

## Re: Methodological Concerns Suspend Interpretations

The article by Geers et al contains significant methodological issues that should moderate any findings and claims of sign language's role in implanted deaf children's spoken and written English development.

First, the study sample is (understandably) nonrandomized; thus, categorization factors may be related to outcomes. Asserting a causal conclusion from a correlational, nonrandomized study is inappropriate, especially when a simpler explanation may exist: parents of deaf children who are not progressing with their cochlear implant (CI) may be more likely to begin (or continue) signing with their child. This would imply that poor oral outcomes encourage the use of signing rather than the use of signing limiting oral outcomes.

Secondly, although the authors reported no statistically significant differences between groups at baseline, the actual data suggest clinically significant differences that were statistically nonsignificant because of small group sizes. Multilayered and complex variables such as maternal education (69% vs 50%), income <\$50 000 (32% vs 43%), and age of onset (0.3 months versus 1.2 months) are well known to influence language and reading outcomes (it is also unclear if age of onset is actually age of diagnosis). Additionally, auditory perception abilities at baseline were much lower in the group that continued to sign; indeed, the authors recognize that early speech recognition predicts later speech intelligibility. Furthermore, type and frequency of postimplant rehabilitation, an educational experience independent of actual CI benefits, was unaccounted for.

Thirdly, it is unclear how the authors characterized "signing" and "percent of time signing" or whether parents understood how American Sign Language (ASL) differs from other signing systems. Moreover, parents may have differed widely in interpreting and

estimating the "percent" of time using sign at home. Hence this measurement may not reflect the actual amount of signing and may not constitute a valid measurement of sign language exposure.

Finally, the suggestion that using sign language interferes with English language development for all deaf children requires acknowledging critical limitations of subject selection that were not discussed. As with other CI studies, subject selection was biased toward including children who succeed with their CI. The 40 children who met eligibility criteria but were excluded because of a lack of follow-up data may have influenced the outcomes. Families experiencing poor progress with their child's CI may stop their follow-up appointments, for instance. Or families may decide to stop continuing with the CI and focus on sign language only. Because race and maternal education differed significantly between selected and nonselected groups, baseline data on the excluded families should be reported and evaluated for any "dropout" associations from the study. Additionally, some excluded families may comprise a fourth, unreported group: families who did not sign at baseline but began signing during the follow-up periods. The absence of this group is particularly striking.

To satisfactorily demonstrate that sign language exposure harms spoken language development, the authors must demonstrate the following: (1) all baseline measures were equivalent, (2) groups were not self-selected, and (3) participant attrition was not systematic. This study design met none of these conditions; we thus find the authors' conclusions unconvincing at best.

Amber J. Martin  
*Developmental Psychologist, Lecturer, Doctoral  
Department of Psychology, Hunter College of the  
City University of New York*  
E-mail: am2631@hunter.cuny.edu

Donna Jo Napoli  
*Professor, Linguist*

Scott R. Smith  
*Developmental Pediatrician, Research Associate  
Professor*

**CONFLICT OF INTEREST:** The authors have indicated they have no potential conflicts of interest to disclose.

doi:10.1542/peds.2017-2655A

## Operationalization and Measurement of Sign Language

We outline a number of fundamental issues in how sign language exposure and proficiency were operationalized and reported by Geers et al. Most importantly, the authors did not distinguish between those exposed to ASL versus English signing systems (eg, signing exact English, sign-supported English, baby sign) when classifying children. This is a fatal flaw because, in contrast to artificial English signing systems, natural sign languages such as ASL are legitimate languages (as long-affirmed by the Linguistic Society of America<sup>1</sup>), with all the cognitive benefits a natural language provides. The study is recklessly misleading because of this inappropriate conflation, especially given that the authors' conclusions contribute to long-standing bias, resistance, and misperceptions against natural sign languages in clinical recommendations for deaf children.

Among other issues, there is not enough information provided about participants' sign language proficiency and exposure. At minimum, it is critical to know the number of children exposed to only ASL (as opposed to artificial signing systems), the age of first exposure to ASL, the number of ASL language models, and the ASL proficiency of parents and children. Effects of "sign language exposure" may have been carried by participants who used an artificial signing system, received late exposure relative to the critical period of language acquisition, had only 1 ASL model, and families with limited to no ASL proficiency. The little information provided about sign language exposure was not collected by using direct measurement; rather, it appears to have been

measured by using an unvalidated parental report questionnaire. The criterion for positive indication of sign language exposure was, in our view, low (>10% of the time), and there was no rationale offered for why 10% is minimally sufficient. It is possible that the sample in this study represents a straw man hypothesis; no one would argue that such language conditions are sufficient for a child to thrive.

ASL is typically used within a bilingual approach encouraging both natural sign language and spoken and written English acquisition,<sup>2</sup> and it should be evaluated as such. Because those children are emerging bilinguals, their combined proficiency in both ASL and English must be considered to draw any conclusions about ASL-based intervention efficacy. In addition, because bilingual and monolingual language acquisition differs, bilingual-signing children's appropriate comparison group is other bilingual children, and they should not be compared with monolingual norms.

Although this study was designed for the authors to look narrowly at English-based outcomes, the authors overinterpret the results as evidence against the assertion<sup>3</sup> that a natural sign language can be beneficial for deaf children. Although English proficiency is certainly 1 route to success, it is not a necessary condition for it. The results of this study have no bearing on whether exposure to a natural sign language has any effect on the holistic well-being and health-related outcomes of deaf children, but they are dangerously framed and misinterpreted as such.

Naomi K. Caselli  
Cognitive Psychologist, Assistant Professor  
School of Education, Boston University  
E-mail: nkc@bu.edu

Wyatte C. Hall  
Psychologist and Public Health Scientist,  
Postdoctoral Fellow

Diane Lillo-Martin  
Linguist, Distinguished Professor

**CONFLICT OF INTEREST:** The authors have indicated they have no potential conflicts of interest to disclose.

## REFERENCES

1. Perlmutter D; Linguistic Society of America. Resolution: sign languages. 2001. Available at: <https://www.linguisticsociety.org/resource/resolution-sign-languages>. Accessed June 18, 2017
2. Lange CM, Lane-Outlaw S, Lange WE, Sherwood DL. American Sign Language/English bilingual model: a longitudinal study of academic growth. *J Deaf Stud Deaf Educ*. 2013;18(4):532–544
3. Napoli DJ, Mellon NK, Niparko JK, et al. Should all deaf children learn sign language [published correction appears in *Pediatrics*. 2015;136(4):781]? *Pediatrics*. 2015;136(1):170–176

doi:10.1542/peds.2017-2655B

## Failure to Distinguish Among Competing Hypotheses

The data presented by Geers et al are consistent with 3 mutually exclusive theories about the impact of natural sign languages on the development of English language skills: that natural sign languages (1) impede, (2) facilitate, or (3) have no impact on English development. Although Geers et al clearly favor theory 1, it is neither the only nor the best interpretation of their data.

Theory 2 is consistent with the data because although the authors quantified exposure to manual communication (including ASL), they did not measure proficiency in ASL. Children who have not acquired the grammar of ASL are not predicted to benefit from it as a foundation for subsequent mastery of English. We applaud these authors for considering variation in the amount of exposure to manual communication; however, we are dismayed to see ASL lumped together with other types of manual communication. Such coarse grouping prevents this crucial hypothesis from being adequately

tested. (That is, children who are exposed mainly to English-based signing systems will not acquire the grammar of ASL; their performance is therefore uninformative about theory 2.) Contrary to their claims, the authors have not falsified the theory that mastering the grammar of a sign language helps a child master the grammar of a spoken language.

Theory 3 is consistent with the data because observed differences might be attributable to other demographic factors that likely affected initial inclusion, attrition over time, and/or performance on the assessments (eg, socioeconomic status, etiology of deafness). Because sign language use may covary with these additional factors, the reported effects might well disappear if these factors were controlled. Theory 3 therefore remains viable.

In our view, the best interpretation of this study is that families self-select their method of communication as a result of their child's development in English. Under this view, the use of manual communication is a consequence of limited progress in English, not a cause. Both of these interpretations remain available because the study used a correlational design that was particularly vulnerable to self-selection.

To successfully discriminate among the 3 competing theories, future researchers will need to do the following: (1) distinguish ASL from other forms of manual communication, (2) assess ASL proficiency as a function of ASL exposure, (3) adopt a research design that minimizes the potential impact of reverse causality and self-selection effects (for example, studying the impact of exposure during a prespecified time window on outcomes after that time window, regardless of the family's communication choices at the time of testing).

Finally, it is important to remember that English language skills are only 1 aspect of child development.

## Operationalization and Measurement of Sign Language

Naomi K. Caselli, Wyatte C. Hall and Diane Lillo-Martin

*Pediatrics* 2017;140;

DOI: 10.1542/peds.2017-2655B originally published online October 31, 2017;

### Updated Information & Services

including high resolution figures, can be found at:  
<http://pediatrics.aappublications.org/content/140/5/e20172655B>

### References

This article cites 2 articles, 1 of which you can access for free at:  
<http://pediatrics.aappublications.org/content/140/5/e20172655B.full#ref-list-1>

### Subspecialty Collections

This article, along with others on similar topics, appears in the following collection(s):  
**Developmental/Behavioral Pediatrics**  
[http://classic.pediatrics.aappublications.org/cgi/collection/development:behavioral\\_issues\\_sub](http://classic.pediatrics.aappublications.org/cgi/collection/development:behavioral_issues_sub)  
**Cognition/Language/Learning Disorders**  
[http://classic.pediatrics.aappublications.org/cgi/collection/cognition:language:learning\\_disorders\\_sub](http://classic.pediatrics.aappublications.org/cgi/collection/cognition:language:learning_disorders_sub)  
**Ear, Nose & Throat Disorders**  
[http://classic.pediatrics.aappublications.org/cgi/collection/ear\\_nose\\_-\\_throat\\_disorders\\_sub](http://classic.pediatrics.aappublications.org/cgi/collection/ear_nose_-_throat_disorders_sub)

### Permissions & Licensing

Information about reproducing this article in parts (figures, tables) or in its entirety can be found online at:  
<https://shop.aap.org/licensing-permissions/>

### Reprints

Information about ordering reprints can be found online:  
<http://classic.pediatrics.aappublications.org/content/reprints>

Pediatrics is the official journal of the American Academy of Pediatrics. A monthly publication, it has been published continuously since . Pediatrics is owned, published, and trademarked by the American Academy of Pediatrics, 141 Northwest Point Boulevard, Elk Grove Village, Illinois, 60007. Copyright © 2017 by the American Academy of Pediatrics. All rights reserved. Print ISSN: .

American Academy of Pediatrics

DEDICATED TO THE HEALTH OF ALL CHILDREN™



# PEDIATRICS®

OFFICIAL JOURNAL OF THE AMERICAN ACADEMY OF PEDIATRICS

## **Operationalization and Measurement of Sign Language**

Naomi K. Caselli, Wyattte C. Hall and Diane Lillo-Martin

*Pediatrics* 2017;140;

DOI: 10.1542/peds.2017-2655B originally published online October 31, 2017;

The online version of this article, along with updated information and services, is located on the World Wide Web at:

<http://pediatrics.aappublications.org/content/140/5/e20172655B>

Pediatrics is the official journal of the American Academy of Pediatrics. A monthly publication, it has been published continuously since . Pediatrics is owned, published, and trademarked by the American Academy of Pediatrics, 141 Northwest Point Boulevard, Elk Grove Village, Illinois, 60007. Copyright © 2017 by the American Academy of Pediatrics. All rights reserved. Print ISSN: .

American Academy of Pediatrics

DEDICATED TO THE HEALTH OF ALL CHILDREN™

