

1 Quantifying sources of variability in infancy research using the infant-directed speech
2 preference

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Abstract

86

87 The field of psychology has become increasingly concerned with issues related to
88 methodology and replicability. Infancy researchers face specific challenges related to
89 replicability: high-powered studies are difficult to conduct, testing conditions vary across
90 labs, and different labs have access to different infant populations, amongst other factors.
91 Addressing these concerns, we report on a large-scale, multi-site study aimed at 1) assessing
92 the overall replicability of a single theoretically-important phenomenon and 2) examining
93 methodological, situational, cultural, and developmental moderators. We focus on infants'
94 preference for infant-directed speech (IDS) over adult-directed speech (ADS). Stimuli of
95 mothers speaking to their infants and to an adult were created using semi-naturalistic
96 laboratory-based audio recordings in North American English. Infants' relative preference for
97 IDS and ADS was assessed across 67 laboratories in North America, Europe, Australia, and
98 Asia using the three commonly-used infant discrimination methods (head-turn preference,
99 central fixation, and eye tracking). The overall meta-analytic effect size (Cohen's d) was 0.35
100 [0.29 - 0.42], which was reliably above zero but smaller than the meta-analytic mean
101 computed from previous literature (0.67). The IDS preference was significantly stronger in
102 older children, in those children for whom the stimuli matched their native language and
103 dialect, and in data from labs using the head-turn preference procedure. Together these
104 findings replicate the infant-directed speech preference but suggest that its magnitude is
105 modulated by development, native language experience, and testing procedure.

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reproducibility; experimental methods

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110 preference

111 The recent focus on power, replication, and replicability has had important
112 consequences for many branches of psychology. Confidence in influential theories and classic
113 psychological experiments has been shaken by demonstrations that much of the experimental
114 literature is under-powered (Button et al., 2013), that surprisingly few empirical claims have
115 been subject to direct replication (Makel, Plucker, & Hegarty, 2012), and that the direct
116 replication attempts that do occur often fail to substantiate original findings (Open Science
117 Collaboration, 2015). As disturbing as these demonstrations may be, they have already led
118 to important positive consequences in psychology, encouraging scientific organizations,
119 journals, and researchers to work to improve the transparency and replicability of
120 psychological science.

121 To date, however, researchers in infancy have remained relatively silent on issues of
122 replicability. This silence is not because infant research is immune from the issues raised.
123 Indeed, the statistical power associated with infant psychology experiments is often unknown
124 (and presumably too low (Oakes, 2017)), and the replicability of many classic findings is
125 uncertain. Instead, one reason for the infancy field's silence is likely related to the set of
126 challenges that come with collecting and interpreting infant data – and developmental data
127 more generally. For example, it can be quite costly to test large samples of infants or to
128 replicate past experiments. Another challenge for infancy researchers is that it is often
129 difficult to interpret contradictory findings in developmental populations, given how
130 children's behavior and developmental timing varies across individuals, ages, context,
131 cultures, languages, and socioeconomic groups. While these challenges may make
132 replicability in infancy research more difficult, they do not make it any less important.

133 Indeed, it is of primary importance to evaluate replicability in infancy research (see

134 Frank et al., 2017). But how can this evaluation be done? Here we report the results of a
135 large-scale, multi-lab, pre-registered infant study. This study was inspired by the ManyLabs
136 studies (e.g., Klein et al., 2014), in which multiple laboratories attempt to replicate various
137 social and cognitive psychology studies, and moderators of study replicability are assessed
138 systematically across labs. Given the reasons discussed above, it would be prohibitively
139 difficult to examine the replicability of a large number of infant studies simultaneously.
140 Instead, we chose to focus on what developmental psychology can learn from testing a single
141 phenomenon, assessing its overall replicability, and investigating the factors moderating it.
142 As a positive side effect, this approach leads to the standardization and delineation of
143 decisions concerning data collection and analysis across a large number of labs studying
144 similar phenomena or using similar methods. For this first “ManyBabies” project, we selected
145 a finding that the field has good reason to believe is robust – namely, infants’ preference for
146 infant-directed speech over adult-directed speech – and tested it in 67 labs around the world.
147 This phenomenon has the further advantage that it uses a dependent measure – looking time
148 – that is ubiquitous in infancy research. In the remainder of this Introduction, we briefly
149 review the literature on the relevance of infant-directed speech in development, and then
150 discuss our motivations and goals in studying a single developmental phenomenon at scale.

151 **Infant-Directed Speech Preference**

152 Infant-directed speech (IDS) is a descriptive term for the characteristic speech that
153 caregivers in many cultures direct towards infants. Compared to adult-directed speech
154 (ADS), IDS is often higher pitched, with greater pitch excursions, and shorter utterances,
155 among other differences (Fernald et al., 1989). While caregivers across many different
156 cultures and communities use IDS, the magnitude of the difference between IDS and ADS
157 varies (Englund & Behne, 2006; Farran, Lee, Yoo, & Oller, 2016; Fernald et al., 1989;
158 Newman, 2003). Nevertheless, the general acoustic pattern of IDS is readily identifiable to

159 adult listeners (Fernald, 1989; Grieser & Kuhl, 1988; Katz, Cohn, & Moore, 1996; Kitamura
160 & Burnham, 2003).

161 A substantial literature has observed infants' preference for IDS over ADS using a
162 range of stimuli and procedures. For example, Cooper and Aslin (1990), using a contingent
163 visual-fixation auditory preference paradigm, showed that infants fixate on an unrelated
164 visual stimulus longer when hearing IDS than when hearing ADS, even as newborns. Across
165 a variety of ages and methods, other studies have also found increased attention to IDS
166 compared to ADS (Cooper & Aslin, 1994; Cooper, Abraham, Berman, & Staska, 1997;
167 Fernald, 1985; Hayashi, Tamekawa, & Kiritani, 2001; Kitamura & Lam, 2009; Newman &
168 Hussain, 2006; Pegg, Werker, & McLeod, 1992; Santesso, Schmidt, & Trainor, 2007; L. Singh,
169 Morgan, & Best, 2002; Werker & McLeod, 1989). In a meta-analysis by Dunst, Gorman, and
170 Hamby (2012), which included 34 experiments, the IDS preference typically had an effect
171 size of Cohen's $d = 0.67$ [0.57 – 0.76] – quite a large effect size for an experiment with
172 infants (Bergmann et al., 2018).

173 The evidence suggests that IDS augments infants' attention to speakers (and
174 presumably what speakers are saying) because of highly salient acoustic qualities such as
175 frequency modulation (Cusack & Carlyon, 2003). In addition, it is hypothesized that the IDS
176 preference plays a pervasive supporting role in early language learning. For example, young
177 infants are more likely to discriminate speech sounds when they are pronounced with typical
178 IDS prosody than with ADS prosody (Karzon, 1985; Trainor & Desjardins, 2002). There are
179 also reports that infants show preferences for natural phrase structure in narratives spoken in
180 IDS but not in ADS (cf., Fernald & McRoberts, 1996; Hirsh-Pasek et al., 1987). In addition,
181 word segmentation (Thiessen, Hill, & Saffran, 2005) and word learning (Graf Estes & Hurley,
182 2013; Ma, Golinkoff, Houston, & Hirsh-Pasek, 2011) are reported to be facilitated in IDS
183 compared to ADS. Naturalistic observations confirm that the amount of speech directed to
184 US 18-month-olds (which likely bears IDS features), rather than the amount of overheard

185 speech (which is likely predominantly ADS), relates to the efficiency of word processing and
186 expressive vocabulary knowledge at 24 months (Weisleder & Fernald, 2013). Finally, infants
187 show increased neural activity to familiar words in IDS compared to ADS, and also
188 compared to unfamiliar words in either register (Zangl & Mills, 2007). From a theoretical
189 perspective, the IDS register has been claimed to trigger specialized learning mechanisms
190 (Csibra & Gergely, 2009) as well as boost social preferences and perhaps attention in general
191 (Schachner & Hannon, 2011), as it even has been reported to improve performance in
192 non-linguistic associative learning (e.g., Kaplan, Jung, Ryther, & Zarlengo-Strouse, 1996).

193 **The Current Study: Motivations and Goals**

194 Despite the large body of research on infants' preference for IDS and its positive effects
195 on the processing of linguistic and non-linguistic stimuli, a number of open questions remain
196 regarding this effect. This study was designed to answer some of these IDS-specific questions
197 as well as questions about methods for assessing infants' cognition, including concerns about
198 the interaction between statistical power and developmental methodologies. We describe the
199 key questions for our study below (as well as our predictions, where applicable), in rough
200 order of decreasing specificity, highlighting methodological decisions that follow from
201 particular goals.

202 What is the magnitude of the IDS preference? First and foremost, our study serves as
203 a large-scale, precise measurement of IDS preference across a large number of labs. Based on
204 evidence summarized in a previous meta-analysis (Dunst et al., 2012), we expect that the
205 preference will be non-zero and positive. We suspect, however, that this phenomenon, like
206 many others, suffers from a file-drawer effect, in which studies with low effect sizes (or large
207 p values) often do not get published. Also, there is reason to believe that effect sizes in
208 infancy research are often incorrectly reported; for example, partial eta-squared η_p^2 is often
209 misreported as eta-squared η^2 . This confusion is likely to inflate the practical significance of

210 the findings, leading to an overestimation of the statistical magnitude and importance of
211 effects (Mills-Smith, Spangler, Panneton, & Fritz, 2015). Therefore, the mean effect size of
212 0.67 reported by Dunst et al. (2012) is likely an overestimate of the real effect size.

213 How does IDS preference vary across age? We could plausibly predict that, all else
214 being equal, older infants can more effectively process ADS than younger infants, and so the
215 attraction of IDS over ADS might attenuate with age (Newman & Hussain, 2006). On the
216 other hand, older infants might show a stronger preference for IDS over ADS, given that
217 older infants have had more opportunity to experience the positive social interactions that
218 likely co-occur with IDS, including but not limited to eye contact, positive facial expressions,
219 and interactive play.

220 How does IDS preference vary with linguistic experience and language community?
221 Preference for IDS might be affected by infants' language experience. Across many areas of
222 language perception, infants show a pattern of perceptual narrowing. They begin life as
223 "universal listeners" ready to acquire any language(s), but with experience gain sensitivity to
224 native language distinctions and lose sensitivity to non-native distinctions (Maurer &
225 Werker, 2014). If preference for IDS follows a similar pattern, then we predict that older
226 infants tested in their native language will show a stronger preference for IDS over ADS than
227 infants tested in a non-native language.

228 Faced with several competing concerns, we made the decision that all infants in our
229 study, regardless of native language, would be exposed to ADS and IDS stimuli in North
230 American English (NAE). This design choice had several practical advantages. Most
231 importantly, every infant was tested with the same stimulus set. Creating different stimulus
232 sets in different languages would add methodological variability across labs that would be
233 statistically indistinguishable from lab identity and language environment. Further, creating
234 a single high-quality stimulus set shared across labs would reduce the time and cost of
235 conducting the study.

236 There are both design-related advantages and drawbacks to this decision. A limitation
237 of our design is that NAE stimuli are unfamiliar to infants from other language or dialect
238 communities; thus these infants might show less interest for NAE speech overall and/or may
239 have a harder time recognizing IDS features as such when they differ from those used in their
240 native language or dialect. In fact, previous work even suggests that infants' IDS preference
241 depends on the characteristics of the type of IDS addressed to children their own age
242 (McRoberts, McDonough, & Lakusta, 2009). Although this is a relevant concern, previous
243 research has documented some IDS preference in the face of language and age mismatches
244 (McRoberts et al., 2009; Werker, Pegg, & McLeod, 1994); and corpus studies suggest that, if
245 anything, the distinction between IDS and ADS is more salient in NAE than in other
246 linguistic variants (e.g., Fernald et al., 1989; Shute, 1987). Further, although this design does
247 not allow us to disentangle the effects of stimulus language (native vs. non-native) from the
248 effects of infants' cultural background, we can explore how aspects of these factors influence
249 infants' preference for IDS.

250 After weighing these considerations, we adopted NAE stimuli to provide the maximal
251 chance of recovering a positive effect, ensure that stimuli are not a source of variance across
252 labs, allow comparability with previous work, and also minimize the barriers to entry (i.e.,
253 the need to create lab-specific stimuli) for each participating lab. So as to be able to assess
254 children's language background at the group level, we also chose to focus our primary
255 analyses on monolingual infants (a separate effort analyzed IDS preferences in bilingual
256 children; Byers-Heinlein et al., accepted pending data collection).

257 We focused here on three primary methods: single screen central fixation, eye tracking,
258 and the head-turn preference procedure (HPP). All three methods are widely used in the
259 field of infant language acquisition, and yield measurements of preference for a given type of
260 auditory stimulus, indexed by infants' looking to an unrelated visual stimulus. In the single
261 screen central fixation method, infants were shown an uninformative image (a checkerboard)

262 on a single, centrally-located monitor, while listening to either IDS or ADS, and looking time
263 to the monitor was manually coded via a closed-circuit video camera. In the eye tracking
264 method, infants saw a similar display, but looking times were measured automatically via a
265 remote corneal-reflection eye tracker. In the HPP method, infants saw an attractor visual
266 stimulus (often a flashing light bulb) appear to either their left or their right, and the
267 duration of their head turn while IDS or ADS played was manually coded via a closed-circuit
268 video camera (Nelson et al., 1995).

269 Each lab tested the same phenomenon, using the same stimuli and the same general
270 experimental parameters (including, e.g., trial order, maximum trial length), varying only in
271 the method of measuring preference. We thus can analyze whether this theoretically
272 irrelevant methodological choice influences effect size, helping to guide future
273 decision-making.

274 What are the effects of testing infants in multiple experiments during a single lab visit?
275 Labs vary in whether each infant visiting the lab completes a single experiment only, or
276 whether some infants participate in a second study as well. These “second session”
277 experiments are thought by some researchers to yield greater dropout rates and less reliable
278 measurements, but the existence and magnitude of a “second session” effect has not been
279 tested, to our knowledge. In our study, a number of participating labs ran the IDS
280 preference study with some infants who had already been tested on additional studies;
281 measurements from these infants can inform future lab administration practices.

282 What should our expectations be regarding replicability and statistical power in
283 studies of infancy? Although we are only replicating a single phenomenon, the importance
284 and assumed robustness of the IDS preference means that our study still provides data
285 relevant to developing a more nuanced understanding of replicability and power in infancy
286 research. Because of the large number of participating labs, data from some labs does not
287 support an IDS preference (i.e., yields a small – or even negative – effect size when analyzed

288 individually). Some variability is expected due to the mathematics of estimating an effect at
289 so many independent sites. Nonetheless, we inspect whether there is systematic variability
290 explained by lab effects.

291 In addition, by providing an unbiased estimate of effect size for an important
292 developmental phenomenon (including estimates of how that effect varies across ages,
293 language backgrounds, and tasks), this work gives a rough baseline for other scientists to use
294 when planning studies. Existing attempts to estimate the statistical power of infant
295 experiments have been contaminated by publication bias, which leads to an overestimation of
296 typical effect sizes in infant research. Such overestimates can lead subsequent studies to be
297 under-powered (expecting to see larger effects than are truly present). Though our report
298 estimates the effect for a particular developmental preference, we can compare our unbiased
299 estimate, calculated both across all three methods and for each method, to the meta-analytic
300 effect extracted from previously published studies. This calculation can provide a rough
301 estimate of the effect size inflation in general, and for each method in particular, at least for
302 this particular phenomenon.

303 How should we think about the relationships between experimental design, statistical
304 significance, and developmental change? Previous work often employs a contrast between
305 two ages to suggest that a developmental change has taken place; for example, by showing
306 that 7-month-old infants show a statistically reliable preference in a task, but 5-month-old
307 infants do not. Such a finding (the pairing of a significant difference and a non-significant
308 difference) is not sufficient to show a difference between two time points (Nieuwenhuis,
309 Forstmann, & Wagenmakers, 2011). Even in the case where a significant difference is found
310 between the two age groups, such a result is not sufficient to elucidate the developmental
311 pattern underlying this discrete test. By measuring how effect sizes change over age with a
312 much denser sampling approach, our data and continuous analytic approach illustrate what
313 stands to be gained with a more gradient approach to testing behavior over development.

314 **Summary**

315 This broad replication of IDS preferences helps to answer basic questions about the
316 replicability of developmental psychology findings and will also provide useful benchmarks
317 for how to design infant cognition studies going forward. Just as projects such as ManyLabs
318 have led to important improvements in research practices in cognitive and social psychology,
319 we hope that ManyBabies will play a similar role for developmental cognitive science.

320 **Methods**

321 **Participation Details**

322 **Time frame.** We issued an open call for labs to participate on February 2nd, 2017.
323 Data collection began on May 1st, 2017. Data collection was scheduled to end on April 30th,
324 2018 (one year later). In order to allow labs to complete their sample, however, a 45 day
325 extension was granted, and data collection officially ended on June 15th, 2018. Data
326 collection from one laboratory extended beyond this timeframe (see below in Methods
327 Addendum).

328 **Age distribution.** Each participating lab was asked to recruit participants in one or
329 more of four age bins: 3;0 - 6;0, 6;1 - 9;0, 9;1 - 12;0, and/or 12;1 - 15;0 months. Each lab was
330 tasked with ensuring that, for each age bin they contributed, the mean age fell close to the
331 middle of the range and the sample was distributed across the bin. We selected three-month
332 bins as a compromise, on the assumption that tighter bins would make recruitment more
333 difficult while broader bins would lead to more variability and would blur developmental
334 trends (i.e., by introducing possible interactions between age and lab-specific effects, for
335 instance, if a particular method turned out to be most appropriate for a subset of the ages
336 tested). This flexibility was necessary because labs differ in their ability to recruit infants of

337 different ages.

338 **Lab participation criterion.** During study planning, we used data from MetaLab
339 (Bergmann et al., 2018) to compute the meta-analytic mean effect size for IDS preference;
340 the resulting value was Cohen’s $d = .72$. In a paired t -test, 95% power to detect this effect
341 requires 27 participants, and 80% power requires 17. On the basis of these calculations, we
342 asked participating labs to commit to samples with a minimum of $N = 32$ in a single age
343 group. However, given that for many of our analyses, power across labs is more critical than
344 within a lab (Judd, Westfall, & Kenny, 2017), we allowed labs to contribute a “half sample”
345 of $N = 16$, with the assumption that this would increase the number of laboratories capable
346 of participating and allow more laboratories to contribute samples from multiple age bins.
347 We specified that labs should recruit with respect to the desired demographic characteristics
348 of the study (e.g., full-term infants; see below for full list of exclusion criteria). Given this
349 recruitment strategy, however, we asked that sample N s be calculated on the basis of the
350 number of total infants tested, not the infants retained after exclusions (which were
351 performed centrally as part of the broader data analysis, not at the lab level).

352 We included data from a lab in our analysis if they were able to achieve the minimum
353 N required for a half-sample in their age bin ($N = 16$) by the end date of testing and if, after
354 exclusions, they contributed 10 or more data points. If a lab collected more than their
355 required sample, we included the extra data as well. Laboratories were cautioned not to
356 consider the data (e.g., whether a statistically significant effect was evident) in their lab
357 internal decision-making regarding how many infants to recruit/when to stop recruitment.

358 **Participants**

359 Our final sample was comprised of 2329 monolingual infants from 67 labs (mean
360 sample size per lab: 34.76, $SD = 20.33$, range: 10 – 93; 45 contributed data at multiple

ages). Demographic exclusions were primarily implemented during recruitment; despite this, additional infants were tested and excluded based on preset criteria (see Exclusions below for percentages). In addition, 2 labs registered to participate but failed to collect data from at least 10 included infants, and so their data were not included. Information about all included labs is given in Table 1.

The mean age of infants included in the study was 291.99 days (range: 92 – 456). There were 310 infants in the 3- to 6-month-old bin (23 labs), 772 infants in the 6- to 9-month-old bin (49 labs), 554 infants in the 9- to 12-month-old bin (35 labs), and 693 infants in the 12- to 15-month-old bin (42 labs). Many labs collected data in more than one bin. Of the total sample, 1066 infants (from 30 labs) were acquiring NAE, and 1263 infants (from 37 labs) were acquiring a language other than NAE. As discussed above, a separate sample of bilingual children was tested in a parallel investigation, but these data are not reported in the current manuscript.

Table 1

Statistics of the included labs. N refers to the number of infants included in the final analysis. English from the US and Canada are both treated as North American English.

lab	Mean age (days)	<i>N</i>	Method	Language	Country
babylabbrookes	255	53	central fixation	English	UK
babylabvuw	224	15	central fixation	English	Australia
babylabyork	268	32	central fixation	English	UK
baldwinlabuoregon	320	16	central fixation	English	US
bchdosu	269	67	central fixation	English	US
bcrlnlv	411	29	central fixation	English	US
bounbel	411	31	central fixation	Turkish	Turkey
icclbc	222	15	central fixation	English	US
infantcoglablouisville	325	35	central fixation	English	US
ldlottawa	276	59	central fixation	English	Canada
madlabucsd	234	10	central fixation	English	US

minddevlabbicocca	158	15	central fixation	Italian	Italy
udssaarland	332	43	central fixation	German	Germany
unlvmusiclab	138	20	central fixation	English	US
weescienceedinburgh	213	32	central fixation	English	UK
wsigoettingen	274	88	central fixation	German	Germany
infantcogubc	165	39	central fixation, eye tracking	English	Canada
lancaster	326	42	central fixation, eye tracking	English	UK
babylablangessex	289	27	eye tracking	English	UK
babylablmu	368	62	eye tracking	German	Germany
babylabshimane	195	28	eye tracking	Japanese	Japan
babylabuclajohnson	408	22	eye tracking	English	US
babylabumassb	308	30	eye tracking	English	US
babylingoslo	227	31	eye tracking	Norwegian	Norway
callab	369	30	eye tracking	English	US
cdcecu	272	27	eye tracking	Hungarian	Hungary
cfnuofn	298	15	eye tracking	English	Australia
childlabmanchester	269	26	eye tracking	English	UK
cogdevlabbyu	161	29	eye tracking	English	US
dcnlabtennessee	345	19	eye tracking	English	US
earlysocogfm	310	35	eye tracking	English	US
escompicbsleipzig	159	14	eye tracking	German	Germany
ethosrennes	187	90	eye tracking	French	France
irlconcordia	310	37	eye tracking	English	Canada
jmucdl	340	17	eye tracking	English	US
kokuhamburg	305	25	eye tracking	German	Germany
kyotobabylab	281	30	eye tracking	Japanese	Japan
labunam	302	36	eye tracking	Spanish	Mexico
lcdfsu	354	23	eye tracking	English	US
lcduleeds	413	14	eye tracking	English	UK
lllliv	302	36	eye tracking	English	UK
lscppsl	404	14	eye tracking	French	France
pocdnorthwestern	409	30	eye tracking	English	US
socialcogumiami	131	19	eye tracking	English	US

weltentdeckerzurich	414	30	eye tracking	German	Switzerland
nusinfantlanguagecentre	337	21	eye tracking, central fixation	Mandarin	Singapore
babylabkingswood	312	32	HPP	English	Australia
babylabkonstanz	235	15	HPP	German	Germany
babylableiden	319	15	HPP	Dutch	Netherlands
babylabnijmegen	279	49	HPP	Dutch	Netherlands
babylabparisdescartes1	403	16	HPP	French	France
babylabplymouth	332	34	HPP	English	UK
babylabprinceton	307	24	HPP	English	US
babylabutrecht	276	61	HPP	Dutch	Netherlands
bllumanitoba	281	79	HPP	English	Canada
chosunbaby	313	77	HPP	Korean	Korea
infantlanglabutk	323	65	HPP	English	US
infantllmadison	316	93	HPP	English	US
infantstudiesubc	228	20	HPP	English	Canada
islnotredame	411	28	HPP	English	US
isplabmcgill	411	11	HPP	French	Canada
langlabucla	250	63	HPP	English	US
lppparisdescartes2	241	30	HPP	French	France
musdevutm	229	31	HPP	English	Canada
purdueinfantspeech	355	58	HPP	English	US
trainorlab	241	24	HPP	English	Canada
babylabpotsdam	306	46	HPP, central fixation	German	Germany

374

375 **Materials**

376 **Visual stimuli.** For labs using central fixation or eye tracking methods, a brightly
377 colored static checkerboard was used as the fixation stimulus, and a small engaging video (an
378 animation of colorful rings decreasing in size) as an attention-getter. For labs using HPP, we

379 asked labs to use their typical visual stimulus, which varied considerably across laboratories.
380 Some labs used flashing lights as the visual fixation stimulus (the original protocol that was
381 developed in the 1980s), while others used a variety of other visual displays on video screens
382 (e.g., a looming circle).

383 **Speech stimuli.** The goal of our stimulus creation effort was to construct a set of
384 recordings of naturalistic IDS and ADS gathered from a variety of mothers speaking to their
385 infants. To do so, we gathered a set of recordings of mothers speaking to their infants and to
386 experimenters, selected a subset of individual utterances from these (see below), and then
387 constructed stimulus items from this subset. All other characteristics of the recordings
388 besides register (IDS vs. ADS) were as balanced as possible across clips. Based on our
389 intuitions and the data from the norming ratings described below, we consider these stimuli
390 to be representative of naturally produced IDS and ADS across middle- and high-SES
391 mothers in North America. Although future studies could attempt to vary particular aspects
392 of the IDS systematically (e.g., age of the mother, age of the infant being spoken to, dialect),
393 we did not do so here. Our stimulus elicitation method was designed to meet the competing
394 considerations of laboratory control and naturalism.

395 Source recordings were collected in two laboratories, one in central Canada and one in
396 the Northeastern United States. The recorded mothers had infants whose ages ranged from
397 122 – 250 days. The same recording procedures were followed in both laboratories.
398 Recordings were collected in an infant-friendly greeting area/testing room using a simple
399 lapel clip-on microphone connected to a smartphone (iPhone 5s or 6s), with the “Voice
400 Record” or “Voice Record Pro” apps (Dayana Networks Ltd.) in the Canadian lab, and the
401 “Voice Memos” app (Apple Inc.) in the US lab. The targets for conversation were objects in
402 an opaque bag: five familiar objects (a ball, a shoe, a cup, a block, a train) and five
403 unfamiliar objects (a sieve, a globe, a whisk, a flag, and a bag of yeast). To ensure that
404 mothers used consistent labels, a small sticker was affixed to each object showing its name.

405 Each object was taken out of the bag one at a time and the mother was asked to talk about
406 the object, either to her baby (for the IDS samples) or to an experimenter (for the ADS
407 samples) until she ran out of things to say; at this point the next object was taken out of the
408 bag. Recording stopped when all the objects had been removed from the bag and had been
409 talked about. Order of IDS and ADS recording was counterbalanced across participants. A
410 total of 11 mothers were recorded in Canada and four in the United States.

411 There were a total of 179 unedited minutes of recording from Canada and 44 from the
412 United States. A first-pass selection of low-noise IDS and ADS samples yielded 1281
413 utterances, for a total of 4479 s. From this first pass, 238 utterances were selected that were
414 considered to be the best examples of IDS and ADS and met other basic stimulus selection
415 criteria (e.g., did not contain laughter or the baby’s name).

416 This library of 238 utterances was then normed on five variables: accent, affect,
417 naturalness, noisiness, and IDS-ness. The goal of this norming was to gather intuitive
418 judgments about each variable so as to identify utterances that were clearly anomalous in
419 some respect and exclude them. In each case, a set of naïve, North American
420 English-speaking adults recruited from Amazon Mechanical Turk (MTurk) listened to all 238
421 of the utterances and rated them on a 7-point Likert scale. Raters were assigned randomly
422 to one of the five variables, with the number of participants assigned to a particular rating
423 task ranging between eight and 18 due to variability in random assignment. Affect and IDS
424 ratings were made using low-pass filtered recordings (a 120-Hz filter with standard rolloff was
425 applied twice using the `sox` software package). These ratings were intended to give us a
426 principled basis on which to exclude clips that were outliers on particular dimensions (such
427 as having odd affect or background noise). In general, with the exception of IDS-ness,
428 ratings were not highly variable across clips (the largest *SD* was .85, for noise ratings).

429 Ratings from the tasks were then used to produce a set of utterances such that accent
430 was rated similar to “standard English” (ratings < 3, with 1 being completely standard),

431 naturalness was rated high (> 4 , with 7 being completely natural), noisiness was rated low
432 (< 4 , with 1 being noiseless), and IDS and ADS clips were consistently distinguished (with
433 IDS having ratings > 4 and ADS having ratings < 4 , with 7 being clearly directed at a baby
434 or child). This procedure resulted in 163 total utterances that met our inclusion criteria.

435 Our next goal was to create eight IDS and eight ADS stimuli that were exactly 18 s in
436 length, each containing utterances from the set we created. To do so, we assembled
437 utterances from our filtered set. All clips were root mean square amplitude-normalized to 70
438 dB sound pressure level (SPL) before assembly, and then the final stimuli were
439 amplitude-renormalized to 70 dB SPL. We assembled the final stimuli considering the
440 following issues:

- 441 • *Identity.* Audio stimuli were constructed using clips from more than one mother. The
442 number of different mothers included in a given stimulus was matched across IDS and
443 ADS stimuli. In addition, multiple clips from the same mother were grouped together
444 within a given stimulus in order to match the number of “mother transitions” across
445 registers.
- 446 • *Lexical items.* We matched the presence of object labels in the clips across IDS and
447 ADS contexts. We also ensured an even distribution of the order in which each
448 particular word was presented across stimuli and registers (ADS vs IDS).
- 449 • *Questions.* IDS tends to include a much higher proportion of questions compared with
450 ADS (Snow, 1977; Soderstrom, Blossom, Foygel, & Morgan, 2008). However, because
451 the nature of the recording task may have served to inflate this difference, we
452 preferentially selected declaratives over questions in the IDS sample. The final stimulus
453 set contained 47% questions in the IDS samples and 3% questions in the ADS samples.
454 We felt that retaining this naturally-occurring difference in IDS and ADS within our
455 stimuli was more appropriate than precisely and artificially controlling for

utterance-type across registers.

- *Duration of individual clips.* As expected, the utterances in IDS were much shorter than those in ADS, so it was not possible to match on duration or number of clips. Because there were more clips per stimulus in the IDS samples, there were also more utterances boundaries. This property is consistent with the literature on the natural characteristics of IDS (Martin, Igarashi, Jincho, & Mazuka, 2016).
- *Total duration.* We fixed all stimuli to have a total duration of 18 s by concatenating individual utterance files into single audio files that were > 18 s in length, trimming these down to 18 s and fading the audio in and out with 0.5 s half-cosine windows.

Table 2 and Figure 1 provide additional details regarding the final stimulus set. Measurements were made using STRAIGHT (Kawahara & Morise, 2011), using default values for F0 extraction. For Figure 1, F0 values for voiced portions of the stimuli were collapsed into a series of logarithmically-spaced bins spanning the algorithm's F0 search range of 32-650 Hz.

Table 3 provides a comparison of our stimuli to a sample of others that have been used previously in the IDS preference literature. Across studies, the only statistic that was reported reliably across papers was the mean pitch (F0) for IDS and ADS and even this one was only reported in about half the studies we sampled. Various measures of variability were reported in some studies (e.g., range within each sample, range across samples, standard deviation), but due to variation in the length and number of different samples used in each study, and a lack of systematicity in reporting, it was difficult to compare directly. Numerically, the average IDS/ADS pitch difference in our materials was less extreme than that found in previous studies.

To confirm that our composite IDS and ADS stimuli were rated as natural and that the more limited pitch difference between registers still led to the stimuli being categorized

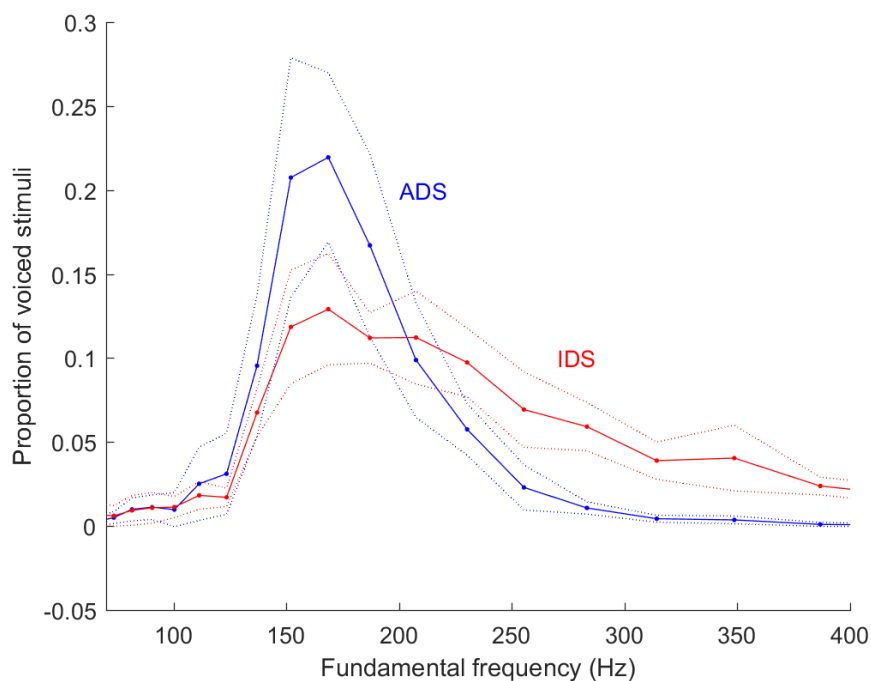


Figure 1. The distribution of F0 values for IDS and ADS is displayed as the proportion of voiced segments that fell in each F0 bin. Dashed lines show mean plus or minus one standard error across stimuli.

481 differently, we conducted another norming study. Using the same basic paradigm as above,
 482 we collected a new sample of judgments from MTurk participants. Raters were randomly
 483 assigned to listen to all 16 stimuli and judge either whether they were directed at
 484 infants/children or adults ($N = 22$) or else whether the stimuli sounded natural ($N = 27$).
 485 All IDS clips were judged extremely likely to be directed at infants or children ($M = 6.74$,
 486 $SD = .09$, on a 1 – 7 rating scale), while all ADS clips were judged highly likely to be
 487 directed to adults ($M = 2.12$, $SD = .38$). Both were judged to be relatively natural, with
 488 the ADS, if anything, slightly more natural ($M = 5.18$, $SD = .19$) than the IDS ($M = 4.47$,
 489 $SD = .31$). In sum, because our stimuli were created from naturalistic productions from a
 490 wide range of mothers, they were less extreme in their intonation, but they were judged as
 491 natural and were easily identified as infant-directed.

Table 2

Characteristics of the IDS and ADS stimuli, with standard deviations computed across stimuli.

Measurement	IDS Mean	IDS <i>SD</i>	ADS Mean	ADS <i>SD</i>
Number of mothers speaking per stimulus	4.00	0.00	3.75	0.46
Number of clips per stimulus	6.88	1.13	4.50	0.76
Number of objects mentioned per stimulus	2.75	0.71	2.75	0.71
Mean F0 (Hz) per stimulus	206.90	19.50	174.90	13.20
10th percentile F0 (Hz) per stimulus	131.40	26.10	139.00	17.70
90th percentile F0 (Hz) per stimulus	340.00	21.50	232.00	13.80
Mean number of utterances per stimulus	7.75	1.04	6.63	0.92
Mean duration (sec) of utterances	1.58	0.74	2.12	1.41
Mean inter-utterance interval (sec)	0.75	0.30	0.59	0.33

Table 3
Comparison of our study's stimuli to those of previous studies on infant-directed speech preferences.

Study	Mean Ages (Months)	Context of Recording	Quantity of Stimuli	Mean IDS F0 (Hz)	Mean ADS F0 (Hz)	IDS-ADS (Hz)	IDS/ADS
Present Study	3 – 15	semi-structured, 4-8 month old child present	8 full trial lengths ¹ worth for each type	206.90	174.90	32.00	1.18
Cooper & Aslin (1990)	0, 1	read speech, no infant present	4 sentences produced in each type	315.88	259.58	56.30	1.22
Newman & Hussain (2006)	4.5, 9, 13	read speech, no infant present	4 passages produced in each type	225.70	189.65	36.05	1.19
Thitessen et al. (2005)	7	nonsense strings of syllables, no infant present	12 sentences in each style	292.00	230.00	62.00	1.27
Cooper et al. (1997)	1, 4	naturalistic speech to own infants	20s of each style	219.30	184.30	35.00	1.19
Schachner & Hammon (2011)	5	elicited speech, with speaker looking at a picture	1 min long videos, 2 in each style	273.00	224.70	48.30	1.21

492 Procedure

493 **Basic Procedure.** Each lab used the testing paradigm(s) with which they were
494 most familiar, among variants of three widely-used measurement methods: 20 laboratories
495 used the HPP, 16 used the single-screen central visual-fixation preference procedure (CF),
496 and 27 used single-screen central visual fixation with fixations recorded by a
497 corneal-reflection eye tracker (ET); four labs contributed data using two different methods.
498 All procedural instructions to participant labs can be found at <https://osf.io/s3jca/>.

499 To minimize researcher degrees of freedom, we asked participating labs to adhere to
500 our instructions closely. Deviations from the basic protocol for each paradigm were necessary
501 in some cases due to variation in the software and procedures used in each laboratory and
502 were documented for future analysis.

503 **1st vs. 2nd test session.** In some laboratories, infants were sometimes tested in an
504 unrelated experiment during their visit, either prior to or following the IDS preference
505 experiment. Each lab noted whether infants completed the IDS preference experiment as
506 their 1st (and possibly only) or 2nd test session.

507 **Onset of each trial.** At the beginning of each trial, a centrally positioned visual
508 stimulus (typically the study's standard attention getter, or a light in some HPP labs) was
509 used to attract the infant's attention. Upon fixation, this event was followed by a visual
510 stimulus (a checkerboard for CF and ET, a light or a similar video for HPP). The stimulus
511 appeared to the left or right of the infant in HPP setups and in the center in CF and ET
512 setups.

513 **Trials.** At the beginning of the session, there were two warm-up trials that
514 familiarized infants with the general procedure. The auditory stimulus for warm-up trials
515 was an 18-second clip of piano music, and the visual stimulus was identical to the test trials.

516 These trials familiarized infants to the general experimental setup and highlighted the
517 contingency between looking at the visual display and the onset of the auditory stimulus.
518 We did not analyze data from these trials. Training trials were then followed by up to 16 test
519 trials presenting the IDS and ADS auditory stimuli.

520 **Minimum looking time.** There was no minimum required looking time during data
521 collection (i.e., trials were never repeated). A minimum looking time of 2 s was used during
522 analysis for inclusion of a trial. The 2-s minimum trial time was chosen after discussion
523 across laboratories regarding typical standards of practice on minimum trial length, which
524 varied considerably across laboratories. This criterion was selected to ensure that the infant
525 had sufficient time to hear enough of the stimulus to discriminate IDS from ADS.

526 **Maximum looking time.** On each test trial, infants could hear speech for a
527 maximum of 18 s, corresponding to the duration of each sound file. For labs whose software
528 could implement infant-controlled trial lengths, the trial ended if the infant looked away
529 from the visual stimulus for two consecutive seconds. Otherwise, the trial continued until the
530 stimulus ended.

531 **Randomization.** Four pseudo-random trial orders were created. Each order
532 contained four blocks, with each block containing two IDS and two ADS trials in alternating
533 order. Two blocks in each order began with IDS and the other two began with ADS. To
534 facilitate analyses of preference scores by item, the same IDS and ADS stimuli were always
535 paired with one another.

536 **Volume.** Each lab was asked to use a stimulus volume level that was consistent with
537 their general lab practices – this decision was not standardized across labs. Labs were
538 instead instructed to measure and report their average dB SPL level with and without a
539 white noise reference audio clip playing, though not all contributing labs reported these
540 measurements ($N = 47$). From these values, we calculated a signal to noise ratio for each lab,

541 $M = 1.95$, $SD = 0.43$, range: 1.25 – 3.30.

542 **Minimizing caregiver bias.** We created a custom blend of instrumental music and
543 a pastiche of stimulus materials triggered at random times and with random amplitude
544 (available as part of the study materials). This masking stimulus was played to the caregiver
545 over noise-attenuating headphones, to mask the IDS/ADS stimuli that the infant was
546 hearing via external loudspeakers. Experimenters were instructed to play the masking music
547 at a high (but comfortable and safe) volume.

548 **Coding.** Coding of looking times was conducted via the standard procedure in each
549 lab. There were three methods of coding infant eye gaze: online coding by an experimenter
550 via button press during the experimental session, offline coding of a video after the
551 experimental session, or automatic coding collected by an eye tracker. In the case that we
552 received online and offline coding data, we used the offline coding.

553 **Minimizing experimenter bias.** Experimenters making online coding decisions (in
554 CF and HPP methods) were blind to the particular stimulus presented during testing trials,
555 as they were either located in a different room from the infant, or were in the same room but
556 were wearing noise-attenuating headphones and hearing the same masking stimuli as the
557 infant’s caregiver. Offline coding was conducted without direct access to the auditory stimuli.

558 **Demographics.** All labs were instructed to collect a set of basic participant
559 demographic information: sex, date of birth, estimated proportion language exposure for the
560 language(s) that they hear in their daily life, race/ethnicity (using categories appropriate for
561 the cultural and geographic context), preterm/fullterm status, history of ear infections,
562 known hearing or visual impairments, and known developmental concerns (e.g.,
563 developmental disorders). Parents were also asked to report information about themselves
564 (gender, level of education, and native language/languages) and the child’s siblings
565 (sex/gender and date of birth). A standard recommended participant questionnaire was

566 distributed to participating labs as part of the instructions, although labs were permitted to
567 use their own forms as long as they gathered the necessary information. In addition, a subset
568 of participating laboratories provided extensive additional information about infants and
569 testing circumstances (not analyzed here), for use in planned followup projects.

570 **General Lab Practices**

571 **Training of research assistants.** Each lab was responsible for maintaining good
572 experimenter training practices, and was expected to use the same rigor with the
573 ManyBabies study as with any other study in their laboratory. Laboratories reported on
574 which research assistant ran each infant using pseudonyms or numerical codes. Each
575 laboratory completed a questionnaire regarding their training practices, the experience and
576 academic status of each experimenter, and their basic participant greeting practices.

577 **Reporting of technology mishaps and infant/parent behavior.** Laboratories
578 were asked to note relevant concerns, anomalies and comments according to their standard
579 lab practices and these were provided along with the looking time data and converted to a
580 standardized form during the central analysis. Examples of relevant concerns included the
581 infant crying during testing, parents intervening in a way that would affect their infant's
582 looking behavior (e.g., talking or pointing), or technical problems that prevented the normal
583 presentation of experimental stimuli.

584 **Videos**

585 All laboratories provided a “walk-through” video that detailed their basic processes
586 including greeting, consent and data collection and showing the physical characteristics of
587 their laboratory. (In our preregistration we stated that further procedural documentation
588 would be available, but standardized reporting for procedural decision-making proved

589 difficult to develop and deploy.) In addition, we strongly encouraged laboratories to collect
590 and share video recordings of their data collection according to what was permissible given
591 their ethics approval and participant consent. If labs could not provide participant videos,
592 they were asked to provide a video showing a run-through of their procedure and/or pictures
593 and information regarding the study setup. A number of laboratories contributed these video
594 recordings to Databrary, where they can be found by searching for “ManyBabies 1.”

595 **Exclusion Criteria**

596 All data collected for the study (i.e., every infant for whom a data file was generated,
597 regardless of how many trials were completed) were given to the analysis team for
598 confirmatory analyses. Participants were only included in analysis if they met all of the
599 criteria below. All exclusion rules are applied sequentially, and percentages reflect this
600 sequential application to an initial sample prior to exclusions of 2754. N.B.: the first three
601 criteria preemptively prevent participation (except in case of erroneously running the
602 experiment with children outside of the inclusion guidelines).

- 603 • *Monolingual.* Monolingual infants of any language background were included in the
604 sample. Monolingual was defined as 90% parent-reported exposure to the native
605 language. This cutoff score struck a balance between including most infants who are
606 typically considered monolingual in infant language studies, while excluding those who
607 might be considered bilingual (Byers-Heinlein, 2015). 162 (5.88%) infants were tested
608 but did not meet this criterion.
- 609 • *Full-term.* We defined full term as gestation times greater than or equal to 37 weeks.
610 Of the remaining sample, 62 (2.39%) infants were tested but did not meet this criterion.
- 611 • *No diagnosed developmental disorders.* We excluded infants with parent-reported
612 developmental disorders (e.g., chromosomal abnormalities) or diagnosed hearing

613 impairments. Of the remaining sample, 2 (0.08%) infants were tested but did not meet
614 this criterion. Due to concerns about the accuracy of parent reports, we did not
615 exclude infants based on parent-reported ear infections unless parents reported
616 medically-confirmed hearing loss.

617 • *Contributed usable data.* A child must have contributed non-zero looking time on a
618 pair of test trials (i.e., one trial each of IDS and ADS from a particular stimulus pair),
619 after trial-level exclusions were applied, to be included in the study. Of the remaining
620 sample, 41 (1.65%) infants were tested but did not meet these criteria. We adopted
621 this relatively liberal inclusion criterion even though it is at variance with the more
622 stringent standards that are typically used in infancy research. We were interested in
623 maximizing the amount of data from each lab we were able to include in the initial
624 analysis, and our paradigm was, by design, less customized for any particular age
625 group (and hence likely to produce greater data loss, especially for older children, who
626 tend to habituate more quickly). In the exploratory analyses below, we consider how
627 exclusion decisions affected our effect size estimates.

628 After these exclusions were applied, participants could also be excluded for analysis
629 based on session-level errors, including: equipment error (e.g., no sound or visuals on the
630 first pair of trials), experimenter error (e.g., an experimenter was unblinded in setups where
631 infant looking was measured by live button press), or evidence of consistent parent/outside
632 interference noted by participating labs (e.g., talking or pointing by parents, construction
633 noise, sibling pounding on door). 78 (3.18%) infants for whom we had other reported data
634 were dropped from analysis due to session-level error. This number is likely an underestimate,
635 however. Many participating labs did not provide data for all children with session-level
636 errors; in addition, session-level errors were not classified consistently across labs, so an
637 accurate classification of the proportion of different types of errors was not possible.

638 We further excluded individual trials that were reported as having issues (e.g.,

639 fussiness, incorrect stimulus, single instance of parent or sibling interference). A total of 4471
640 (10.61%) trials were affected by such errors. As with session level errors, classification of
641 these was inconsistent across participating labs, but the most common source of trial-level
642 errors was infant fussiness.

643 Based on our trial-length minimum, we also excluded 6027 (16.13%) trials with total
644 looking times shorter than 2 s. These trials are analyzed as “missing” in our planned
645 analysis below.

646 As discussed above, we included a lab’s data if they were able to achieve the minimum
647 N required for a half-sample and if, after exclusions, they contributed 10 or more data points.
648 11 (0.47%) infants from 2 labs were not included in the final sample because of this criterion.

649 **Post-Data Collection Methods Addendum**

650 As the first experimental cross-laboratory infant study of this scale, there were a
651 number of unanticipated issues that arose during data collection within individual labs and at
652 the study level, which resulted in deviations from our registered protocol. All such cases were
653 documented and decisions were made without consideration of their impact on the results.
654 Fuller documentation can be found accompanying our shared data; here we summarize the
655 nature and extent of these deviations. Note that some of these deviations were the result of
656 typical within-laboratory protocol deviation (experimenter error, etc.) while others stemmed
657 from the additional challenges inherent in harmonizing methodology and data format across
658 such a large number of laboratories with different lab-internal protocols and standards.

659 These protocol deviations include the following:

- 660 • Before labs had commenced data collection, we altered our attention-getter stimulus to
661 be a precessing annulus accompanied by chimes (to address the concern that a

662 laughing baby might be more associated with infant-directed speech); some labs used
663 the old stimulus.

- 664 • Variation in trial length beyond the assumed maximum of 18 s emerged due to
665 deviations in lab's protocols for a variety of reasons. In all cases, looking times on
666 these trials were truncated to 18 s.
- 667 • A number of labs provided data from infants that were within the 3–15 month age
668 range, but outside of the submitting lab's pre-registered age bin. These infants were
669 included in the analyses.
- 670 • Many labs deviated from their pre-registered sample size due to constraints on testing
671 resources. We included these labs provided they met the minimum inclusion criteria for
672 the study as a whole. All such labs certified that they did not make decisions regarding
673 sample size on a data-dependent basis.
- 674 • A number of laboratories marked participants as session-level errors for reasons other
675 than equipment error, experimenter error or outside interference.

676 This last point bears further discussion. Some labs marked participants as exclusions
677 at the participant level for trial-level errors (e.g. infant fussy, parental interference), even
678 though there was sufficient trial-level data available for analysis. Similarly, individual trials
679 were sometimes marked as errors for reasons related to participant-level issues. All trial-level
680 and participant-level errors were reviewed centrally by at least two coders using all available
681 information in the spreadsheet to determine whether a trial-level or participant-level error
682 was appropriate. Specific information about each trial or participant error coding that was
683 changed during this process can be found by reviewing metadata within the data analysis
684 codebase.

685 In total, 313 participants from 50 labs previously marked as participant-level exclusions
686 were retained for further processing and analysis. Participants originally coded as having
687 session-level errors were recoded for the following reasons: when the participant-level

688 exclusion was based solely on the existence of trial-level errors (190 infants), when exclusion
689 was based on a different exclusion criterion (e.g., participants were out of the age range or
690 were preterm) (93 infants), or if an issue identified by the lab at the participant level was
691 deemed acceptable by the central analysis team (e.g., if a lab implemented a slightly different
692 look-away criterion, see below) (30 infants). Note that many of the retained participants
693 were subsequently excluded at other points in the analysis pipeline because, although they
694 did not meet the criteria for session-level errors, they did meet the conditions for other
695 exclusion criteria (e.g., participants did not contribute enough useable trials or were excluded
696 based on language exposure).

697 In addition to recoding session-level errors, we also corrected the coding of trial-level
698 errors where appropriate. 778 total trial-level errors from 62 participants in 16 different labs
699 were recoded. The majority of trials were corrected when labs coded a participant-level error
700 (e.g. age exclusion) on the trial level (584 trials) or coded a trial-level error on the
701 participant level (e.g., if labs marked a participant as a session-level error for fussiness on a
702 specific trial, but did not code the affected trials as errors) (133 trials). Other trials were
703 corrected when subsequent investigation of lab notes and discussion with lab members
704 revealed that the original trial-level error code needed to be changed (61 trials).

705 In addition, a variety of errors were found (e.g., pilot participants not properly
706 excluded but noted in the comments) and fixed within the spreadsheets. Video data were
707 not reviewed centrally, although in some cases where a question arose, the laboratory
708 reviewed their own video in-house in order to respond. The entire process has been carefully
709 documented and can be accessed upon request, but because in some cases this included
710 identifiable information about participants, it is not possible to share it publicly.

711 Other reported protocol deviations included: No preregistration form submitted (1
712 lab); trial look-away time set to 3 s for some participants (1 lab); lab temporarily moved
713 location during data collection (1 lab); minor protocol technical changes after start of data

714 collection (2 labs); alternated left-right presentation and tested skin conduction during
715 procedure (1 lab); procedural differences related to high-chair usage (1 lab); attention-getter
716 deviation (4 labs); use of a pinwheel rather than checkerboard as the main visual fixation
717 stimulus in HPP (1 lab).

718 We also detected a large number of data submission errors (typographical or otherwise)
719 as a result of the comprehensive checking process in analysis. These were resolved when
720 necessary by contacting the original lab. In general, we were inclusive of data with minor
721 protocol deviations, and erred on the side of excluding data, when necessary, at the trial
722 rather than participant level. A few demographic variables required greater central scrutiny
723 than originally anticipated. Most notably, there was considerable variability in the
724 interpretation of preterm and bilingual designations (despite centrally-dictated standards).
725 When necessary, we recoded lab data so as to conform to the original protocol definitions.

726 There was an ambiguity in our lab-level exclusion criteria between whether labs would
727 be included if they contributed 10 or more datapoints, or more than 10 datapoints. We chose
728 the more liberal of these two criteria.

729 Finally, two labs submitted data after the deadline. In one case this was due to a
730 communication error; in the other case, the lab continued data collection, resulting in 8
731 additional infants being tested. Both datasets are included in the final analysis here.

732 Results

733 Confirmatory Analyses

734 **Data processing and analytic framework.** All planned analyses were
735 pre-registered in our initial registered report submission (available at <https://osf.io/vd789/>).
736 Our primary dependent variable of interest was looking time (LT). Looking time was defined

737 as time spent fixating the screen (for central fixation and eye tracking methods, and some
738 HPP set-ups) or light (HPP) during test trials; LT scores did not count any time spent
739 looking away from the screen, even if looks away were below the threshold for terminating a
740 trial. Since looking times are non-normally distributed, following Csibra, Hernik, Mascaro,
741 Tatone, and Lengyel (2016), we log-transformed all looking times prior to statistical analysis
742 (we refer to this transformed variable as “log LT”).

743 We adopted two complementary analytic frameworks: meta-analysis and mixed-effects
744 regression. In the meta-analytic framework, we conducted standard analyses within each lab
745 and then estimated variability in the result of this analysis across labs. The meta-analytic
746 approach has a number of advantages over the mixed-effects approach, including the use of
747 simple within-lab analyses, the ability to estimate cross-lab variability directly, and the
748 possibility of making direct comparisons with the standardized effect sizes that have been
749 estimated in previous meta-analyses. However, the standard random-effects meta-analytic
750 model is designed for a case where the raw data are unavailable and procedures and
751 data-types are not standardized. In contrast, in our situation, procedures and data were
752 standardized across labs and relevant moderators were recorded. The availability of
753 trial-by-trial data across all labs allows us to use mixed-effects models, which account for the
754 nesting and crossing of random effects (e.g., subjects nested within labs, items crossed across
755 labs), and can provide more accurate estimates of the main effect and moderators. Both
756 analyses were therefore included to allow for the most comprehensive understanding of the
757 variance in the data.

758 Our meta-analyses were conducted as follows. The datasets provided by each lab were
759 considered as separate “studies.” For each lab’s dataset, we first computed individual infants’
760 IDS preference by 1) subtracting looking times to each IDS trial from its paired ADS trial
761 (excluding trial pairs with missing data) and 2) computing a mean difference score (across
762 trial pairs). Then we computed a group IDS preference for each lab and infant age group

763 using dz , a version of Cohen’s standard d statistic, computed as the average of infants’ IDS
764 preference scores divided by the standard deviation of those scores. We then used standard
765 random effects meta-analysis fit using REML with the `metafor` package (Viechtbauer, 2010).

766 In our initial analysis plan, we did not anticipate that a large number of labs would
767 collect data outside of their planned samples. For example, many labs contributed a sample
768 of children within a specific age bin as well as several children that fell outside of that age
769 bin, or a sample of children using one method and a handful of children with another. While
770 we include these children in the mixed-effects analyses described below, we worried that the
771 inclusion of many unplanned samples of just one or two infants in the meta-analytic models
772 would excessively increase lab-level variance. Thus, for only the meta-analyses, we include
773 only samples (e.g., age, language, or method groups) with ten or more infants.

774 Our mixed effects models, fit to the entire dataset collected from the 67 labs, were
775 specified as:

$$DV \sim IV_1 + IV_2 + \dots + (\dots|\text{subject}) + (\dots|\text{item}) + (\dots|\text{lab})$$

776 The goal of this framework was to examine effects of the independent variables
777 (notated IV) on the dependent variable (DV), while controlling for variation in both the DV
778 (“random intercepts”) and the relationship of the IV to the DV (“random slopes”) based on
779 relevant grouping units (subjects, items, and labs). The use of mixed-effects models also
780 allowed us to move away from using difference scores as the dependent variable of interest.
781 While difference scores simplify the process of calculating effect sizes for the meta-regression,
782 their use requires that trials be paired, so some collected data (i.e., unpaired trials) cannot
783 be analyzed. In the mixed effects framework, in contrast, looking time on individual trials is
784 the dependent measure, ensuring that all trials can be included.

785 In our mixed-effects models, we planned a maximal random effects structure (Barr,
786 Levy, Scheepers, & Tily, 2013), which entails specifying all random effects that are
787 appropriate for the experimental design (e.g., IDS/ADS trial type can be nested within
788 subjects – since each infant heard stimuli in both conditions — but cannot be nested within
789 items since each item is unique to its trial type). In cases of mixed-effects models that failed
790 to converge, we pursued an iterative pruning strategy. We began by removing random slopes
791 nested within items (as that grouping was of least theoretical interest) and next removing
792 random slopes nested within subjects and then labs. We then removed random intercepts
793 from groupings in the same order, retaining effects of trial type until last since these were of
794 greatest theoretical interest. We fit all models using the `lme4` package (Bates, Mächler,
795 Bolker, & Walker, 2015) and computed p values using the `lmerTest` package (Kuznetsova,
796 Brockhoff, & Christensen, 2017).

797 **IDS preference.** What was the overall magnitude of the IDS preference we
798 observed? This question is answered within the cross-lab meta-analysis by fitting the main
799 effect model specified by $dz \sim 1$ to the 108 separate group means and variances (after
800 aggregating by lab and age group). The mean effect size estimate was 0.35 (CI = [0.29 -
801 0.42], $z = 10.67$, $p < .001$). A forest plot for this meta-analysis is shown in Figure 2. Further,
802 1373/2329 infants (58.95%) showed a numerical preference for IDS.

803 **Independent relationship of IDS preference to moderating variables.** We
804 next fit a set of moderated meta-analytic models. We began by examining the relationship of
805 IDS preferences to age, using the average age in months for each lab’s contributed sample as
806 the moderator value. Labs that contributed samples from two age bins had values added
807 separately for each age (because of the small number of these, we did not model this
808 dependency between labs). For ease of interpretation, we centered age in this analysis. The
809 age-moderated model, $dz \sim 1 + \text{age}$, yielded an estimated main effect of 0.35 (CI = [0.29 -
810 0.41], $z = 11.47$, $p < .001$) and an age effect of 0.05 (CI = [0.03 - 0.07], $z = 4.89$, $p < .001$).

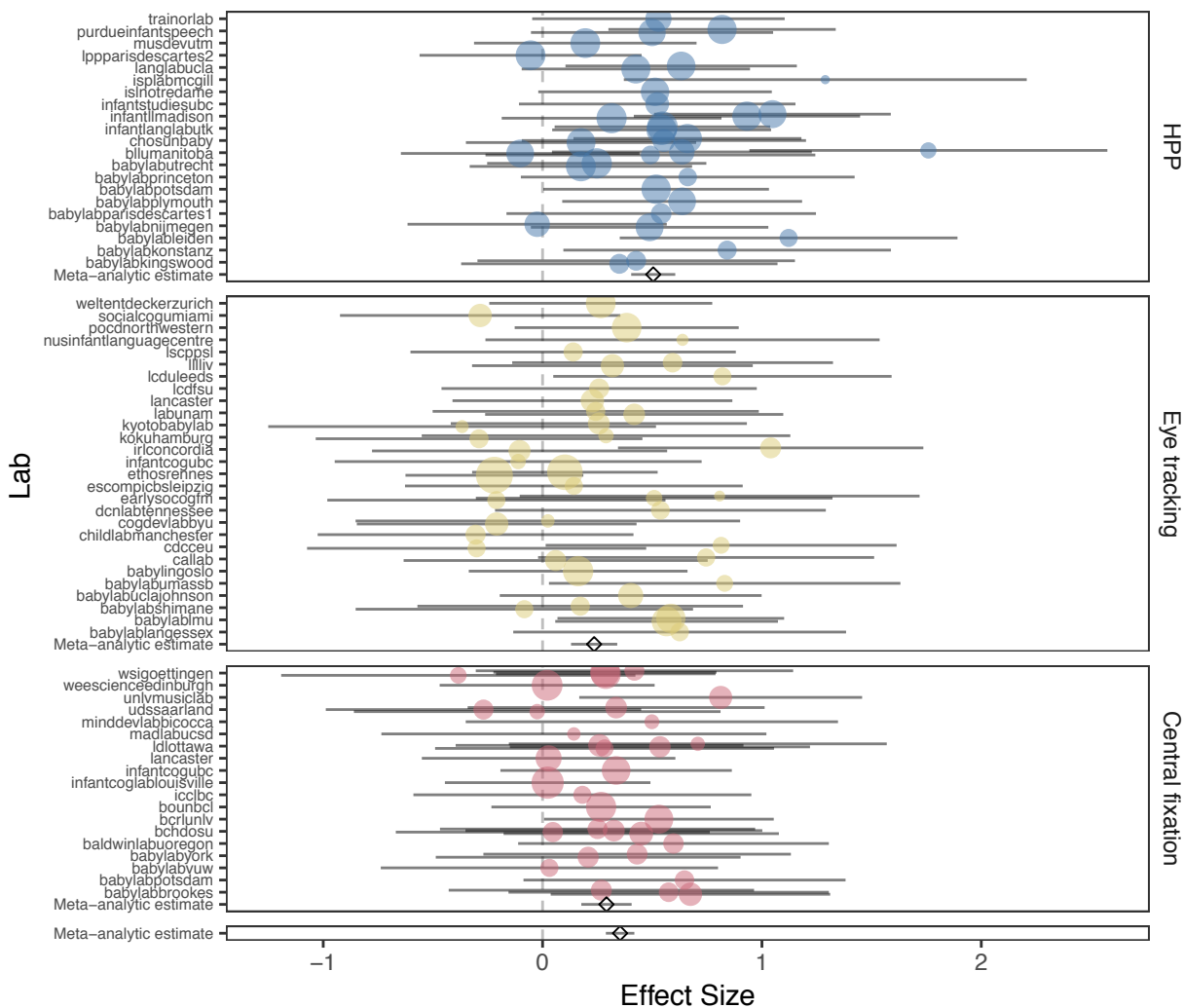


Figure 2. Forest plot. Standardized effect sizes are shown for each lab, with error bars showing 95% confidence intervals. Labs are grouped by method. Points are scaled by inverse variance and colored by experimental method. In each panel, the diamond and associated interval represents the meta-analytic estimate from the method-moderated model and its 95% confidence interval. The bottom panel shows the global meta-analytic estimate from the unmoderated model.

811 This positive age coefficient indicated that the measured IDS preference was on average
 812 larger for older children. Age trends are plotted in Figure 3.

813 We next investigated effects of experimental method, with method dummy-coded using

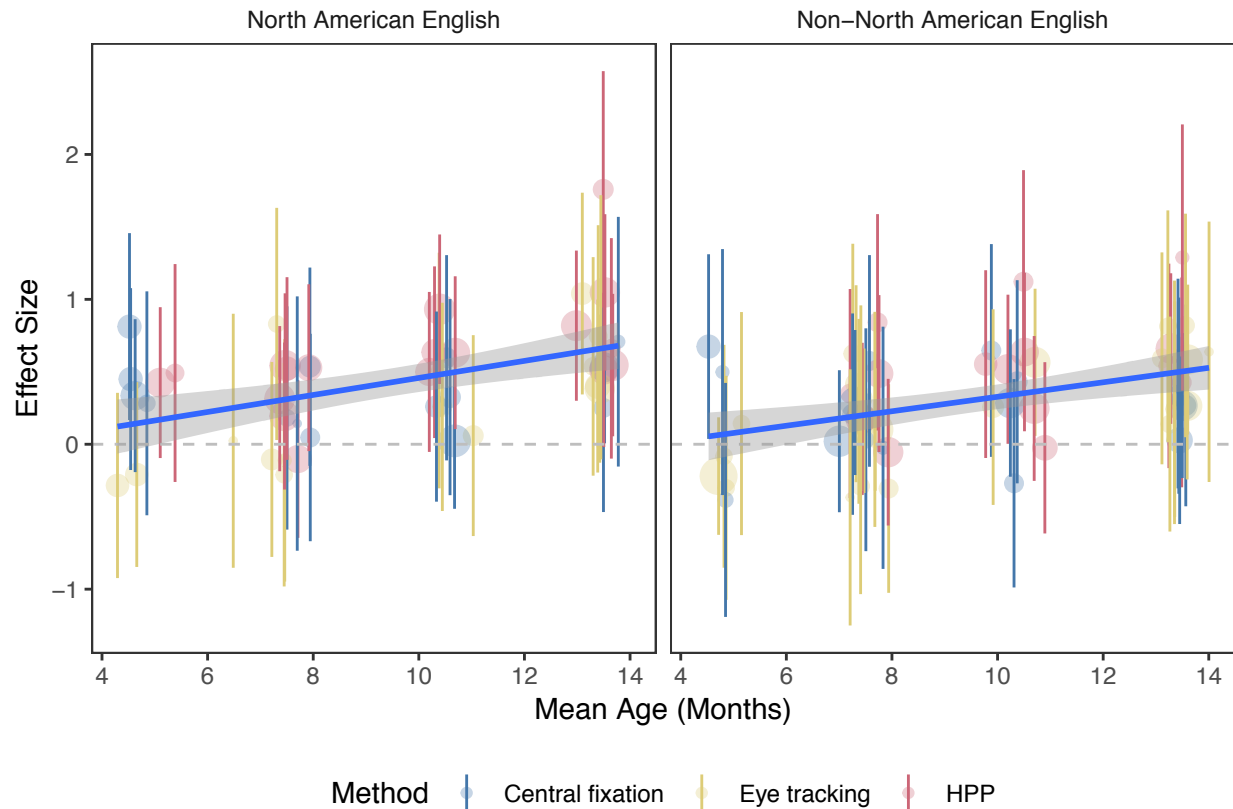


Figure 3. Lab effect size estimates plotted by age and method. Subplots show language groups. Standardized effect sizes are shown for each lab, with error bars showing 95% confidence intervals. Points are scaled by number of participants and colored by experimental method; they are slightly transparent to avoid overplotting.

814 single-screen central fixation as the reference level. The method-moderated model
 815 ($dz \sim 1 + \text{method}$) yielded a reference-level intercept of 0.29 (CI = [0.18 - 0.41], $z = 4.98$,
 816 $p < .001$), reflecting the mean effect size for single-screen presentation. The HPP yielded an
 817 additional effect of 0.21 (CI = [0.06 - 0.37], $z = 2.74$, $p = .006$), indicating a substantial gain
 818 in measured IDS preference for those labs using HPP as compared with single-screen central
 819 fixation. In contrast, eye-tracking yielded an effect of -0.06 (CI = [-0.21 - 0.10], $z = -0.71$,
 820 $p = .479$), indicating a slight, non-significant decrease in measured effect size for eye-tracking
 821 relative to single-screen central fixation.

822 The language-moderated model ($dz \sim 1 + \text{language}$) was fit with language group coded

823 as a categorical variable indicating whether infants were tested in a lab in which NAE was
 824 the standard language (e.g., in the United States or Canada). The reference level effect (i.e.,
 825 not NAE) was 0.29 (CI = [0.20 - 0.37], $z = 6.56$, $p < .001$), while for infants in North
 826 American labs, the effect was increased by 0.15 (CI = [0.02 - 0.27], $z = 2.26$, $p = .024$).
 827 Thus, measured IDS preferences were higher in those infants for whom the stimuli were
 828 native-language congruent.

829 **Joint relationship of IDS preference to moderating variables.** Because
 830 infant age, language, and method were confounded across labs (labs with particular methods
 831 also chose specific sample age ranges, and these choices were not independent), we next turn
 832 to the mixed-effects modeling framework to estimate subject-level age effects and lab-level
 833 method effects. To help visualize the spread of subject-level effects, Figure 4 shows IDS
 834 preferences for individual participants.

835 Our main model was:

$$\begin{aligned}
 \log \text{lt} \sim & \text{trial type} * \text{method} + \text{trial type} * \text{trial num} + \text{age} * \text{trial num} + \\
 & \text{trial type} * \text{age} * \text{language} + \\
 & (\text{trial type} * \text{trial num} \mid \text{subid}) + \\
 & (\text{trial type} * \text{age} \mid \text{lab}) + \\
 & (\text{method} + \text{age} * \text{language} \mid \text{item})
 \end{aligned} \tag{1}$$

836 Trial type, language, and method were dummy-coded (with ADS trials, non-NAE, and
 837 single-screen method) as the reference level; thus, coefficients are interpretable such that e.g.,
 838 positive effects of trial type indicate longer looking to IDS. To increase the interpretability of
 839 coefficients, age (in months) was centered and trial number was coded with trial 1 as the
 840 reference level.

841 We specified this model to minimize higher-order interactions but preserve
 842 theoretically-important interactions. We included main effects of trial type, method,
 843 language, age, and trial number, capturing the basic effects of each on looking time (e.g.,
 844 longer looking times for IDS, shorter looking times on later trials). In addition, we included
 845 two-way interactions of trial type with method (modeling the possibility that some methods
 846 show larger IDS preferences) and trial type with trial number (modeling the possibility of
 847 faster habituation to ADS) as well as age and trial number (modeling faster habituation for
 848 older children). We also included two- and three-way interactions of age, trial type, and
 849 language (modeling possible developmental changes in IDS preference across age and
 850 language group). Both developmental effects and trial effects are treated linearly in this
 851 model; although both likely have non-linear effects, adding quadratic or other effects would
 852 have substantially increased model complexity. After pruning random effects for
 853 non-convergence,¹ our final model specification was:

$$\begin{aligned}
 \log lt \sim & \text{trial type} * \text{method} + \text{trial type} * \text{trial num} + \text{age} * \text{trial num} + \\
 & \text{trial type} * \text{age} * \text{language} + \\
 & (1 | \text{subid}) + \\
 & (1 | \text{lab}) + \\
 & (1 | \text{item}).
 \end{aligned}
 \tag{2}$$

854 Table 4 shows coefficient estimates from this model.

855 Overall, the fitted coefficients of the mixed effects model were consistent with the
 856 results of the individual meta-analyses. Within the structure of the mixed effects model, IDS
 857 preferences are shown by positive coefficients on the IDS predictor (reflecting greater looking
 858 times to IDS stimuli). The fitted model shows a significant positive effect of IDS stimuli,

¹ Pruning was done using models fitted with ‘lme4’ version 1.1-21.

Table 4

Coefficient estimates from a linear mixed effects model predicting log looking time.

	Estimate	SE	<i>t</i>	<i>p</i>
Intercept	2.180	0.051	43.100	0.000
IDS	0.099	0.036	2.740	0.010
Eye-tracking	-0.265	0.046	-5.790	0.000
HPP	-0.052	0.051	-1.020	0.308
Trial #	-0.038	0.002	-25.000	0.000
Age	-0.035	0.004	-7.950	0.000
NAE	-0.016	0.049	-0.335	0.738
IDS * Eye-tracking	-0.009	0.017	-0.548	0.584
IDS * HPP	0.034	0.015	2.270	0.023
IDS * Trial #	-0.003	0.002	-1.370	0.172
Trial # * Age	0.001	0.000	3.140	0.002
IDS * Age	0.012	0.003	4.300	0.000
IDS * NAE	0.039	0.013	3.060	0.002
Age * NAE	0.001	0.006	0.198	0.843
IDS * Age * NAE	0.004	0.004	1.050	0.292

859 consistent with a global IDS preference. Consistent with the age- and language-moderated
860 meta-analyses, there were significant and positive two-way interactions of IDS with age and
861 with NAE, suggesting greater IDS preferences for older children and for children in NAE
862 contexts. Further, there was a positive interaction with the HPP method, consistent with
863 the method-moderated model. There was not a significant three-way interaction of IDS, age,
864 and NAE, however, suggesting that there was not a reliable differential change in IDS
865 preference for older children in NAE contexts over and above that expected based on each of

866 these factors alone.

867 In addition to these results, a number of other factors were significant predictors of
 868 looking time. Looking time decreased across trials, and did so especially for older children,
 869 generally confirming that all infants habituated to our experimental stimuli and older infants
 870 did so more quickly. Further, eye-tracking led to lower looking times overall across stimulus
 871 classes.

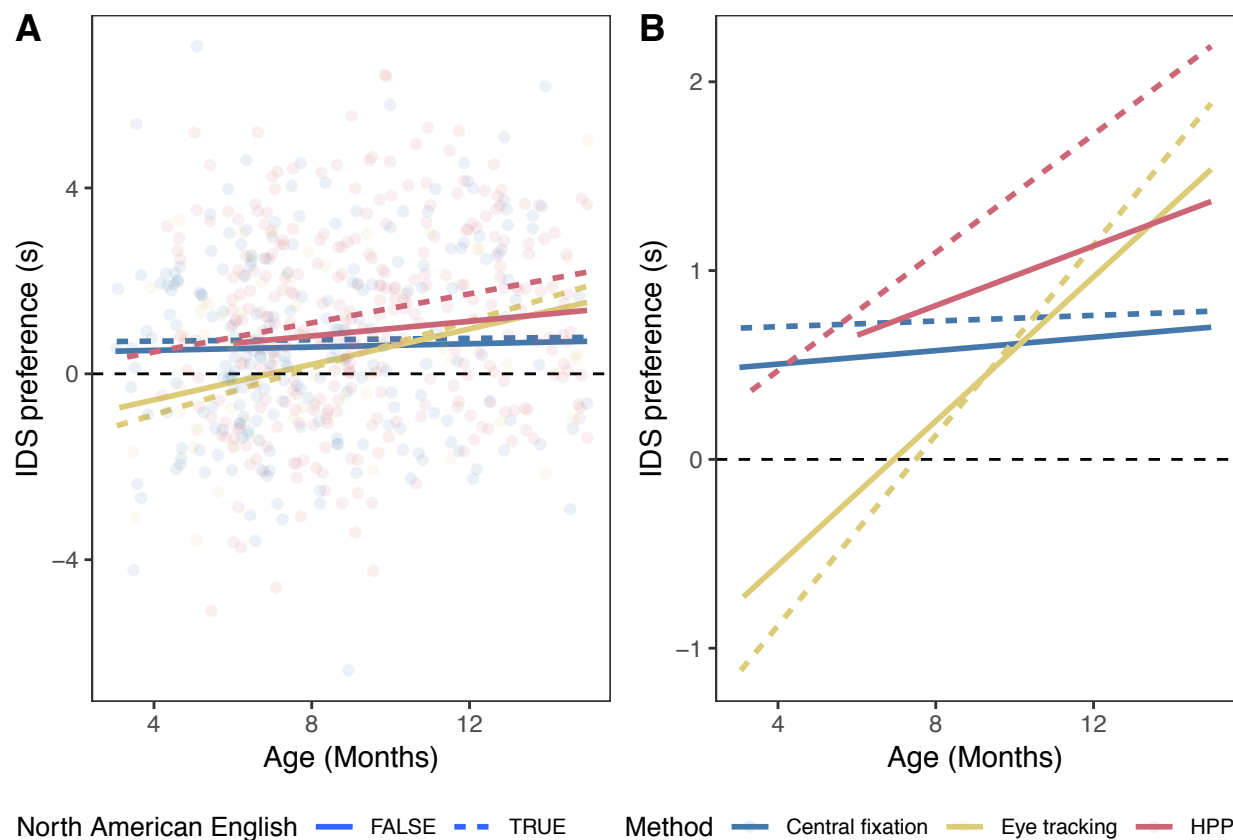


Figure 4. Simple linear trends for IDS preference by age and language group, plotted (A) with individual participants' preferences and (B) without individual participants' preferences to show trends more effectively.

872 **Effects of second-session testing on IDS preference.** We preregistered an
 873 analysis of whether second-session infants showed a different pattern of infant-directed
 874 speech preference. Only 6 labs contributed second-session infants, however, with a total of

875 only 0 infants represented. Thus, we did not fit the full, pre-registered mixed-effects model
876 for this variable as we did not have enough variability on the important covariates to
877 estimate this variable. As an exploratory analysis, we note that 19/41 second-session infants
878 (46.30% [31.60 - 61.30]) showed a numerical preference for IDS. This number was numerically
879 different but not distinguishable statistically from the 58.95% of IDS preferences in the
880 first-session infants, likely due to the small sample of second-session infants.

881 **Sex and IDS preference.** In order to investigate effects of biological sex on IDS
882 preference, we fit the model specified above with the addition of a sex main effect and trial
883 type by sex interaction.² Female was coded as the reference level, so effects are stated in
884 terms of changes for male infants. The main effect of sex $\beta = 0.01$ ($SE = 0.02$, $p = 0.67$)
885 and the interaction with trial type was $\beta = -0.01$ ($SE = 0.01$, $p = 0.56$). These predictors
886 were small and nonsignificant, suggesting that sex was not a strong determinant of measured
887 IDS preferences in our data.

888 **Moderator effects on missing data.** One further question regarding our data was
889 whether particular moderator variables affected not just the amount of looking time we
890 recorded, but whether children looked at all during a trial. To test for effects of moderators
891 on the presence of missing data, we constructed a categorical variable (missing), which was
892 true if a trial had no included looking time (e.g., no looking recorded, a look under 2 s, or no
893 looking because the infant had already terminated the experiment) and false otherwise. We
894 fit a logistic version mixed-effects model with all two-way interactions between method, age,
895 and trial number, using the specification:

² Because this model did not converge, following our protocol, we pruned random effects of item.

$$\begin{aligned}
& \text{missing} \sim \text{method} * \text{age} + \text{method} * \text{trial num} + \text{age} * \text{trial num} + \\
& \quad (1 \mid \text{subid}) + \\
& \quad (\text{trial num} * \text{age} \mid \text{lab}) + \\
& \quad (\text{method} + \text{age} \mid \text{item}).
\end{aligned} \tag{3}$$

896 After pruning for non-convergence, our final model specification was:

$$\begin{aligned}
& \text{missing} \sim \text{method} * \text{age} + \text{method} * \text{trial num} + \text{age} * \text{trial num} + \\
& \quad (1 \mid \text{lab}).
\end{aligned} \tag{4}$$

897 Table 5 shows coefficient estimates from this model. To aid convergence, we centered and
898 scaled age and trial number, and set single screen presentation as the reference level. Positive
899 coefficients indicate a higher probability of missing data. Older children and later trials had
900 greater amounts of missing data, consistent with the idea that all children habituated to the
901 stimuli, but that older children habituated faster. There was also a significant negative
902 interaction of age and eye-tracking, suggesting that data loss for eye-tracking was
903 substantially greater in younger children and lower in older children (we return to this issue
904 in the general discussion). Other coefficients were relatively small and nonsignificant.

905 Exploratory Analyses

906 **Meta-analytic heterogeneity.** One question of interest was whether we observed
907 any meta-analytic heterogeneity in the data. When a meta-analysis shows heterogeneity,
908 that finding indicates the presence of unexplained variance in effect size over and above that
909 due to sampling variation; the τ^2 provides an estimate of the total heterogeneity in our
910 models. We further assess heterogeneity using the I^2 statistic (Higgins, Thompson, Deeks, &

Table 5

*Coefficient estimates from a linear mixed effects model
predicting whether an observation was missing.*

	Estimate	SE	z	p
Intercept	-1.090	0.152	-7.140	0.000
Eye-tracking	0.167	0.130	1.290	0.198
HPP	-0.178	0.195	-0.913	0.361
Age	0.356	0.038	9.380	0.000
Trial #	0.663	0.030	22.100	0.000
Eye-tracking * Age	-0.238	0.047	-5.090	0.000
HPP * Age	-0.059	0.051	-1.150	0.251
Eye-tracking * Trial #	0.068	0.036	1.850	0.064
HPP * Trial #	0.046	0.040	1.130	0.257
Trial # * Age	-0.003	0.014	-0.208	0.835

911 Altman, 2003), which quantifies the proportion of total variation in estimates that is due to
 912 heterogeneity. We also report the results of a standard hypothesis test for heterogeneity, the
 913 Cochran Q test; when this test is statistically significant, that indicates that the null
 914 hypothesis of homogeneity of variance can be rejected (Huedo-Medina, Sanchez-Meca,
 915 Marin-Martinez, & Botella, 2006).

916 In our primary, intercept-only meta-analytic model, $\tau^2 = 0.01\%$, $I^2 = 12.39\%$, and
 917 $Q(107) = 122$, $p = 0.15$. In the language-moderated model, $\tau^2 = 0.01\%$, $I^2 = 7.76\%$, and
 918 $Q(106) = 116.18$, $p = 0.23$. In the age-moderated model, $\tau^2 = 0\%$, $I^2 = 0\%$, and
 919 $Q(106) = 98.06$, $p = 0.70$. Finally, in the method-moderated model, $\tau^2 = 0\%$, $I^2 = 3.20\%$,
 920 and $Q(105) = 106.78$, $p = 0.43$. In none of these could we reject the null hypothesis of no
 921 heterogeneity beyond sampling variation, and in no case was the magnitude of observed

922 heterogeneity large. Although there were reliable moderators (see meta-analytic results
 923 above), these moderators were quite small in magnitude relative to the sampling variation in
 924 individual lab effect size estimates (because of the small median sample size within each lab).

925 **Exclusion criteria.** Because our criterion for including infants in the analysis was
 926 so liberal (infants needed to contribute data from only two trials to be included), we next
 927 conducted an exploration of the effects of different inclusion rules on the results we reported
 928 above. In particular, we calculated the meta-analytic effect size with 4 trials and 8 trials as
 929 minimum inclusion criteria. For a minimum of 4 trials, the effect size was 0.42 (CI = [0.35 -
 930 0.48], $z = 12.05$, $p < .001$) and for a minimum of 8 trials the effect size was 0.48 (CI = [0.40 -
 931 0.57], $z = 11.23$, $p < .001$). In comparison, our original results showed a meta-analytic effect
 932 size of 0.35 (CI = [0.29 - 0.42], $z = 10.67$, $p < .001$). Furthermore, we computed effect sizes
 933 for each method for each of these additional exclusion criteria (see Table 6). Overall, more
 934 stringent inclusion criteria yielded substantially larger effects, although they also led to
 935 substantial data loss (especially for eye-tracking labs).

Table 6

Meta-analytic effect size (dz), standard error (SE) and percentage of included participants for three different exclusion criteria

method	2 Trials			4 Trials			8 Trials		
	estimate	SE	%	estimate	SE	%	estimate	SE	%
Central fixation	0.29	0.06	0.98	0.34	0.06	0.88	0.40	0.06	0.73
Eye tracking	0.24	0.06	0.85	0.33	0.06	0.59	0.41	0.10	0.36
HPP	0.51	0.06	0.98	0.56	0.06	0.92	0.63	0.07	0.78

General Discussion

936

937 We designed a large-scale, multi-lab study of infants' preference for IDS and invited
938 infancy researchers to participate. Our call for participation resulted in contributions from
939 69 labs, representing a total of 2845 infants from 16 countries, 2329 of which were included
940 in the final sample used for analysis (see Table 1). We believe that the resulting dataset
941 represents the largest laboratory study of infancy to date. We begin our discussion by
942 summarizing the principal results of the study with respect to four critical analytic questions
943 and then discuss limitations of the study as well as future directions.

944 Summary of Findings

945 Our first goal was to address the issue of replicability by providing a pre-registered,
946 unbiased measure of the magnitude of infants' preference for IDS over ADS. We expected to
947 replicate prior demonstrations of the existence of an IDS preference in infant listeners, and
948 our study indeed confirms the expected effect. Our overall meta-analytic mean is smaller in
949 size than the effect found in a preceding meta-analysis of the literature, however (Bergmann
950 et al., 2018; Dunst et al., 2012).

951 While one possible interpretation of this finding is that previous effect sizes were
952 inflated by publication bias, there are other possible explanations as well. In an individual
953 laboratory, the methodology would be tailored to the specific research question, age range
954 and other characteristics of the infants tested (or conversely, research questions would be
955 tailored to the existing methodological expertise of the laboratory). The approach used here,
956 namely applying multiple methodologies to the same research question across diverse age
957 ranges and samples of infants including non-native English learning infants, may have led to
958 an underestimate of the true effect size (i.e., because an ideal choice of presentation details
959 that would maximize effect sizes might differ between methods and across ages, versus the

960 compromise protocol used here). Further, our protocol included several decisions that might
961 have decreased effect size, including both our stimuli's relatively less extreme acoustic
962 characteristics, the use of multiple speakers, and our less stringent participant inclusion
963 criteria (both discussed below).

964 Our second goal was to examine possible age effects in the preference for IDS.
965 Consistent with the prior published meta-analysis (Dunst et al., 2012) and with idea that
966 preference for IDS grows in response to experience with positive social interactions – but in
967 contrast with some other reports in the literature (e.g., Hayashi et al., 2001; Newman &
968 Hussain, 2006; Segal & Newman, 2015) – we found an increase in IDS preference across
969 development. Further, the magnitude of the positive developmental change is considerable,
970 at 0.05 standard deviations per month. This finding suggests that the preference for IDS is
971 at a minimum modulated by experience and/or maturation.

972 As with any other developmental trend, however, age-related change may be driven by
973 changes in factors other than the underlying construct. First, as we will discuss in detail
974 below, characteristics of the stimuli may be best suited for an older age range. Second,
975 stronger effects may result from a more robust or more measurable behavioral response on
976 the part of older infants, independent of an underlying preference. Some evidence in favour
977 of this possibility stems from examining the data in MetaLab, an online databank for
978 meta-analysis in infant research: most meta-analyses show an increase in absolute effect size
979 as infants mature, independent of the research question (see e.g., Bergmann et al., 2018).

980 Our third goal was to examine how the preference for IDS varies based on the differing
981 linguistic experiences of infants growing up across different linguistic communities. We found
982 a preference for North American English IDS over North American English ADS even for
983 participants for whom this was not their native language or dialect. This finding replicates
984 previous work (Werker et al., 1994). However, in our study, North American English-exposed
985 infants showed the strongest preference. Note that our findings do not support the idea of a

986 simple attentional effect (infants attending more to speech overall when presented in their
987 native language): The effect of language background on overall (as opposed to preferential)
988 looking times is not large in our regression models.

989 There are several possible interpretations of the native language effect we observed.
990 One possibility is that as infants become experts in their native language phonology and
991 begin to acquire word meanings, they listen to speech in their own language differently,
992 starting to process what’s being said not just as “speech” or “register” per se but as
993 meaningful language (Gervain & Mehler, 2010; Johnson, 2016). For infants hearing a foreign
994 language or even dialect, the ability to listen in this “deeper” or more predictive way is not
995 available. Another possibility is processing speech in an unfamiliar language requires more
996 attentional resources, leaving fewer attentional resources to process some of the
997 characteristics that may differentiate IDS and ADS. In either situation, preference for IDS
998 may depend in part on the similarity to one’s native language experiences with IDS. This
999 idea is somewhat supported by the age effect we observed; however, we did not observe a
1000 three-way interaction between age, stimulus type, and language background, which would
1001 have been a prediction of this interpretation. Companion data in several non-North
1002 American English language communities using native language stimuli created using the
1003 ManyBabies 1 protocol are currently under development and may shed further light on this
1004 issue.

1005 Our fourth and final goal was to examine differences across methodological approaches
1006 in the measured experimental effect. We found a stronger effect when using HPP than
1007 central fixation or eye-tracking approaches. One potential interpretation of this finding is
1008 that the greater effort on the part of the infant in HPP (i.e., a turning of the head, as
1009 opposed to small eye movements) leads to stronger engagement in the task and therefore to
1010 stronger effects.

1011 It is important to keep in mind, however, that methodology was not randomly assigned

1012 to laboratories, and the characteristics of laboratories probably varied systematically with
1013 their methodological choices. It may well be, for example, that laboratories with more
1014 expertise in infant language acquisition research were more likely to use HPP. Furthermore,
1015 these findings should not be interpreted as suggesting that HPP would be best suited for all
1016 research questions. Instead, a more modest interpretation is simply that a theoretically
1017 irrelevant variable related to laboratories and their methodological decisions appears to have
1018 a substantial and systematic effect on measured effect size (see also Bergmann et al., 2018
1019 for a similar conclusion based on meta-analytic data). We hope to undertake future
1020 secondary analyses of our dataset to better understand factors that may have covaried with
1021 methodological choices. Moreover, further large-scale projects that include methodological
1022 contrasts of this type – perhaps with random assignment – may allow us to draw more
1023 specific conclusions about the sources of methodological variability, and their interactions
1024 with phenomenon and participant age.

1025 Another methodological contribution of this project was our investigation of how
1026 different infant-level inclusion criteria affect the magnitude of the obtained effect size. For
1027 our main analysis, we included all infants who completed at least one IDS and one ADS trial.
1028 This is somewhat a departure from the literature using this paradigm, as most participating
1029 labs reported using a stricter inclusion criterion in their own independent work. Our original
1030 meta-analytic effect size was 0.35 when we included all infants with a minimum of two trials,
1031 grew to 0.42 with a minimum of four trials, and 0.48 with a minimum of eight trials.
1032 Moreover, there was substantially more missing data from younger infants in the
1033 eye-tracking paradigm compared with the other methods. While missing data increased
1034 across the length of the experiment, this increase was particularly prevalent for eye tracking.
1035 Setting stricter inclusion criteria necessarily decreases sample size with the same number of
1036 total infants tested, but at the same time stricter criteria appear to lead to more robust
1037 effects in this paradigm.

1038 **Challenges and Limitations**

1039 As with any study, the current experiment required specific methodological choices,
1040 several of which influence the generalizability of our results. Two aspects of the
1041 decision-making regarding the stimuli in particular are worth further discussion. The first is
1042 the choice to use North American English (as opposed to, say, the native language or dialect
1043 for each infant group tested). This choice was based on the need to use consistent stimuli
1044 across laboratories to limit cross-lab variation and ensure feasibility of the overall project,
1045 and to use stimuli from a language in which there was robust evidence of a strong IDS
1046 preference effect, both in a native and non-native setting. However, our design necessarily
1047 complicates the interpretability of our findings from laboratories outside of North America.
1048 They confound native-language/dialect effects (infants prefer listening to their native
1049 language) and true cultural variation in IDS preference. Further, there is substantial
1050 diversity in the non-North American English samples that is obscured in our pre-registered
1051 analyses. Together with the previously-mentioned native-language follow-up studies using
1052 the ManyBabies 1 protocol, further analyses of our dataset on specific sub-samples with
1053 sufficient sample size (e.g. French, German, Dutch, British English) will shed additional light
1054 on how the differences between the North American and other infants in the current study
1055 should be interpreted.

1056 The second challenging decision hinged around the elicitation of the IDS stimuli.
1057 Stimuli used in previous IDS preference literature range from scripted speech with no infant
1058 present (e.g., Cooper & Aslin, 1990; Newman & Hussain, 2006), which maximizes
1059 experimental stimulus control, to more naturalistic samples collected from free-play,
1060 unscripted contexts (e.g., Hayashi et al., 2001; Werker et al., 1994), which maximizes
1061 generalizability to real-world contexts. We opted for a relatively naturalistic approach, with
1062 an elicitation protocol using real mothers and their infants centred around concrete objects.
1063 It is likely that this approach may have led to the reduction in the distinctiveness of the

1064 acoustic characteristics of the IDS samples that we observed, and it limited our ability to
1065 fully control the characteristics of the samples. Other aspects of our elicitation approach are
1066 important to keep in mind in interpreting findings such as our developmental effects –
1067 namely the age range of the “target” infants (4-8 months) and the objects-focused nature of
1068 the task (something likely best suited to infants at the older range of our age bins). The
1069 extent to which these age-related characteristics of IDS affect the magnitude of infants’ IDS
1070 preference across development merits further inquiry. Further, and as noted above, the use of
1071 multiple speakers in the stimuli may have increased the processing load for infants.

1072 As the first collaboration of its kind, ManyBabies 1 revealed a number of important
1073 challenges in conducting multilab infant collaborations. As any lab that has tested infant
1074 participants knows, data collection is slow and labour intensive. Over a period of
1075 approximately 13 months, 69 labs were able to collect data from 2845 infants. In contrast,
1076 ManyLabs 1, a similar initiative with adults participants (Klein et al., 2014), was able to
1077 collect data from more than 6000 participants tested in 36 labs over just a handful of months.
1078 Moreover, while adults can often be tested in multiple studies in a single session, this option
1079 is very limited for infants.

1080 We expected challenges in implementing a standardized data collection procedure
1081 across infant labs, but the depth of these challenges, and the diversity of methodological
1082 implementation across laboratories, was surprising. Infant laboratories are highly diverse in
1083 both the software and hardware they have available to implement experimental infant testing
1084 methods. We planned flexibility in the specific setup (eyetracking, HPP, central fixation) due
1085 to known variability, but despite this several labs were forced to deviate from aspects of the
1086 protocol, for example due to limitations of how stimuli could be presented (e.g., the ability
1087 to implement infant-controlled trial lengths, software settings for repeating trials, etc.). One
1088 important conclusion from our work, as evidenced in the “walk through videos” laboratories
1089 provided to illustrate their protocols (see below), is the extent to which a typical methods

1090 section fails to capture this methodological diversity.

1091 **Additional Benefits of Large-Scale Collaboration**

1092 While our primary goal was an empirical one, the ManyBabies 1 project had numerous
1093 additional benefits to both individual researchers as well as the field at large. All of the
1094 questionnaires, and how-tos, and stimuli (e.g., attention getters) used in the project are freely
1095 available for re-use in future studies. Each participating lab created a walkthrough video
1096 that showed their lab and study setup. These videos provide an unprecedented peek “behind
1097 the curtain” of other infancy labs, which was previously only possible through visiting labs in
1098 person. Such information could be a particularly helpful resource for investigators setting up
1099 an infant lab for the first time. It also provides a unique dataset whereby the field of infant
1100 research can begin to understand the variety of lab setups and study implementations.

1101 This large-scale collaborative effort also had broader benefits for the field. It created a
1102 strong collaborative network of infancy researchers. Informal “ManyBabies” gatherings are
1103 now organized at developmental conferences, enabling researchers who have previously
1104 collaborated only virtually to meet in person. It also was many researchers’ introduction to
1105 open and cumulative science practices and tools, such as pre-registration and the Open
1106 Science Framework.

1107 Finally, ManyBabies 1 has launched several “knock-on” projects. For example,
1108 ManyBabies Bilingual (Byers-Heinlein et al., accepted pending data collection) is comparing
1109 bilingual infants’ preference for infant directed speech with our results from monolinguals.
1110 Other projects will examine the test-retest reliability of infants’ IDS preference, examine
1111 whether IDS preference predicts vocabulary size at 18 and 24 months (Soderstrom et al.,
1112 accepted pending data collection), and test whether lab-specific variables affect infant
1113 performance and attrition. We believe that these additional benefits are not unique to

1114 infancy research, and that other scientific communities embarking on large-scale
1115 collaborative projects will garner similar benefits.

1116 **Conclusion**

1117 Replication research can go far beyond simply asking whether an effect is present: it
1118 can allow for an assessment of how an effect varies and how it develops. We observed a
1119 robust and statistically significant preference for IDS over ADS, confirming previous
1120 observations in the literature. Yet the value of our experiment lies not purely in this binary
1121 result – or even in the quantitative estimate of the overall magnitude of the IDS preference –
1122 but in the further theoretical and methodological opportunities that the data afford. By
1123 measuring the relationship of IDS preferences to age and language community, this
1124 experiment provides a starting point for developing a more nuanced theory of how IDS
1125 preferences relate to children’s language experiences. Further, by revealing the substantial
1126 contributions of methodological decision-making to effect size, our study points the way
1127 towards developing best-practices templates in further infancy work of this kind. In sum, we
1128 hope our work here illustrates the power of large-scale collaboration for the study of
1129 developmental variation and change.

1130

Author Contributions

1131 Author contribution initials reflect authorship order. MCF, EB, CB, KBH, BF, JG,
1132 JKH, MK, CL, CLW, CM, TN, RP, HR, AS, MS contributed to the study concept. MCF,
1133 CB, KBH, CF, JG, NGG, JKH, EEH, MK, CLW, TN, RP, HR, JLR, SW, DY, MS
1134 contributed to the study design. MCF, RC, CF, DJK, KK, CLW, RP, MS, MS contributed
1135 to stimulus creation. NGG, JKH, DJK contributed to piloting. MCF, CB, RB, KBH, LR,
1136 CDL, BF, IJ, MK, JFK, MM, KT, DY contributed to the final protocol. MCF, CB, KBH,

1137 JG, MK, CLW, MM, MS contributed to study documentation. MCF, CB, KBH, RLAF,
1138 JKH, MK, CLW, KT, MS contributed to study management. KJA, NAT, GA, DB, SB,
1139 AKB, MPB, PB, AB, SMB, BB, AB, KBH, LEC, CC, MC, JC, LKC, SC, SC, CC, AC, CD,
1140 MK, LR, CDL, DD, KCD, VD, SD, CF, AF, PF, TF, CF, MF, TF, RLAF, AG, JG, NGG,
1141 AG, LEH, JKH, EEH, NH, JH, MH, BH, DMH, LHH, MI, SI, IJ, KVJ, MJ, SPJ, CJ, DK,
1142 NK, TKP, KK, ESK, JEK, HEK, AARK, FK, JL, RJL, ML, CL, CL, UL, LL, SGL, RAL,
1143 VMC, NM, CM, AM, MM, VM, JM, KM, CM, YM, BM, KMN, CN, MAN, NMO, AJO,
1144 MO, RP, SPE, MP, CP, LP, CP, HR, SR, JLR, GDR, KCR, CR, DR, YR, JS, AS, SS, AS,
1145 GS, MSS, AS, EAS, LS, BS, GS, MS, AT, AT, LJT, SET, AST, ASMT, KT, KVH, YW,
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1147 ML, HR, ASMT, AW, MZ, MS contributed to data analysis. MCF, EB, CB, KBH, AC, RC,
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1151

Conflicts of Interest

1152 The authors declare that there were no conflicts of interest with respect to the
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1154

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1167

Prior Versions

1168 Our pre-registered protocol was posted prior to data collection at
1169 <https://psyarxiv.com/s98ab/>.

1170

Disclosures

1171 Preregistration

1172 Our manuscript was reviewed prior to data collection; in addition, we registered our
1173 instructions and materials prior to data collection (<https://osf.io/gf7vh/>).

1174 Data, materials, and online resources

1175 All materials, data, and analytic code are available at <https://osf.io/re95x/>; the specific
1176 code and data required to render this document are available at <https://osf.io/zaewn/>.

1177 Reporting

1178 We report how we determined our sample size, all data exclusions, all manipulations,
1179 and all measures in the study.

1180 Ethical approval

1181 All labs collected data under their own independent ethical approval via the
1182 appropriate governing body for their institution. Central data analyses used exclusively
1183 de-identified data. Identifiable video recordings of individual infant participants were coded
1184 and archived locally at each lab; where IRB protocols permitted, video recordings were also
1185 uploaded to Databrary, a central controlled-access database accessible to other researchers
1186 (Databrary, n.d.).

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