Response by Drs. Linda Acredolo and Susan Goodwyn to. . . .

L. H. Nelson, K. R. White, & J. Grewe. Evidence for website claims about the benefits of teaching sign language to infants and toddlers with normal hearing, *Infant and Child Development,* Wiley Online Library, 2012.

OVERVIEW

The authors of this review raise a number of valid issues. Overall, the article is a clarion call for additional research exploring the benefits of teaching sign language to hearing infants. As longtime university researchers, we are always in favor of adding competently conducted research to the body of knowledge.

However, we take strong issue with the conclusion they draw that our own NIH-funded data provide only "fair and non-convincing" support (3 on a 1 – 5 scale) for our conclusion that signing has a positive effect on the verbal development of hearing children. Their conclusion was based on a very narrow reading of our paper which ignored the overarching statistical evidence also provided in the paper that supports our conclusion. Specific points in this regard are addressed in detail below. In addition, we think it is relevant to point out that that our study was carefully critiqued before publication in a highly respected psychological journal by four researchers with extensive expertise in early language development and research methodology.

BIAS: A DANGER FOR ALL RESEARCHERS

One issue the authors raise repeatedly is the danger that "bias" can creep into the analysis of research results. True. However, in this regard "turnabout is fair play." The methodology the authors employed to determine the merit of the research studies they evaluated consisted of each of the authors applying a rating scale (1 = Good, 3 = Fair, 5 = Poor) to the studies, a procedure that is certainly vulnerable to subtle bias should the raters be predisposed to reach a particular conclusion. Even when unintentional, it's all too easy for raters to overlook or downplay certain points in favor of others. In the case of the current article, there are reasons to suspect that the authors (particularly the first two) might have been intentionally or unintentionally biased against finding support for a positive role for sign language in the studies they reviewed. To see why that might have been the case, we need to take a look at the professional backgrounds of Nelson and White.

- Lauri Nelson is an Assistant Professor of Psychology at Utah State who is affiliated with the "Auditory Learning and Spoken Language Program" and whose publications focus on the value of cochlear implants and auditory enhancement systems for Deaf children.

- Karl White is a Professor of Psychology at Utah State whose career has been devoted to early diagnosis of hearing loss and early intervention. He is founder of the "Sound Beginnings" program at Utah State, a program that "provides home and center-based services to children with hearing loss whose families want their children to learn to listen and talk." The emphasis is on auditory enhancement and cochlear implants.

As anyone with any connection to the Deaf Community is well aware, there is currently a strong difference of opinion (one could say "war") as to the role that sign language should play in the development of children with hearing loss. The introduction of cochlear implants and the concern that sign language might impede the development of language skills among children with such implants (or using enhanced auditory systems) has angered many within the Deaf Community who are passionate about the utility of ASL.

In an effort to refute the notion that exposing Deaf babies to signing is detrimental, many within the Deaf Community have pointed to our research with hearing babies precisely because it documents a positive effect of signing on verbal development. (See, for example, the March 2007 issue of *Deaf Life* magazine: https://www.babysigns.com/pdf/DeafLife_BabySignsBoom.pdf .) Given this argument, it makes sense that individuals dedicated to eschewing sign language in favor of promoting aural methods would be motivated to find fault with studies that support a positive role for sign language. In other words, this controversy over appropriate interventions for Deaf children has put our study "in the crosshairs" of those advocating for aural over signed interventions. Simply put, discrediting our research furthers their goals.

Now to address specific issues raised by the authors.

POINT #1:

The size of the movement is *way* out of proportion to the number of scientific studies that have been done.

RESPONSE TO #1

That may be true. However, we have certainly done our part – and done it well (see below) – by contributing a longitudinal study of 103 families that took us over 5 years to complete and required the help of over 100 students, most working in our lab for a full year.

Furthermore, we would argue that the size of the movement *is* in proportion to word-of-mouth "data" attesting to the benefits of signing. In other words, now that signing is so widespread, parents are not being swayed as much by research as by word-of-mouth reports from neighbors and friends who have experienced the benefits of signing "up close and personal." One only has to look to our network of over 1000 instructors for evidence supporting this conclusion. What has motivated practically every instructor to sign on and become passionate about teaching others about signing is not any research but, rather, their own experiences of signing with the babies in their lives.

Of course, in the authors' view, word-of-mouth is the same as "parental reports" and, therefore, highly suspect. We challenge you to tell that to parents who've witnessed how signing has enriched their interactions with their children.

POINT #2

No data exist supporting the claims of social and emotional benefits like reduced frustration, enriched parent-infant relationship, etc.

RESPONSE TO #2

As far as our own claims go, we are confident that they are accurate because from the beginning they have been based on interviews with the signing families in our NIH study conducted during the 15 and 19 month visits to our lab, interviews which were then analyzed using "content analysis," a method Nelson, White, & Grewe use themselves. Because our main concern in that study was documenting effects of signing on verbal development, we omitted those findings from our published papers. Perhaps we should have gone back and remedied the situation, but as parental reports of such benefits began to flood in from the population at large, we didn't feel it necessary to expend the time and energy to do so. There are times when "common sense" is enough: The idea that being better able to understand what your child needs results in less frustration, a better relationship, and feelings of efficacy on the child's part seems self-evident.

Fortunately, Dr. Claire Vallotton from Michigan State University is picking up the slack. In a paper entitled "Infant signs as intervention? Promoting symbolic gestures for preverbal children in low-income families supports responsive parent-child relationships" (*Early Child Research Quarterly*, in press), she documents the positive effects of signing on maternal attunement to children's emotions and responsiveness to distress as evidenced during videotaped interactions, and feelings of satisfaction with the child as evidenced in questionnaires. Moreover, Dr. Vallotton is leading an international group of scholars who are collecting data from parents all over the world about perceived benefits of signing including social and emotional benefits. Of course, once again the data are based on parental reports—a method of data collection that is, according to Nelson, White, and Grewe, highly suspect and is the issue to which we now turn.

POINT #3

A major complaint voiced by Nelson, White, and Grewe concerns our reliance for part of our data on parental reports to gather information about the acquisition of words and signs by infants and toddlers. They argue that parental reports can be biased, thereby artificially affecting the results.

RESPONSE TO #3.

In response, we would like to point out that at least 80% of published research dedicated to the study of language acquisition has relied on parental reports. Why? Seriously, how else are you going to get details about a slowly developing skill like vocabulary development? Which would you rather trust to provide accurate information: words or signs produced within a 15 minute laboratory session with a baby/toddler or the report of someone who spends enormous amounts of time with the child and who has been asked to watch and record the information as it happens? In fact, one of the most respected and widely used measures of vocabulary development, the *MacArthur Communicative Developmental Inventory*, is totally reliant on parent report in the form of a vocabulary checklist. What's more, we didn't only rely on parental reports. We also used standardized laboratory based methods like the "Peabody Picture Vocabulary Test," the "Sequenced Inventory of Communicative Development," and "Mean Length of Utterance" assessments.

POINT #4

The absence of random assignment "means that the selection bias is a strong alternative explanation for the differences in outcomes between groups."

RESPONSE TO #4

Perfectly designed studies are not always feasible in the real world. Our need to eschew random assignment is a case in point. Instead of randomly assigning subjects to the three groups (e.g., 1st family to group A, 2nd family to group B, 3rd to group C, and so on), we found it necessary to complete each group's data collection in turn as if each was a separate study and assuring participant families that that was the case. This choice was necessitated by the fact that we were conducting the research in a small town where too many moms knew each other. At the very beginning we did try random assignment; however, we quickly ran into problems of intergroup communication. A mom in the control group, for example, would learn that her friend was teaching signs to her baby, knowledge which had led her to conclude she was, in fact, in a control group—where she didn't *want* to be: "I'd rather be in the signing group, okay?" You can see the problem. Collecting the data from the groups sequentially helped deal with this problem.

In any study, whether or not subjects are randomly assigned, there is always the possibility that groups will differ at the start of the study, thereby accounting for differences at the end. That's why we followed the standard practice of using statistical analyses to make sure that the groups didn't differ on variables researchers agree are very likely to affect language and cognitive development. Relevant quote from Goodwyn, Acredolo, and Brown, (*Jn. of Nonverbal Behavior*, 2000):

> "*Baseline measures*. To assess the comparability of the groups at the beginning of the study, the three groups (ST, NC, and VT) were compared on a variety of demographic variables and baseline language measures. Demographic variables included sex, birth order, maternal and paternal education (4-pt scale…) and family income (6-pt scale…). The language measures at 11 mos included a maternal report of verbal vocabulary (the MacArthur Communicative Development Inventory, Fenson et al., 1999) and a measure of vocalization frequency during a 15-min play session. No significant differences between the groups were found for any measure (p. 87)."

Of course, we didn't rule out every possible difference, but these are the variables that are recognized to predict more direct factors like the number of books in the home, the likelihood of a child being read to, quality nutrition, exposure to lead, etc. In other words, we followed appropriate, standard practice and are confident that no selection bias occurred. The four language development researchers who reviewed our study for publication agreed.

POINT #5

Differences in language skills between the signing and control group weren't statistically significant at every age tested.

RESPONSE TO #5

1. The authors completely overlooked 4 important analyses reported in our paper, all of which indicated a statistically significant, positive effect on language development when the signing group was compared to the main control group. These analyses include the following:

    a. Comparison of the signing and control groups on a composite, *expressive* language measure (ability to use words to communicate), meaning a measure that statistically combined all the various expressive language measures we used across the 2 years of the study to give an overall view.
    b. Comparison of the signing and control groups on a composite, *receptive* language measure (ability to *understand* words), meaning a measure that statistically combined all the various receptive language measures we used across the 2 years of the study to give an overall view.
    c. Comparison of the signing and control groups on a composite language measure that combined receptive *and* expressive language scores "to provide a 'bottom line' summary of the effects of symbolic gesturing on language development" (Goodwyn, Acredolo & Brown, 2000, p. 97).
    d. Comparison of the number of language measures (out of a total of 17) on which the signing children averaged higher scores than the control children (16 of the 17) to the number that would be expected if signing was irrelevant to language development (50%, or 8.5).

Why are these composite tests appropriate? When the number of subjects in the groups being compared are small, as ours were (32 to 39 is small compared to medical studies that use thousands of subjects), it's hard to reach "statistical significance." Specifically, the difference between the groups has to be much greater than would be necessary with large subject groups. (Actually, the fact that we found "statistically significant" difference in so many cases is a tribute to the power of signing to influence verbal development.) In cases where the number of subjects is small, it's very easy for REAL, meaningful differences between groups on *individual* language measures to approach but not reach the level of "statistically significance." When researchers suspect that there is an effect that is being artificially suppressed due to small sample sizes, the standard practice is to combine measures – as we did. The result was robust in supporting our conclusion that signing has a positive effect on verbal development.

2. They completely overlooked one of our most important, statistically significant findings: The signing children used significantly longer sentences (a measure of grammar development rather than just vocabulary) at 24 months.

POINT #6

We should have spent more time analyzing the data from the second control group, the "Verbal Training Group."

RESPONSE TO #6

We used this group for the purpose for which it was intended, as a way to detect what are called "training effects" – the effect on parents' behavior of simply knowing they are part of an intensive, longitudinal study of their child's language development (e.g., maybe they interact with their children differently because they are part of a language study). The question was whether the advantage we were seeing among the signing families was simply due to "training effects." To find out, we went to the trouble of adding the Verbal Training Group – where parents were treated in almost identical ways to the signing group (minus the signing). We then compared the language results from the Verbal Training Group's children to the results from the children from the main control group and found no differences – indicating that "training effects" were not artificially improving language development. Given this result we were able to argue that signing, and *not* "training effects," were most likely responsible for the fact that the signing children were doing better. Other comparisons were not necessary.

POINT #7

The authors seem to be criticizing our use of strategies to "jog the memories" of parents during the biweekly phone calls we used to track progress in the acquisition of signs and words.

RESPONSE TO #7

As we explain in our report, the "jog" was provided by asking parents standard questions about the context in which a word or sign was used (for example, to label a real, pictorial, toy or other exemplar of the object). Obtaining this information was crucial in order to be sure individual words or signs were being used (1) spontaneously and (2) as *true*, generalized symbols rather than linked to a single item. Children were not given credit for having acquired either a word or sign until these important criteria were met.

Which brings us to the next criticism of our research.

POINT #8

"None of the studies described whether parents and children were using sign language in a functional way in their daily activities…"

RESPONSE TO #8

This was precisely the goal of the questions described above. Was the child producing the sign or word spontaneously (rather than it being elicited or just in imitation)? And what was the child using the sign or word for? What was the context? Was it to label a single item or being used more generally? As the table below from a longer report of our NIH study indicates, we were very conservative in our determination whether or not our subjects were using signs in a truly communicative way.

TABLE 6.1. Criteria for assigning symbolic status to gestures

1. Frequency: The gesture had to be produced repeatedly in reference to conceptually similar objects or events. (Purpose: to distinguish from one-time-only pantomining of salient characteristics or actions.)
2. Form: (a) The physical movement had to be "empty handed" (i.e., not involving an object) and relatively consistent from one time to another"; (b) if a sound was involved, a nonauditory component also had to be clearly discernible. Onomatopoeic sounds occurring on their own (e.g., "rrrr" for truck, "grr" for bear) were treated as potential vocal symbols.
3. Context-flexible usage: The gesture had to be used beyond the specific situation or specific object to which it was initially attached (i.e., multiple exemplars of the concept).
4. Communicative intent: The gesture had to be serving the child as a tool for communicative interactions. Evidence included eye contact, pointing, or use during book reading. (Purpose: to distinguish between gestures used to communicate and gestures used in the context of pretend play.)
5. Noninstrumental: The gesture could not be one that the child might use to achieve a goal directly rather than by communicating with others (e.g., blowing directly on food to cool it off would not qualify, while blowing at a distance might qualify).

POINT #9

"No data were collected on the spontaneous or naturally acquired use of gestures in either control group." We assume the authors are faulting us here for not indicating in our NIH-study report how many signs the control groups developed spontaneously in comparison to the signing group

RESPONSE TO #9

This criticism doesn't make much sense given that the goal of the study was *not* directed to documenting child created signs but rather to determining whether *purposefully* encouraging children to use signs would affect verbal development. This means that the spontaneous development of signs by the control groups (where parents were *not* purposefully encouraging signing) would actually work *against* our hypothesis by decreasing the verbal advantage of the signing babies. Besides, we had previously reported in studies of spontaneous signing by children that the average number to be expected was about 5. Given that the signing group in the NIH study averaged over 20 signs, it's clear that parental modeling and encouragement were effective.

POINT #10

The authors also throw stones at the follow-up to our NIH study in which we used a standard IQ test to retest the children in the two important comparison groups of the original study at age 8—those who had signed as babies and those in the non-intervention group who had not.

RESPONSE TO #10

The results of this comparison indicated a significant advantage in IQ for those who had signed as babies. *We stand by these results and, once again, dispute their criticisms.*

(a) The authors complain about attrition—the fact that, after 8 years, we were unable to find some of the original families. We were disappointed, too, but this is a common problem in long term studies. However, we dealt with this issue in a number of standard ways:
    1) First, we compared the two groups of 8 year olds who we did relocate on demographic factors known to affect IQ to make sure the groups were not significantly different at this later point. There were no differences between the groups in mother's education, father's education, family income, gender, or first vs. later born status.
    2) We also looked at each group separately to see if those we *did* find at age 8 differed from those we *didn't* find on (a) a standard intelligence measure at age 2, (b) the number of signs they had used when babies, and (c)

demographic variables. We found no differences on any of these measures—making us confident that attrition was not affecting our findings.

(b) We have already dealt with the issue of non-random assignment to the groups in the original study. (See Point #4.)

(c) We didn't include the 2nd control group—the Verbal Training group because it had served its purpose (ruling out training effects) in the earlier portion of the study. (See Point #6.) Logically, then, the relevant comparison was between children whose parents had encouraged their babies to sign and parents whose language-relevant behavior toward their babies had not been influenced in any way.

(d) The paper did undergo peer review in order to be accepted for presentation at the International Conference for Infant Studies.

CONCLUSION

As we said at the beginning, Nelson, White & Grewe raise a number of valid issues in their paper, and we always applaud the call for additional research. However, as we hope the discussions above have made clear, we feel the criticisms of our research are not valid and do not justify a description of our studies as only "fair" in quality and "non-convincing" in terms of the support they provide for signing with hearing babies.

Fortunately, we are confident that critiques such as Nelson, White & Grewe's will have little impact on families of hearing babies who are considering using signs. These days all it takes is to view one or more of the many, many parent videos on YouTube showing babies using signs to convince other families to give this easy and enormously rewarding experiment a try.

On the other hand, we do hope that this very lengthy response will bolster the efforts of the Deaf Community to persuade professionals with whom they deal of the effectiveness and importance of sign language for the very youngest among us.