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A systematic review suggests marked differences in the prevalence of infant-directed vocalization across groups of populations

Alejandrina Cristia

Laboratoire de Sciences Cognitives et de Psycholinguistique, Département d'Etudes cognitives, ENS, EHESS, CNRS, PSL University, Paris, France

Correspondence
Alejandrina Cristia, Laboratoire de Sciences Cognitives et de Psycholinguistique, Département d'Etudes cognitives, ENS, EHESS, CNRS, PSL University, 29, rue d'Ulm, 75005 Paris, France.
Email: alecristia@gmail.com

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Abstract

Anthropological reports have long suggested that speaking to young children is very infrequent in certain populations (notably farming ones), which is in line with scattered quantitative studies. A systematic review was undertaken to use available literature in order to estimate the extent of population variation. Database searches, expert lists, and citation searches led to the discovery of 29 reports on the frequency of vocalizations directed to infants aged 24 months or younger, based on systematic observations of spontaneous activity in the infant's natural environment lasting at least 30 min in length. Together, these studies provide evidence on 1314 infants growing up in a range of communities (urban, foraging, farming). For populations located outside of North America, the frequency with which vocalization was directed to urban infants was much higher than that for rural infants (including both foraging and farming, medians = 12.6 vs. 3.6% of observations contained infant-directed vocalization behaviors). We benchmarked this effect against socio-economic status (SES) variation in the United States, which was much smaller. Infants in high SES American homes were spoken to only slightly more frequently than those in low SES homes (medians = 16.4 vs. 15.1% of observations contained infant-directed vocalization behaviors). Although published research represents a biased sample of the world's populations, these results invite further cross-population research to understand the causes and effects of such considerable population group differences.

KEYWORDS
behavioral observations, child-directed speech, cognitive development, human diversity, time sampling

1 | INTRODUCTION

Both empirical and theoretical work in a range of disciplines have discussed the importance (or lack thereof) of infant-directed speech. In addition, some empirical research suggests that the prevalence of behaviors including infant-directed vocalizations varies across populations. This Introduction summarizes both lines of work to motivate a systematic review of behavioral observations allowing an estimation of differences in the prevalence of infant-directed vocalization behaviors across populations.

1.1 | Relevance of infant-directed speech

Infant-directed speech has been discussed in a wide range of disciplines, including psycholinguistics, developmental psychology, and anthropology. A full review of how infant-directed speech behaviors integrate with theories and observations across all of these fields is beyond the scope of the present work. Nonetheless, we provide here a brief summary of theories according to which infant-directed speech is a fuel for early language acquisition in order to justify an interest in the accurate measurement of its prevalence.

In many populations, speech directed to infants and young children has a special acoustic profile (Moser et al., 2020), and may be characterized by lexical simplicity and other adjustments (Cristia, 2013). Even for populations in which infant-directed speech is not marked in this way, it is likely easier to learn certain things (saliently words) from the speech that is addressed to oneself, rather than speech that is overheard and does not take into account our perspective (Shneidman & Woodward, 2016; Shneidman et al., 2013).

Several results have been interpreted as suggesting that speech addressed to the infant themself may be more useful for language acquisition than merely overheard speech. For instance, individual differences in language processing among Californian low socioeconomic status (SES) children tested at 18–24 months of age correlated with, and were statistically explained by, infant-directed input quantity (Weisleder & Fernald, 2013). Importantly, individual differences in outcomes were not significantly correlated with individual variation in total speech quantity (i.e., adding up infant-directed speech with speech directed to others and merely overheard by the child). A similar pattern has been observed among children growing up in a Yucatec Mayan village: Only adult infant-directed speech significantly predicted children's vocabulary, whereas overheard speech did not (Shneidman & Goldin-Meadow, 2012). Neuroimaging studies have been used to argue that, if children get a chance to interact linguistically with adults around them, this entails changes in brain structure, based on a correlation between quantity of adult-child conversational turns (above and beyond adult word counts) and white as well as gray matter structure in children's brains (Romeo et al., 2018). Other research is even more specific: It is only one-on-one interactions that positively impact language development, and not group-based interactions, even if they involve an adult (Ramírez-Esparza et al., 2014, 2017). In addition, some work suggests that variation in children's vocabulary as a function of maternal socioeconomic status is mostly explained by the quantity and quality of speech addressed to the infant (Hart & Risley, 1995; Huttenlocher et al., 2010; Rowe, 2008).

An emergent view from this work is that infant-directed speech, particularly from adults in dyadic interactions with the infant, both facilitates learning of language structures and promotes the development of efficient speech processing routines, which further speed up acquisition (Hurtado et al., 2008). These views are consistent with usage-based theories, in which language representations emerge primarily as a function of processing and/or production pressures (e.g., Christiansen & Chater, 2016). Work causally linking infant-directed speech to child vocabulary (e.g., Ramírez et al., 2020) has inspired developmental economists (e.g., Ma et al., 2021), pediatricians (e.g., Suskind, 2012), and others (e.g., Mahoney et al., 2019) interested in creating and rolling out interventions to, for instance, reduce disparities in educational outcomes. However, it should be noted that other researchers believe the importance of infant-directed speech may have been overestimated, and the relevance of these theories for language acquisition across diverse populations has been called into question (Casillas et al., 2020; Ochs & Schieffelin, 2001; Sperry et al., 2019). We return to this when pointing out limitations of this study in the section Discussion.

RESEARCH HIGHLIGHT

- A systematic review yielded 29 papers on ~1300 infants and their families observed in naturalistic situations.
- Infant-directed vocalization behaviors were three times more frequent in samples classified as urban compared to rural samples.
- This ratio was much larger than that found for samples of high versus low socioeconomic status in the United States.
- Results invite further cross-cultural work and may have implications for theories and applications on infant language development.

1.2 | Differences in the prevalence of infant-directed vocalization across diverse populations

Previous qualitative and quantitative work suggests that the prevalence of infant-directed vocalization behaviors varies across populations. According to Ochs and Schieffelin (2001), Samoan and Kaluli children were more rarely addressed by their own mothers than American children were. More recent quantitative reports have started to pinpoint the extent of variation in the prevalence of infant-directed vocalization. Although not including a direct cross-population comparison, Cristia et al. (2019) report low levels of infant-directed speech in six Tsimane' villages in Bolivia. Direct comparison of data collected in the same way in rural and urban settings are also beginning to emerge. For instance, Vogt and Mastin (2013) report a seven-fold difference in the number of utterances addressed to 14-month olds in rural, as opposed to urban, settings in Mozambique; and Shneidman and Goldin-Meadow (2012) report an 11-fold difference between toddlers growing up in a Yucatec Mayan village compared to toddlers growing up in Chicago. Casillas et al. (2019) and Casillas et al. (2020) also reported low levels of infant-directed speech in a Tseltal-speaking village in Mexico and several Yélî Dnye-speaking hamlets respectively. A recent reanalysis of those data also included similarly-coded data from urban sites suggesting wide variation in child-directed speech quantities among urban sites, with some overlap across the five urban and two rural sites (Bunce et al., 2021).

These individual reports raise the question of the extent of variation in the prevalence of infant-directed vocalization across different groups of populations. Our key interest in this paper is estimating the extent of this variation, for instance via a ratio of how much more common infant-directed vocalization behaviors are in some populations as compared to others. Knowing whether this ratio is large or not benefits from a comparison point. For this reason specifically, we also consider whether such variation is equivalent to that found across socio-economic strata in North America, the population that is most often studied (Nielsen et al., 2017), and one key source of evidence for the view that infant-directed speech fuels language acquisition summarized in Section 1.1.

1.3 | Potential reasons why the prevalence of infant-directed vocalization behaviors may vary across populations

Previous work suggests several reasons why the prevalence of infant-directed vocalization may vary across populations in systematic ways. Robert A. LeVine's proposal of a set of caregiving practices that are adaptive to a variety of settings may serve to illustrate this (for a discussion in the context of other proposals, see Hewlett & Lamb, 2000). For instance, in Sharma and LeVine (1998, p. 56), the authors explain that "human parental care can be seen as behavior adaptive to historical, socioeconomic, and cultural stability and change. Each major type of socioeconomic and cultural adaptation to the environment (hunting-gathering, foraging, agrarian, urban-industrial, and postindustrial) demands different strategies from parents in caring for their young. These are called parental investment strategies and consist of allocation of time and resources devoted to child care."

According to Sharma and LeVine (1998), parents in farming societies try to optimize the survival of their children, likely to compensate historically high levels of child mortality. In contrast, parents in urban/industrialized societies have few children, and stimulate them cognitively so that they may have a better chance of getting integrated into a competitive job market, in which success in formal education is a key constraint. Recent improvements in health care allow a greater proportion of all children to survive, leading to larger families in the former than the latter populations. As for foragers, Draper and Harpending (1987) highlight similarities with urban parents, with a greater degree of investment in individual infants and overall lower fertility than in farming populations.

The very fact of having more versus fewer children may affect the likelihood that a given child is addressed: If parents need to divide their attention across more children, they may provide less input to each, and particularly to the younger (potentially less attention-demanding) infant (Havron et al., 2019; Sharma & LeVine, 1998). Moreover, in some populations, families are integrated into a hamlet where other people, including older children, look after the infant without necessarily talking to them. For instance, Harkness (1977, p. 311) reports that Kipsigis toddlers in a farming community spent a considerable portion of their day with other children, but the frequency of speech was lower at these times than when toddlers were with an adult (Harkness, 1977).

Additionally, parents from different populations may have distinct goals for the communicative and social abilities of their children. Rogoff (2003) has proposed that many human populations do not engage in infant-centric, dyadic interactions, because the expectation is for the child to become a proactive member of the community. Therefore, adults in these populations prefer to provide opportunities for the child to observe others' conversations, so that they can develop skills to track third-party interaction and decide when to jump in themselves (e.g., pp. 134–135 and 141–146). Other researchers have highlighted the importance of training children in producing appropriate speech in specific social contexts. For instance, Smith-Hefner (1988) reports that urban Javanese children are trained from an early age on honorifics and other adaptations which are crucial when the child interacts with people varying in social rank. It stands to reason that there is no pressure for generally high prevalence of infant-directed interactions in such cases, because caregivers are focused in getting children to talk correctly in specific situations.

As in the research on individual and socioeconomic variation summarized earlier, these factors can vary within a population (see Kline et al., 2018 for the importance of considering variation). Richman et al. (1992) highlight differences in caregiving styles with relative increases in the prevalence of infant-directed vocalization that correlate with mothers' formal schooling in a longitudinal design (see also LeVine et al., 2011). Although in Richman et al. (1992) more formal education led to increases in the prioritization of verbal communication during caregiving, the opposite has also been observed: Crago et al. (1993) describe an interesting shift in parenting habits based on observations and interviews in two pre-industrial Inuit villages. The authors report that older mothers (who tended to have fewer or no years of formal education) employed more infant-centric strategies (such as simplifying utterances and using Parentese intonation), than younger, more acculturated mothers with more schooling.

1.4 | The present study

In sum, the prevalence of infant-directed vocalization is hypothesized to be affected by variation in dimensions including overall fertility, parental investment strategies, prevalence of allocare by children, expectations of infants' role in the community, and many others, which vary within but also across populations. The present study aimed at taking one step toward understanding how these factors affect young children's experiences by estimating the extent of population differences across two groups of populations likely varying along many of these dimensions: on the one hand, rural, small-scale, subsistence-level populations; and on the other hand, urban, industrialized or post-industrial populations. For simplicity of expression, in what follows we refer to the former populations as "rural" and the latter as "urban," although we remain mindful of the fact that these are shorthand for a cluster of characteristics differing across the two groups of populations. Additionally, any conclusions on their differences reflect group-based comparisons, and are not depictions of individual populations. A last caveat is in order: Within the "rural" group, we are combining populations that have been predicted to differ in terms of parental investment (Draper & Harpending, 1987), and perhaps infant-directed vocalization prevalence. This was simply due to the fact that relatively few studies represented each.

To pinpoint the extent of population variation, and benchmark it against the more commonly studied socio-economic variation within North America, we carried out a thorough review and quantitative integration of work employing systematic behavioral observations. A long tradition in both psychology and anthropology employs systematic behavioral observations to study theoretical questions as diverse as caregiving practices (Belsky, 1979), investment strategies (Hewlett & Lamb, 2000), and infant-caregiver attachment (Leifer et al., 1972). In systematic behavioral observations, investigators typically observe for a certain length of time (for instance 5 s), and then note what they have seen (e.g., over the next 5 s). Often, the observer checks items on a short list, with a much smaller proportion of studies instead requiring the observer to write down a set of codes describing behaviors, to write/speak a running commentary, or to code behavioral categories with a machine. Regardless of whether behavior is coded continuously or intermittently, all such studies provide estimates of the prevalence or frequency of certain behaviors. Very often, infant-directed vocalization constitutes one of the coded behaviors.

This paper presents the results of a quantitative integration of data collected over a period of 62 years, across a large variety of populations, including some that have since been irrevocably changed by cultural contact. The questions addressed were as follows:

1. In the body of literature that was discovered, and which provided quantitative reports of frequency of infant-directed vocalization behaviors, what is the extent of differences in prevalence of infant-directed vocalization across groups of populations, and particularly across the rural and urban groups?
2. What is the extent of differences in prevalence of infant-directed vocalization behaviors across populations varying in socio-economic status in North America?

Importantly, this second question is subordinate to our primary interest, which is to estimate cross-population differences. We are motivated by the increasing attention given in behavioral sciences not just to the presence of an effect, but to the actual estimation of the *size* of that effect (Amrhein et al., 2019; Coe, 2002). The problem is that once we have estimated the size of cross-population differences, it will be difficult to know whether it is large enough to warrant attention or not. Therefore, we want to also estimate socio-economic status effects in the same body of data in order to put any urban/rural differences we observe in the context of the much richer literature that already exists on socio-economic status differences within the United States. We would like to stress that it is not our goal to engage in the ongoing discussions regarding socio-economic effects on language environment per se (e.g., Golinkoff et al., 2019; Sperry et al., 2019). Instead, we look at socio-economic effects merely as a benchmark. It may be that urban/rural differences are much smaller than socio-economic status differences within the United States, and thus it will be hard to study them further, for instance to understand what leads to urban/rural differences in this behavior and to document patterns of language acquisition as a function of infant-directed vocalization prevalence. In contrast, if the prevalence of infant-directed vocalization is as large as, or larger than, socio-economic status differences, then additional detailed studies should be feasible.

2 | MATERIALS AND METHODS

The review was not registered because no registry exists for this kind of systematic review. However, the present work follows all recommendations in the PRISMA(P) Statements (Moher et al., 2009; Shamseer et al., 2015). Additionally, transparency was ensured by publicly posting all of our materials (Cristia, 2022), including this manuscript, which is reproducible thanks to the use of RMarkdown (Baumer & Udwin, 2015) and Papaja (Aust & Barth, 2017) on R (R Core Team, 2013). We have also created a ShinyApp interface that allows readers to interact with the data and view the effects of changes in free parameters, available from https://acristia.shinyapps.io/vocXcult/.

2.1 | Search and selection protocol

Literature was found using an initial list (Cristia et al., 2019), PubMed, scholar.google.com and other searches, direct recommendations, and citations found in all of the above between May, 2018 and December, 2021. For example, one search was carried out in PubMed on 2019-11-03 with keywords "observation infant mother vocalization." The full list of searches is found in online Supplementary Materials (direct link: https://osf.io/jwpbq/?view_only=f9af0cf7d2574234a8517c38151e4210, "sources" tab).

Studies were first screened based on title and abstract (see Figure 1 for the PRISMA flowchart). Full text was inspected even in ambiguous cases. To be included in the present paper, studies had to report on systematic observations of infants (aged on average 24 months or younger) in their natural environment (home, daycare, hospital in the case of newborns, hospice in the case of institutionalized children, village), in an unconstrained situation. That is, studies where the experimenter deliberately tried to engage the family in conversation, asked the caregiver to do something specific like play with toys, or scheduled the visit to only sample one or a few specific situations like meal time were excluded, because they sample non-randomly from the child's experiences.

Each time the investigator came to observe the infant is called a *visit*. Each individual visit needed to last at least 30 continuous minutes for the study to be included. The reason for this was to increase the likelihood that participants have habituated to the observation. Of course, we cannot be certain that participants habituated, and the 30-min minimum may seem arbitrary. Interested readers who would like to set a higher threshold can download the data from the online materials, and re-do analyses with a different threshold. We made an exception for studies where the observer was residing in the community, as these may have employed spot observations, which are only a few minutes long.

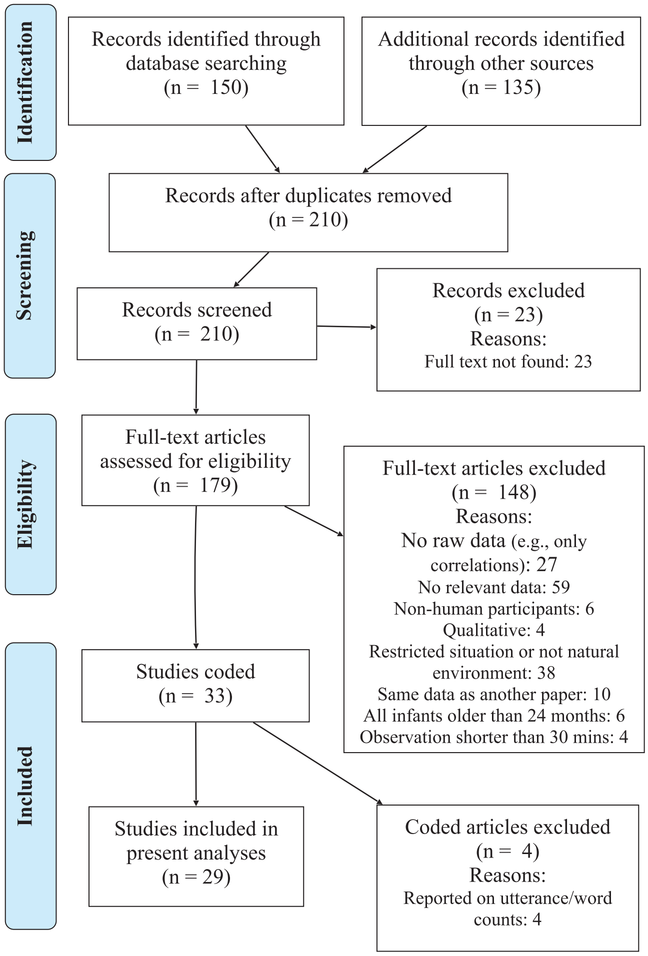

FIGURE 1 PRISMA Flow Diagram detailing number of studies considered and finally included

Often, investigators visited families several times in close succession, when the infant can be considered to have the same age. Typically, these studies integrate across all visits at the same age, and we did the same here. Thus, the minimal unit of data entry is constituted by a group of infants observed at a given age (perhaps over several visits). We call these *infant-age samples*. Some studies have a single group of infants, and others have several (e.g., one group of infants being raised in a hospice, and another group being raised in homes).

Data was entered maximizing the number of infant-age samples so as to increase precision, declaring data from groups of visits made to each group of children at a given age separately. That is, if there were two groups of children visited twice at one age, then two infant-age samples or rows (one for each independent infant group) were entered; if they were visited twice at two separate ages (e.g., at 4 and 10 months), then four infant-age samples or rows were entered, one for each infant-age sample. Notice that samples thus defined are not mutually independent (because the same infants can be seen at different ages).

Whenever a study reported visits to infants aged more than 24 months on average, these were also coded, but not analyzed here because there were very few such data points. Inspection of age trends also considering these older age groups does not change our conclusions in the least.[1] Other studies were coded but not included here because they relied on substantially different methods (counting words or sentences from video- or audio-recordings). Integrating across studies using very different methods requires parameters of how numbers of words or utterances can be converted to frequency of infant-directed vocalization or vice versa. For more information on them, see the online Supplementary Materials document (https://osf.io/9zgw6/, sections A and B). The complete dataset is available for download (direct link https://osf.io/jwpbq/), and results including all studies can be inspected via our ShinyApp (https://acristia.shinyapps.io/vocXcult/).

2.2 | Data entry

All data was initially extracted by a single coder (the author). We also attempted to contact corresponding authors of all included work to fill in missing information. For each infant-age sample, the following key demographic characteristics were entered whenever possible:

1. Socio-economic status, following the authors' description, collapsed onto a three-level scale: high, mid, low; mixed indicates the authors said the sample was mixed across these levels; NA if not reported;
2. location type, rural if clear that the families were hunter-gatherers, farmers, or villagers; urban otherwise;
3. number of children included in the observation;
4. the average and range of infant age in months; and
5. whether the infants deviated from a norm in some sense the authors of the original paper thought noteworthy (and which was not coded by the other demographic characteristics; e.g., preterm, institutionalized, difficult).

Key methodological characteristics coded at the level of the study were: length of each visit and total observed time; and the duration of each "observe" period for time-sampling studies with intermittent observation periods.

Frequency of infant-directed vocalization behaviors was extracted for subsequent quantitative analyses. Authors either reported this frequency as such, or they reported the number of observation intervals in which vocalizations were addressed to the observed infants, as well as the total number of observation intervals. Dividing the former by the latter and multiplying by 100 gives a frequency potentially ranging from 0% (= no interval contained infant-directed vocalization behaviors) to 100% (all observation intervals contained infant-directed vocalization behaviors). Some studies reported cumulated minutes per hour (Bunce et al., 2021; Casillas, et al., 2019, 2020; Cristia et al., 2019); this was divided by 60 to derive a frequency estimate like that of the other papers. Notice that included data is based on reported prevalence of infant-directed vocalization as a behavior, and not on counting utterances. Variance (standard deviation or range) was infrequently reported, but was coded when present.

A second coder unaware of the goals and results of the study was hired through a freelancer platform and provided with the codebook and the papers. After he had independently coded these data, the author compared all entries, going back to the articles whenever there were disagreements. There were 19 papers with secondary coding, containing 50 infant-age samples, which constitute 69% of the samples contributing to the current analyses. Together, these contained 1402 cells of data entered. Coders agreed in 93% of the cells, and the majority of divergences concerned details of the descriptors (e.g., the precise age range) rather than the dependent variables. More information can be found in supplementary materials (direct link https://osf.io/jwpbq/, tab "cf.coder1-2" and https://osf.io/wfpkt/ for the second coder's entries). After several months, the lead author reviewed all entries again, verifying them against the original articles (annotated articles available from https://osf.io/wt8fr/).

PRISMA(P) Statements require explicit discussion of bias. An attempt was made to avoid bias in study selection: Inclusion criteria bore exclusively on methodology, and no study was excluded on the basis of observed frequencies of infant-directed vocalization behaviors. Please note a decision sheet is available from supplementary materials (direct link https://osf.io/jwpbq/, tabs "criteria" and "papers"). Regarding bias at the level of individual studies, oft-cited sources of bias are financial conflicts of interest and/or theoretical preferences. The former is irrelevant here. As to the latter, given that data were collected by researchers who had diverse theoretical backgrounds and interests, it is unlikely that results reflect theoretical biases on the specific question of interest here, namely the frequency of infant-directed vocalization and its variation across urban/rural populations.

2.3 | Integration across papers

Time sampling studies vary in the length of the "observe" period, going from instantaneous scans (only code as present behavior that happened this past second) or continuous coding (using running narration and/or a machine for online coding) to one minute (code as present any behavior that happened at any point during the past 60 s). Previous methodological work demonstrates long observation periods lead to behavior over-estimation (Mann et al., 1991). We implemented three levels of correction to compensate for over-estimation: A null one (raw data, no correction applied); a steep one (based directly on Mann et al., 1991) in which estimations based on 5-s blocks are discounted by 40%, those based on 60-s blocks are discounted by 300%, and intermediate block durations get intermediate discounts; and a more lenient one where corrections were 80% of the steep correction. Observations based on continuous coding, instantaneous scans, or 1-second observation blocks were not corrected. Since there is independent data on the over-estimation induced by longer blocks, we focus on data emerging from the steep correction in the present paper. The other two correction levels lead to similar conclusions, as discussed in the online Supplementary Materials document (https://osf.io/9zgw6/, section C).

PRISMA(P) Statements require explicit discussion of analyses of consistency and bias. Answering our research questions involved using unstandardized effects. As a result, statistical analyses of consistency and bias could not be incorporated. Consistency was considered conceptually. The potential effect of several types of inconsistency is discussed in the online Supplementary Materials document (direct link https://osf.io/9zgw6/, sections C and D), including the duration of observe periods and whether only the mother or a broader set of speakers was considered.

3 | RESULTS

The final dataset draws from 29 papers, containing data for 72 infant-age samples, based on 60 independent samples of infants, totaling 1,314 infants. Table 1 summarizes the characteristics of included samples, identifying each paper with the last name of the lead author and year regardless of the number of authors (Belsky, 1979; Bunce et al., 2021; Campbell, 1979; Casillas et al., 2019, 2020; Caudill & Weinstein, 1969; Clarke-Stewart, 1973; Cristia et al., 2019; Feiring & Lewis, 1981; Fouts et al., 2012; Fracasso et al., 1997; Gordon et al., 2020; Hewlett & Lamb, 2000; Hewlett et al., 1998; Klein et al., 1977; Konner, 1977; Leifer et al., 1972; Lewis & Ban, 1977; Lewis & Wilson, 1972; Leyendecker et al., 1997; Lusk & Lewis, 1972; Moss, 1967; Rheingold, 1956, 1960; Richman et al., 1988; Roopnarine et al., 2005; Scelza, 2009; Tulkin, 1977; Whaley et al., 2002). We note that in two cases, data was reused in two separate papers, so entries reflect our best knowledge combining both reports; that is, Roopnarine et al., 2005 actually also includes information reported in Fouts et al. (2007); and the same for Tulkin, 1977 and Tulkin and Kagan (1972). A table with key information for papers that were coded but not included in the quantitative analyses presented here is provided in the online Supplementary Materials document (direct link https://osf.io/9zgw6/, sections A and B).

A majority of the data points (43 infant-age samples, 60%) was collected in the United States of North America (specified collection sites are CA, CT, DC, IL, MA, NJ, NY, various) or Canada. In terms of socio-economic status, the distribution in these samples was 8 low, 16 mid, 7 high, 3 mixed. Socio-economic status was missing for nine infant-age samples, mostly because the study pertained to infants growing up in a hospice. All of these data points came from urban or suburban sites.

The remaining 29 data points (40% of included infant-age samples) were collected in a wide variety of countries (Argentina, Australia, Bolivia, Botswana, Brazil, Central African Republic, Costa Rica, Guatemala, Italy, Japan, Kenya, Mexico, PNG, Senegal, Sweden, the United Kingdom, Yugoslavia). Among these, a majority were rural (18 infant-age samples, 62% of the non-American infant-age samples). There were seven infant-age samples from five populations described as forager, hunter-forager, hunter-forager-farmer groups (!Kung, Aka, Mardu, Pirahã, Tsimane'); and 11 samples from seven populations described as (subsistence) farmers (Embu, Guatemala, Gusii, Ngandu, Tseltal, Yélî, Yugoslavia).[2]

TABLE 1 Included studies

Paper	Geog	Group	Loc	SES	N	Age	%IDV
Belsky, 1979	NY	NY 1979	Urban	Mid	40	15	44
Bunce et al., 2021	various	NorthAm English	Urban	mixed	29	10	6
Bunce et al., 2021	UK	NA UK	Urban	mixed	10	20	6
Bunce et al., 2021	Argentina	Argentina	Urban	mixed	10	17	8
Campbell, 1979	Canada	Montreal 1977 difficult	Urban	Mid	6	3	15
Campbell, 1979	Canada	Montreal 1977	Urban	Mid	6	3	20
Campbell, 1979	Canada	Montreal 1977 difficult	Urban	Mid	6	8	12
Campbell, 1979	Canada	Montreal 1977	Urban	mid	6	8	21
Casillas et al., 2019	Mexico	Tseltal 2015	Rural[a]	[c]	10	16	6
Casillas 2021	PNG	Yélî 2015	Rural[a]	[c]	10	14	5
Caudill, 1969	Japan	Japan 1962	Urban	mid	30	3	13
Caudill 1969	DC	DC 1963	Urban	mid	30	4	15
Clarke, 1973	CT	Yale 1973	Urban	low	36	12	14
Clarke, 1973	CT	Yale 1973	Urban	low	36	16	16
Cristia et al., 2019	Bolivia	Tsimane'	Rural	[c]	24	6	1
Cristia et al., 2019	Bolivia	Tsimane'	Rural	[c]	9	18	1
Feiring, 1981	NJ	Princeton high 1980	Urban	high	92	3	17
Feiring 1981	NJ	Princeton mid 1980	Urban	mid	101	3	15
Fouts et al., 2012	[c]	US EA low 2003	Urban	low	20	4	12
Fouts et al., 2012	[c]	US EA mid 2003	Urban	mid	20	4	14
Fracasso 1997	DC	DC Euro-Am 1997	Urban	high	21	3	16
Fracasso 1997	DC	DC Central-Am 1997	Urban	low	17	4	13
Gordon et al., 2020	Brazil	Pirahã	Rural	[c]	8	24	<0.5
Hewlett et al., 1998	CAR[b]	Aka 1998	Rural	[c]	8	10	1
Hewlett et al., 1998	CAR[b]	Ngandu 1998	Rural[a]	[c]	20	10	2
Hewlett, 2000	CAR[b]	Aka 1998	Rural	[c]	20	4	1
Hewlett 2000	CAR[b]	Ngandu 1998	Rural[a]	[c]	21	4	1
Hewlett 2000	DC	DC 2000	Urban	high	21	4	10
Klein et al., 1977[b]	Guatemala	Guatemala 1970	Rural[a]	[c]	22	8	2
Klein et al., 1977[b]	Guatemala	Guatemala 1970	Rural[a]	[c]	21	12	3
Klein et al., 1977[b]	Guatemala	Guatemala 1970	Rural[a]	[c]	21	16	7
Konner, 1977	Botswana	!Kung 1970	Rural	[c]	7	10	7
Leifer et al., 1972	CA	US preterm sprtd 1972	Urban	mid	22	0	17
Leifer et al., 1972	CA	US preterm cntct 1972	Urban	mid	22	0	16
Leifer et al., 1972	CA	US fullterm 1972	Urban	mid	24	0	15
Leifer et al., 1972	NJ	Princeton 1971d	Urban	high	3	3	24
Lewis, 1972	NJ	Princeton 1971e	Urban	high	7	3	20
Lewis 1972	NJ	Princeton 1971a	Urban	low	9	3	19
Lewis 1972	NJ	Princeton 1971b	Urban	low	5	3	18
Lewis 1972	NJ	Princeton 1971c	Urban	mid	8	3	26
Lewis, 1977[b]	Yugoslavia	Yugoslavia Village 1977	Rural	low	9	3	19
Lewis 1977[b]	Yugoslavia	Yugoslavia City 1977	Urban	high	9	3	26
Leyendecker et al., 1997	Costa Rica	San Jose low 1997	Urban	low	20	3	12
Leyendecker et al., 1997	Costa Rica	San Jose mid 1997	Urban	mid	20	3	14
Lusk, 1972	Senegal	Wolof B 1972	Urban	low	6	6	7

(Continues)

TABLE 1 (Continued)

Paper	Geog	Group	Loc	SES	N	Age	%IDV
Lusk 1972	Senegal	Wolof A 1972	Urban	mid	6	4	19
Moss, 1967	DC	DC 1967 m	Urban	[c]	14	1	5
Moss, 1967	DC	DC 1967f	Urban	[c]	15	1	4
Moss, 1967	DC	DC 1967 m	Urban	[c]	13	3	6
Moss, 1967	DC	DC 1967f	Urban	[c]	12	3	6
Rheingold, 1956	IL	IL hospice t1 1956	Urban	[c]	4	6	13
Rheingold, 1956	IL	IL hospice c1 1956	Urban	[c]	4	6	2
Rheingold, 1956	IL	IL hospice t2 1956	Urban	[c]	4	6	11
Rheingold, 1956	IL	IL hospice c2 1956	Urban	[c]	4	6	3
Rheingold, 1960	DC	DC 1960	Urban	high	5	3	13
Rheingold, 1960	DC	DC hospice 1960	Urban	[c]	5	4	1
Richman et al., 1988	Kenya	Gusii 1988	Rural[a]	[c]	9	4	3
Richman et al., 1988	Kenya	Gusii 1988	Rural[a]	[c]	17	10	4
Richman et al., 1988	Mexico	Yucatec 1988	Urban	low	13	4	5
Richman et al., 1988	Italy	Italy 1988	Urban	low	20	10	28
Richman et al., 1988	MA	Boston 1988	Urban	mid	9	4	18
Richman et al., 1988	MA	Boston 1988	Urban	mid	9	10	21
Richman et al., 1988	Sweden	Sweden 1988	Urban	mid	20	10	40
Roopnarine et al., 2005	[c]	Afr–Am high 2003	Urban	high	21	4	9
Roopnarine et al., 2005	[c]	Afr-Am low 2003	Urban	low	20	4	6
Roopnarine et al., 2005	[c]	Afr-Am mid 2003	Urban	mid	21	4	8
Scelza, 2009	Australia	Mardu 2006	Rural	[c]	10	20	3
Tulkin, 1977	MA	Boston low 1968	Urban	low	30	10	8
Tulkin, 1977	MA	Boston mid 1968	Urban	mid	26	10	11
Whaley et al., 2002	Kenya	Embu 2002	Rural[a]	[c]	132	3	8
Whaley et al., 2002	CA	LA preterms 1973	Urban	mixed	112	2	24
Whaley et al., 2002	CA	LA fullterms 1973	Urban	mixed	27	2	29

[a]Farmers.
[b]Central African Republic.
[c]Not reported.
Papers represented by last name of first author and year. Geog = US state, otherwise country. Group = unique infant group identifier. Loc = location. Age (months). %IDV = percent observations containing infant-directed vocalization behaviors.

3.1 | Differences between urban and rural samples

What is the extent of differences in prevalence of infant-directed vocalization across populations, and particularly across our rural and urban groups? To answer this question, we averaged across infant-age samples and therefore ages so as to have mutually independent data points (i.e., based on independent infant groups).

We fit a linear regression on these sample averages, weighting samples as a function of the number of infants included, to assess whether the differences in prevalence of infant-directed vocalization between rural and urban groups apparent in Figure 2 were statistically significant. This revealed that infant-directed vocalization behavior in the samples classified as rural was less frequent than in both urban non-USA/Canada samples ($\beta = 11.51$, $t = 3.54$, $p < 0.01$); and in USA/Canada urban samples ($\beta = 9.41$, $t = 4.90$, $p < .001$). Assumptions for linear regressions were checked using the gvlma package (Pena & Slate, 2014), which suggested there were two urban outliers, with the aforementioned effects still being significant when these two outliers were included. Additionally, a direct comparison between urban samples within USA/Canada and urban samples outside USA/Canada revealed no significant difference in a two sample unpaired t-test: $t(13.63) = -0.49$, $p = 0.63$. Taking medians as our best estimate, the ratio between the frequency of infant-directed vocalization in urban non-USA/Canada as opposed to rural non-USA/Canada samples was 12.62% to 3.64%, or 3.47 when outliers were included. When they were excluded, it was 12.55% to 3.64% or 3.45. The ratio between

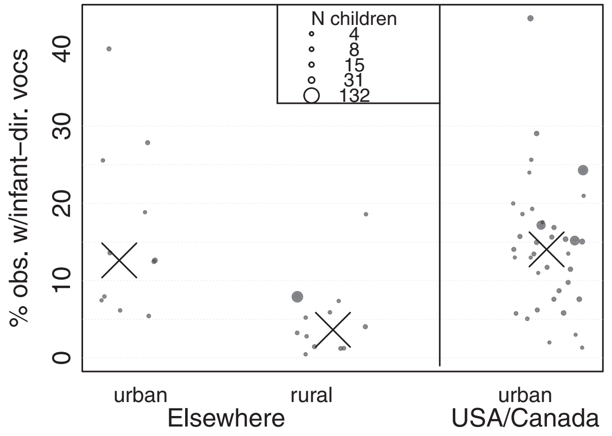

FIGURE 2 Frequency with which speech/vocalizations directed to the child are observed (averaged across repeated measures), as a function of population group. Each point is an independent sample. The size of the point indicates the sample size (in number of children included, averaged across repeated measures). Position along the x axis indicates whether observations were gathered in the United States/Canada or elsewhere, in an urban or rural location (jittered). Crosses indicate medians

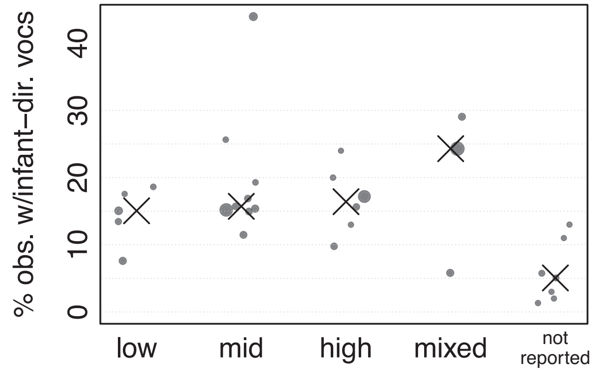

FIGURE 3 Frequency with which speech/vocalizations directed to the child are observed (averaged across repeated measures) in American urban or suburban samples, as a function of SES. Each point is an independent sample. The size of the point indicates the sample size (in number of children included, averaged across repeated measures). Position along the x axis indicates socioeconomic status (jittered). Crosses indicate medians. Five out of the nine samples for which SES was not reported were infants growing up in a hospice

the frequency of infant-directed vocalization in urban USA/Canada as opposed to rural samples was 3.86 when outliers were included, and 3.79 when they were excluded.

There was some variability within rural populations, which did not seem to relate to subsistence types. Among gatherer, hunter-gatherer, or hunter-forager-farmer populations, Aka and Tsimane' children had two of the lowest estimates (1.26% of observations contained infant-directed vocalization behaviors, in both groups), with slightly higher estimates for Mardu infants (2.80% of observations contained infant-directed vocalization behaviors), whereas !Kung infants had much higher estimates (7.37%). Predominantly farming populations spanned this same range (Ngandu 1.46%, Gusii 3.24%, Guatemalan 4.04%, Yucatec Mayan 5.43% Yélî Dnye 5.22%, Tseltal Mayan 5.90%, Embu 7.91%). The highest estimate was for a group of children growing up in a Yugoslavian village (18.59%), which had recently transitioned from farming to work-for-pay according to study authors (Lewis & Ban, 1977). The lowest prevalence documented was 0.47%, based on data from three Pirahã villages where farming was rare (Peter Gordon, personal communication, 2021-04-28).

3.2 | Differences between urban and rural samples

The goal of this subsection is to put the urban/rural population differences described in the previous subsection in the context of the oft-discussed socio-economic status (SES) differences among children in the United States. Figure 3 shows a weak trend for levels of infant-directed vocalization behaviors to correlate with SES, suggesting that this effect may be smaller than can be detected with the present approach, since there was no significant difference in a two sample unpaired t-test comparing high and low SES American samples: $t(9.47) = 0.78$, $p = 0.46$. Taking medians as our best estimate, the ratio between the frequency of infant-directed vocalization in high as opposed to low SES samples was 16.41 to 15.06%, or 1.09. This is not to deny that SES differences exist in the United States: Individual studies can capture smaller effects by studying low and high SES participants while keeping the methodology (including the definition of SES) perfectly constant, which is not possible in a meta-analytic framework. Additionally, we only looked here at the prevalence of infant-directed vocalization but not other features studied in previous work (including number of words, see meta-analyses in Dailey & Bergelson, 2020; Piot et al., 2021).

4 | DISCUSSION

We sought to assess how large population differences in the prevalence of infant-directed speech may be, starting with a contrast between rural, small-scale, subsistence-level populations and urban, industrialized populations. It is likely that this is one of the largest contrasts one can find, with populations differing along most dimensions thought to affect infant-directed vocalization prevalence (overall fertility, parental investment strategies, prevalence of allocare by children, expectations of infants' role in the community, prevalence and importance of formal education, prevalence of literacy, and others). Additionally, we are comparing groups of populations, and our conclusions should not be thought to describe each and every individual population (nor every individual family within a given population), a point to which we return below. To benchmark urban/rural population differences, we also included an analysis of socio-economic differences within US samples, who have so far constituted the focus of most cognitive development research (Nielsen et al., 2017) and who provide key data in the argument for infant-directed speech fueling language acquisition (as described in Section 1.1).

Although most published studies came from urban sites, and from American urban sites more particularly, there were enough observations in non-US/Canada urban and rural sites to carry out statistical comparisons. The results were clear: Infant-directed vocalization behaviors were observed less frequently in rural populations (including groups relying mainly on hunting, foraging, and/or farming for subsistence) than urban ones. Our follow-up analyses suggested this effect was much larger than that found for socioeconomic status within the US urban data. Together, these data quantitatively support previous qualitative ethnographic and anthropological reports, and scattered quantitative efforts, stating that the prevalence of infant-directed vocalization can be much lower in certain populations than others who are more frequently studied.

4.1 | Towards accurate population variation estimates

Using medians as our best estimate, the present behavioral observation data suggest infants in our urban populations group are addressed about three or four times as frequently as non-American children growing up in our rural populations group. One may wonder whether analyses using other data collection methods lead to a similar conclusion. Cross-population work using transcriptions of video-taped interactions leads to larger estimates when very young infants are considered, with 7 (Vogt & Mastin, 2013, comparing rural and urban families in Mozambique) to 10 (Shneidman & Goldin-Meadow, 2012, comparing urban families in Chicago with rural families in a Mayan village in Mexico) times as many utterances addressed to urban compared to rural 14-month-olds. In those same studies, results are also reported for infants aged 17 months or more, at which age differences drop to three times for the Chicago–Mayan comparison in Shneidman and Goldin-Meadow (2012), but remain stable at seven for the urban-rural Mozambique comparison in Vogt and Mastin (2013). When considering these ratios together, the three-fold difference we observe may seem small, and readers may worry that this is due to using a systematic review, which may contain increased noise due to the combination of estimates from diverse studies. However, this noise is offset by the increase in sample size (and thus precision): Our conclusions are based on 1,314 children, which is nearly ten times the number of children included in the largest study ($N = 132$) and nearly 70 times larger than the median sample size in the included studies ($N = 16$). In fact, it is also possible that previous work contains inflated estimates if the rural and urban populations studied differ in their reaction to the presence of video-cameras (see Shneidman & Goldin-Meadow, 2012 for evidence that Yucatec Mayan participants spoke more when not video-recorded). Vogt and Mastin (2013) attempted to reduce observer effects by setting up a camera on a tripod without an operator/investigator, but it would still be interesting to revisit the question of what the precise estimate is with even more unobtrusive technology, namely long-form recordings (Vandam et al., 2016), as used by Casillas et al. (2019). These are typically collected with a very small device worn by the child. Observer effects may be lower in this work because (a) the equipment is unobtrusive; (b) it can be used for very long periods of time (often 16 h), which may allow habituation; and (c) families may feel more at ease if they believe the resulting audio will be analyzed using a computer algorithm, rather than inspected by a human. These features of long-form recordings raise a variety of ethical and legal questions that would be important to bear in mind, particularly in cross-population research (Broesch et al., 2020; Cychosz et al., 2020). Recent analyses of long-form recordings align with the prediction that urban/rural population differences may be smaller than suggested by previous work using short video-recordings, with the largest difference being between an estimate for Yélî Dnye infants (5% in our metric) and Argentinean Spanish learners (8%) (Bunce et al., 2021).

4.2 | Speculation on the causes of cross-population variation

As estimations of the extent of cross-population variation accumulate, we can also ideally make progress in understanding why the prevalence of infant-directed vocalization behaviors varies. Some of this variation may be arbitrary, but other may be theoretically meaningful, as discussed in Section 1.3.

Are the urban/rural population differences we observe simply due to socioeconomic status? The differences in the prevalence of infant-directed vocalization behavior we observed in our urban/rural comparison were much greater than the socio-economic differences within North America in the same body of literature, so at the very least this suggests that the *socio-economic* differences between urban/rural populations may be larger than those within North American data.

That said, we hope that readers will not assume that the urban/rural population differences uncovered here do not need to be studied further on their own right. Indeed, our two groups of populations differed on many dimensions that cannot at present be teased apart, including not only a rural versus urban lifestyle but also access to formal education, overall fertility, and other factors, mentioned in the Introduction, that could all affect the prevalence of infant-directed speech. As a result, the differences we observe may be over-determined, with the particular rural populations represented here exhibiting lower frequencies of infant-directed vocalization behaviors for several, conceptually diverse reasons. Additionally, SES is itself a concept that conflates conceptually different variables (access to material wealth, formal education, but also exposure to stressors and many others Ellwood-Lowea et al., 2021; Farah, 2018).

Instead, we trust these robust differences will trigger interest in using a quantitative and cumulative scientific approach to tease apart the specific mechanistic explanations that have been posited and which require large enough data sets to distinguish the dimensions that are confounded here. Given that systematic observations are regularly used in anthropological work today, we have made all data open using the "community-augmented meta-analyses" framework (Tsuji et al., 2014). The Supplementary Materials (Cristia, 2022) include information that researchers can use to contribute additional data. This is ideal to make the best use of data collected in collaboration with many communities all over the world, including those that are changing via globalization and cultural contact. As explained in the next section, we hope this data set can grow not only in size but also in coverage.

With more power and better representation of population diversity, we can hope to start to shed light on the reasons why populations vary in the frequency with which infant-directed vocalization behaviors are observed. For example, we had hoped to address some of the factors previously proposed as potentially leading to population variation in infant caregiving practices, such as subsistence models among rural populations, but data were so sparse as to prevent meaningful statistical analyses. Descriptively, observations spanned a similar range for populations described as foragers, hunter-foragers, or hunter-forager-horticulturalist (Pirahã, Aka, Tsimane', Mardu, !Kung) and for populations described as primarily farmers (Ngandu, Gusii, Guatemalan, Yélî Dnye, Embu, Yucatec, Tseltal Mayan). It is possible that the overlap is due to inaccurate population classification, and a more appropriate analysis would have been based on a continuous variable (such as time spent foraging versus farming). By using an open meta-analytic model, these data are available to other researchers who may want to augment the data set with variables relevant to their own hypotheses (such as family size, inter-birth interval, or average per capita revenue) to evaluate the explanatory adequacy of pertinent theories (Cristia et al., 2021).

4.3 | Limitations and future directions

The present study synthesizes previous research to show that potential population differences can be studied because they are sizable. However, a great deal more work remains to be done.

First, although only studies with comparable methodologies were included, there were still some differences that were impossible to control for. A mega-analytic approach, in which raw data are combined via a standardized analysis pipeline, would be best to address this limitation (Costafreda, 2009; Sung et al., 2014; Verhage et al., 2020). Second, by drawing from published work, samples could not systematically represent variability even across the two key parameters of interest, namely the population types and the frequency of infant-directed vocalization. As highlighted by other work suggesting a strong bias in developmental data for North American locations (Nielsen et al., 2017), three fifths of the data points had been gathered in North America, and none of them in rural locations. Moreover, notice that only 12 rural populations were represented. Additionally, we are uncertain that within-population diversity (which may be crucial to test certain explanations for behavior variation, Kline et al., 2018) has been properly captured, given the relatively small sample sizes. Furthermore, there is no representation in our sample of families that lead a rural lifestyle but have relatively high SES (including in the otherwise well-represented North American subcontinent), and limited representation of families that live in urban locations but have relatively low SES, resulting in a data set in which these factors (and several others) cannot be teased apart. Given that systematic reviews cannot go beyond public information, improving upon these issues awaits that further data are collected and/or shared.

This leads us to a third limitation: We suspect there is more relevant data that was not discovered because the time sampling method did not feature prominently in the paper, or because they have not been published. We trust that the publication of the present report may allow authors of such data sets to come forward. Fortunately, our open community-augmented approach to both data and code will allow us or others to integrate later-emerging data points and update results.

Another limitation that we hope future work addresses pertains to the connection between two bodies of literature investigating infants' experiences, namely anthropological/social research and psycholinguistic research. The present study benefited from the massive literature employing behavioral observations that provide estimates of behavior frequencies; but there is no straightforward way to relate these results with those typically reported in published research employing video- or audio-recordings, which are based on counting words or utterances. Some rules of thumb could be employed (such as extrapolating from frequency of infant-directed vocalization to quantity of speech per hour, and then converting this into words or utterances per hour using an estimate of word and utterance duration). A fruitful avenue of research would employ both techniques concurrently. Long-form recordings can be systematically sampled in order to fill in the checklists observers would normally employ, similarly to how videorecordings were used to establish the accuracy of time sampling for other behaviors (Mann et al., 1991).

Along the same lines, we hope more research assesses potential downstream effects on child development, not only for language acquisition, but potentially also for other domains of cognitive development. It is possible that the prevalence of infant-directed vocalization behaviors is relevant to learning about the world (e.g., Csibra & Gergely, 2009), including about appropriate interpersonal behavior (e.g., Keller, 2012; Keller & Otto, 2009; Ochs & Schieffelin, 2001). Infant-directed speech may also be a way in which caregivers respond to children's needs and signals, and thus promote attachment according to some theories (e.g., Ainsworth, 1979; Bowlby, 1986; but see Keller, 2016). However, for both learning about the (social) world and attachment, infants' key experiences include non-verbal communicative signals, which would need to be integrated to make testable predictions regarding potential consequences of any diversity in experiences (Tsuji et al., 2021).

We can also study potential population differences in the acquisition of spoken languages, for which vocalization experiences are arguably more important than alternative sources of information (Landau et al., 2009). Previous work on socioeconomic status differences within USA in the prevalence of infant-directed speech suggests that downstream effects are visible in the lexicon (e.g., Hart & Risley, 1995). In our data, urban/rural population differences in the prevalence of infant-directed vocalization were much larger than socioeconomic differences; therefore, assuming mainstream theories about the crucial role of infant-directed speech hold across all populations, one predicts massive effects in children's vocabulary sizes (provided that these can be studied in a culture- and language-neutral way, Frank et al., 2021). However, it is very plausible that these theories do *not* hold true of all populations, if nothing else because they have been generated based on biased data (Broesch et al., 2020; Nielsen et al., 2017). For instance, it is possible that urban children's remarkable reliance on child-directed input is a side effect of its very salient prevalence (see Shneidman & Woodward, 2016 for a review of learning from third-party interaction).

Additionally, it would be important to also look at other levels of language expertise beyond the lexicon. Consideration of how phonology, morphosyntax, and pragmatics develop in more diverse populations could be another way in which the research community comes to revise the hypothesis that infant-directed speech fuels language acquisition (see Cristia, 2020 for a discussion of diverse theories explaining phonological acquisition, only some of which predict population differences in outcomes due to differences in infant-directed vocalization frequencies). For instance, Casillas and colleagues studied two farming populations with very similar levels of prevalence in infant-directed vocalization behaviors and found the development of several production milestones, including the emergence of canonical syllables, first words, and word combinations, to be comparable to that reported for urban North American children (Casillas et al., 2019, 2020). We echo Kachergis et al. (2021)'s call that a true understanding of the cognitive strategies subtending language acquisition across diverse settings and language levels requires us to move from relative comparisons of input and outcomes (e.g., comparing groups of children in low versus high prevalence settings) to absolute measurements (i.e., linking language experiences to outcomes within individuals or groups). Dupoux (2018), in particular, proposes a reverse-engineering approach, where computer models learn language from samples of children's everyday input as captured using long-form recordings. Building on this idea, Lavechin et al. (2021) suggest that children's productions in long-form recordings are benchmarked against those of adults in the same recordings. Ideally, such a move would help us avoid being trapped by inappropriate assumptions and unfair between-population comparisons – although we recognize that this is never easy (Broesch et al., 2020).

5 | CONCLUSIONS

The extant body of literature using systematic behavioral observations of infants showed considerable variation in the prevalence of infant-directed vocalization behaviors, with marked differences across two groups of populations. This difference was much larger than that found across SES groups within the United States in the same body of data. We believe these results invite further research to understand the factors leading to population variation in input quantity, in addition to research documenting stability and variation in language outcomes, and on the mechanisms linking the two.

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CONFLICT OF INTEREST

The author declares no conflict of interest.

DATA AVAILABILITY STATEMENT

All data are made available in a repository in the Open Science Framework, DOI 10.17605/OSF.IO/C86EW.

ORCID

Alejandrina Cristia https://orcid.org/0000-0003-2979-4556

ENDNOTES

[1] We originally intended to assess whether differences across urban/rural populations became smaller as children aged. Following a reviewer's suggestion that the data were too sparse, this analysis has been moved to the supplementary material (https://osf.io/4uc8r/?view_only=f9af0cf7d2574234a8517c38151e4210, Section F).

[2] The Guatemalan sample is described as follows: "four isolated subsistence farming communities in eastern Guatemala"; and the Yugoslavian village sample as follows: "As recently as one generation ago, all the residents of both villages were dependent on farming for subsistence. The peasant-style homes and most of the fields remain today; however, a dramatic transition is in evidence." All infants' fathers traveled to do work-for-pay.

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