

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.

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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 America¹), 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

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