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Responsive Caregiving, Home Learning Materials, and Subsequent Language Development in Rural Pakistan and India: A Secondary Analysis

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ABSTRACT

Nurturing care is associated with both responsive care and access to learning materials, but the independent and combined associations of these variables with subsequent language development remain poorly understood in rural South Asian contexts. We analyzed linked 12- and 18-month assessment data from the SPRING cluster-randomized trials in Rawalpindi District, Pakistan (n = 915) and Rewari District, India (n = 1,272) using an observational secondary analysis. We assessed caregiver responsivity (11 items) and learning materials (9 items) using the HOME inventory at about 12 months, and measured Bayley-III language composite scores at about 18 months. Country-specific regressions adjusted for maternal and child characteristics and trial assignment, using CR2 small-sample cluster-robust standard errors. Four major tests were evaluated with the Holm correction. In Pakistan, learning materials were positively associated with subsequent language scores (B = 0.334, 95% CI [0.011, 0.657]), but the association was not statistically significant after Holm correction (adjusted p = .087). In India, later language scores were determined by learning materials, whereas responsivity was not (B = 0.329; p = .267). No statistically significant interaction emerged between caregiver responsivity and learning materials in either setting (p > .32). Exploratory comparisons did not identify statistically significant differences in the associations between countries. The findings suggested distinct patterns of association between the two home-environment dimensions and subsequent language development, although evidence for these associations varied across settings. The results did not support compensatory or complementary interactions. Given the observational nature of the analysis, we cannot draw causal conclusions.

Introduction

Language development in the first 2 years of life relies on children's opportunities to hear meaningful speech, engage in reciprocal exchanges, and explore the environment (Tamis-LeMonda et al., 2001; Weisleder & Fernald, 2013). These early experiences are unequally distributed within and across populations, reflecting differences in material circumstances, caregiver resources, and access to developmentally supportive environments (Black et al., 2017; Walker et al., 2011). In low- and middle-

income countries, improving the quality of early environments is consequently regarded as an important component of strategies to reduce developmental disadvantage (Britto et al., 2017; Richter et al., 2017).

The Nurturing Care Framework distinguishes responsive caregiving from early learning opportunities and emphasizes their joint contribution to children's developmental environments (Britto et al., 2017; World Health Organization et al., 2018). Responsive caregiving refers to adults' ability to perceive and respond appropriately to a child's signals, whereas early learning opportunities include interactions and accessible activities/materials that support exploration and communication (Britto et al., 2017; World Health Organization et al., 2018). They are conceptually similar but do not necessarily relate to developmental outcomes in the same way, especially when measured by caregiver behaviors and the types of household resources available (Bronfenbrenner & Morris, 2006; Hamadani et al., 2010).

According to bioecological theory, repeated, reciprocal relationships between the developing child and the immediate environment form the core of developmental change, including social, personal, and symbolic relationships with humans and other symbols and objects (Bronfenbrenner & Morris, 2006). Applied to early communication, this view focuses not just on the presence of a caregiver, but on the timing, reciprocity, and quality of communication exchanges children engage in (Bronfenbrenner & Morris, 2006; Tamis-LeMonda et al., 2001). A longitudinal study of maternal responsiveness found that contingent maternal responses were associated with children's attainment of language milestones, supporting the relevance of responsive exchanges during early language acquisition (Tamis-LeMonda et al., 2001).

Research on language input offers a complementary account of how everyday interaction may be related to later language skills (Rowe, 2012; Weisleder & Fernald, 2013). Among Spanish-speaking families with relatively low socioeconomic resources, greater child-directed speech was associated with more efficient language processing and larger expressive vocabularies in infancy and toddlerhood (Weisleder & Fernald, 2013). Longitudinal evidence further indicates that the qualities of caregiver speech relevant to vocabulary development vary with children's developmental stage, cautioning against treating all verbal input as interchangeable (Rowe, 2012). These studies support attention to the interactive language environment, although child-directed speech and a global HOME responsivity score are not equivalent measures (Rowe, 2012; Tamis-LeMonda et al., 2001).

Learning resources may support early communication by creating opportunities for naming, pointing, joint attention, exploration, and interaction with adults, although the presence of a resource does not determine how it is used (Bronfenbrenner & Morris, 2006; Dowdall et al., 2020). Evidence from rural Bangladesh is particularly pertinent because researchers examined family-care indicators of play activities and play materials alongside developmental assessments in a resource-constrained setting (Hamadani et al., 2010). Their analyses found that, after adjusting for socio-economic outcomes, play materials and play activities predicted several developmental outcomes for a child (Hamadani et al., 2010). This includes separating the material opportunities from caregiving activities. In addition, they proposed reducing the home environment to a single total score (Hamadani et al., 2010).

Experimental research also suggests that structured activities involving simple learning resources can support language, while showing that materials typically operate within an interactional practice (Dowdall et al., 2020; Vally et al., 2015). In a randomized trial in a disadvantaged South African community, training caregivers in dialogic book sharing improved caregiver-reported infant language and attention outcomes compared with a control condition (Vally et al., 2015). A systematic review and meta-analysis of 19 randomized trials reported improvements in children's expressive and receptive language following shared picture-book reading interventions, alongside larger improvements in caregivers' book-sharing competence (Dowdall et al., 2020). These trials evaluate supported shared-reading activities, however, rather than the isolated effect of owning books or scoring higher on a home-materials inventory (Dowdall et al., 2020; Vally et al., 2015).

One theoretically plausible possibility is that when adults are responsive, they make objects more meaningful through shared attention, labeling, and guided exploration, and thus they are more likely to develop an association between materials and their development when adults are more responsive (Bronfenbrenner & Morris, 2006; Dowdall et al., 2020). An alternative possibility is that frequent responsive exchanges provide language-learning opportunities even when few materials are available, implying a compensatory pattern rather than an amplifying one (Bronfenbrenner & Morris, 2006; Tamis-LeMonda et al., 2001). These competing propositions motivate examining a responsivity-by-materials product term, but existing evidence from observational HOME indicators and shared-reading interventions does not, by itself, establish either interaction (Dowdall et al., 2020; Hamadani et al., 2010).

South Asian literature on this topic supports the modifiable aspects of home caregiving. Interventions and observations on these aspects also address different questions (Sikander et al., 2023). A cluster-randomized factorial trial delivered through Pakistan's Lady Health Worker program found benefits of an integrated responsive-stimulation intervention for early child development. It also demonstrated that caregivers focused on programming can improve developmental outcomes (Yousafzai et al., 2014).

More broadly, a global systematic review of parenting interventions in the first three years of life found benefits for child development and parenting practices, but also documented variability across programs and outcomes (Jeong et al., 2021).

The SPRING program evaluated a scalable home-visiting intervention in parallel cluster-randomized trials in Rawalpindi District, Pakistan, and Rewari District, India (Kirkwood et al., 2023; Sikander et al., 2023). This primary analysis found no intervention effect on 18-month developmental composites in either setting, and the investigators noted implementation issues in their interpretation (Sikander et al., 2023). This null randomized intervention contrast does not determine the associations of naturally varying aspects of observed caregiving within each trial population, because intervention assignment and HOME-IT responsivity or learning-material scores are distinct exposures (Kirkwood et al., 2023; Sikander et al., 2023).

Recent secondary work using Indian SPRING records evaluated postpartum depressive symptoms, related psychosocial risks, and developmental outcomes, underscoring the value of additional analyses of the released assessment data (Kumar et al., 2026). That analysis did not center its research question on the independent and joint relationships of the two HOME-IT domains with continuous language scores across both trial settings (Kumar et al., 2026; Sikander et al., 2023). Consequently, a focused two-setting analysis can extend, not replace, the original program evaluation and existing psychosocial research by specifying which aspects of the 12-month home environment show associations with the 18-month language outcome (Sikander et al., 2023; Kumar et al., 2026).

This secondary analysis examined mutually adjusted associations of 12-month caregiver responsivity and home learning materials with 18-month Bayley-III language composite scores in Pakistan and India (Kirkwood et al., 2023; Rahman et al., 2023; Sikander et al., 2023). Previous work on responsive caregiving and home stimulation anticipated positive independent associations (Hamadani et al., 2010; Tamis-LeMonda et al., 2001; Weisleder & Fernald, 2013). Because both compensatory and complementary patterns were plausible, we used a nondirectional interaction test (Bronfenbrenner & Morris, 2006; Dowdall et al., 2020). We examined cognitive and motor composites as secondary outcomes, and interpreted all estimates as observational associations rather than causal effects (Hamadani et al., 2010; Jeong et al., 2021; Walker et al., 2011).

Methods and Materials

Design and Setting

This study used anonymized public-use impact-assessment records from the Sustainable Program Incorporating Nutrition and Games (SPRING) program in the World Bank Microdata Library (Kirkwood et al., 2023; Rahman et al., 2023). The parent program comprised parallel cluster-randomized trials in Rawalpindi District, Pakistan (20 union-council clusters), and Rewari District, Haryana, India (24 health sub-center catchment clusters). The original intervention assignments were cluster-randomized, whereas the caregiving exposures considered here were observed, not randomized. The unit of analysis was the child; assessments were linked within each impact-assessment record. The data are not nationally representative. Sikander et al. (2023) describe details of original recruitment, trial registration, measurement, and ethical approvals.

Participants and analytical sample

The Pakistan and India impact-assessment releases contained 5,048 and 5,049 records, respectively. For Pakistan, eligibility required both the official 12-month HOME completion flag (home12m = 1) and the 18-month Bayley outcome flag (bsid18m = 1). Of n=1,232 children with valid HOME assessments, n=916 had linked Bayley assessments; one lacked a model covariate, leaving n=915 complete cases. For India, n=1,303 children had a completed HOME assessment in the designated 12-month evaluation cohort (homeitdone = 1 and twelvebin = 1); n=1,272 also had the 18-month assessment (eighteenbin = 1), and all n=1,272 had complete model covariates. Nine additional India records with HOME data were outside the designated 12-month cohort; none entered the linked analysis. The final sample thus included n=2,187 children, with 20 and 24 randomized clusters represented. The eligible sub-cohorts reflect the original assessment schedule and sample design as well as nonparticipation; they must not be interpreted as pure attrition rates.

Measures

At approximately 12 months, trained assessors administered the 45-item Home Observation for Measurement of the Environment Infant-Toddler (HOME-IT) inventory. The responsivity domain (11 binary-scored items; possible range 0–11) captures communicative and affective caregiver interactions, while the learning-materials domain (9 binary-scored items; possible range 0–9) reflects age-appropriate play and learning resources. Higher domain scores indicate more observed/elicited nurturing features. The main outcome was the Bayley Scales of Infant and Toddler Development, Third Edition (Bayley-III), language composite score at approximately 18 months. Cognitive and motor composite scores were secondary outcomes. The original investigators culturally adapted and standardized the assessment procedures (Kirkwood et al., 2023; Sikander et al., 2023).

Country-specific models controlled for child sex, maternal age at delivery, maternal education, within-country household socioeconomic quintile, child age at the 18-month developmental assessment, and original SPRING trial arm. We entered Pakistan maternal education as years of schooling, whereas India maternal education as a seven-category factor, consistent with the mentioned variables. Unassessed cases may have nonstandard numeric placeholders in some score columns. We therefore applied documented final-analysis flags before HOME and examined outcome scores. Complete-case analysis was conducted without imputation; only one of the linked n=2,188 cases was missing a primary model covariate.

Statistical Analysis

We first estimated country-specific descriptive statistics and exploratory Pearson correlations. The primary model was a linear regression of language composite score on mean-centered responsivity and mean-centered learning materials, adjusted for the covariates above. Country-specific models were fitted because sampling contexts and maternal education coding differed. Inference used cluster-robust CR2 variance estimation with coefficient-specific Satterthwaite degrees of freedom, with observations clustered by the original randomization unit. This is preferable to uncorrected asymptotic GEE standard errors with 20 and 24 clusters (Pustejovsky & Tipton, 2018). We reported unstandardized coefficients with 95% confidence intervals. We also reported CR2 standard errors and exact p-values. Holm familywise error adjustment was applied to the four country-by-predictor primary tests (Holm, 1979). The interaction model additionally included the product of the two centered HOME dimensions. We treated the cognitive and motor models as secondary and assessed eight country-by-outcome-by-predictor tests separately using Holm correction. Exploratory comparisons of country coefficients used independent-estimate contrasts with small-sample degrees-of-freedom approximation; these are not tests of population equivalence.

Model diagnostics examined continuous-predictor variance inflation factors, residual distributions, influential observations, and heteroscedasticity. Robustness analyses included refitting each of the primary regressions with each cluster omitted one by one, building n=2,500 cluster-pairs bootstrap resamples of the data within each trial arm for each country, and fitting alternative covariate specifications that also included the maternal symptom score on the PHQ-9 and the original baseline cluster-context indices in eligible subsamples. Within the targeted 12-month HOME cohort, we compared sample inclusion. We screened data and ran initial models in IBM SPSS Statistics, and independently calculated reported OLS/CR2 results and sensitivity checks in Python using statsmodels, SciPy, and a documented CR2 routine. The study is presented as an observational secondary analysis in accordance with the STROBE principles (von Elm et al., 2007).

Results

Table 1

Participant inclusion and final sample in the two SPRING settings

Stage	Pakistan	India
Source impact-assessment records	5,048	5,049
Completed 12-month HOME; designated trial cohort	1,232	1,303
Linked 12- and 18-month assessments	916	1,272
Not linked to valid 18-month assessment	316	31
Excluded because of covariate missingness	1	0
Final analytical sample	915	1,272
Randomized clusters represented	*20*	*24*

Note. The source files are not baseline cohort denominators. For India, n=1312 HOME records were completed, whereas 9 were designated as the 12-month-old cohort and entered the linked analysis. The final sample was n=2187, and designated HOME records were 74.4% and 97.6%, respectively

Table 1 summarizes the sample flow. Linkage between the 12- and 18-month assessments was less complete in Pakistan than in India, suggesting a greater potential for selection-related limitations in the Pakistani sample.

Table 2

Demographic characteristics, HOME scores, and developmental outcomes

Characteristics	Pakistan (*n* = 915)	India (*n* = 1,272)
Maternal age at delivery, years	26.84 (4.50)	23.25 (3.75)
Age at Bayley assessment, months	18.74 (0.27)	18.36 (0.29)
HOME-IT total score (0–45)	27.25 (5.11)	31.61 (4.11)
HOME-IT responsivity (0–11)	6.73 (1.95)	9.17 (1.62)
HOME-IT learning materials (0–9)	3.89 (2.29)	4.94 (1.78)
Language composite	93.45 (12.45)	90.74 (14.18)
Cognitive composite	90.09 (9.53)	92.64 (10.62)
Motor composite	97.99 (11.93)	94.50 (9.72)
Girls, n (%)	434 (47.4%)	592 (46.5%)
SPRING intervention arm, n (%)	458 (50.1%)	629 (49.4%)
Maternal education, years	7.85 (4.30)	—

Note. Values are M (SD) except where indicated. Maternal education was measured as years of schooling in Pakistan and seven categories in India; category distributions were included in India's adjusted models. HOME-IT = Home Observation for Measurement of the Environment Infant-Toddler version. We obtained Bayley-III composite scores at approximately 18 months.

HOME-IT scores differed descriptively across settings (Table 2). In particular, 22.5% of Indian children had the maximum responsivity score of 11, compared with 2.7% in Pakistan, suggesting a potential ceiling constraint on estimated associations in India. The formal comparison of populations is not mentioned in these summaries. Table 3 presents cluster-robust adjusted coefficients.

Table 3

Adjusted associations of 12-month home exposures with 18-month language composite scores

Country and exposure	B	CR2 SE	95% CI	*df*	*p*	Holm *p*
Pakistan – Responsivity	0.657	0.158	[0.325, 0.989]	17.7	< .001	.002
Pakistan – Learning materials	0.334	0.154	[0.011, 0.657]	18.3	.044	.087
India – Responsivity	0.329	0.288	[-0.273, 0.931]	19.9	.267	.267
India – Learning materials	0.789	0.264	[0.242, 1.337]	22.0	.007	.020

Note. The country-specific linear models are controlled for various variables, including household wealth, child sex, and maternal variables. The CR2 correction for the clustering of randomized sample units was also included using df accounts. - Observational estimates were used with Holm correction.

As shown in Table 3, after adjusting for the four primary tests, the evidence lies in caregiving responsivity in Pakistan. It also supports learning materials in India. The learning materials coefficient was not significant, as the confidence interval for the other country included zero. These country-specific models explained 7.6% and 11.4% of the observed variance in language scores for Pakistan and India, respectively.

Table 4

Exploratory analysis between setting slope contrasts and coefficients

Contrast	B	SE	95% CI	*p*
Pakistan	-0.062	0.062	[-0.194, 0.069]	.328
India	0.124	0.147	[-0.189, 0.436]	.412
Responsivity: Pakistan – India	0.328	0.329	[-0.343, 0.999]	.327
Materials: Pakistan – India	-0.456	0.306	[-1.077, 0.165]	.145

Note. The first two rows are within-setting responsivity × materials interaction terms from the adjusted language model. The remaining rows contrast independently fitted main-effect coefficients, with independent-sample variance and Satterthwaite df; different maternal-education coding limits direct site equivalence. The comparison does not imply causal effect modification.

Table 4 presents interaction coefficients and exploratory between-setting slope contrasts. The confidence intervals for both responsivity-by-materials interaction terms included zero (Table 4). All between-setting coefficient contrasts were exploratory and not statistically significant; therefore, do not use differences in the underlying slopes between countries to infer differences in between-country p-values. Caution is also needed when interpreting contrasts, as they were coded differently across settings. The cognitive and motor models are provided in Table 5.

Table 5

Adjusted secondary associations with 18-Month cognitive and motor development

	Outcome	Exposure	B	95% CI	*p*	Holm *p*
Pakistan	Cognitive	Responsivity	0.213	[-0.274, 0.700]	.369	.738
Pakistan	Cognitive	Learning materials	0.227	[-0.079, 0.533]	.137	.549
Pakistan	Motor	Responsivity	0.347	[-0.195, 0.890]	.195	.585
Pakistan	Motor	Learning materials	0.411	[0.037, 0.784]	.033	.197
India	Cognitive	Responsivity	-0.020	[-0.419, 0.378]	.916	.916
India	Cognitive	Learning materials	0.652	[0.183, 1.121]	.009	.069
India	Motor	Responsivity	0.412	[0.025, 0.799]	.038	.197
India	Motor	Learning materials	0.462	[0.060, 0.863]	.026	.183

Note. Country-specific adjusted models use the same analytic samples and CR2 small-cluster correction as the language models. Holm p corrects the eight secondary tests as one separate family

Although several cognitive and motor associations were nominally significant, none were statistically significant after Holm correction for the eight secondary tests (Table 5). These secondary analyses did not provide sufficient statistical evidence for associations with cognitive or motor development after correction for multiple testing. Multicollinearity was low, and diagnostic analyses showed no highly influential individual observations. Cluster-bootstrap and leave-one-cluster-out analyses generally corroborated the direction of the main associations. The estimate for the association between learning materials and India remained positive after further covariate adjustment, while the estimate for the association between learning materials and Pakistan was slightly less precise. We found no significant differences in the measured aspects of caregiving between included and non-included children, but we cannot rule out selection bias. The supplementary material provides detailed diagnostic and sensitivity results.

Discussion

This analysis distinguished between relational and material indicators of nurturing care in two samples from rural SPRING areas. Table 3 illustrates the language association of the items after small-cluster and multiple-testing adjustments; in Pakistan, responsivity showed a clear association, and in India, learning materials did. The interaction tests and exploratory contrasts of site-specific slopes did not suggest any statistically demonstrable effect modification (Table 4).

These associations were modest and should not be interpreted as intervention effects or as evidence of clinical significance. The positive association between caregiver responsivity and language development in Pakistan is consistent with previous longitudinal evidence linking contingent maternal responses to the subsequent attainment of language milestones (Tamis-LeMonda et al., 2001). Studies of CDS offer a plausible explanation: when conversations are addressed to the child, not just overheard, these interactions have been linked to the child's processing efficiency and vocabulary at later ages (Weisleder & Fernald, 2013). In the current study, these mechanisms remain speculative, as HOME-IT responsivity was not measured as a verbatim count of conversational turns or contingent verbal response (Sikander et al., 2023; Rowe, 2012).

The learning-materials association more consistently supported in India aligns with evidence linking a stimulating home environment and shared learning activities to early developmental outcomes (Dowdall et al., 2020; Hamadani et al., 2010). While shared-reading interventions can enhance children's language skills, the benefits likely come when adults engage with children in the material, not from having objects read to them (Dowdall et al., 2020; Vally et al., 2015). Thus, the current association cannot separate the effects of resources from other family routines, caregiver preferences, socioeconomic opportunities, and prior child capabilities (Bronfenbrenner & Morris, 2006; Hoff, 2003; Walker et al., 2011).

Measurement context will also affect the meaning of magnitude differences across settings (Kirkwood et al., 2023; Sikander et al., 2023). Both the Indian analytical cohort's average HOME-IT responsivity score and its clustering near the upper limit may limit the variation needed to estimate an association with later language (Table 2). The items may also affect HOME differently across the two populations, and distinct socioeconomic and maternal-education coding further limits direct cross-setting comparison (Sikander et al., 2023). The formal contrasts between settings were not statistically significant, and the observed pattern is not sufficiently compelling to support claims that it reflects national caregiving norms or that different developmental processes are involved in each setting (Table 4).

The evidence for an additive scale compensatory or amplifying pattern from the responsivity-by-learning-materials interaction was not statistically significant in either setting; thus, the data provide insufficient evidence for such a pattern (Table 4). Bioecological theory does not require that the observed indicators in one regression model include a product term in the other; it permits interaction between the observed indicators and the observable product term, but does not require a product term (Bronfenbrenner & Morris, 2006). However, a null interaction does not rule out more specific developmental thresholds, developmental changes over time, or interactions in which the quality of shared play is the focus rather than the number of available objects (Dowdall et al., 2020; Rowe, 2012). To differentiate these possibilities, repeated observation of how adults and children use particular learning resources would be required (Bronfenbrenner & Morris, 2006; Vally et al., 2015).

None of the secondary coefficients remained significant after familywise correction (Table 5), though some indicated potential associations with cognitive or motor development. The results do not, however, provide generalizable links to other developmental areas, despite previous reports of generalizations to other areas of development from stimulation-based interventions (Hamadani et al., 2010; Jeong et al., 2021).

Strengths and Limitations

The study has strengths, including the temporal ordering of caregiving and developmental assessments, examination of two dimensions of the home environment, and use of data from Pakistan and India. The results are further bolstered by the adjustment for trial allocation and relevant family characteristics, cluster-robust inference, and sensitivity analyses. There are a number of limitations to be noted. First, the analysis is observational; therefore, one cannot draw causal conclusions, and residual confounding cannot be ruled out. The lack of previous language assessments also limits consideration of the potential impact of children's current skills on caregiver responses. Second, the HOME assessment provides limited observation of caregiving practices, and in India, relatively high responsiveness scores may lead to ceiling effects. Differential sample inclusion may have introduced selection bias, especially in Pakistan. In addition, the authors do not formalize measurement-invariance testing, and differences in measurement and socioeconomic coding across countries limit direct cross-country comparisons. Finally, the relatively small number of clusters can also limit statistical precision, even with small-sample corrections.

Conclusion

In rural SPRING settings in Pakistan and India, early caregiver responsivity and learning materials showed distinct patterns of association with subsequent language scores, with the strongest familywise-corrected evidence for responsivity in Pakistan and for learning materials in India. These observational findings do not establish causal effects, statistically different country mechanisms, or compensatory interactions.

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