aggravation, but so be it.

INTRODUCTION

At the invitation of KU's Department of Psychology, Morton Ann Gernsbacher (University of Wisconsin) gave its Fern Forman Lecture on September 27, 2007. It was titled

the broader diagnosis of the autism spectrum disorders, which include autism, Asperger syndrome, and pervasive developmental disorder not otherwise specified.

“The Science of Autism: Beyond the Myths and Misconceptions.” Gernsbacher is an award-winning educator, a well-funded and well-published researcher, and the 2006–2007 president of the Association for Psychological Science (APS). Her research is on cognitive mechanisms hypothesized to underlie language comprehension (e.g., Traxler & Gernsbacher, 2006). When her son, Drew, was diagnosed with autism at the age of 2 years in the spring of 1998, she became “motivated by personal passion” to address autism, too, in particular, why children with autism do not speak (www.Gernsbacherlab.org). Since then, she has become an active researcher and professional speaker in this and related areas, as well as a public advocate for the rights of individuals with autism (e.g., Dawson, Mottron, & Gernsbacher, 2008; Gernsbacher, 2007a, 2007b; Gernsbacher, Sauer, Geye, Schweigert, & Goldsmith, 2008). At KU, her lecture (a paid public lecture) filled a 990-seat on-campus auditorium largely, it appeared, with students earning course credit. In addition, it was simulcast to 200 more students and community members at KU’s Edwards Campus in Kansas City. For the record, Gernsbacher had given four previous invited lectures by the same title at (a) a September, 2005, colloquium at Washington University, (b) the August, 2006, conference on Brain Development and Learning: Making Sense of the Science (Vancouver, British Columbia, Canada), (c) the February, 2007, meeting of the Southeastern Psychological Association, as a William James Distinguished Lecturer (New Orleans), and (d) the April, 2007, John S. Kendall Lecture Series at Gustavus Adolphus College (St. Peter, Minnesota).

In her lecture, Gernsbacher addressed several assumptions about autism’s diagnosis and etiology, for instance, that it is epidemic (Maugh, 1999); that it was once caused by emotionally cold “refrigerator moth-

ers” (Bettleheim, 1967); and that it is today caused by childhood measles-mumps-rubella vaccinations (Kirby, 2005). Emphasizing the importance of rigorous research methods and experimental designs, she concluded from her review of the literature, some of it her own research, that these assumptions were myths and misconceptions (see, e.g., Gernsbacher, Dawson, & Goldsmith, 2005; Gernsbacher, Dissanayake, et al., 2005). In the final section of her lecture, she addressed autism intervention and therapy, specifically the assumption that applied behavior analysis is an effective treatment. Before addressing her review of this literature and her conclusions, though, I put applied behavior analysis in a broader disciplinary framework and then in a local and historical context. This material is intended, in part, as a scholarly resource, so it is a tad academic.

Applied Behavior Analysis

Applied behavior analysis is more than intervention and therapy. It is a subdiscipline of the field of behavior analysis (J. Moore & Cooper, 2003; see *The Behavior Analyst*; www.abainternational.org; www.behavior.org). The field comprises (a) a natural science of behavior (i.e., basic behavioral principles and processes; e.g., reinforcement, shaping; see Catania, 2007; *Journal of the Experimental Analysis of Behavior*), (b) related conceptual commitments (i.e., philosophy of science; e.g., naturalism, empiricism; see J. Moore, 2008; *The Behavior Analyst*), and (c) applied research on problems of societal importance and means for ameliorating them (Cooper, Heron, & Heward, 2007; *Journal of Applied Behavior Analysis* [*JABA*]; *Behavior Analysis in Practice*). For concise overviews, see Michael (1985) and Reese (1986).

Although applied behavior analysis arose at several U.S. and Canadian sites in the late 1950s and early

1960s (Kazdin, 1978), its first institutional base was KU's Department of Human Development and Family Life (established 1965), now the Department of Applied Behavioral Science (ABS; established 2004). This is where ABA's flagship journal (*JABA*) was founded (Wolf, 1993), the subdiscipline's basic dimensions were first articulated (Baer, Wolf, & Risley, 1968), and some of its earliest innovative programs of research were undertaken. These include the Juniper Gardens Children's Project for youth, school, and community development (Hall, Schiefelbusch, Greenwood, & Hoyt, 2006) and Achievement Place for juvenile offenders (i.e., the Teaching Family Model; Wolf, Kirigin, Fixsen, Blase, & Braukmann, 1995), both of them in collaboration with the Bureau of Child Research, now the Schiefelbusch Institute for Life Span Studies (Schiefelbusch & Schroeder, 2006; see Baer, 1993a; Goodall, 1972).²

Applied behavior analysis involves an integration of research and application, including use-inspired basic research (i.e., basic research in the interests of application; e.g., stimulus control of stereotyped behavior; Doughty, Anderson, Doughty, Williams, & Saunders, 2007), discovery research (i.e., research on unplanned findings; e.g., on the overjustification effect; Roane, Fisher, & McDonough, 2003), and translational research (i.e., the translation of basic research into practice; e.g., reinforcer

magnitude and delay; Lerman, Addison, & Kodak, 2006). In the main, however, ABA addresses atypical behavior (e.g., stereotypy; Reeve, Reeve, Townsend, & Poulson, 2007), methods for its assessment and analysis (e.g., functional assessment and analysis; R. H. Thompson & Iwata, 2007), behavior-change procedures (e.g., desensitization for phobias; Ricciardi, Luiselli, & Camare, 2006), packages of behavior-change procedures (e.g., self-management; peer-mediated treatments; Stahmer & Schreibman, 1992), and comprehensive programs of treatment (e.g., early intensive behavioral interventions; T. Smith, Groen, & Wynn, 2000).

Applied behavior analysis also ranges across several domains (Luiselli, Russo, Christian, & Wilczynski, 2008), for instance, (a) from individual procedures for specific behavior to comprehensive programs for problems in daily living (e.g., Iwata, Zarcone, Vollmer, & Smith, 1994; McClannahan & Krantz, 1994), (b) from inpatient to on-site service delivery (e.g., Hagopian, Fisher, Sullivan, Acquisto, & LeBlanc, 1998; Nordquist & Wahler, 1973), and (c) from staff training to organizational behavioral management (e.g., McClannahan & Krantz, 1993; J. W. Moore & Fisher, 2007; Sturmey, 2008; see Cuvo & Vallenga, 2007). Finally, the field's interventions are, ideally, research, too, in that clinical decisions are data based (e.g., when to alter or amend them). In fact, the ethical guidelines of the Behavior Analysis Certification Board® (BACB) require data-based decision making (see Bailey & Burch, 2005, pp. 104–106, 212–214).

Gernsbacher's Review and Conclusions

Gernsbacher did not review all the applied behavior-analytic research in autism. That would have been too great a task. Over 750 articles were published between 1960 and 1995 (DeMyer, Hingtgen, &

² As for my potential conflicts of interest, I am the ABS department chairperson. However, although I have published applied research and reviews (e.g., Altus & Morris, 2004; Atwater & Morris, 1988; Morris & Braukmann, 1987) and am a Board-Certified Behavior Analyst, I am not an applied behavior analyst. My interests lie largely in history and theory (e.g., Morris, 1992, 2003; Morris, Altus, & Smith, 2005). As a result, I am not deeply attuned to applied behavior analysis's every nuance in science and practice, especially in autism, so I apologize to my applied colleagues if I am clumsy or insensitive in representing their field.

Jackson, 1981; Matson, Benavidez, Compton, Paclawskyj, & Baglio, 1996) and hundreds more since then. They appear in *JABA*, other applied behavioral science journals (e.g., *Behavioral Interventions*), and journals in related fields (e.g., *American Journal on Mental Retardation*, *Journal of Consulting and Clinical Psychology*). What Gernsbacher reviewed was a subset of the comprehensive programs for early intensive behavioral interventions (ABA-EIBI) that she referred to as “the Lovaas-style of behavioral treatment.”³ Based on her review, she concluded that the effectiveness of applied behavior analysis for autism was another myth and misconception and that the gains made during treatment were due to the children’s “development,” not to ABA-EIBI.

These conclusions upset some audience members. A parent of an adolescent with autism, for whom applied behavior analysis had dramatically improved their lives, asked me what he should use instead. An ABS major bemoaned that her course of study was apparently for naught. A faculty member criticized Gerns-

bacher for overlooking the extensive literature on which Lovaas-style ABA-EIBI is based. This criticism, though, was not fully justified. Gernsbacher had to be selective in her review, given the size of the literature, the breadth of her audience, and the interests of time.

As for my reaction to her conclusions, I was stunned. However, I was stunned not so much by her conclusions per se. I had heard them before in antiscience rhetoric about autism’s etiology and treatment, as well as in sentiment against applied behavior analysis in general (e.g., Meyer & Evans, 1993; www.AutCom.org; www.autistics.org; see “Is ABA the Only Way?” at <http://www.autismnz.org.nz/articlesDetail.php?id=23>; contra. Baer, 2005; Eikeseth, 2001; Green, 1999; J. E. Jacobson, Foxx, & Mulick, 2004; Leaf, McEachin, & Taubman, 2008; Lovaas, 2002, pp. 287–407; T. Thompson, 2007a, pp. 187–203; in general, see Offit, 2008).

Sentiment against applied behavior analysis is not, of course, necessarily antiscience. No matter what Gernsbacher’s sentiments may be, her achievements are anything but antiscience. What stunned me, then, was how she reached her conclusions: She inaccurately represented research reviews, wrongly characterized applied behavior-analytic interventions, misleadingly appealed to history, inaccurately conveyed research designs, selectively omitted research results, and incorrectly interpreted intervention outcomes. Although misrepresentations are often only a minor nuisance in science, they can have harmful consequences, which I believe hers did (and do), both locally and more broadly.

The local consequences included misinforming KU’s community members about ABA-EIBI; hundreds of KU students about a science of behavior and its application; current and prospective ABS majors about a course of study at KU (and careers); and KU staff, faculty, and adminis-

³Equating applied behavior analysis with any one intervention, for example, with Lovaas-style ABA-EIBI or, more narrowly, with discrete-trial training (DTT), is a conceptual error. Lovaas’s is just one of several ABA-EIBI programs, of applied behavior-analytic programs in general, and of programs based in the science of behavior (Luiselli et al., 2008; T. Thompson, 2007a, pp. 43–46; see, e.g., Koegel & Koegel, 2006; Schreibman, 2000; Strain, McGee, & Kohler, 2001). In fact, the number of applied behavior-analytic programs is huge, limited only by the permutations on the number of basic behavioral principles (e.g., reinforcement, stimulus control), behavioral processes (e.g., chaining, shaping), behavior-change procedures (e.g., activity schedules), and packages of behavior-change procedures (e.g., verbal behavior interventions), all constrained, of course, by ethical considerations (see Green, 1999; Hayes, Hayes, Moore, & Ghezzi, 1994). Finally, although Lovaas-style ABA-EIBI is today’s best evidence-based treatment for autism, it may not be the best treatment. That remains an empirical question. It is also not likely the last best treatment. Science, both basic and applied, is a process; it evolves (see T. Thompson, 2008).

trators about scholarship in a department renowned for its research in applied behavior analysis. The broader consequences include Gernsbacher's probable influence on behavioral, social, and cognitive scientists who teach, conduct research, and provide services in autism; funding agencies and foundations who set priorities and allocate resources for autism research and applications; and state and federal agencies that set standards for autism services and funding. She has standing and stature in most, if not all, of these venues: in APS, of course, but also in the American Association for the Advancement of Science (AAAS), where she is a psychology section member at large, and in the National Science Foundation (NSF), where she is on the Advisory Committee for the Social, Behavioral, and Economic Sciences. Although Gernsbacher surely gained these highly respected positions by conducting first-rate science, the hallmarks of her science were largely absent in this section of her lecture.

In Response

In what follows, I respond to Gernsbacher's misrepresentations, but remain agnostic, yet curious, about their source or sources. No matter what, though, misrepresentations remain misrepresentations. In addressing them, I reproduce this section of her lecture below,⁴ inserting bracketed

material to provide context and continuity. Then, where they occur, I address the misrepresentations. For the sake of brevity, such as it is, I restrict my comments to her lecture and note her ABA-EIBI-related publications only in passing (e.g., M. Dawson et al., 2008; Gernsbacher, 2003). As a result, I do not address important issues in autism research and application that she did not cover, for instance, the incomplete reporting of treatment variables in research (Lechago & Carr, in press; see Kazdin & Nock, 2003), among them, therapist competence (Shook & Favell, 1996), treatment intensity (Graff, Green, & Libby, 1998), and treatment fidelity or integrity (Wolery & Garfinkle, 2002). I also set aside the literatures on treatment effects on brain structure (G. Dawson, 2008; T. Thompson, 2007b), autism recovery and its mechanisms (Helt et al., 2008), and ABA-EIBI's long-term costs and benefits (Chasson, Harris, & Neely, 2007; J. W. Jacobson & Mulick, 2000).

My response may give offense to Gernsbacher, but none is intended. I am concerned about scientific communication and reasoning, not about a person or persons. Indeed, my comments are made in the spirit of the behavior-analytic maxim: "The organism is always right." It is not always right, of course, in a moral or factual sense, but it is "right" in the sense that behavior is a lawful subject matter for a science unto its own. In that science, behavior is a function of the organism's biology, its environment, and the history of their transactions in which organisms become

⁴The text was transcribed from KU's Instructional Development and Support's digital recording of Gernsbacher's lecture for KU's Department of Psychology. The section on applied behavior analysis runs from about the 48- to the 55-min mark. I acquired a URL of it from David S. Holmes, a KU professor of psychology, who introduced Gernsbacher. When I asked him if I could forward it to students and friends, he responded, "You can distribute the URL to anyone who is interested. In fact, I want to encourage you to do that as widely as possible" (D. S. Holmes, personal e-mail communication, November 27, 2007). The URL is <http://merlin.cc.ku.edu:8080/asxgen/ids/holmes/autismlecture.wmv>. As for

Holmes's perspective on ABA-EIBI, his introductory psychology review of it is dated (Holmes, 2008, pp. 368–370); it associates ABA-EIBI with aversive control that has not been used in decades; and it is not supported by any citations to any literature. Given its content, though, it is seemingly based on Lovaas et al. (1973), Lovaas (1987), and articles on the late 1980s aversives controversy in behavior analysis (e.g., Johnston, 1991; Sherman, 1991).

individuals.⁵ Unfortunately, English grammar is not neutral in this matter. Its agent-action syntax implicates organisms as the agents of their actions (Hineline, 1980, 2003). As a result, in acquiring English, we acquire a philosophy of mind woven thread-by-thread unconsciously into the fabric of our lives. This philosophy is both inimical to a science of behavior qua behavior (e.g., mind-body dualism; Koestler, 1967; C. R. Rogers & Skinner, 1956) and a basis for counter-Enlightenment, postmodern critiques of it (e.g., humanistic, revelatory; Krutch, 1954; Rand, 1982). Its press (that science's press) is worse than that for evolution in Kansas (Frank, 2004). This syntax may also make my comments appear ad hominem and bereft of compassion for Gernsbacher as a parent of a child with autism. Where this occurs, I apologize (see Skinner, 1972, 1975). ABA-EIBI's critics are always right, too.

AUTISM INTERVENTION AND THERAPY

I now turn to Gernbacher's lecture. I begin where she began on autism intervention and therapy:

Finally, since I'm starting to talk about intervention and therapy, I am going to go to the last section of my talk and that is the empirical evidence for claims such as this: "There is little doubt that early intervention based on the principles and practices of applied behavior analysis can produce large, comprehensive, lasting, and meaningful improvements in many important domains for a large proportion of children with autism." As you might know, the author is referring to what is known as the Lovaas-style of behavioral treatment for autistic children.

⁵I do not mean to perpetuate the nature-nurture dichotomy, that is, the false dichotomy between nature and nurture as independent variables, even if they putatively interact. Among the best contemporary alternatives to the dichotomy is developmental systems theory (Gottlieb, 1998; D. S. Moore, 2001; Oyama, 2000; see Midgley & Morris, 1992; Schneider, 2003, 2007).

At this point, I offer a seemingly trivial observation, for which I beg the reader's indulgence. As I noted, I am curious about the sources of Gernsbacher's misrepresentations. One means of discerning them is to address them all, no matter how seemingly innocuous, to see if any patterns emerge. I begin with first instances.

Improvements in Children with Autism

The quotation above about "improvements ... for a large proportion of children" was taken out of context. Its author, Gina Green (1996), qualified it in her next sentence: "For some, those improvements can amount to ... completely normal intellectual, social, academic, communicative, and adaptive functioning" (p. 38). "Some" children is not "a large proportion of children." Quoting material out of context is not inherently misleading, of course. Moreover, Gernsbacher could not quote ad infinitum; she had to be selective. In any event, the consequence was probably negligible because ABA-EIBI's effectiveness has been overstated by some of its advocates, too (Green, 1999; Herbert, Sharp, & Gaudiano, 2002). Many critics of these overstatements, however, also support ABA, as in, "ABA is one of the most—if not the most—promising interventions for childhood autism" (Herbert & Brandsma, 2001, p. 49). For an overview of applied behavior analysis in autism, see Harris and Weiss (2007).

Lovaas-Style ABA-EIBI Treatment for Autistic Children

The first ABA research on children with autism was published in 1964 by Wolf, Risley, and Mees.⁶ The first systematic report of Lovaas-style

⁶DeMyer and Ferster (1962) were arguably the first to apply the principles of operant conditioning to the socially important behav-

ABA-EIBI was published in 1973 by Lovaas, Koegel, Simmons, and Long. The first report of a comprehensive ABA-EIBI program was published in 1985 by Fenske, Zaleski, Krantz, and McClannahan. And, the first clinical trial of Lovaas-style ABA-EIBI was published in 1987 by Lovaas (see also Celiberti, Alessandri, Fong, & Weiss, 1993; Maurice, Green, & Luce, 1996).

In that trial, the experimental group ($n = 19$; chronological age = 2 years 11 months) received 2 years of 40 hr per week of one-on-one in-home ABA-EIBI from their parents and staff members from the UCLA Young Autism Project. The primary control group was a treatment comparison control group ($n = 19$; chronological age = 3 years 5 months) that received fewer than 10 hr per week of ABA-EIBI plus community treatment (e.g., special education). This controlled for maturational effects—or what Gernsbacher called “development”—over the course of the study; any such effects would presumably have been the same in both groups. A matched secondary control group ($n = 21$; chronological age = 3 years 6 months) was drawn largely from the same population and received community treatment. This controlled for selection bias and permitted a comparison between ABA-EIBI and treatment as usual (Freeman, Ritvo, Needleman, & Yokota, 1985).

Lovaas (1987) did not randomly assign his participants to the experimental and control groups, as he had planned, because of “parent protest and ethical considerations” (p. 4; Lovaas, 2002, pp. 388–389). Instead, he assigned them on the basis of staff

availability for the experimental group. This is an accepted practice in clinical research, especially if the treatment and control groups can be matched a priori or are equivalent on pretreatment measures (Baer, 1993b; Eikeseth, 2001; Kazdin, 1992). In Lovaas’s case, his groups were statistically equivalent on 19 of 20 pretreatment measures, among them, their IQs, which were 53 and 46, respectively (McEachin, Smith, & Lovaas, 1993). After treatment, the experimental group had significantly higher IQs than the control groups (83 vs. 52 and 58) and a significantly higher probability of passing first grade in regular education classrooms (9 of 19 vs. 1 of 40). The 9 participants who passed first grade had a mean IQ of 107 and were considered to be “recovered.” In a follow-up study, the experimental group was found to have maintained these and other gains (e.g., in adaptive behavior; McEachin et al.).

In describing Lovaas-style ABA-EIBI, Gernsbacher continued, “as illustrated in the intro to this 1980s film.” The film was *Behavioral Treatment of Autistic Children* (E. Anderson, Aller, & Lovaas, 1988), which reviewed and followed up on Lovaas et al. (1973) and Lovaas (1987). Its 15-s introduction showed a therapist and a child sitting at a table across from each other engaged in DTT. DTT is one of many technologies that has evolved from ABA research (T. Smith, 2001; Tarbox & Najdowski, 2008), but none of them is meant to be applied in a cookie-cutter fashion. Ideally, applications are individualized, taking into account developmental and individual differences (Schreibman, 2000), as well as differences in families and settings (on values, see e.g., Wolf, 1978).

DTT ranges along a continuum from more to less structured trials and from massed to distributed trials. Highly structured and massed DTT may consist of a therapist’s request or

ior of children with autism, but they failed to address so many of the defining dimensions of applied behavior analysis (e.g., behavioral, analytic, and technological; see Baer et al., 1968) that it probably does not warrant being called applied behavior analysis.

instruction (e.g., to imitate a vocal or nonvocal model), a child's response (e.g., imitation), and a therapist's consequence (e.g., "yes," "no," hugs). The film's introduction shows the end of one such trial, in which the therapist says, "Oh, good boy; that's good" and leans in for a kiss. In the next trial, the therapist says "Sit up; get doll a drink," the child gives the doll a drink, and the therapist says the child's name and "very nice." In the next trial, the therapist says "Kiss doll," but the child again gives the doll a drink, and the therapist says "No, kiss doll," which ends that trial and begins another.

When possible, DTT moves from more to less structure and from massed to distributed trials, that is, to those that are more naturalistic (e.g., incidental teaching; see Allen & Cowan, 2008). Incidental teaching is also an applied behavior-analytic technology (Hart & Risley, 1975; see McGee, Krantz, & McClannahan, 1985), as well as DTT: Therapists set toys aside, children request them, and therapists provide them if requested correctly (or else are prompted). Structured and massed DTT is used to build the basic linguistic, social, and academic repertoires necessary for moving to less structured, more distributed DTT, which then builds repertoires necessary for functioning more fully in everyday life (e.g., functional communication, social reciprocity, and self-guidance; Leaf & McEachin, 1999; Lovaas, 1981, 2002; T. Smith, 2001). Where ABA-EIBI begins on this continuum and how quickly it moves toward more naturalistic procedures depend on children's developmental and individual differences and their rates of progress (see R. R. Anderson, Taras, & Cannon, 1996), not on developmental norms and theories, the latter of which remain largely unfounded.⁷

⁷In observing that therapists sometimes draw eclectically from the behavior-analytic and developmental perspectives, Lovaas

As for the film, Gernsbacher could not have played its full 43 min. She had to be selective again. However, the segment she played was not representative. It showed only structured, massed DTT, not the children later in social play and conversation as teenagers with peers without autism (and indistinguishable from them). In Gernsbacher's defense, no 15-s segment could have fairly represented the film. Thus, any such segment would merit a disclaimer, but none was provided. She continued,

I truly cannot underestimate how much attention this style of intervention has received. As just one metric, the *Clinical Practice Guideline*, distributed by the New York State Department of Public Health recommends that virtually no other intervention be con-

(1981) pointed out that important differences between them need to be recognized because, "each involves certain risks that can be assumed to affect a student's progress. The behavioral approach runs the risk of failing to teach prerequisite behaviors in its concerns with teaching age-appropriate skills as rapidly as possible. In defense of the behavioral approach, it may be argued that this problem is picked up when the data show the student's lack of progress; attempts are then made to determine what additional behaviors need to be taught and to teach them. The developmental approach involves a much more serious risk. In attempting to stimulate maturational changes indirectly through procedures of often dubious scientific validity, it runs the risk of spending so much time on prerequisite behaviors (or 'readiness' skills) that age-appropriate behaviors are never taught, nor do they emerge spontaneously. 'Developing,' in the sense of acquiring new behaviors without direct instruction, is the thing that developmentally disabled children are least able to do, whether 'stimulated' or not. Further, the lack of socially significant progress may not be noticed and addressed because the developmental position does not include a strong emphasis on data-based decision making" (p. 225). Lovaas ended on an ecumenical note, though. He expected that the education of "developmentally disabled persons" would become more effective because "A blending of developmental and behavioral educational approaches, at least to some degree, seems likely, with the strengths of each approach contributing to an integrated curriculum" (p. 233).

ducted with young autistic children except for that one style of intervention [ABA-EIBI] because other interventions like speech therapy or physical therapy would take precious time away from the necessary treatment supposedly needed for that style of intervention. But what do the data show? Are there, as stated on the Surgeon General's Web site, "thirty years of research" demonstrating "the efficacy of applied behavioral methods in reducing inappropriate behavior and in increasing communication, learning, and appropriate social behavior"? [Gernsbacher, 2003, p. 20; the quoted material is from the U.S. Surgeon General's Web site: www.surgeongeneral.gov/library/mentalhealth/chapter3/sec6.html]

The New York State Department of Health Clinical Practice Guideline

In mentioning only the New York State Department of Health's (not Public Health's) (NYSDH, 1999a, 1999b, 1999c) *Clinical Practice Guideline (Guideline)*⁸ and the U.S. Surgeon General's Mental Health Report (1999), Gernsbacher omitted ABA-EIBI's endorsement by other academies, institutes, and councils at the time of her lecture, among them, the American Academy of Pediatrics (2001), the National Institute of Mental Health (2007), the National Research Council (2001), California's Collaborative Work Group on Autism Spectrum Disorders (1997), Maine's Task Force Report for Administrators of Services for Children with Disabilities (1999), and other state reports

⁸For some background on the NYSDH *Guideline*, here is part of its preface: "In 1996, a multi-year effort was initiated by the New York State Department of Health (DOH) to develop clinical practice guidelines to support the efforts of the statewide Early Intervention Program. As lead agency for the Early Intervention Program in New York State, the DOH is committed to ensuring that the Early Intervention Program provides consistent, high-quality, cost-effective, and appropriate services that result in measurable outcomes for eligible children and their families. This guideline is a tool to help assure that infants and young children with disabilities receive early intervention services consistent with their individual needs, resources, priorities, and the concerns of their families" (NYSDH, 1999a, p. xi).

and guidelines (e.g., Alaska, Vermont). Again, time constraints may have kept her from mentioning these, which was fair, as long as her omissions were not systematically biased.

The claims. Turning to her claim that the NYSDH recommended that "virtually no other intervention be conducted with young autistic children except for that one style of intervention [ABA-EIBI]," I could not find this in the *Guideline*. So, perhaps it was an interpretation. For instance, although applied behavior analysis was just one of seven "experiential approaches" the NYSDH reviewed, it was the only one that was recommended as a primary treatment. This was not, however, a recommendation for Lovaas-style ABA-EIBI. The NYSDH (1999b) recommended only that the "principles of applied behavior analysis and behavior intervention strategies be included as important elements in any intervention program for young children with autism" (p. 33).

As for the claim that the NYSDH recommended that no other interventions be conducted because they "would take precious time away from the necessary treatment supposedly needed for [ABA-EIBI]," this was similar to Gernsbacher's (2003) assertion that the *Guideline* recommended that "some interventions not even be included in a child's therapeutic program because those interventions might take time away from an intervention that had been scientifically proven" (p. 20). Not only did I fail to find this in the *Guideline*, but the *Guideline* contradicts it. It notes that applied behavior analysis "may also incorporate some elements of other approaches, such as developmental and cognitive approaches" (NYSDH, 1999a, chap. 4, p. 14) and cites this as an advantage (p. 24), although some advocates of ABA-EIBI and treatment efficacy would disagree because those approaches generally lack empirical

support (e.g., Green, 1996; Lilienfeld, 2007).

The closest the NYSDH (1999a) comes to Gernsbacher's claim is in describing another experiential approach: the developmental, individual difference, relationship (DIR) model also known as floor time (chap. 4, pp. 55–70). DIR seeks to alleviate the symptoms of autism as a psychiatric disorder by enhancing affective parent–child relations through child-led play and interactive motor, sensory, and spatial activities, taking the children's developmental level into account. In particular, it recommends that therapists and parents spend six to ten 20- to 30-min sessions per day on the floor “working on the child's ability for affective-based interactions” (NYSDH, 1999c, p. 153). DIR, however, seems little more than a program of intensive free-operant differential reinforcement of desired behaviors through successive approximations (i.e., shaping), along with some incidental teaching. The NYSDH, however, found no empirical support for it in the only study published at the time (a chart-review study; Greenspan & Wieder, 1997) and thus did not recommend it as a primary treatment. Furthermore, the NYSDH (1999a) cautioned that DIR “may interfere with an intensive behavioral educational program unless steps are taken to coordinate the two” and that, being intensive itself, DIR “may take time away from interventions that have been shown to be effective” (chap. 4, p. 56). These cautions were not admonitions against using ABA-EIBI.

If the source of Gernsbacher's claim was not in the *Guideline*, then it presumably lay elsewhere. In her 2003 article, she attributed the following to Behavior Analysts, Inc.: “Diverting attention, even for a brief period of time, away from treatment methods that have been scientifically proven to be effective is a disservice and can have serious consequences” (p. 20; see www.behavioranalysis.org/

level2/EvaluatingTreatmentEffectiveness.htm). Behavior Analysts, Inc., however, was silent about ABA-EIBI; it was only offering a general precaution. So, too, was Green (1996), in arguing for using the most effective treatments (ABA-EIBI or not) as opposed to less effective or ineffective ones. Lilienfeld (2007) refers to the harm caused by the latter as “opportunity costs.” These include “lost time and the energy and the effort expended in seeking out interventions that are not beneficial” (p. 57), to which the benefits lost by delaying treatment need to be added.

As for Gernsbacher's claim that the NYSDH recommended against “speech therapy or physical therapy,” I also could not find this in the *Guideline*. Moreover, Behavior Analysts, Inc. recommends otherwise. Its answer to a frequently asked question (“How does speech therapy fit into your approach?”) was this: “Our program supervisors determine when speech (or other) therapy would benefit the child and make the appropriate referral. In fact, we offer speech therapy at some of our centers and clinics.” The book in which Green's (1996) chapter appeared also contradicts the claim: It contains a chapter on how to incorporate speech-language therapy into applied behavior analysis (Parker, 1996). As T. Thompson (2007a) has noted,

An experienced speech therapist can be invaluable in developing effective treatment methods that should be used by all therapists and teachers as well as the child's parents. ... Many children with ASD have subtle perceptual-motor coordination problems, which can be addressed by occupational therapists. (pp. 42–43; see also Koenig & Gerenser, 2006)

By this, Thompson meant therapists who provide evidence-based treatments that are integrated with ABA-EIBI, not empirically unsupported pull-out services.

This is all I could find about the source of Gernsbacher's claim that “virtually no other intervention [than

ABA-EIBI] be conducted.” If a source does exist, she should have cited it and then distinguished between quoting from it and providing an interpretation of it, so that the audience could have responded effectively to her claim. She continued,

[What do the data show?] Well, to answer that question, we can go back to the New York State *Guideline* books because, in formulating their guidelines, they conducted a thorough literature review. They found 232 articles that reported using behavioral and educational approaches in children with autism and these articles were systematically screened and five articles reporting four studies were found that met established criteria. So, of the 232 articles, they found in their exhaustive literature review, only five articles met their own standards [see also Gernsbacher, 2003, p. 20]. And, these are the people who believe that this [ABA-EIBI] is a very scientifically supported intervention.

Gernsbacher’s description of the NYSDH’s literature review elided so many details that it misrepresented the ABA-EIBI research. The NYSDH’s (1999a) goal was to “identify relevant scientific articles that might contain evidence about intervention methods for young children with autism” (Appendix B, p. 3; see Noyes-Grosser et al., 2005). To identify them, its reviewers searched the 1980–1998 MEDLINE, PsychINFO, and ERIC databases under autism, infantile autism, and autistic children and read the abstracts of all the articles for those “that might contain evidence about intervention” and then obtained those articles. These were the 232 articles the NYSDH screened in its search of reports of original data on intensive behavioral treatment (see below).

Several consequences arise from eliding these and other details. First, in asking, “What do the data show?” Gernsbacher was asking, rhetorically, what the 232 articles that reported “using behavioral and educational approaches” showed about “the efficacy of applied behavioral methods.” This implied that the 232 articles were applied behavior-analytic arti-

cles, but this misrepresented the *Guideline* on three counts: (a) The keywords in the NYSDH’s (1999a) search were “behavior therapy, behavior modification, psychotherapy, psychoanalytic therapy, psychotherapeutic techniques, instructional programs, and special education” (Appendix B, pp. 4–5). Psychoanalytic therapy is not applied behavior analysis. (b) Not all the 232 articles reported using behavioral and educational approaches. Many of them were descriptions of interventions, literature reviews, theoretical articles, and commentaries and critiques. (c) Of the behavior-analytic reports of research, most of them used within-subject replication (single-subject) designs to evaluate the effects of individual interventions for discrete behaviors (e.g., MacDuff, Krantz, & McClannahan, 1993). These were not ABA-EIBI or the comprehensive programs of research the NYSDH was selecting for.

Second, the claim that only five of the 232 articles “met established criteria” for ABA-EIBI confused the criteria. Of the 232 articles the NYSDH screened, a subset of “articles meeting criteria” (NYSDH, 1999a, Appendix B, p. 4) “was selected for more in-depth review if [they] appeared to contain original data about [a] ... treatment method for autism” (NYSDH, 1999a, chap. 1, p. 9). The articles also had to meet “general criteria” (e.g., include participant age; NYSDH, 1999a, chap. 1, p. 16) and “additional criteria” (e.g., evaluate functional outcomes; NYSDH, 1999a, chap. 1, p. 17). Among these articles, those that reported intensive behavioral and educational programs had to “involve [the] systematic use of behavioral teaching techniques and intervention procedures, intensive direct instruction by the therapist, and extensive parent training and support” (NYSDH, 1999c, p. 229).

Given these criteria, eight of the 232 articles were selected for in-

depth review, all of them control-group studies. These were Birnbrauer and Leach (1993), Koegel, Bimbela, and Schreibman (1996), Layton (1988), Lovaas (1987), McEachin et al. (1993), Ozonoff and Cathcart (1998), Sheinkopf and Siegel (1998), and T. Smith, Eikeseth, Klevstrand, and Lovaas (1997) (see NYSDH, 1999c, p. 57). From these, the NYSDH selected the articles that provided evidence for efficacy on the basis of several methodological criteria (e.g., controlled trials; NYSDH, 1999a, Appendix B, p. 4; 1999c, p. 229). These were the five articles reporting four studies that met what Gernsbacher referred to as the “established criteria,” all of them Lovaas-style ABA-EIBI studies: Birnbrauer and Leach (1993), Lovaas (1987), McEachin et al. (1993), Sheinkopf and Siegel (1998), and T. Smith et al. (1997) (see NYSDH, 1999a, chap. 4, pp. 17–21; Appendix 7, pp. 7–11). Thus, in the end, four of the seven studies (67%) the NYSDH reviewed in depth and four of the four (100%) ABA-EIBI studies met its criteria for efficacy, not five out of the 232 (2.2%), as implied. In eliding the distinctions among what the NYSDH searched and screened and the “articles meeting criteria” for in-depth review and those that met the criteria for efficacy, Gernsbacher misrepresented the quantity and quality of the ABA-EIBI research and the efficacy of applied behavior-analytic treatment overall. She continued,

However, as even the New York State *Guideline* notes [what follows is a quotation from the *Guideline*], “None of the four studies that met criteria for efficacy used random assignment of the children to the groups, such as to the group receiving intensive behavioral intervention versus the group receiving a comparison intervention” (see NYSDH, 1999a, chap. 4, p. 22). And, I believe everyone who has studied behavioral research realizes how absolutely critical it is to randomly assign participants to the treatment versus the control. For example, I could say, “Ah, I’m going to give out new iPhones tonight and I’m

going to do it, you know, randomly. In fact, I’m going to give the first ten people sitting right over there [my iPhones.]” I think those of you up there [in the balcony] would get a little miffed, right? [She paused for the answer, “Yes.”] I would, too. Random assignment is absolutely critical. It is what enables you to draw scientifically supported conclusions.

Random assignment is indeed important, a point I address shortly, but first I note that Gernsbacher’s claim that none of the four studies met what she called the NYSDH’s “established criteria,” “own standards,” or “criterion for efficacy” was misleading. The four studies did meet the NYSDH’s criteria for assigning participants to groups because the NYSDH had two criteria: The studies had to “assign subjects to groups either randomly *or* [italics added] using a method that did not appear to significantly bias the results” (NYSDH, 1999a, chap. 1, p. 17; 1999c, p. 199; e.g., Lovaas, 1987). The studies thus met the NYSDH’s either-or criterion and thus its criteria overall.

Misrepresenting ABA-EIBI Research I

Gernsbacher continued,

But, of the four studies that were mentioned from this review, the first two weren’t even experiments [Sheinkopf & Siegel, 1998; T. Smith et al., 1997]. In fact, they were just record reports, where we go back in time and we say, “This person has a 4.0. Let’s see if she ate pasta every night her freshman year.”

The claim that Sheinkopf and Siegel (1998) and T. Smith et al. (1997) “weren’t even experiments” and “were just record reports” misrepresented them, but then much depends on the meaning of “experiment.” It differs across the sciences. In the social sciences, control-group designs compare (a) the effects of a condition for one group of participants to (b) its absence (or another condition) for another group, after which the statistical significance of any differences in their correlated outcomes is inferred. In the natural sciences, within-subject

and within-group replication designs are more the norm (T. Thompson, 1984). In these, experimental conditions are systematically applied, removed, and replicated within individuals or groups, with the differences between them displayed in graphs (on the greater use of graphs in “harder” vs. “softer” psychology, see L. D. Smith, Best, Stubbs, Archibald, & Roberson-Nay, 2002). This is also the applied behavior-analytic approach (Johnston & Pennypacker, 2009; Sidman, 1960), which is increasingly appreciated in clinical psychology (Barlow & Nock, 2009; Borckardt et al., 2008). For its use in autism research, see Wacker, Berg, and Harding (2008). I am not taking sides in this matter, just noting that *experiment* has a range of meanings.

In any event, although Sheinkopf and Siegel (1998) and T. Smith et al. (1997) were not planned experiments, they were not “just record reports” of a relation between treatment and its outcome. They were record reports that used treatment comparison control groups, another point Gernsbacher omitted. Sheinkopf and Siegel, for instance, found 11 children in a longitudinal study of autism whose parents had provided 19 hr per week of Lovaas-style ABA-EIBI. The authors then formed a matched treatment comparison control group from the same study; its participants had been provided 11 hr per week of treatment as usual (i.e., school-based interventions). Over the course of 18 to 20 months, the experimental group made a significant 25-point gain in IQ over the control group and had a significant reduction in symptom severity. See Lovaas (2002, pp. 399–400), however, for a critique of the study. As for T. Smith et al., they created an experimental group and a treatment comparison control group of preschool children with mental retardation and pervasive developmental disorder on the basis of records at the UCLA project and other sites. The experimental group

($n = 11$) had received 30 hr of Lovaas-style ABA-EIBI per week, while the treatment comparison control group had received 10 or fewer hours per week. In the 2 to 3 years between intake and follow-up, the experimental group made a significant 12-point gain in IQ and a significant gain in expressive speech over the control group. Gernsbacher continued,

The other two studies were experiments [Birnbauer & Leach, 1993; Lovaas, 1987; McEachin et al., 1993], but they didn’t include the critical piece of random assignment. Instead, the participants were assigned to either the treatment or the control group by factors such as who lived closer, whose parents wanted them to be in the treatment group, who could pay for some of the treatment, et cetera, et cetera.

As for Gernsbacher’s claims about participant assignment, first, her claim that children were assigned on the basis of “who lived closer” was presumably a rewording of who lived too far away, but this rarely occurred. Lovaas (1987) assigned only 2 of his 38 children to the control group “because they lived further away from UCLA than a 1-hr drive, which made sufficient staffing unavailable to those clients” (p. 4) And, although Birnbauer and Leach (1993) excluded three families because they “lived too far away” (p. 64), the families were excluded from both the experimental and the control groups. Second, her claim that children were assigned on the basis of “whose parents wanted them to be in the treatment group” was presumably a rewording of “parent protest,” but is not true. This would have yielded groups that likely differed in parental involvement in treatment (e.g., effort, motivation), which is why the children were assigned on the basis of therapist availability. Third, I found nothing to support the claim that children were assigned on the basis of “who could pay for some of the treatment.” Rewording, overstating, and misstat-

ing research methodology are bound to misrepresent it.

As for the findings of these studies, I have already reviewed Lovaas (1987) and McEachin et al. (1993) and so here only describe Birnbrauer and Leach (1993). They provided 19 hr per week of ABA-EIBI to 9 children with autism and pervasive developmental disorder; the control group was comprised of 5 children who received unknown treatment. Although the groups were similar at pretreatment, the experimental group made more gains after 2 years than the control group on standardized and descriptive measures of intelligence, language, personality, and adaptive functioning. However, no statistical analyses were conducted.

For pre-2000 applied behavior-analytic research Gernsbacher did not review, see S. R. Anderson, Avery, DiPietro, Edwards, and Christian (1987), Fenske et al. (1985), Handleman, Harris, Celierti, Lilleheht, and Tomchek (1991), Harris, Handleman, Gordon, Kristoff, and Fuentes (1991), Harris, Handleman, Kristoff, Bass, and Gordon (1990), Hoyson, Jamieson, and Strain (1984), Perry, Cohen, and DeCarlo (1995), and Weiss (1999). For literature reviews, see S. J. Rogers (1998) and Matson et al. (1996).

Experimental Control

Gernsbacher continued, "Well, the New York State Guideline says it's been argued that the [nonrandom] method for group assignment probably did not bias the results [NYSDH, 1999a, chap. 4, p. 22; see Gernsbacher, 2003, p. 21]." The *Guideline* did not argue that nonrandom assignment will not bias results. It only described the outcome of nonrandom assignment in these studies: "In all cases the authors analyzed the pretreatment ... data to see if the groups were equivalent in important variables. Most of the authors concluded that such analyses found no system-

atic bias in the assignment of subjects to the intervention or comparison group" (NYSDH, 1999a, chap. 4, p. 22). Furthermore, the NYSDH (1999a) noted that "all studies showed similar and consistent results" (chap. 4, p. 24). This does not mean that no biases existed, only that no (or few) biases were found among the important variables; that is, the variables were balanced across groups.

Other critics have also noted the possibility of bias on pretreatment measures, as well as the use of nonequivalent pretest–posttest measures and weak assessment measures (e.g., Foxx, 1993; Gresham & MacMillan, 1997; Kazdin, 1993; Munday, 1993; Schopler, Short, & Mezibov, 1989). This is not perfect science. These criticisms, though, have been subject to counter-criticisms (e.g., changes in pretest–posttest language skills, for instance, may require different measures; Eikeseth, 2001; Lovaas, 1993; Lovaas, Smith, & McEachin, 1989; McEachin et al., 1993; T. Smith & Lovaas, 1997; T. Smith, McEachin, & Lovaas, 1993), the counter-criticisms to counter-counter-criticisms (e.g., Gresham & MacMillan, 1998) and the counter-counter-criticisms to counter-counter-counter-criticisms (e.g., Lovaas, 2002, pp. 387–407)—science red in tooth and claw.

Nevertheless, Gernsbacher's criticism of the foregoing studies for not using random assignment has obvious merit. However, it is not as straightforward as it seems (S. J. Rogers & Vismara, 2008, p. 30). First, although the American Psychological Association (APA, 2002a) states that "Randomized controlled experiments ... are the most effective way to rule out threats to internal validity in a single experiment" (p. 1054), it notes that the experiments remain subject to threats of external and construct validity and need replication.

Second, random assignment is but one component of randomized con-

trolled trials (RCTs). The gold standard requires double-blind (or triple-blind) placebo control groups in which the experimenters, participants, therapists, evaluators, and statisticians do not know which participants are assigned to which group. Even then, it does not guarantee that statistically significant treatments are clinically significant.

Third, even when random assignment is planned for or used, practical problems ensue. (a) The treatment's intensity often makes it discriminable from control groups, which allows families to distinguish ABA-EIBI from other treatments (J. E. Jacobson, 2000). (b) Parents will protest the random assignment of their children to experimental and control groups, and withdraw them from research if assigned to the latter. (c) Given the empirical evidence for ABA-EIBI, institutional review boards and parents will balk at the ethics of assigning children to control groups. And (d), because of ABA-EIBI's intensity, experimental groups are often so small that random assignment can, by chance, create groups unbalanced on variables that are critical to the outcome (e.g., language, age, IQ; see Reichow & Wolery, in press).

Fourth, the claim that random assignment is absolutely critical may be overly conservative if, failing that standard, public health initiatives are delayed and treatments are withheld at irreversible risk to individuals and populations. For instance, given the standard of random assignment, no proof exists that smoking causes lung cancer in humans, yet a convergence of evidence was sufficient for the Surgeon General to take action regarding it.

In the end, scientific conclusions are supported by a range and convergence of methods, with logically permissible conclusions nested hierarchically within them (see T. Smith et al., 2006). Among the methods, randomization is a means for assign-

ing participants to groups, not an end in itself. It does not guarantee unbiased assignment, except in the long run. Presumably, methods such as Lovaas's, could assign participants in an unbiased manner. Bias is an empirical matter (Baer, 1993b).

Appeals to History

Referring to what the NYSDH (1999a) argued about group assignment, Gernsbacher continued, "My academic great-grandfather [Wilhelm Wundt] would be rolling over in his grave." (In her introduction, she mentioned tracing her academic lineage back to Wundt who is "typically credited with establishing the first experimental psychology laboratory, and who therefore earned the status of father of experimental psychology"; see Boring, 1950, pp. 316–347.)

Appeals to history can be perilous. First, in this case, most doctorates of psychology can trace their lineage back to either Wundt (1832–1920) or William James (1842–1910), so Gernsbacher's appeal to Wundt was rhetorical, not scholarly. Second, using history to justify apparently winning traditions (e.g., cognitivism), as opposed to apparently losing traditions (e.g., behaviorism), is a breach of historiographical method called presentism (Samelson, 1974; Stocking, 1965; see Furumoto, 1989). Third, citing Wundt on participant assignment was misleading. Although he likely knew of John Stuart Mill's (1843) "method of differences," he was not expert in group designs. His research was mainly case studies of individuals who reported their introspectively observed experiences (e.g., mental elements and processes; Wundt, 1874/1904), studies that do not meet the standards of within-individual replication designs (e.g., Kennedy, 2005; Sidman, 1960). Moreover, his participants were a highly trained, nonrandom sample of the adult population. Wundt's research program died for methodo-

logical reasons: often poor reliability within studies and, more often, poor replicability across laboratories (Boring, 1950).

Research Review: Separating Fact from Fiction

Gernsbacher continued,

And, in fact, [Wundt] would probably have drawn the same conclusions as those drawn in an article titled, "Separating Fact from Fiction in the Etiology and Treatment of Autism" [Herbert et al., 2002]. This article states that "Methodological weaknesses of the existing studies severely limit the conclusion that can be drawn about their efficacy." [p. 35; see Gernsbacher, 2003, p. 21]

This quotation from Herbert et al. (2002) inaccurately portrayed their conclusions about ABA-EIBI. First, they addressed ABA-EIBI in a section titled "Promising Treatments for Autism" (pp. 33–38), in which ABA-EIBI was a "fact," not "fiction." Second, although ABA-EIBI research has mainly used nonrandom assignment, Herbert et al. concluded that "the intervention programs ... are based on sound theories, are supported by at least some controlled research, and clearly warrant further investigation" (p. 33). Third, after reviewing the ABA-EIBI research, Herbert et al. wrote, "Taken together, the literature on ABA programs clearly suggest that such interventions are promising" (p. 35). Gernsbacher, however, quoted the next sentence as their conclusion: "Methodological weaknesses of the existing studies [however] severely limit the conclusions that can be drawn [about] their efficacy." Fourth, although Herbert et al. admonished the proponents of ABA-EIBI for their uncritical advocacy, they concluded, "Clearly, ABA does not possess most of the features of pseudoscience that typify many of the highly dubious treatments for autism. ABA programs are based on well-established theories of learning and emphasize the value of scientific methods in

evaluating treatment effects" (p. 35; for critiques of pseudoscience in autism, see J. E. Jacobson et al., 2004; Offit, 2008).

Evidence for the Other Experiential Approaches

Although the NYSDH (1999a, 1999b, 1999c) and Herbert et al. (2002) noted limitations in the ABA-EIBI research, they also pointed out that the treatment was evidence based, which was more than they said of the other approaches, none of which they recommended as primary interventions. Among those the *Guideline* reviewed were DIR, sensory integration therapy, touch therapy, auditory integration therapy, facilitated communication (FC), and medical and diet therapies. Herbert et al. addressed these and other approaches under "Questionable Treatments for Autism": sensory motor therapies (e.g., FC, sensory integration training); psychotherapies (e.g., psychoanalysis, holding therapy), and biological treatments (e.g., secretin, gluten- and casein-free diets, Vitamin B6). Of these, the NYSDH (1999a) and Herbert et al. were most critical of FC (see Biklen, 1990, 1993). As Herbert et al. described it,

Facilitated communication (FC) is a method designed to assist individuals with autism and related disabilities to communicate through the use of a typewriter, keyboard, or similar device. The technique involves a trained "facilitator" holding the disabled person's hand, arm, or shoulder while the latter apparently types messages on the keyboard device. The basic rationale behind FC is that persons with autism suffer from a neurological impairment called *apraxia*, which interferes with purposeful motoric functioning. (p. 28; see also NYSDH, 1999a, chap. 4, p. 64; 1999b, p. 43)

In its literature search, the NYSDH (1999a) screened 11 FC articles, none of which met its criteria for an in-depth review (NYSDH, 1999c, p. 245; see also Herbert et al., pp. 27–28). Of FC, the NYSDH (1999c) commented,

In studies of facilitated communication used in older children with autism, the messages typed by the children are often far beyond their capabilities as evidenced by their behavior or language. Studies of facilitated communication suggest that communication that exceeds baseline levels for a subject originates from the facilitator rather than the child. Use of facilitated communication has brought up a number of ethical and legal issues. There have been cases where messages produced with facilitated communication have caused emotional distress to parents or have led to accusations of abuse that resulted in legal proceedings [see also Herbert et al., pp. 28, 38; and the Public Broadcasting Service's *Frontline* report at video.google.com/videoplay?docid=3439467496200920717]. Recommendations: Because of the lack of evidence for efficacy and possible harms of using facilitated communication, it is strongly recommended that facilitated communication not be used as an intervention method in young children with autism. (p. 160; see also the American Academy of Pediatrics, 2001; APA's 1994 resolution on FC at <http://www.apa.org/divisions/div33/fcpolicy.html>; J. W. Jacobson, Mulick, & Schwartz, 1995; Lilienfeld, 2007; Offit, 2008, pp. 6–13)

In critiquing FC, Herbert et al. properly distinguished it from augmentative and alternative forms of communication (e.g., keyboards and picture exchange systems; see Bondy & Frost, 1994; Reichle, York, & Sigafos, 1991). Children with autism often benefit from such technologies and may need hands-on help in mastering them, but the content of their communication is their own, not the facilitators'.

Misrepresenting the ABA-EIBI Research II

Gernsbacher continued,

However, skip ahead to 2007 and there are now two studies of Lovaas-style ABA intervention that did employ the ever so important random assignment [Sallows & Graupner, 2005; T. Smith, Groen, & Wynn, 2000]. And, you're probably curious: What do those studies show? In one study [T. Smith, Groen, & Wynn], there was a slight but nonsignificant advantage for the autistic children. [Gernsbacher presented two figures of treatment gains graphed from intake to follow-up for expressive and receptive language. The lines in the

figures were labeled the "ABA" and "Control."]⁹

T. Smith, Groen, and Wynn (2000). This was the first independent replication of Lovaas's (1987) ABA-EIBI study. Gernsbacher's description of it, though, contained serious omissions and misrepresentations. First, her claim that "there was a slight but nonsignificant advantage for the autistic children" has two meanings, neither of them accurate. (a) The reference to "the autistic children" was seemingly to the experimental group, which implies that the control group was composed of children without autism, but it was not. Both groups were drawn from the same population. (b) The reference might have been to the autism subgroups in the experimental and control groups whose gains were less than those of the pervasive developmental disorder subgroup, but they were still greater (and more frequently greater) than those of the control group.

Second, the control group in her figures was a treatment comparison control group, not a no-treatment control group, which the audience members (and I) had expected and which her conclusion would require: that the children's gains were due to their development. This does not mean that T. Smith, Groen, and Wynn's (2000) methods were impeccable; they were not (e.g., they used one-tailed instead of two-tailed tests of significance; M. A. Gernsbacher, personal communication, December 1, 2007). However, critical reviews should describe research accurately enough that audiences can draw correct conclusions about them. For the record, T. Smith, Groen, and Wynn's control group was composed

⁹I thank Gernsbacher for sending me the figures she constructed from T. Smith, Groen, and Wynn's (2000) and from Sallows and Graupner's (2005) results and for her additional comments on the ABA-EIBI research (M. A. Gernsbacher, personal communication, December 1, 2007).

of children (a) whose parents were trained and supported in providing ABA-EIBI for 5 hr per week for 3 to 9 months, (b) whose parents were asked to provide this treatment for an additional 5 hr per week during those months, and (c) who were enrolled in 10 to 15 hr per week of special education. The experimental group received about 25 hr of ABA-EIBI per week for 2 to 3 years.

Third, in claiming that “there was a slight but nonsignificant advantage for the autistic children,” Gernsbacher selectively reported T. Smith, Groen, and Wynn’s (2000) findings. She reported only the nonsignificant differences between the experimental and the control groups on the measures of expressive and receptive language, omitting the differences that favored the experimental group: (a) significantly higher IQs (e.g., 16 vs. 0 points) and Merrill-Palmer developmental ages (+42.7 vs. +27.3), (b) significantly less restrictive school placements, and (c) higher academic achievement scores (75.7 vs. 58.0 on the Wechsler Individual Achievement Test).

Fourth, she failed to note that both the experimental and the control groups made gains from intake to follow-up. The former made gains on seven of the nine standardized measures (e.g., >300% on the Merrill-Palmer), and the latter made gains on four of them (e.g., >200% on the Merrill-Palmer). The findings, however, were not subjected to statistical analysis, so we do not know if they were significant. Even if they were, without a no-treatment control group, we would not know if the gains were due to treatment or development.

Gernsbacher’s omission of T. Smith, Groen, and Wynn’s (2000) significant findings was not due to her lack of familiarity with them. When the study was published, she read it carefully enough to find an error in its calculation of the statistical differences in the two language measures. The calculation was

wrong; there were no differences (T. Smith, personal e-mail communication, October 5, 2007; see Errata, 2001). She was also familiar enough with the findings to report the significant results in her 2003 article. In her lecture, she continued,

“In the other study [Sallows & Graupner, 2005], there was a slight but nonsignificant advantage for the control children. And this was after 40 hours a week of a minimum of 2 years of intensive therapy, which is a bit depressing.” (For this, Gernsbacher presented figures, labels, and measures that were the same as T. Smith, Groen, & Wynn’s, 2000, except for the data, of course.)

Sallows and Graupner (2005). This study was the second independent replication of Lovaas (1987), albeit a partial replication because the intervention included other treatments (e.g., pivotal response training; Koegel & Koegel, 2006). Here, Gernsbacher’s description contained the same omissions and misrepresentations. First, what she and Sallows and Graupner called a “control group” was not a no-treatment control group but rather a treatment comparison control group. It was composed of children whose parents chose the number of ABA-EIBI hours they received each week (31 to 32 hr), but who had less in-home staff supervision than the experimental group. The latter received 37 to 39 hr per week of ABA-EIBI, not much more than the control group. Second, the claim that “there was a slight but nonsignificant advantage for the control children” was more false than true. On the one hand, the control group had slight but nonsignificant advantages in expressive and receptive language and four other outcomes. On the other hand, the experimental group had slight but nonsignificant advantages on seven outcomes. Thus, the experimental group had a seven to six advantage across the outcomes. Third, Gernsbacher selectively reported Sallows and Graupner’s findings: She failed

to note that, when combined, the ABA-EIBI experimental and control groups made significant gains on eight of the 13 posttreatment measures (e.g., 25 IQ points). Again, though, without a no-treatment control group, the gains could have been due to development.

Summary. In summarizing T. Smith, Groen, and Wynn's (2000) and Sallows and Graupner's (2005) findings, Gernsbacher said, "One study showed a non-significant advantage to the treatment [T. Smith, Groen, & Wynn, 2000], but the other study showed a nonsignificant advantage to the control group [Sallows & Graupner, 2005], meaning it's a wash." It's a wash" misrepresented the studies in ways just described. It also dismissed other post-1999 ABA-EIBI studies, albeit none of them randomized controlled trials. These include control-group studies (e.g., Howard, Sparkman, Cohen, Green, & Stanislaw, 2005), two of which used Lovaas-style ABA-EIBI (e.g., Cohen, Amerine-Dickens, & Smith, 2006; Eikeseth, Smith, Jahr, & Eldevick, 2002, 2007); pretest–posttest group comparisons (e.g., Stahmer & Ingersoll, 2004); single-subject studies (e.g., Green, Brennan, & Fein, 2002), one of which used Lovaas's methods (i.e., T. Smith, Buch, & Gamby, 2000); case studies (e.g., Butter, Mulick, & Metz, 2006); and retrospective analyses (e.g., Boyd & Corley, 2001; Luiselli, Cannon, Ellis, & Sisson, 2000), at least two of which used Lovaas's methods (i.e., Bibby, Eikeseth, Martin, Mudford, & Reeves, 2001; Eldevik, Eikeseth, Jahr, & Smith, 2006). For comparisons of ABA-EIBI to community-based treatments, see Cohen et al. (2006), Eikeseth et al. (2002, 2007), and Howard et al. (2005), some of whose control groups (e.g., time-intensive eclectic, public-school-based interventions) regressed on many outcome measures. Notwithstanding the appeal of individualized over more standardized interven-

tions, the eclectic application of non-evidence-based treatments needs to be questioned (S. J. Rogers & Vismara, 2008).

For post-1999 reviews of the literature, see Campbell (2003), Eikeseth (2009), Goldstein (2002), Helt et al. (2008), Horner, Carr, Strain, Todd, and Reed (2002), McConnell (2002), Odom et al. (2003), Odom and Strain (2002), Reichow and Wolery (in press), S. J. Rogers and Vismara (2008), and Schreibman (2000). Helt et al. concluded this about the effects of interventions on the likelihood of recovery from autism:

Almost no controlled studies directly compare outcomes between behavioral vs. other therapies (e.g., developmental stimulation, Denver developmental model, "floor time") or with "biomedical" treatments. Therefore no definitive statements can be made about which treatments can produce recovery in the greatest number of children. However, although it cannot be stated categorically that behavioral intervention is necessary for recovery, the majority of the studies that report actual recovery used behavioral techniques, alone or in combination with other therapies, for some or all of the children, and therapies that include behavioral methods are the most empirically validated. In addition to the well-described learning principles that govern behavior therapy, competent behavioral therapy requires a highly affective, emotionally positive set of interactions that promote the reward value of social interactions and more or less continuous social engagement, especially in very young children. (p. 350)

Those learning principles may also explain why effective therapy works, whether it is behavioral or not. The latter may work for behavioral reasons without our knowing it.

Treatment or Development

[In concluding her review of Smith, Groen, and Wynn (2000) and Sallows and Graupner (2005), Gernsbacher noted that the studies were not really a wash]: But, you know, I'm an optimist by nature, in addition to being a curious person, and I like to look at these data a little differently [i.e., a Sallows and Graupner figure]. And when I look at these data, I say, you know what, these data might not show us that that particular style of early

intervention is dramatically more effective than control [usually a less intensive version of the same treatment, but often not much less]. But these data show us something very important and that is that autistic children develop. In fact, it doesn't appear from these data to matter whether they're in the control group or the intensive treatment group. Autistic children develop. And you see that pattern in these data as well [i.e., one of her T. Smith, Groen, & Wynn (2000) figures].

Gernsbacher's claim that the children's gains were due to their development was empirically and logically unfounded. For it to be true, the control groups would have to have been no-treatment control groups that had made similar gains, but no such groups existed, although, by implication, she suggested they did. Gernsbacher's claim was also inconsistent with the methodological rigor she promoted in her lecture. Critical reviews that emphasize methodological standards ought not violate their own standards. This calls into question not only their logic but also their integrity and impartiality. Unfounded conclusions may also imply consequences that do not necessarily follow, as in this case.

First, if development explained the children's gains, then Gernsbacher's audience should have assumed that children with autism needed no treatment at all. This, however, overlooks individual differences in diagnosis and developmental outcome. In autism, most outcomes are gravely suboptimal (Johnson, Myers, & the Council on Children with Disabilities, American Academy of Pediatrics, 2007; T. Smith, 1999; Volkmar, Lord, Bailey, Schultz, & Klin, 2004), and the need for treatment is obvious (Green, 1996; Myers, Johnson, & the Council on Children with Disabilities, American Academy of Pediatrics, 2007). In fact, if left untreated, few children with autism spontaneously recover (Helt et al., 2008). Most of them require intensive services and, as adults, institutionalization, both of which are ultimately more expensive than ABA-EIBI (Bilstedt,

Gillberg, & Gillberg, 2005; Howlin, 2005; Howlin, Goode, Hutton, & Rutter, 2004). Second, if development rather than ABA-EIBI explained the children's gains, then the audience should have assumed that children with autism do not respond to ABA-EIBI. In study after study, however, ABA-EIBI demonstrates significant gains. Third, if development explained the children's gains, then the audience should have assumed that the gains were genetically programmed and unalterable by any intervention, but this is neither supported by the literature (T. Thompson, 2007a) nor consistent with recent advances in developmental science (e.g., G. Dawson, 2008; see D. S. Moore, 2001; Oyama, 2000). Gernsbacher continued,

So, lastly, let me just leave you with the question of how can we, as parents and teachers and society and members of this lovely audience tonight, how can we foster that development, even through adulthood? And, how can we improve the lives of all autistic citizens? And for that message, I want to turn to a brief video that my son has made that I think you are going to enjoy and probably will not mind staying another six-ish minutes [actually, about 10 minutes], so let me get that going.

Written with intelligence and creativity, the video made the important point that individual differences are not necessarily deficits. Agreed. When the video ended, Gernsbacher concluded,

And, given how late it is, I think I will just put up the last slide that gives you places to go if you want any more of my work [e.g., www.gernsbacherlab.org] and also places to go if you want to see any more of my son's films, which is his YouTube account [psych.wisc.edu/lang/drew/New/drew.html].¹⁰ Thank you so much. You have been a great audience tonight. I appreciate it. [She allowed no time for questions.]

¹⁰ Presumably posing as "DeeDeeMom," Gernsbacher has posted images from Drew's video on YouTube (www.youtube.com/profile?user=DeeDeeMom). See also "D's Autism Society Presentation, November 2006," in which Drew, I presume, then age

DISCUSSION

In summary, Gernsbacher (a) drew methodologically unfounded conclusions about the outcome of ABA-EIBI research, (b) described nonsignificant findings to the exclusion of significant ones, (c) failed to identify control groups as other than no-treatment control groups, (d) mischaracterized a research review as wholly critical of ABA-EIBI, (e) appealed to psychology's history on misleading points, (f) described participant assignment criteria that did not exist, and (g) made unfounded claims about professional guidelines. She also omitted significant material throughout. These misrepresentations and omissions increased in frequency and significance over this section of her lecture and were uniformly biased against ABA-EIBI.¹¹ I shall not belabor these points; I have already done that. Instead, I update the literature, address some issues concerning professional conduct, and conclude.

10, offered engaging and sophisticated answers to questions about having autism (www.youtube.com/watch?v=cJK4SvQ3s4A).

¹¹The pattern reflects what cognitive psychologists call a confirmation bias (Evans, Barston, & Pollard, 1983; Lord, Ross, & Lepper, 1979; Mahoney, 1977; Wason, 1960; see also the Tolstoy, 1894, quotation at the beginning of this article). In critical reviews of research, confirmation bias may be found in tendencies to misrepresent the literature in ways that credit or discredit a particular position or sentiment. In the context of the "autism wars," Catherine Maurice (2005a), a mother of two children with autism, commented on this: "I think I was naive about the willingness of people who have a vested interest in something to change their minds. Whether it's a question of income, status in the field, or the fear of saying 'I was wrong,' people just have had a hard time changing their views about anything. For the purveyors of therapeutic nurseries, play therapy, relationship therapy, or any other model on which they had built their reputations, it was just too much to admit that behavioral intervention was actually capable of taking children with autism farther than had ever been possible before" (p. 35).

Further Endorsement and Research

ABA-EIBI is endorsed by many academies, councils, institutes, and agencies, whereas other treatments receive little, if any, support or, like FC, are found to be harmful. In fact, a few weeks after Gernsbacher's lecture, the American Academy of Pediatrics (AAP) published a revision of its 2001 recommendations about the management of children with autism (Myers et al., 2007). In 2001, it referenced the applied behavior-analytic literature, but in 2007, it reviewed the literature and concluded, "The effectiveness of ABA-based intervention in ASDs has been well documented through 5 decades of research using single-subject methodology and in controlled studies of comprehensive early intensive behavioral intervention programs in university and community settings" (p. 1164). In turn, the AAP was critical of "complementary and alternative medicine," including FC, which was "not a valid treatment for ASD" (Myers et al., 2007, p. 1173). As S. J. Rogers and Vismara (2008) point out, according to Chambliss et al.'s (1996, 1998) criteria for identifying empirically validated therapies, Lovaas-style ABA-EIBI is the only "well-established psychosocial intervention for improving the intellectual performance of young children with autism spectrum disorders" (p. 25; see Chambliss & Hollon, 1998).

Overall, ABA-EIBI has subjected itself to far more empirical self-scrutiny in published peer-refereed journals than any other comprehensive program of intervention (Eikeseth, 2009; Helt et al., 2008; Myers et al., 2007; S. J. Rogers & Vismara, 2008). In fact, soon after Gernsbacher's lecture, two more articles provided further support, albeit without random assignment. In England, Remington et al. (2007) compared a treatment group ($n = 23$) that had received 26 hr per week of in-home

ABA-EIBI to a publicly funded treatment comparison control group ($n = 21$). The groups did not differ in pretreatment, but after 2 years, the treatment group had made significant gains over the control group on measures of intelligence (e.g., 24 IQ points), language, daily living skills, and positive social behavior. In Israel, Zachor, Ben-Itzhak, Rabinovich, and Lahat (2007) compared a treatment group ($n = 20$) that received 35 hr per week of center-based ABA (e.g., DTT, incidental teaching), along with speech and occupational therapy, to a matched treatment comparison control group ($n = 19$) that received eclectic center-based treatment (e.g., DIR). After a year, the ABA treatment group had made significant gains on measures of intelligence and core autism deficits (e.g., communicative and social interactions), whereas the control group made only a gain in social interactions, but with a smaller effect size. Research continues apace.¹²

Professional Conduct

Not only did Gernsbacher's lecture have deleterious consequences at KU and in the local community, it also raised questions concerning professional conduct. Professional conduct is, of course, a touchy subject, one on which reasonable people will disagree. Scientists and practitioners, for instance, disagree about the interpretation of research designs,

¹²Magiati, Charman, and Howlin's (2007) postlecture publication reported no advantage for Lovaas-style ABA-EIBI over autism-specific nursery school classrooms, and Solomon, Necheles, Ferch, and Bruckman (2007) reported on the effectiveness of DIR (see Greenspan & Wieder, 2006). However, the poor quality of their research methods, suspect treatment fidelity, and inadequate reporting make any conclusions by Gernsbacher's standards debatable at best (see Eikeseth, 2009; Lloyd, Pullen, Tankersley, & Lloyd, 2006). For research that contradicts Magiati et al., see Cohen et al. (2006), Eikeseth et al. (2002, 2007), and Howard et al. (2005).

methods, results, clinical guidelines, reviews, and applications. Rarely, though, are they harmed by these disagreements in their own and related fields; indeed, the disagreements are often an impetus for further research. However, across the psychological sciences (and outside them), misrepresentation may raise issues regarding professional conduct.

Given that the fundamental ethical dictum in human research and clinical practice is Hippocrates' "First, do no harm," the question arises about whether Gernsbacher's lecture violated that ethic by misrepresenting ABA-EIBI to scholars inside and outside psychology; to students attending her lecture for course credit; and to family and community members possibly looking for clinical advice. In research and practice, psychologists are acutely aware of APA's (2002b) *Ethical Principles of Psychologists and Code of Conduct* because state licensing boards, accrediting organizations, funding agencies, and institutional review boards routinely apply those principles to their work. In the areas of competence, public statements, and teaching, however, we are less likely aware of APA's principles (see Keith-Spiegel, 1994). They are as follows.

Competence. APA's Standard 2.01 on the Boundaries of Competencies states, "Psychologists provide services, teach, and conduct research with populations and in areas only within the boundaries of their competence, based on their education, training, supervised experience, consultation, study, or professional experience" (p. 4). For instance, behavioral, social, and cognitive scientists should be wary of offering advice, pro or con, about clinical treatment to consumers, especially to vulnerable ones, among them the families of children with autism (J. E. Jacobson, 2000; T. Thompson, 2007a, pp. 187–203). In *Ethics in Plain English*, Nagy (2005) elaborates,

Your work must be firmly grounded in established scientific and professional knowledge. Do not make “factual statements” in your classroom, your consulting office, the courtroom, on radio or TV, in print, on the Internet, or anywhere, about psychological matters that go beyond supporting facts, unless you use a disclaimer. Resist the temptation to overgeneralize or oversimplify, regardless of the setting or pressure from others. It’s better that your statements are a little more tentative, but accurate, rather than flashy and flawed. (p. 57)

Few of us likely violate APA’s standards for competence, but we may sometimes engage in the activities Nagy describes: We may overgeneralize and simplify our views, both in criticism and advocacy, on a variety of topics in a variety of venues. How much harm this causes is difficult to judge, but when misrepresentations occur, our colleagues are often quick to point them out (e.g., see Catania, 1991, on Mahoney, 1989; Morris, 1993, on Meyer & Evans, 1993; Wolf, 1991, on Proctor & Weeks, 1990; see Todd & Morris, 1992).

Public statements. APA’s Standard 5.01 on the Avoidance of False or Deceptive Statements states, “Psychologists do not knowingly make public statements that are false, deceptive, or fraudulent concerning their research, practice, or other work activities or those of persons or organizations with which they are affiliated. [These] include but are not limited to ... lectures and public oral presentations” (p. 8). With respect to RCTs, the Consolidated Standards of Reporting Trials note that reporting them inadequately “borders on unethical practice when biased results receive false credibility” (Moher, Schultz, & Altman, 2001, p. 1191). Ethical practices include the “precise reporting of the interventions intended for each group” (p. 1192). Although this standard concerns the submission of RTC manuscripts for publication, the ethical reasoning behind it applies to public statements about research methods in general.

Teaching. APA’s Standard 7.03b on Accuracy in Teaching states, “When engaged in teaching or training, psychologists present psychological information accurately” (p. 10). Fisher (2003) elaborates, “Standard 7.03b reflects the pedagogical obligation of psychologists to share with students their scholarly judgment and expertise along with the right of students to receive an accurate representation of the subject matter enabling them to evaluate where a professor’s views fit within the larger discipline” (p. 138). Nagy (2005) cautions, “Do not exaggerate, minimize, spin, or otherwise distort or bend the facts to suit your opinion or bias” (p. 182; see Keith-Spiegel, Whitley, Balogh, Perkins, & Wittig, 2003). I expect we rarely violate APA’s standard for teaching, but we may sometimes exaggerate or minimize the facts to suit our sentiments.

In “Ethics and the Persuasive Enterprise of Teaching Psychology,” Friedrich and Douglass (1998) speak to this point directly. They argue that we should not only make balanced presentations (Matthews, 1991), but that we should also be aware of how our “instructional persuasion” may affect students’ beliefs. Beliefs are not only a function of research, but also of how we present the research. For students to act effectively on it, we must not only avoid misrepresenting it, but also avoid false assumptions, rhetorical tactics, and insincere “cues” (e.g., perceived but false caring). On this point Friedrich and Douglass recommend,

Not only should instructors help students weigh special bias or opinion in source materials, they should also make available for scrutiny their own positions and opinions when pertinent. The object ... is not to indoctrinate students but rather to encourage them to see the affective, personal dimension to knowledge and to develop in them the habit of critical consideration of all sources. By the same justification, instructors should disclose to students their classroom, or public, persuasion agendas. (pp. 555–556)

Conclusion. Whether or not Gernsbacher's lecture violated APA's ethical standards is open to interpretation. I, myself, take no stance. As the standards for competence are written, the answer is no. As for the standards on public statements, everything depends on "not knowingly," as in not knowingly made false or deceptive statements about research and application. As for the standards on accuracy in teaching, their violation may also depend on "not knowingly," as in not knowingly presented inaccurate information; however, APA does not qualify this standard with "not knowingly."

Sources of Misrepresentation

As for the source or sources of Gernsbacher's misrepresentations, I can only conjecture, having just the form and content of her lecture and Web site (www.gernsbacherlab.org) from which to make inferences about their function. The misrepresentations may have been due to a series of unhappy accidents or poor scholarship, but the latter would have been uncharacteristic of Gernsbacher as a professional.¹³ They may reflect her seemingly dismissive sentiment towards applied behavior analysis, based perhaps in a philosophy of mind that conflicts with behavior analysis. They may also have had

¹³The professional standards of scientists in public advocacy are not always their standards in science (Shermer, 2002). For example, Gernsbacher is associated with autism advocacy groups—the Autism National Committee (AutCom) and autistics.org—whose standards are not scientific by the standards she promoted. For AutCom, she has published an article in its newsletter (Gernsbacher, 2005; see www.autcom.org/pdf/AutcomNLSpring2005.pdf), offered "support" for its 2005 and 2006 conferences (e.g., www.autcom.org/pdf/AutcomNLFall2006.pdf), and given a keynote conference address by the same title as her KU lecture (www.autcom.org/pdf/AutcomNLSpring2006.pdf). In addition, AutCom has posted an anti-DTT memorandum that contains scientifically unsupported assertions (e.g., the outcome of DTT is due to

conscious or unconscious metacognitive origins, for instance, an intent to strengthen a developmental approach to theory, research, and practice in autism by misrepresenting the hold of applied behavior analysis on evi-

development; see www.autcom.org/articles/DTT.html), an article promoting a non-evidence-based treatment (i.e., DIR; see www.autcom.org/articles/Behaviorism.html), and a position paper advocating for a harmful intervention—facilitated communication (www.autcom.org/articles/Poition2.html)—that Gernsbacher has endorsed in a prepublication book commentary (www.reasonable-people.com/advanced-praise.html). AutCom also promotes pseudoscience: One of its officers, Gail Gillingham Wylie, offers a \$1,000 diagnostic and treatment service using a "QXCI Quantum Xeroid Consciousness Interface EPIX/SCIO." This device putatively gathers so-called bioenergetic data from clients to diagnose hundreds of ailments and then treat them, autism included, through feedback, even through subspace, that is, without clients being physically present. This is mere quackery (www.quackwatch.com/01_QuackeryRelatedTopics/Tests/xrroid.html; see Offit, 2008). See Maurice (2005b) for how to distinguish science from pseudoscience in autism treatment and for warning signs of the latter. See Lerman et al. (2008) on using behavior analysis to examine unproven therapies in the context of ABA-EIBI.

As for autistics.org, as of December 10, 2007, it had posted a reprint of Gernsbacher's 2004 article from the *Wisconsin State Journal*—"Autistics Need Acceptance, Not Cure" (see www.autistics.org). This is an important piece about individual rights, empowerment, and social justice, but it obscures the fact that, although children with autism deserve acceptance as individuals, their behavior sometimes does not. It requires treatment, lest it severely limit their freedom for the rest of their lives (Rimland, 1993).

The positions these groups advance are inconsistent with the scientific standards Gernsbacher promoted in her lectures and are ill-suited to an NSF panel member, AAAS representative, and past president of APS, whose mission is "to promote, protect, and advance the interests of scientifically oriented psychology in research, application, teaching, and the improvement of human welfare" (www.psychologicalscience.org/about/). For autism advocacy groups that promote science-based interventions, see the Association for Science in Autism Treatment (www.asatonline.org) and Families for Early Autism Treatment (www.feat.org/); see also, the Cambridge Center for Behavioral Science (www.behavior.org) and Maurice (2005a).

dence-based treatments, especially now that significant competitive funding is finally available. This, though, would have been an error in logic: Weakening one approach does not strengthen the intellectual content of another, but then, audiences do not always reinforce logically.¹⁴

Gernsbacher's misrepresentations may also be related to her personal experiences with her son; decisions about treating his autism; and relations with autism advocacy groups that harbor sentiments against applied behavior analysis and science in general. These may have compromised her objectivity in reporting the ABA-EIBI literature.

This may sound patronizing and *ad hominem*. If so, I apologize; it is not meant to. I am only conjecturing about the personal, social, and cultural sources of ABA-EIBI's misrepresentations, which extend far beyond Gernsbacher's (see Baer, 2005; J. E. Jacobson et al., 2005; Leaf et al., 2008). In some cases, addressing their sources might reduce the probability of future misrepresentations (Morris, 1985; Todd & Morris, 1992). My conjectures notwithstanding, I have deep and abiding professional and personal compassion for the parents of children with autism. They are part of my life. They telephone and e-mail me about services in Kansas, and I fail them as often as not; good services are lacking. They consult with me about how to improve

services, but have had to band together later to found their own school. They work with some of my colleagues, who work with their children, but often on waiting lists that are too long. They are also my neighbors.

CONCLUSION

Gernsbacher concluded by asking how we can foster the development and improve the lives of persons with autism across the life span. Given an agreement on the meaning of *foster* and *improve*, her question can be answered empirically through research in the biological, behavioral, and developmental sciences. It can be answered better, though, through interdisciplinary research across them. Autism is not an essence that lies in any one of their subject matters. It is a product of the transactions among biology, the environment, and behavior that occur over the course of biological and behavioral development and that are unique to each individual (G. Dawson, 2008). Although research in each of these sciences controls for factors in the others, holding those factors constant does not thereby privilege the factors that any one of them investigates. Autism does not exist outside the factors in all of these sciences. As long as research is empirical, discoveries in any one of them will ultimately be consistent with those in the others (Warren, 2002).¹⁵ Finally, given that our understanding of autism and our ability to discover effective treatments for it require transdisciplinary research, misrepresenting any one of the sciences will only impede our overall progress. Gernsbacher is presumably

¹⁴Given Gernsbacher's undisclosed association with antisience advocacy groups, her promotion of FC, and her sentiments against applied behavior analysis, her conclusions about ABA-EIBI may reflect a conflict of interest. APA's Standard 3.06 on Conflict of Interest is this: "Psychologists refrain from taking on a professional role when personal, scientific, professional, legal, financial, or other interests or relationships could reasonably be expected to (1) impair their objectivity, competence, or effectiveness in performing their functions as psychologists or (2) expose the person or organization with whom the professional relationship exists to harm or exploitation" (p. 6).

¹⁵I am surely naive about the ability of data to resolve philosophical and political differences, at least in the short run (see Howard, 1999). A case in point is Project Follow Through, a follow through on the Head Start programs of the early 1960s. Begun in 1968, the project identified and funded 22 different early education programs not just to discover

aware of this. As she noted of a 2006 AAAS symposium she organized and chaired, "With the surge in both scientists and society turning their attention toward autism, there comes responsibility. It behooves us as scientists to distinguish uninformed stereotypes from scientific reality and to move beyond myths and misconceptions" (Gernsbacher, 2006, retrieved December 11, 2007, from www.news.wisc.edu/12198).

It also behooves us to distinguish misrepresentations of ABA-EIBI from scientific reality to foster the transdisciplinary research needed to solve the problem of autism.

I conclude by returning to my opening. I wrote this response to Gernsbacher's KU lecture for a parent who asked me how he could defend the effective use of ABA-EIBI with his son. They can now go fishing together; before, they could not. I wrote it for the ABS major who asked for counterarguments to the misrepresentations of ABA-EIBI so that she could defend her major to her peers in other departments. She is now a research assistant in a program of use-inspired basic research in developmental disabilities. Mainly, though, I wrote it for the families of children with autism and, ultimately, for those children who need and deserve evidence-based treatments, of which ABA-EIBI so far has the best support. Unfortunately, many parents are dissuaded from using it by misinformed, misguided, or misleading advocates of other approaches. As a result, they often use these approaches until they see their chil-

what worked but also to adopt and then fund the programs that did work, not the others. Although the results demonstrated the clear superiority of the two behavior-analytic programs—direct instruction (Englemann & Carnine, 1982) and behavior analysis (Bushell, 1973)—the project funded many models, irrespective of their effectiveness (Carnine, 1983, 1984). For a review of the project and how various stakeholders undermined it, see Watkins (1988, 1997).

dren's poor progress. When they begin using ABA-EIBI to good effect, they speak of their great regret and guilt for not having used it earlier, when their children had the most to gain and the most time to make those gains. The opportunity cost of not using ABA-EIBI, or any equally effective intervention, is that their children will be delayed in achieving their full potential or never achieve it at all. As a result, their children will need more supportive services and institutionalization later into their lives and perhaps for the rest of their lives at significant personal and social costs to them, and financial costs to us all.¹⁶ This is a crime.

POSTSCRIPT

On March 28, 2008, Gernsbacher gave a lecture by the same title as her KU lecture at the Midwest Conference on Professional Psychology in Owatonna, Minnesota, hosted by the Psychology Department at Minnesota State University at Mankato. The conference's goal was "to promote the dissemination of empirically oriented research from a variety of disciplines within the field of psychology" (Society for Teaching of Psychology Discussion List). I was told by three colleagues who independently attended the lecture and who had either read an earlier draft of this manuscript or viewed the URL of Gernsbacher's KU lecture, that, minor changes aside (e.g., labeling the control groups as comparison groups), she misrepresented ABA-EIBI in the same ways she did at KU. Given that I had sent her a draft of this manuscript at least a month before her Mankato lecture, she would have knowingly misrepresent-

¹⁶Gernsbacher has been fortunate to have had choices in addressing her son's developmental course. Most parents of children with autism do not. For the barriers and frustrations they face in seeking evidence-based treatments, see Maurice's *Let Me Hear Your Voice: A Family's Triumph over Autism* (2001; see also Maurice, 2005b; Offit, 2008).

ed ABA-EIBI if she had read it. She also presented a paper by the same title that addressed, in part, “Does ABA cure autism” at the April, 2008, Web Conference Series on Practices to Promote Inclusion for People with Autism Across the Lifespan, sponsored by the Association for the Severely Handicapped. And, in November, 2008, she offered an online course through neurodiversity.com (<http://www.neurodiversity.com/autismbasic.html>) titled “Understanding Autism: Myths and Misconceptions” (<http://144.92.102.54/autism-sample.html>) that examined “approaches to remediating behaviors that are considered autistic” (Gernsbacher, 2004; see <http://www.ls.wisc.edu/L&STODAYv9no2/L&STODAYv9n2p4.pdf>, retrieved February 3, 2009). As of February 3, 2009, however, the link was broken.

To address her continued misrepresentations, I consulted one of her colleagues for advice. She proposed that I invite Gernsbacher to participate in an APA symposium on the evidence for the efficacy of ABA-EIBI. I also consulted one of the senior APS administrators, who agreed. Thus, last summer, I invited Gernsbacher to participate in such a symposium, but she never responded. Finally, I wrote her department chairperson to suggest that he counsel her against continuing to give this lecture, lest a teacher, therapist, or parent file ethics charges against her, but he never acknowledged my concern. At the urging of many colleagues, this was the point at which I submitted this manuscript to *The Behavior Analyst*.¹⁷ I end with an admonition and hope:

Let us pay tribute to the courage of children with autism and their families, as they strive every day to confront the disability with a powerful combination of determination, crea-

tivity, and hope. Let us empower them and respond to their needs today, so as to make our societies more accessible, enabling and empowering for all our children tomorrow. (Ban Ki-Moon, United Nations Secretary General, April 2, 2008, First World Autism Day)

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¹⁷Two of the BACB guidelines concern “statements by others” that also apply here (Bailey & Burch, 2005). Guideline 10.04b:

“Behavior analysts make reasonable efforts to prevent others ... from making deceptive statements concerning behavior analysts’ practices or professional or scientific activities” (p. 195). Guideline 10.04c: “If behavior analysts learn of deceptive statement about their work made by others, behavior analysts make reasonable efforts to correct such statements” (p. 196).

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