

Supplementary Materials to Establishing the reliability and validity of measures extracted from long-form recordings

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Recalculate everything or not?

If RECALC is set to TRUE, then the ICC tables will be re-generated and the simulation re-ran. Notice that you can only do this if you have access to underlying data. If you are not one of the paper’s authors, please email us for access to reproduce this section. You do not need access to reproduce all the rest.

Simulation to better understand ICC

We were uncertain of how to interpret ICC’s numeric values. It is described as “proportion of variance explained”, but we do not know if it should be considered as a percentage (like R^2) or a correlation (like r). We therefore simulated data controlling the underlying r between paired datapoints to see how ICC recovered that underlying r .

We will inform the simulation by the data we have as follows:

- we’ll have the same N of corpora, and of children in each corpus
- we’ll have the same metrics for each (i.e., CVC, AWC, etc) – and these metrics will have the same mean & SD for day 1 of recordings as in observed data

We’ll depart from reality as follows:

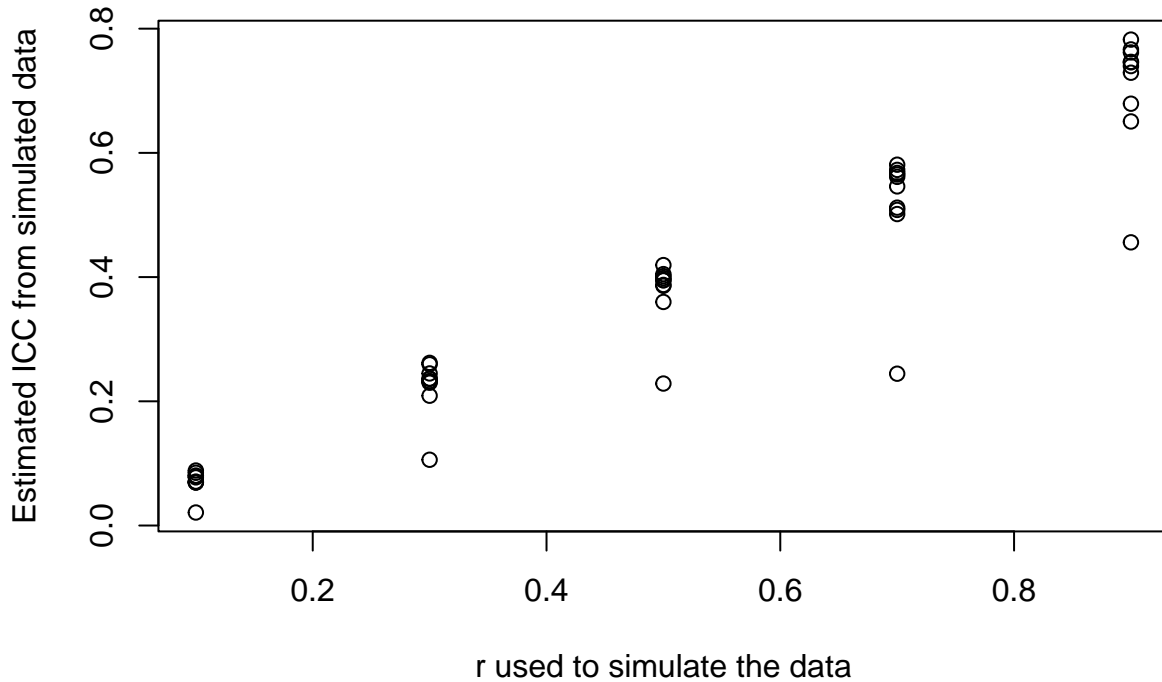
- we will not consider the r across multiple days observed in the data, but instead generate data points to vary r from a small correlation ($r=.1$), a moderate one ($r=.3$), a larger one ($r=.5$). It is unlikely

that test-retest correlations in infancy would be much greater than .5 (based on previous studies using test-retest in experimental tasks), but for completeness, we also use .7 and .9

- we will not consider child age, nor variable re-recording periods
- we will have a single pair of recordings (rather than variable N of re-recordings)

We use `simstudy`, a package created for such simulations, following the vignette <https://cran.r-project.org/web/packages/simstudy/vignettes/correlated.html> to create correlated data providing a correlation matrix

In the following plot, each point represents the ICC extracted from a mixed model applied to one metric, combining data from all corpora. It appears that ICC values reflect underlying r values, but underestimating r more the larger r is.



More information for benchmarking our results against previously reported reliability studies

First, we looked for measures of language development used with observational data that can be employed with children aged 0-3 years, and which are available at least in English. All of the instruments we found rely on reports from caregivers, who are basing their judgments on their cumulative experience with the child (e.g., the Child Observation Record Advantage, Schweinhart, McNair, & Lerner, 1993; the Desired Results Developmental Profile, REF; the MacArthur-Bates Communicative Development Inventory, REF). Readers are likely most familiar with the MB-CDI, Fenson et al., 1994 report a correlation of $r=.95$ in their sample of North American, monolingual infants. We did not find a systematic review or meta-analysis providing more such estimates. However, @frank2021 analyzed data archived in a CDI repository, concentrating on American English and Norwegian, where longitudinal data was available. They found that for both datasets, correlations within 2-4 months were above $r=.8$, with correlations at 16 months of distance (i.e., a form filled in at 8 and 24 months) at their lowest, $r=.5$. These correlations are very high when considering that the CDI tends to have ceiling and floor effects at these extreme ages. Another report looked at correlations when parents completed two versions of the form in two media (e.g., short form in paper and long form online, or vice versa) within a month. Here, the correlation was $r=.8$ for comprehension and $r=.6$ for production. It is worth bearing in mind that test-retest reliability in parental report measures does not depend only on the consistency in the individual infants' ranking for a given behavior, but also on the consistency of the adult in reporting it. Moreover, they are based on cumulative experience, rather than a one-shot observation, as in

the case of long-form recordings. Therefore, they do not constitute an appropriate comparison point, and by and large we can be quite certain that they will yield higher reliability than metrics based on the children themselves. For example, a meta-analysis of infant laboratory tasks, including test-retest data for sound discrimination, word recognition, and prosodic processing, found that the meta-analytic weighted average was not different from zero, suggesting that performance in these short laboratory tasks may not be captured in a stable way across testing days. Thus, parental report (or short lab studies) may not be the most appropriate comparisons for our own study.

Second, we did a bibliographic search for systematic reviews of test-retest reliability of standardized instruments to measure language development up to age three years that are available at least in English. Although reliability in these ones may also partially reflect consistency in the adults' reports, they are at least based on a one-shot observation of how the child behaves, rather than the adult's cumulative experience with the child. Note that some promising tests are currently in development or have only recently been released, including the NIH Baby Toolbox (REF) and GSED (REF), and thus we cannot include them in the present summary.

The Ages and Stages Questionnaire (ASQ) is a screening tool to measure infants and children's development based on observation of age-specific behaviors: For instance, at 6 months, in the domain of communication, one of the items asks whether the child smiles. The ASQ's reliability has been the object of a systematic review of independent evidence (Velikonja et al., 2017). Across 10 articles with data from children in the USA, Canada, China, Brazil, India, Chile, the Netherlands, Korea, and Turkey, only three articles reported test-retest correlations, two in the USA ($r=.84-1$) and one in Turkey ($r=.67$). However, the meta-analysis authors judged these three studies to be "poor" in quality. Moreover, the ASQ is a questionnaire that an observer fills in, but it can also be administered as a parental questionnaire, reducing its comparability with our purely observational method. For the other tests, reliability is mainly available from reports by the companies commercializing the test. The Goldman Fristoe Articulation Test – 3rd edition (GFTA-3) (REF) focuses on the ability to articulate certain sounds through picture-based elicitation and can be used from two to five years of age. Available in English and Spanish for a USA context, it has a reported reliability of $r=.92$ – although we do not know whether it is this high for two-year-olds specifically. The Preschool Language Scales – 5th edition can be used to measure both comprehension and production from birth to 7 years of age. According to Pearson's report, its test-retest reliability is .86 to .95, depending on the age bracket (0;0-2;11, 3;0-4;11, and 5;0-7;11, 0-7 years). The test has also been adapted to other languages, with good reported test-retest reliability ($r=.93$; Turkish, Sahli & Belgin, 2017). One issue we see with both of these reports is that children tested varied greatly in age, and the correlation seems to have been calculated based on the raw (rather than the normed) score. As a result, children's ranking may have been stable over test and retest mainly because they varied greatly in age. The Expressive Vocabulary Test – 2nd Edition (EVT-2) is a picture-based elicitation method that can be used with children from 2.5 years of age. The company Springer reports a test-retest reliability of $r=.95$ by age (REF).

Many other standardized tests exist for children over three years of age. Given that most children included in the present study were under three, these other tests do not constitute an ideal comparison point. Nonetheless, in the spirit of informativeness, it is useful to consider that a systematic review and meta-analysis has been done looking at all psychometric properties (internal consistency, reliability, measurement error, content and structural validity, convergent and discriminant validity) for standardized assessments targeted at children aged 4-12 years (REF). Out of 76 assessments found in the literature, only 15 could be evaluated for their psychometric properties, and 14 reported on reliability based on independent evidence (i.e., the researchers tested and retested children, rather than relying on the company's report of reliability). Among these, correlations for test-retest reliability averaged $r=.67$, with a range from .35 to .76. The authors concluded that psychometric quality was limited for all assessments, but based on the available evidence, PLS-5 (whose test-retest reliability was $r=.69$) was among those recommended for use.

Third, and perhaps most relevant, we looked for references that evaluated the psychometric properties of measures extracted from wearable data. We found no previous work attempting to do so on the basis of completely ecological, unconstrained data like ours. The closest references we could find reported on reliability and/or validity of measurements from wearable data collected in constrained situations, such as having 4.5 year old children wear interior sensors and asking them to complete four tests of balance (e.g., standing with their eyes closed; Liu et al., 2022). It is likely that consistency and test-retest reliability are higher in such

cases than in data like ours, making it hard to compare. Nonetheless, to give an idea, a recent meta-analysis of wearable inertial sensors in healthy adults found correlations between these instruments and gold standards above $r = .88$ for one set of measures (based on means) but much lower for another (based on variability, max weighted mean effect $r = .58$). Regarding test-retest reliability, the meta-analysts report ICCs above .6 for all measures for which they could find multiple studies reporting them. However, those authors point out that the majority of the included studies were classified as low quality, according to a standardized quality assessment for that work.

Code to reproduce Table 2

Code to reproduce Fig. 2

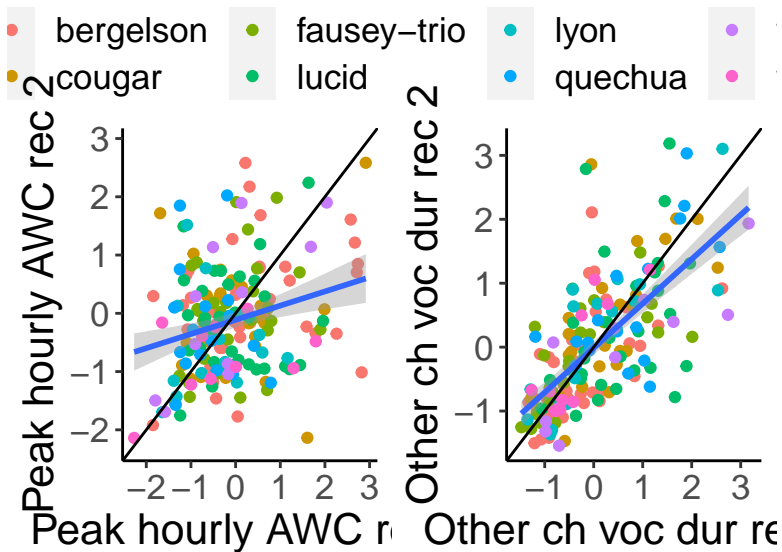


Figure 1: (A) scatterplot for one variable with relatively low ICCs versus (B) one with relatively higher ICCs (see Tables 1-2 for details)

Code to reproduce other text in the section Results, subsection Overall reliability

Out of the 69 fitted models, 65 could be fit with the full model, yielding a measure of Corpus ICC. Of these, 92% had Corpus ICCs smaller than .2, consistent with the idea that LENA and ACLEW metrics are robust to corpus differences. For the 4 for which the full model was singular, we fit the data with the No Corpus model, and none was singular then, allowing us to have Child ICC for all 69 metrics.

Code to reproduce Fig. 3

Code to reproduce text under Figure 3

The majority of measures had ICCs between .3 and .5. 7 measures had higher ICCs, and surprisingly, 6 of them corresponded to the “other child” category, known to have the worst accuracy according to previous analyses (Cristia et al., 2020).

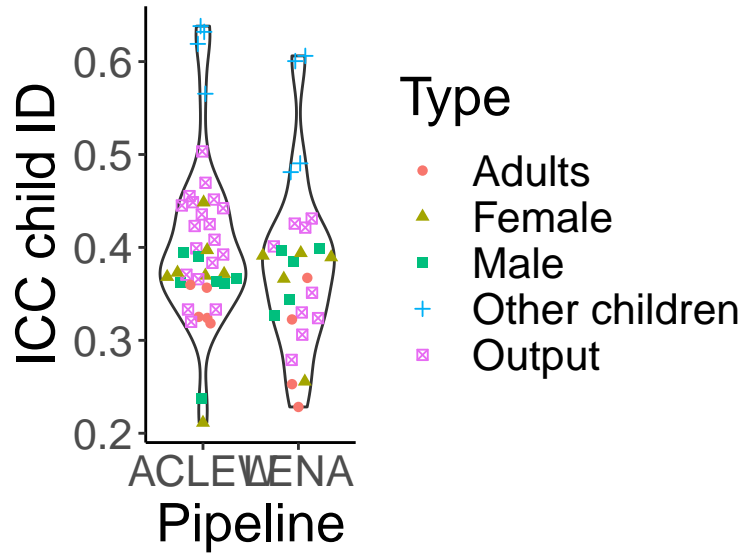


Figure 2: Distribution of ICC attributed to corpus (a) and children (b), when combining data from all corpora.

Checking whether high ICC for other child measures are due to presence of other siblings

We reasoned this may be because children in our corpora vary in terms of the number of siblings they have, and that siblings’ presence may be stable across recordings. To address this possibility, we fit the full model again to predict number of vocalizations from other children, but this time including sibling number as a fixed effect $lmer(metric\ age + sibling_n\ umber + (1|corpus/child))$, so that individual variation that was actually due to sibling number was captured by that fixed effect instead of the random effect for child. We had sibling number data for 874 recordings from 120 in 5 corpora (bergelson, lucid, quechua, warlaumont, winnipeg). We fit this model for the metric with the highest Child ICC, ACLEW’s total vocalization duration by other children. Results indicated the full model was singular, so we fitted a No Corpus model to be able to extract a Child ICC. In fact, there was no difference in Child ICC in our original analysis (0.64) versus this re-analysis including the number of siblings (0.64).

Code to reproduce text before Table 3

Going back to our overarching analyses, we explored how similar Child ICCs were across different talker types and pipelines. We fit a linear model with the formula $lm(icc_child_id\ type * pipeline)$, where type indicates whether the measure pertained to the key child, (female/male) adults, other children; and pipeline LENA or ACLEW. We found an adjusted R-squared of 65%, suggesting much of the variance across Child ICCs was explained by this model. A Type 3 ANOVA on this model revealed only type was a significant ($F(4)=31.2$, $p<.001$), whereas neither pipeline nor the interaction between type and pipeline were significant.

Code to reproduce Table 3

Code to reproduce “Exploratory analyses: Correlations based on paired analyses of recordings done within two months”

```
##          aclew          lena
## AWC    ".55 [.47,.63]" ".52 [.4,.64]"
## CVC    ".8 [.76,.84]" ".66 [.6,.72]"
```

Table 1: Most commonly used metrics.

metric	LENA ICC	ACLEW ICC
wc_adu_ph	0.37	0.33
lena_CVC	0.35	0.42
lena_CTC	0.32	0.36
voc_fem_ph	0.39	0.37
voc_chi_ph	0.42	0.45

```
## CTC      ".69 [.63,.75]" ".68 [.6,.76]"
## Chi vocs ".69 [.63,.75]" ".6 [.52,.68]"
```

Out of our 8 corpora and 191 children, 148 children (belonging to 7 corpora) could be included in this analysis, as some children did not have recordings less than two months apart (in particular, no child from the Warlaumont corpus did).

Code to reproduce Fig. 4

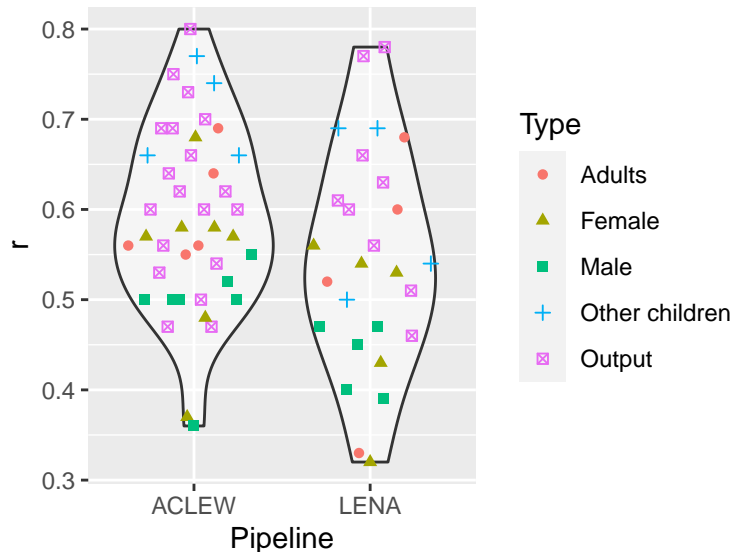


Figure 3: Distribution of correlation coefficients.

Code to reproduce results of regression on correlation values

To see whether correlations in this analysis differed by talker types and pipelines, we fit a linear model with the formula $lm(cor \sim type * pipeline)$, where type indicates whether the measure pertained to the key child, (female/male) adults, other children; and pipeline LENA or ACLEW. We found an adjusted R-squared of 32%, suggesting this model did not explain a great deal of variance in correlation coefficients. A Type 3 ANOVA on this model revealed a significant effect of pipeline ($F = 5.13, p = 0.03$), due to higher correlations for ACLEW ($m = 0.59$) than for LENA metrics ($m = 0.54$). See below for fuller results.

	Sum Sq	Df	F value	Pr(>F)
Type	0.36	4	31.21	0.00
data_set	0.01	1	5.13	0.03
Type:data_set	0.01	4	1.19	0.32
Residuals	0.17	59	NA	NA

Code to reproduce text and figures in “Exploratory analyses: Reliability within corpus”

Figure 5 addresses this question, showing the distribution of ICC across our 69 metrics in each of the 8 included corpora. Out of 552 fitted models (69 metrics times 8 corpora), 23 were singular when including a random intercept per child, and therefore they could not be included in these analyses at all. (Including a random intercept per corpus is not relevant here, since only data from one corpus is included in each model fit.)

The fact that we cannot infer reliability from one corpus based on another one was confirmed statistically: We checked whether Child ICC differed by talker types and pipelines across corpora by fitting a linear model with the formula $lm(Child_ICC \sim type * pipeline * corpus)$, where type indicates whether the measure pertained to the key child, (female/male) adults, other children; pipeline LENA or ACLEW; and corpus the corpus ID. We found an adjusted R-squared of 46%, suggesting this model explained nearly half of the variance in Child ICC. A Type 3 ANOVA on this model revealed several significant effects and interactions, including a three-way interaction of type, pipeline, and corpus; a two-way interaction of type and corpus; and a main effect of corpus. See below for more information.

	Sum Sq	Df	F value	Pr(>F)
Type	0.59	4	8.74	0.00
data_set	0.10	1	6.10	0.01
corpus	2.13	7	17.95	0.00
Type:data_set	0.05	4	0.69	0.60
Type:corpus	4.75	28	10.00	0.00
data_set:corpus	0.48	7	4.03	0.00
Type:data_set:corpus	0.96	28	2.02	0.00
Residuals	7.61	449	NA	NA

Code to reproduce text and figures in “Exploratory analyses: Reliability across age groups”

Out of 414 fitted models (69 metrics times 6 age bins), 4 were singular when including a random intercept per child, and therefore they could not be included in these analyses at all. In addition, 122 were singular when including a random intercept per corpus. The remaining 288 could be analyzed with the full model.

As we did in the previous section for corpus, we checked whether Child ICC differed by talker types and pipelines across age bins by fitting a linear model with the formula $lm(Child_ICC \sim type * pipeline * age_bin)$. We found an adjusted R-squared of 38%, suggesting this model explained about a third of the variance in Child ICC. However, a Type 3 ANOVA on this model revealed only an interaction of type and age bin, as well as a main effect of age bin, suggesting less complex effects than in the case of corpus. See below for more information.

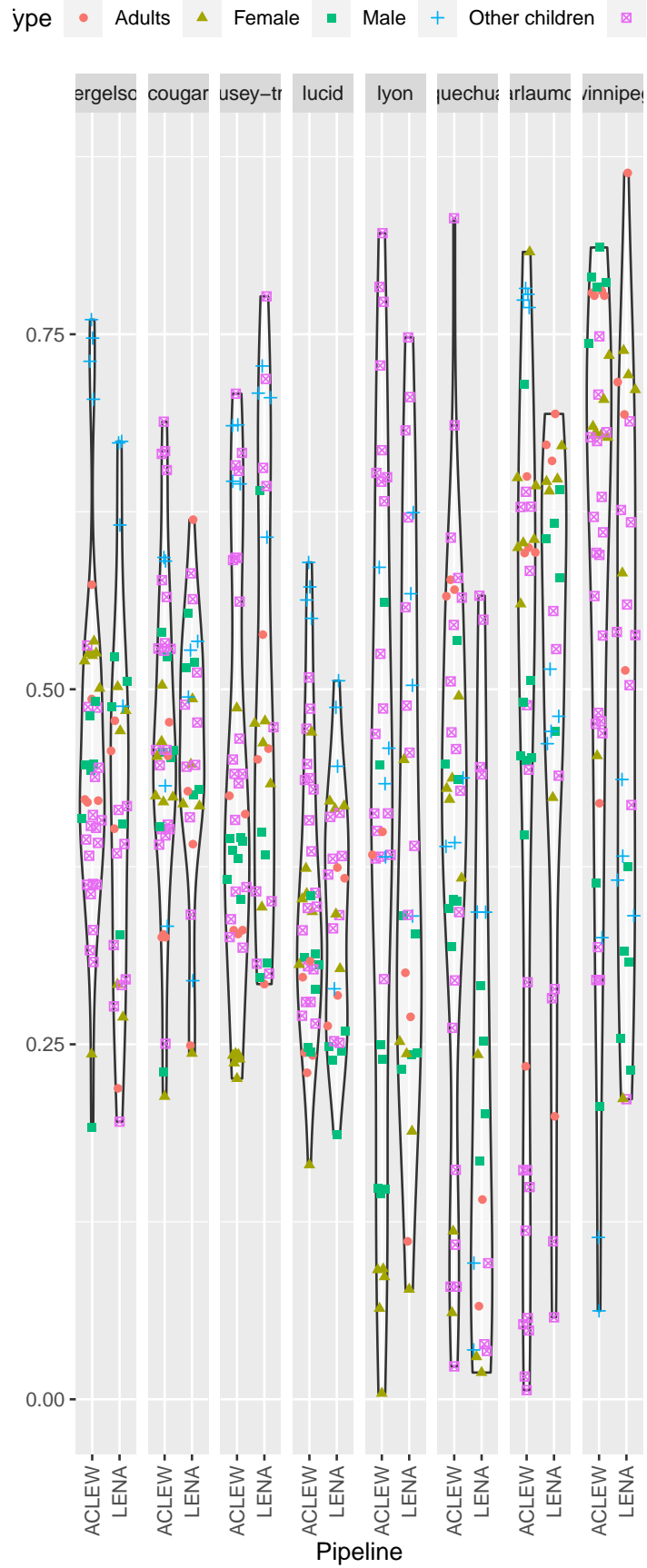


Figure 4: Child ICC by metric type and pipeline, when considering each corpus separately.

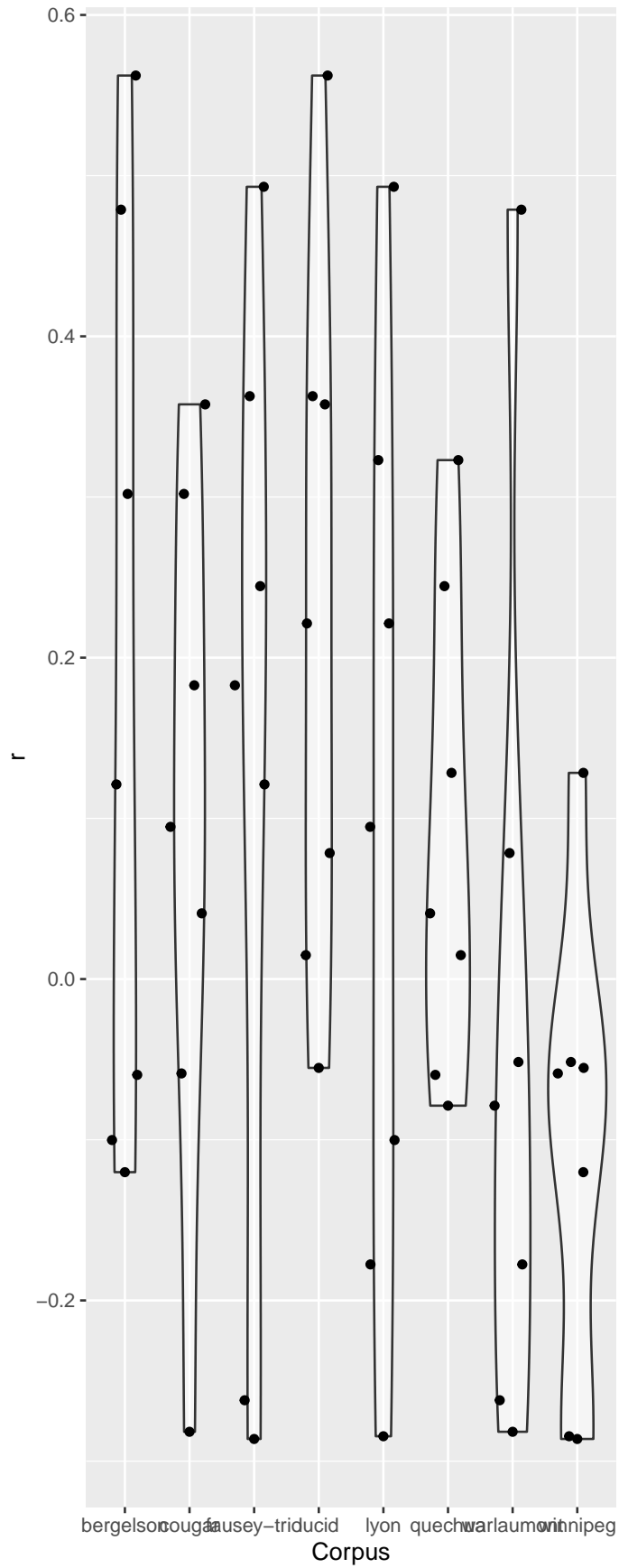


Figure 5: Correlations in Child ICC across corpora. Each point indicates the correlation in Child ICC for the corpus named in the x-axis with every other corpus. ⁹

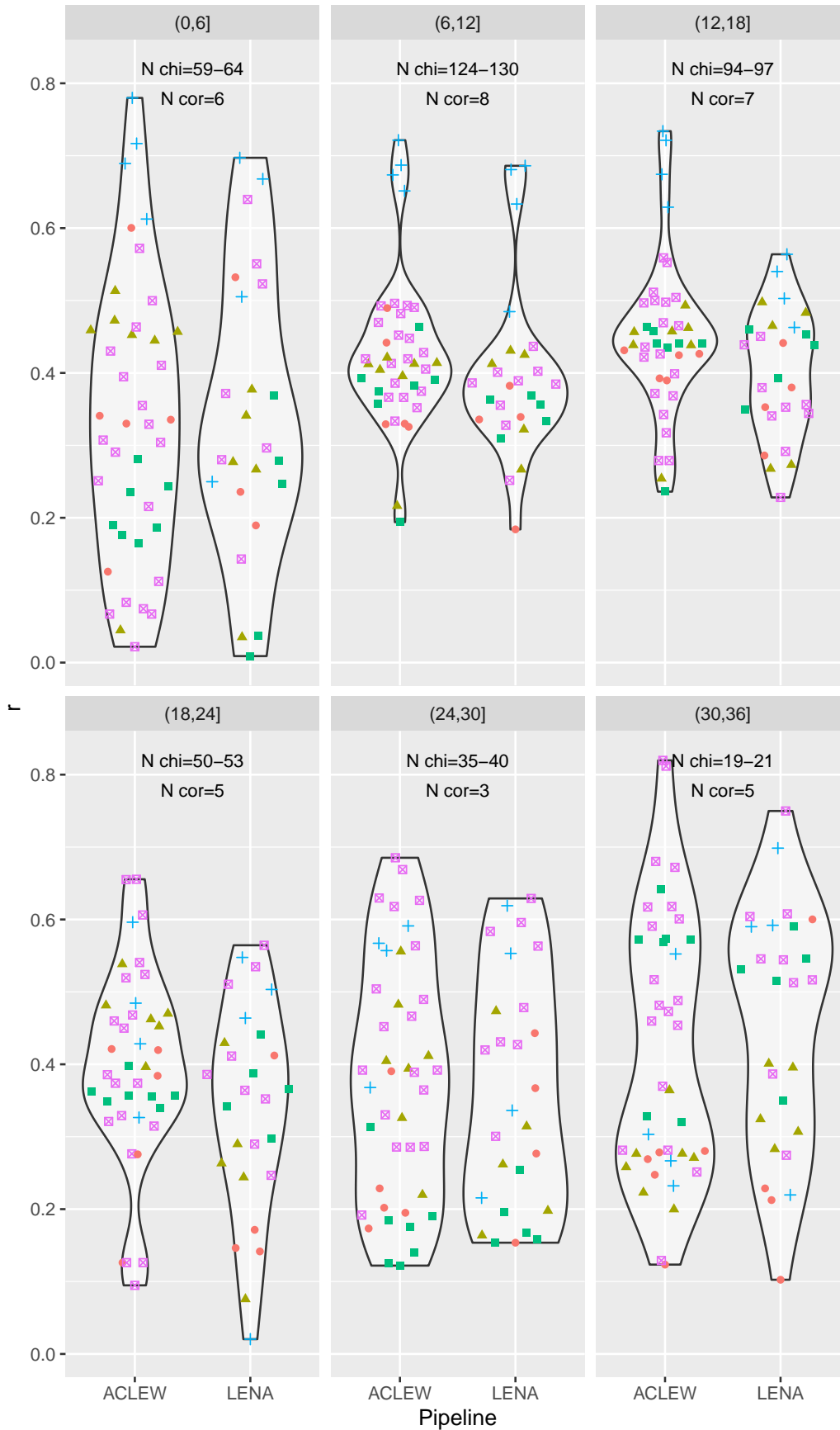


Figure 6: Distribution of ICC attributed to corpus (a) and children (b), when binning children’s age.

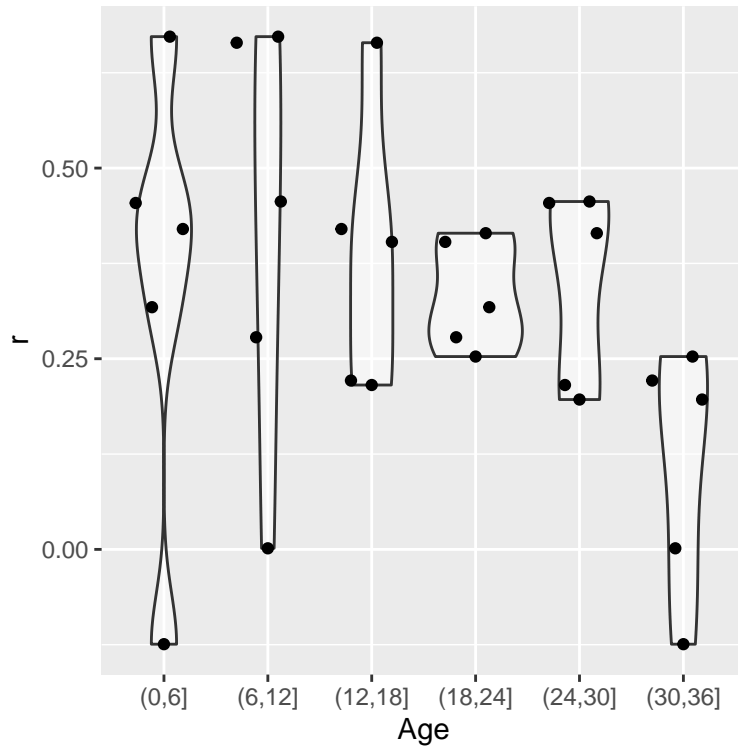


Figure 7: Correlations in Child ICC across corpora. Each point indicates the correlation in Child ICC for the corpus named in the x-axis with every other corpus.

	Sum Sq	Df	F value	Pr(>F)
Type	1.68	4	28.84	0.00
data_set	0.05	1	3.31	0.07
age_bin	0.50	5	6.85	0.00
Type:data_set	0.10	4	1.68	0.15
Type:age_bin	1.76	20	6.03	0.00
data_set:age_bin	0.14	5	1.95	0.09
Type:data_set:age_bin	0.38	20	1.31	0.17
Residuals	5.11	350	NA	NA

Save information about packages used