

Thank you for inviting me to review this report! This is a very exciting study, and I commend the authors for the creativity and innovation in research questions, design, and methods. In general, I think the study is well designed and sound. My comments below are related to the specific questions I was asked to answer for this review:

• **1A. The scientific validity of the research question(s):**

Research questions are sound, important, and timely! The team is ideally situated in a country that actually has paternity leave (unlike many others!). While the findings will definitely be of interest to the scientific community, they could also have practical implications.

• **1B. The logic, rationale, and plausibility of the proposed hypotheses (where a submission proposes hypotheses)**

The hypotheses are generally well-written and sound. I do have a question/concern about H1b and H2b: the authors hypothesize that if paternal IDS modulation is related to their experience as a main caregiver, then their IDS will be more pronounced when fathers have had a higher number of days as the main caregiver. This is plausible, but I wonder whether fathers also vary in terms of their attitudes, knowledge, and beliefs about child development (and perhaps child **language** development). That is, perhaps fathers who are more knowledgeable about child (language) development are also more likely to take paternity leave. That is, IDS modulation may be related to paternal knowledge, rather than experience. We know very little about links between paternal attitudes, knowledge, and beliefs, and their language behaviors, but similar links have previously been identified in mothers (Rowe, 2008), and recently in Latinx fathers (Ferjan Ramírez et al., 2022). Perhaps the researchers could add a questionnaire to test paternal knowledge, attitudes, and/or beliefs to distinguish between these two interpretations? See for example the SPEAK survey developed by Suskind et al.

Rowe, M. L. (2008). Child-directed speech: Relation to socioeconomic status, knowledge of child development and child vocabulary skill. *Journal of Child Language*, 35(1), 185–205. <https://doi.org/10.1017/s0305000907008343>

Ferjan Ramírez, N., Hippe, D. S., Correa, L., Andert, J., & Baralt, M. (2022). Habla conmigo, daddy! Fathers' language input in North American bilingual Latinx families. *Infancy*, 00, 1–23. <https://doi.org/10.1111/inf.12450>.

Suskind, D. L., Leung, C. Y. Y., Webber, R. J., Hundertmark, A. C., Leffel, K. R., Suskind, E., Hernandez, M. W., & Graf, E. (2018). Development of the Survey of Parent/Provider Expectations and Knowledge (SPEAK). *First Language*, 38(3), 312–331. <https://doi.org/10.1177/0142723717737691>

• **1C. The soundness and feasibility of the methodology and analysis pipeline (including statistical power analysis or alternative sampling plans where applicable)**

The power analyses are sound, as are most of the methodological choices. I am a bit concerned about the choice of READING as an activity to elicit IDS. As far as I know, many parents do not read regularly to their child at this age (perhaps Norwegian parents are different, but this is not reviewed in the current report), and this may be even more true for fathers compared to mothers. At what age do Norwegian parents typically start reading to their infants? Do we know that fathers read regularly at this age? If not, they may find it awkward to read to their child, and their reading IDS may sound quite different from the type of IDS that they produce in more naturalistic settings, such as during diaper changes, feedings, object exploration. Another potential issue are fathers with a history of reading impairments or learning differences. What I worry about is that the team might find differences attributable to the amount of experience READING with the child, as opposed to the amount of experience being the main caregiver. These two things may be (but don't have to be) related (for example, it is entirely plausible to imagine a father who reads with their child every day prior to bed time, but chooses to not take paternity leave). Likewise, you could find fathers who are main caregivers, but do not read to their babies (perhaps because they don't think that their child is ready for books yet). Do the authors have a way for controlling how much READING fathers do with their babies and/or controlling for things like reading disorders? An alternative may be to consider recording fathers in other settings, to confirm that their IDS modulations, whatever they may look like, are consistent or similar across settings.

- **1D. Whether the clarity and degree of methodological detail is sufficient to closely replicate the proposed study procedures and analysis pipeline and to prevent undisclosed flexibility in the procedures and analyses**

Yes, the authors provide sufficient methodological detail to replicate the study.

- **1E. Whether the authors have considered sufficient outcome-neutral conditions (e.g. absence of floor or ceiling effects; positive controls; other quality checks) for ensuring that the obtained results are able to test the stated hypotheses or answer the stated research question(s).**

I believe that my listed concerns under 1B and 1C could also go into this category. I encourage the authors to add other quality checks, related to paternal knowledge, paternal reading frequency and reading history.

I look forward to seeing the results of this study!