The productivity of public and private preschools (and schools): Evidence from India*

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Abstract

We study the relative productivity of private and public institutions at the preschool and primary school levels using panel data from 215 villages in Tamil Nadu. Private preschools show higher test score value-added in math and language ($\sim 0.59 - 0.74\sigma$) and outperform government providers in nearly all villages. This productivity difference explains 60% of the socioeconomic test score gap before school entry. These results contrast starkly with primary schooling, where we find no evidence of a private sector premium in math and negative effects in local language. Test score value-added is positively correlated between private and government options in a village, both at the preschool and primary school levels. Quality is also correlated across levels; villages with more productive primary schools also tend to have more productive preschools. Our findings inform debates on achieving universal foundational skills and highlight the need to improve the quality of preschools available to lower-income families.

Keywords: early childhood education, primary schooling, education systems, education markets

JEL Codes: H44, H52, I21, I25, I28, L10, L33

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1 Introduction

Private providers account for a substantial share of education services in low- and middle-income countries (LMICs). They are particularly important in preschool services where, globally, they account for ∼37% of children enrolled in pre-primary institutions, compared to 19% of enrolment at the primary level in LMICs (UNESCO, 2021; UNESCO, 2022). Yet, evidence comparing the productivity of private to public preschools using broad samples remains scarce. This omission is in stark contrast to numerous studies on private primary school effects (Crawfurd et al., 2024), and is especially surprising given broad recognition of the importance of early childhood education (Elango et al., 2015; Holla et al., 2021), the relative importance of private providers in the sector, and international policy targets to universalize quality preschool services (see Sustainable Development Goals Target 4.2).

In this paper, we focus on understanding the relative productivity of private and public preschools using a new child-level panel dataset of ∼ 19,000 children aged 3–10 years in 215 villages in the Indian state of Tamil Nadu. Importantly, the data include tests of student achievement in math and the local language (Tamil), using age-appropriate tests administered at home, as well as multiple measures of socioeconomic status and educational inputs; the test scores are vertically-equated using Item Response Theory models to be comparable over time and across ages. Although preschool is not compulsory in the setting we study, nearly all children are enrolled at age 4. Primary school enrolment is compulsory and near-universal by age 6. In our setting, private providers account for roughly one-third of enrolment at preschool age and one-quarter in primary schooling.

We use these data to conduct three exercises. First, following early studies of private schooling in South Asia (Andrabi et al., 2011; Singh, 2015), we estimate the average private institution effect at the preschool and primary school stages using value-added models of student achievement. In addition to providing novel estimates at the preschool level, we can benchmark these estimates to primary school effects in our sample and to previous work. Second, we assess the extent to which the differential productivity of private institutions, combined with differential enrolment in these institutions by socio-economic status (SES), can explain SES gaps in cognitive achievement. This reflects longstanding concerns about unequal access to quality early childhood education and preschooling. Finally, following recent evidence of substantial heterogeneity within and across markets in primary schooling (Andrabi et al., 2025), we then allow the estimated private institution value-added to vary across villages and levels. This allows us to understand variation in the quality of public and private educational provision across villages.

We document three sets of results. The first of these focuses on the premium of the average private institution. Compared to public options, the private preschool premium in test score value-added is substantial, at 0.74 and 0.59 standard deviations (σ) of the test score distributions in math and Tamil language. This premium is roughly double the (cross-sectional) achievement difference in language between 3- and 4-year-olds enrolled in public preschools, and roughly four times the equivalent difference in math. These effects exceed those typically achieved by early childhood development interventions documented in the literature (e.g., Attanasio et al., 2014; Andrew et al., 2024; Attanasio et al., 2022). In contrast, we find no evidence of a positive effect for private primary schools: value-added in private schools is indistinguishable from that of government options in math and significantly lower in the local language (by $\sim 0.17\sigma$), results that are similar to those reported by Muralidharan & Sundararaman (2015); Singh (2015) in neighboring states.[1]

Second, we quantify the extent to which greater private sector enrolment for students from wealthier households explains the test score gap between them and students from poorer households. Students from households in the top socioeconomic quartile are 35 percentage points more likely to attend private preschools and 39 percentage points more likely to attend private primary schools than students in the bottom quartile. This differential private sector enrolment accounts for approximately 60% of the SES test score gap at preschool ages but, reflecting the absence of average private school effects in primary schools, does not explain SES gaps at later ages.

Third, focusing on *village-specific* estimates, the value-added in government preschools is dominated by that of private options in virtually *all* villages. In both subjects, there is very limited common support in the distributions of value-added between government and private preschools. This is consistent with prior evidence that government care centres typically provide little structured cognitive stimulation (Ganimian et al., 2024), whereas private preschools focus much more on early childhood education (Singh, 2014; Dean & Jayachandran, 2019). In contrast, there is near-complete overlap in value-added in math and substantial overlap in Tamil at the primary school level. Public and private sector value-added is positively correlated within villages: an increase of 1σ of student achievement in the value-added of public preschools in a village predicts 0.61–0.80σ higher private sector productivity; in primary schooling, this correlation is between 0.88–0.99. Value-added estimates are also positively correlated across levels in the same sector in a village — i.e., higher value-added in government (private) preschools also predicts higher value-added in government (private) primary schooling.

[1] The negative effect in the local language likely reflects a greater focus on English over the local language in private primary schools Muralidharan & Sundararaman (2015); Singh (2015).

Taken together, our analyses provide a unified treatment of the productivity of private and public options, and their link to socioeconomic inequality, over an extended span of education from preschool to the completion of primary schooling. These empirical results contribute to multiple distinct strands of economics research.

First, they contribute to an active literature that focuses on the development of cognitive skills in early childhood. Estimates suggest that over 250 million children under 5, mostly in LMICs, do not fulfill their cognitive potential (Engle et al., 2007; Grantham-McGregor et al., 2007; Behrman et al., 2013; Black et al., 2017). In response, much of the economics literature has, appropriately, focused on evaluating novel interventions to address these deficits.[2] In contrast, we aim to identify differences in productivity between the options *currently used* by millions of children. This provides useful complementary information about the institutions where novel interventions may be most needed (in our context, public preschools), as well as the potential for achieving cognitive gains through reallocation of children across providers in the same village (e.g., through vouchers).

Second, these results contribute to a substantial literature examining the private sector in education.[3] This literature has mostly focused on primary schools; we contribute by examining preschools, a shift in focus that is particularly important in the Indian context. Existing estimates, which we confirm in our data, suggest that average private school effects in foundational math and local language are small at the primary level (Muralidharan & Sundararaman, 2015; Singh, 2015). In contrast, we show that the private-public margin is more consequential at the preschool level. These results are directly informative for current policy initiatives aimed at ensuring universal foundational literacy and numeracy in childhood, both in India and globally (see, e.g., Muralidharan & Singh (2021); World Bank (2017)).

Relatedly, we contribute to the literature on socioeconomic inequality in early childhood cognitive skills. This primarily descriptive stream of research has documented the existence and evolution of disparities in the cognitive skills of young children from more- and less-advantaged backgrounds.[4] While differential access to effective preschools

[2]See, for example, evaluations of programs to support parents (e.g., Attanasio et al. (2014); Andrew et al. (2024); Attanasio et al. (2022)), to improve public preschools (e.g., Ganimian et al. (2024); Evans et al. (2024)), or to send children to private preschools (e.g., Dean & Jayachandran (2019); Bjorvatn et al. (2024)).

[3]See Crawfurd et al. (2024) for a recent review in LMICs. Influential studies from LMICs include multiple studies using the LEAPS dataset in Pakistan (Andrabi et al., 2011, 2024, 2025; Carneiro et al., 2024), as well as Muralidharan & Sundararaman (2015); Singh (2015) in the state of Andhra Pradesh, India, and evaluations of private-public partnership initiatives in Pakistan (Barrera-Osorio et al., 2022) and Liberia (Romero et al., 2020; Romero & Sandefur, 2021).

[4]See, e.g., Engle et al. (2011); L. C. Fernald et al. (2012); Rubio-Codina et al. (2015); Schady et al. (2015); Elango et al. (2015); Reynolds et al. (2017). The analogous literature in the US studies racial disparities (Fryer Jr & Levitt, 2004, 2006, 2013), as well as the income-achievement gap (Reardon, 2011, 2021; Nielsen,

is a plausible contributor to these disparities, drawing a conclusive link has been difficult. Our main contribution here is to show that differential access to private preschools, which offer higher value-added, is a substantial driver of the socioeconomic disparities in achievement observed before school entry age.

Finally, our results also contribute to the literature on education markets. Specifically, our results on the near-universal productivity advantage of private preschools over public alternatives across villages (unlike in primary schools), as well as market-level correlations in the productivity of providers across sectors and educational stages, are both novel in the context of LMIC education systems. Although data limitations constrain us from attempting a comprehensive analysis of preschool markets — we do not observe provider-specific quality, prices, attributes and actions in these villages — these results suggest that understanding both the demand and supply-side of educational markets is as important for preschool services as it has been for our understanding of primary schooling in LMICs.[5]

2 Context and data

2.1 Context

Our study is based in rural areas of Tamil Nadu, a large south Indian state with an estimated population of 74 million and an education system that serves 13 million children annually (Government of India, 2019). While Tamil Nadu's public early childhood education system is considered high-performing within India, substantial quality gaps remain: fewer than 10% of children can read individual words when they enter primary school, and 60% cannot recognize individual letters (Pratham, 2022).

Primary education in India is mandatory, and enrolment is nearly universal (Pratham, 2022). The official school starting age is 6, though many children (including in Tamil Nadu) begin Grade 1 at age 5. Government primary schools are free and provide in-kind benefits, including midday meals, textbooks, and uniforms. Instruction is usually in the state's official language (Tamil in Tamil Nadu). In contrast, private primary schools charge fees and often use English as the medium of instruction, and there is substantial heterogeneity in quality and costs (Singh, 2014; Kingdon, 2020). In 2022, private providers accounted for roughly one-quarter of primary school enrolment in rural areas nationally; this figure was similar in rural Tamil Nadu, where they accounted for ∼24% of enrolment (Pratham, 2022).

2023). Our findings are consistent with evidence from high-income settings showing that SES gaps emerge before formal schooling (e.g., Hart & Risley (1995); Lee & Burkam (2002); Noble et al. (2005); A. Fernald et al. (2013)) although a large share in our setting is accounted for by the type of preschool attended.

[5]For influential examples of such work in LMICs in primary schooling, see, e.g., Neilson (2021); Allende (2019); Andrabi et al. (2017); Bau (2022).

Preschool education (ages 3–5) is not compulsory. The main public option is the network of *anganwadi* centres under the Integrated Child Development Services (ICDS) program. ICDS anganwadis constitute the world's largest early childhood program, offering free pre-primary education alongside nutrition and health services to roughly 36 million children aged 3–6 at 1.35 million centres nationwide (Ganimian et al., 2024).[6] enrolment is non-selective and free of charge. Anganwadis are typically staffed by a single Anganwadi worker (and a helper) per center, who is responsible for delivering a mix of preschool instruction, child nutrition, and health monitoring services. Anganwadi centres in Tamil Nadu typically operate for about 4 hours each morning, of which two hours are nominally allotted to pre-school education. In practice, however, only 38 minutes are devoted to instruction per day due to staff constraints (Ganimian et al., 2024).

Private preschools, on the other hand, charge fees and focus on pre-primary instruction in nursery and kindergarten classes, often as part of or attached to private primary schools. These private pre-schools tend to emphasize early academic skills, have more structured instructional time, and frequently introduce English (Dean & Jayachandran, 2019). Overall, in India, preschool enrolment has been rising (and reached 80% for four-year-olds in 2022 (Pratham, 2022)) but varies widely by state, with Tamil Nadu having nearly every 4-year-old enrolled (over 99%) in some educational facility.[7] Private providers account for roughly 20% of preschool enrolment nationally (and 36% in Tamil Nadu).

The annual per-child expenditure differs both across sectors and levels. In 2021, private schools in our study districts charged an average annual fee of INR 7,100 (∼96 USD) at the preschool level and INR 8,460 (∼ 114 USD) at the primary school level.[8] Per-pupil expenditure in public elementary schools is much higher (∼INR 29,000 (∼391.5 USD) in 2019-20, Bordoloi et al. (2020)). The cost in public preschools per child per year was ∼76 USD (∼5630 INR) across all of India in 2020 (UNESCO, 2020a,b,e,f,c,d). These differences in per-child public expenditure across primary and preschool reflect substantially lower salaries paid to anganwadi helpers compared to public school teachers and constrained resources in the public preschool sector.

[6] Throughout this paper, we will refer to all forms of structured pre-primary enrolment as preschooling. We are only investigating differences across sectors (public/private), rather than across individual providers or facilities. Hence, we are unable to account for the potential differences *within* sectors, such as between *anganwadi* centres and formal, public preschools (which account for 2% of enrolment of 4-year-olds (Pratham, 2022)). To avoid introducing additional terminology, we therefore refer to these simply as "preschools".

[7] Near universal enrolment is not unique to Tamil Nadu: rates of non-enrolment among 4-year-olds are under 2% in several other states, including Andhra Pradesh, Telangana, and Karnataka in South India, and Maharashtra, Jharkhand, and Odisha in other parts of the country (Pratham, 2022).

[8] These estimates average fees across private preschools in the four study districts using from data from the Tamil Nadu Private Schools Fee Determination Committee (https://tnfeecommittee.com/). We use 0.0135 USD/INR as the exchange rate, which was the average rate in 2021.

2.2 Data

2.2.1 Sample

Our data covers 215 villages in four districts of Tamil Nadu (see map in Figure A.1 in the Appendix). These districts were selected using probability proportional to size sampling to ensure representativeness of rural Tamil Nadu. In these communities, we administered comparable achievement tests to students aged 3–10 in early 2022 (baseline) and 2023 (endline). Our core analysis sample includes the set of children aged 4–10 in 2023 for which we have access to baseline assessments (N=19,021).[9] Although these villages were not randomly selected (the sample only includes blocks with two or more government preschool centres (*anganwadis*) co-located with middle schools), our sample is similar in observable characteristics to the state's rural population, though slightly less wealthy in terms of asset ownership (see Table A.1).[10]

Our sampling strategy involved enumerating every household located within approximately 2 kilometres of the village *anganwadi*, designated as the reference point for data collection. Villages typically consist of multiple distinct clusters of households and, in practice, either an entire cluster was included in the sample or not. Figure A.2 illustrates the sample in four villages to provide a sense of the coverage of this sampling strategy in practice. In most villages, we enumerated all households. As a result, while we claim that our villages provide examples of disjoint markets, with complete enumeration in sampled geographical areas, we do not claim to have a full enumeration of all students and providers in these markets.[11]

2.2.2 Assessments

The learning assessments were designed to capture student achievement across the preschool and primary populations. Children were tested in math and Tamil (the local

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[9] Attrition between survey rounds was ∼25%, but does not vary by socioeconomic status (SES) or test scores (see Appendix B). The survey waves were administered slightly more than a year apart. Since we only have data on age in completed years (and not months), we measure attrition for children aged 3–9 in 2021/2022. All our results are robust under specifications using inverse probability weighting (see Appendix B.3).

[10] This data was originally collected for an experimental evaluation of a government program to improve preschool education. The intervention and the evaluation were canceled due to the COVID-19 pandemic and subsequent preschool and school closures. See `https://doi.org/10.1257/rct.5599` for more details. We continued to collect data to study the learning loss during the pandemic and the pace of recovery afterward (Singh et al., 2024).

[11] In very dispersed villages, our sampling strategy omits clusters that are distant from the reference school. More generally, in this setting, an education market can span several villages. In data from 2019, ∼20-25% of children, even at preschool level, go to school using school vans and, in 2023, nearly a quarter report attending school further than 2 kilometres away. Our estimates are, therefore, best interpreted as the productivity of educational institutions accessed by children in our sample geographies, rather than only those providers located within the boundaries of our enumeration area.

language) using age-appropriate booklets and overlapping items. For preschoolers (ages 3–4), the tests captured oral comprehension, letter recognition, quantitative comparisons, number recognition, and counting; at ages 5 and 6, they also included word recognition, more complex counting, and basic addition; for children aged 6–10, the tests additionally included more complex arithmetic computation and word problems in math and passage comprehension and reading exercises in Tamil.

Our focus on foundational math and local language skills closely reflects the targets of the National Initiative for Proficiency in Reading with Understanding and Numeracy (NIPUN), the national educational policy on early childhood education (Ministry of Education, 2020). Figure A.3 provides an overview of skills that are targeted by the policy at different ages from preschool through to early primary grades. Our assessments, thus, map directly to stated policy goals while also including broader competences that were either included in Ganimian et al. (2024) or are standard in evaluations of early primary education in this context.

Test booklets included common items across waves and ages, which allows us to link achievement on a common metric using Item Response Theory (IRT) models (Das & Zajonc, 2010). We estimate these scores by pooling all test observations across rounds, separately for math and Tamil. We standardize test scores to have mean zero and standard deviation of one in the sample of children aged 5 in the 2022 survey wave. The test scores display, as expected, a shifting in the distribution of achievement with age (indicating skill acquisition over time; see Figure G.7) and no evidence of Differential Item Functioning by age or by round (see Figures G.8-G.29). See Appendix G for more details on the test and the validation exercises we conduct.[12]

2.2.3 Household survey

We collected extensive household data about their socioeconomic status and children's education in both survey waves. We use detailed information on household ownership of various assets in 2022 to construct a socioeconomic status index using principal component analysis (PCA). We use a household's percentile rank in this index as the primary measure of socioeconomic status (SES) (see Appendix C for details).

[12] In 2022, we divided the sample into two randomly-assigned groups within villages that were administered the tests in a staggered manner between December 2021 and April 2022 (Singh et al., 2024). We administered the tests with a similar staggering and the same assigned groups in 2023 to maintain a similar gap between assessments. In 2023, we observed signs of ceiling effects for school-age children in the first round of testing. We remedied this by adjusting the test booklets for the second (randomly assigned) testing round, retaining common items for linking (see Figures F.1-F.4). No results are sensitive to only using the second round (see Table F.1).

We also use maternal and paternal education information as additional measures of SES and control for them in our regressions.

2.2.4 Strengths and limitations of the data

Our dataset has several important strengths. The most important of these is the availability of panel data on achievement for children over the full span of preschool and primary school ages across a large number of spatially disjoint education markets. A related strength is the comparability of measurement over time and across ages: datasets with vertically-linked IRT scores are uncommon in low- and middle-income countries but are crucial to our goals of expressing preschool and primary school productivity on a common scale. The final strength is the complete enumeration of households in sampled geographical areas: this prevents the attrition typical in many school-based surveys due to student absence on the day of testing.[13]

However, the expansiveness of the dataset also imposes some trade-offs that limit our analysis. Most importantly, we can only provide sector-specific estimates for a village as a whole, rather than estimates for each facility separately. This issue arises for a combination of reasons. First, matching children to centres generates substantial measurement error since we only collected facility names (which are hard to map to individual facilities, especially government facilities that do not have distinctive names). Second, because the total number of children in each facility is often very small, any individual facility estimates would be very noisy even with complete matching. Finally, in large villages, we restricted our censuses to a radius of $\sim$2 kilometres from a reference point (the anganwadi); this does not affect our interpretation of each village as a distinct market, but does affect our ability to interpret our survey as a complete enumeration of the full market.

Second, since we did not collect detailed facility surveys, we only have information about whether a child attends a private or public preschool but no other characteristics, such as staffing, fees, or instructional practices. This prevents us from decomposing the sources of any private/public advantage (as in, e.g., Angrist et al. (2013) for Charter schools) — note, however, that even with extensive data on school inputs and characteristics, Andrabi et al. (2025) can only explain 5% of the heterogeneity in value-added across private schools in Pakistan. Finally, since our data collection focused on foundational math and local language skills, the explicit targets of government policy, we did not administer tests for English language skills (an important differentiator for private schools).

[13] For instance, ASER reports indicate that student absence ranges from 10% to 45% in different states of India (Pratham, 2022). This absence-induced attrition is non-random: it is typically higher in the public sector and for children with lower test scores and from poorer households.

3 Pre- and primary school choices and value-added

The first part of our analysis focuses on selection into and average productivity differences across different schooling options.

3.1 Selection and educational trajectories

We investigate enrolment patterns and child characteristics by age in Table 1. Virtually all children ($\sim$ 96%) are enrolled in private preschools or public care centres (*anganwadis*) at age 4. At age 5, roughly half of all children begin enrolling in primary school.[14] Between the ages of 6 and 10, primary school enrolment becomes nearly universal.

Private operators serve a significant portion of the market in both pre- and primary schools. At age 4, a third of all children are enrolled in a private preschool. The market share of private providers reduces to one-quarter in primary school.

There is a significant SES gap in private enrolment. Private school children are about 25% more likely to have mothers with completed secondary education. The average child in private preschool ranks 18 percentiles higher in the socio-economic distribution compared to children in public preschools. In primary school, this gap increases to around 21 percentiles. Additionally, there is a clear gender gap in private enrolment at the primary school level, amounting to 7 percentage points in favor of boys.

We also document a substantial gap in test scores, as measured in our baseline assessments, between children from private and public schools. For students aged 4 in 2023, this gap amounts to 0.17 standard deviations (σ) in the previous year's test score distribution in both math and Tamil. At age 5, the gap increases to around 0.45σ in both subjects. During the main primary school ages (6–10), the gap in Tamil reduces to 0.12σ, but remains large in math (0.3σ).

3.2 Estimating value-added by sector

We rely on conventional value-added models to measure test score improvements from attending a private pre- and primary school. Specifically, test scores are regressed on school characteristics (e.g., private/public indicators) while conditioning on lagged scores and student socio-demographic characteristics to account for student selection (see, e.g., Todd & Wolpin (2003, 2007)).

[14]This pattern, of a substantial fraction of students already choosing to enroll in Grade 1 at age 5 (although the official age to start formal schooling is supposed to be 6 years), is common in most Indian states (Pratham, 2022). The ubiquity of this empirical pattern motivates our attempt to understand the relative productivity of the four types of institutions — public and private preschools and primary schools — on a comparable scale, since each of these is a viable choice for students between 4–6 years old, and especially so for 5-year-olds.

For each subject (math and Tamil), we estimate the following equation:

$$y_{iv}^{2023} = \lambda y_{iv}^{2022} + \beta Private_i + \mathbf{\Gamma X}_{iv} + \epsilon_{iv}, \tag{1}$$

where i denotes a child and v a village. The variable y_{iv}^t denotes student i's test score in a particular subject in year t, λ is the coefficient on lagged test scores (i.e., a persistence parameter), and $Private_i$ is an indicator for whether student i attended a private preschool or primary school between the assessment waves. $\mathbf{X}_{iv}$ is a vector of additional controls, including village fixed effects, deciles of the SES wealth index, paternal and maternal education levels, and the child's gender. The coefficient of interest is β, which captures the effect of attending a private preschool/school on test scores. This benchmark specification is similar to the dynamic OLS specifications used by Andrabi et al. (2011) and Singh (2015), the two most closely related papers on private formal schooling in South Asia.

We estimate this equation separately for children aged 4, 5, and 6–10 years. This choice reflects multiple distinct concerns. Most importantly, given the enrolment patterns in Table 1, estimates of β at ages 4 and 6–10 can be interpreted as the private school premia at the preschool and primary school levels; at age 5, in contrast, β represents a weighted average of the private premia at the two levels.[15] Second, this allows the coefficient on lagged achievement to differ across the preschool and schooling stages; this is potentially important because both true persistence in achievement and measurement error in test scores may plausibly differ for children at very young ages. In Section 3.4, we further relax the assumption of a common persistence parameter (λ), allowing λ to differ for every age, and find similar results. Finally, by allowing the coefficients on all variables in $\mathbf{X}_{iv}$ to differ across ages, this also provides flexibility in case the nature of selection across sectors, influenced by factors such as parental education or wealth, differs across levels of education.

The causal interpretation of β relies on a conditional exogeneity assumption: our estimates will be unbiased only if our controls are rich enough to account for the selection of children into private vs. public operators. In other settings, both in the United States and in LMICs, similarly-estimated value-added measures appear to agree closely compared to estimates using identification from design-based experimental or quasi-experimental variation (Angrist et al., 2017, 2023; Andrabi et al., 2011, 2025; Singh, 2015, 2020).[16] Thus,

[15] A few children (N=54) at age 4 are recorded as attending primary school (Table 1). This likely reflects response errors, and we do not exclude these children in our analyses. Excluding them has very little impact on any of the main results. The same holds for children aged 6 and above recorded as attending preschool (N=82).

[16] Since our identifying assumption does not require assignment to *individual school units* to be random, it is weaker than the assumption underpinning school value-added models validated across multiple contexts

although the nature of selection may differ across settings, the value-added estimates in our data likely reflect true productivity differences rather than selection effects. Further, we will examine the potential for bias through two (related) strategies in Section 3.4. First, analogously to the investigation of bias in teacher value-added estimates in Chetty et al. (2014), we show that our estimates are invariant to the inclusion of multiple additional controls. Second, we report sensitivity analyses following Cinelli & Hazlett (2019) to examine the strength of the unobserved confounding needed to overturn our results.

3.3 The private premium in preschool and primary school

Table 2 reports the estimated private premium (β) from Equation (1), separately for children aged 4 (preschool), 5 (transition), and 6–10 (primary school). Columns 1, 3, and 5 report differences in test scores by private school attendance conditional only on village fixed effects; Columns 2, 4, and 6 further condition on lagged scores and covariates as specified in Equation (1).

Raw differences in academic achievement by private school attendance are substantial at the preschool level, amounting to 0.81 standard deviations of the baseline test score distribution (σ) in math and 0.64σ in Tamil (Column 1). Most of these gaps reflect stark productivity differences between sectors: conditional on lagged test scores and socioeconomic characteristics, the average private premium in the preschool market is 0.74σ in math and 0.58σ in Tamil (Column 2).[17] This is equivalent to almost *twice* the raw difference in Tamil achievement between children aged 3 and 4 in public preschools in our endline assessments, and *four times* the difference in math.[18]

These patterns differ substantially at the primary school level (Columns 5 and 6). The private premium in math is virtually zero (Column 6). In Tamil, the "premium" at the primary level is negative, which likely reflects a greater focus on English teaching in private schools. Patterns in both subjects are similar to previous estimates of private primary

(Angrist et al. 2021; Andrabi et al. 2025). Imagine a village with two public schools A and B and two private schools X and Y. Typical school value-added models require enrolment into *each* of these options to be conditionally ignorable; our specifications only require sector-level enrolment to be conditionally ignorable (i.e., enrolment in either of school A or B versus school X or Y, but not the choice of school within sector).

[17] Since children aged 5 are a mix of pre- and primary school students, their private premium is positive but muted compared to their younger peers. If we focus on children aged 5 — of which around half will have started primary school — and allow private school effects to differ at the pre- and primary level, we obtain very similar estimates as for children aged 4 and 6–10 (see Table A.2).

[18] In Tables A.3 and A.4, we divide items by competencies being assessed. In math, private pre-schools increase the proportion correct on test items by 19–33 percentage points, relative to public school averages of 19–58%. In Tamil, this figure is 10–22 percentage points, relative to public school averages of 36–50%. These effects are largest, both in absolute and relative terms, in competencies where public preschool children are particularly weak.

school effects in India (Muralidharan & Sundararaman, 2015; Singh, 2015).[19]

We examine these value-added estimates semi-parametrically in Figure 1, following Cattaneo et al. (2024). Specifically, we adjust for the full set of covariates in Equation (1) and allow the relationship between lagged achievement and subsequent test scores to vary non-linearly.[20] We plot these semi-parametric estimates separately for students in private and public sectors in each of the three age categories in Table 2. In each sub-figure, the distance between the estimated fits for the private and government sectors provides an analogue to the estimated private school premium for students in that segment of the achievement distribution. In both subjects, the private school premia at age 4 is present across most of the achievement distribution and not substantively different in magnitude for students with differing achievement levels. In contrast, and reflecting the results in Table 2, for ages 6–10, there is no premium across the baseline achievement distribution in math, and a negative one in Tamil. Figure 1 shows that the estimated premia in Table 2 are not sensitive to controlling for lag scores more flexibly or a potential lack of common support in achievement between the public and private sectors.[21]

Our core analyses identify the difference in productivity between public and private options; determining their absolute levels would require comparing them to no enrolment. The latter margin is less relevant in Tamil Nadu, as preschool enrolment is nearly universal, but remains relevant in many Indian states, such as Uttar Pradesh, where only 33% of children enroll in preschool (Pratham, 2022). In our sample, only ∼4% of 4-year-olds (N=67) are not enrolled in any level. Estimating the value-added of both public

[19]We also estimated the private premium separately for each age within primary grades (see Figure A.4). The premium is zero across all ages for math, except for 9-year-olds, for whom it is positive and statistically significant. For Tamil, the point estimate for the premium is negative for all ages, but closer to zero and statistically insignificant for 9- and 10-year-olds. The similarity between the results for 6-year-olds and the overall primary sample (ages 6–10) is reassuring. Older cohorts faced substantial educational disruptions and learning losses due to COVID-19 (Singh et al., 2024), which could potentially affect relative sectoral productivity if private schools coped with these disruptions more (or less) effectively than public schools. However, disruptions were less severe for 6-year-olds; yet, their estimated private premium closely matches that of the full primary sample, suggesting limited bias from pandemic-related disruptions.

[20]We use within-age percentiles of lagged achievement in 2022 to ensure an even distribution of the sample.

[21]Even with significant mean differences, there is full common support between the distribution of prior achievement in the two sectors (Figure D.1). This is important because, if segments of the achievement distribution were unique to each group, then controlling for baseline achievement would have involved extrapolation rather than comparison of observationally similar students. To address further concerns around the common support of covariates other than baseline test scores that predict private enrolment (e.g., socioeconomic status), we also estimate the private premium restricted to a sample of observationally similar children. In particular, we first estimate the probability of attending a private institution using a probit regression, separately by age groups, on the full set of value-added controls used in the main analysis. We then estimate the private premium when restricting the sample only to children within 5, 10, and 15 percentage points of the age-specific median probability of private enrolment (Tables D.1-D.3). These estimates yield results that are similar to those of the main analysis, and we cannot reject the equality at conventional levels of statistical significance.

and private preschool facilities at age 4, relative to the baseline of no enrolment, suggests attending public care centres leads to learning gains of 0.24σ and 0.3σ in math and Tamil, respectively — roughly a quarter to a third of private preschool value-added (Table A.5).

Reflecting domestic and international policy targets, our data collection and analyses focus only on foundational numeracy and literacy in the local language. A resulting limitation is the absence of measures for English language proficiency, where prior research indicates that private primary schools significantly outperform public schools (Muralidharan & Sundararaman, 2015; Singh, 2015). Consequently, we likely underestimate the full private-sector premium in preschools and schools. Similarly, public or private preschools may place greater emphasis on socio-emotional skills that are unmeasured in our data; our analysis is unable to estimate their differential productivity in these domains, which remain topics of independent interest.

3.4 Robustness of the private premium

The principal threat to our results is that our parsimonious specification of the value-added model (Equation 1), with a limited set of covariates and a linear control for the subject-specific lagged score, does not fully account for selection into the two sectors. We examine the robustness of our estimates to these concerns in several ways.

First, we report results from richer specifications that include a battery of additional covariates, control for lagged achievement non-linearly, and examine the stability of our estimated coefficient of the private school premium; this procedure is similar to validation exercises reported in Chetty et al. (2014). The additional covariates are: quadratic polynomials in lagged scores in *both* math and Tamil; fixed effects at the level of survey month in 2022, 2023 and their interactions; controls for caste groups; and educational inputs measured at baseline from our survey, including whether the child had recently received educational content via internet, TV or books. At the preschool level, the private premium is similar across specifications (0.72–0.74σ in math and 0.56–0.58σ in Tamil; see Panel A of Table 3). Likewise, at the primary level, coefficients are stable across specifications, remaining close to zero in math and consistently negative in Tamil (between -0.17 and -0.19σ; see Panel B of Table 3).

To explicitly quantify the sensitivity of our estimates to potential omitted variable bias, we further report robustness values (RVs) and bounds following Cinelli & Hazlett (2019) (see Table 3).[22] This procedure considers a potential omitted variable Z that predicts both private enrolment and test scores. Robustness values RV and $RV_{\alpha=0.05}$ measure how much

[22]This procedure serves a similar purpose as Oster (2019) bounds, but offers a more intuitive approach to assessing the role of unobserved confounders.

of the residual variation in both test scores (Y) and private enrolment (D), after controlling for all other included covariates, needs to be explained by Z to i) reduce the private premium to zero (RV) or ii) make it statistically insignificant ($RV_{\alpha=0.05}$). These values range from 20% to 24% for Tamil and from 28% to 31% in math at the preschool level. To evaluate whether such a confounder Z is plausible, we consider the extreme scenario where Z is as powerful a predictor of Y and D, respectively, as our SES index, baseline test scores, parental education and child gender taken together. Even a confounder Z as strong as these covariates combined would explain at most ∼7% of remaining test-score variation and ∼16% of private enrolment variation, well below the threshold required to eliminate or substantially weaken our results. Hence, we view such an omitted variable as highly unlikely. For further details on the implementation of this exercise, see Appendix D.

As a final robustness check, we iteratively allow the coefficients on each of the covariates included in the main specification to vary one at a time by village. This addresses concerns about selection patterns being very different across villages. The estimated private premium changes very little when allowing for such interactions, both at the pre- and primary school level (see Figure D.2).

3.5 Socioeconomic learning gaps

Concerns that private education might exacerbate inequality are central to public debates surrounding education policy (UNESCO, 2021). These concerns may be empirically grounded, as private sector institutions disproportionately enroll students from higher-SES families (Table 1) and, at the preschool level, also have significantly higher productivity (Table 2). Therefore, we directly examine the contribution of differential productivity across sectors to SES gaps in achievement at early ages.

We focus on the gap in achievement between students from households in the top and bottom quartiles of socioeconomic status (omitting the middle half of the SES distribution). Specifically, we regress student test scores in 2023 on a dummy indicating whether the child belongs to the top SES quartile, and then sequentially condition on lagged achievement and a dummy variable for attending a private institution. As in our previous analyses, we estimate this separately for 4, 5, and 6 to 10-year-olds (see Table 4).

Raw test score gaps between high- and low-SES students, at age 4, are large (0.44σ and 0.36σ in math and Tamil, respectively). Differences in baseline achievement explain only around 5–10% of this gap (Columns 1 and 2), but approximately 60% of it can be attributed to private preschool attendance (Columns 2 and 3). These patterns differ for primary school children. At ages 6–10, differences in baseline achievement account for around 30% of the test score gap in both subjects (Columns 7 and 8); private enrolment explains little of the

SES gap in math and widens it in Tamil (Columns 8 and 9), which aligns with our previous findings on the primary private premium. Conditional on private preschool enrolment and lagged test scores, the remaining SES gap in test scores is relatively stable across age groups.

Overall, high-SES children enter primary school with substantially stronger academic achievement compared to their low-SES peers, a disparity that persists at later stages. These early gaps are primarily driven by differences in private preschool enrolment.

4 Spatial variation in the private premium

The previous section estimated the *average* productivity differential across sectors. Yet, these averages likely conceal substantial heterogeneity across villages. Further, within educational markets, the productivity of private and government preschools/schools is likely to interact; reducing productivity differentials to a sample-wide average restricts us from investigating such relationships. In this section, we investigate these market-level associations.

4.1 Estimating village-level value-added

Our empirical approach to estimating village- and sector-specific school productivity extends the value-added framework described in Section 3.2. We estimate test score gains for each level-sector-village cell, where level refers to pre-/primary schooling and sector to public/private operators. We define θ_{slv} as a set of dummy variables that indicate attendance at a private or public option s at the pre- or primary level l in village v. To improve precision, we pool all children aged 4–10 (adding subscript a for age) in the same regression and estimate the following equation:

$$y^{2023}_{iaslv} = \lambda_a y^{2022}_{iaslv} + \theta_{slv} + \mathbf{\Gamma X}_{iaslv} + \epsilon_{iaslv}. \tag{2}$$

The coefficient on the lagged score λ_a is allowed to differ by child age a. The vector $\mathbf{X}_{iaslv}$ contains controls for deciles of the SES wealth index, as well as paternal and maternal education, the child's age, and gender. The public preschool sector of one of the villages is left out as the omitted category.

In this model, θ_{slv} captures the improvement in test scores between 2022 and 2023 for a child in a particular sector, level of schooling, and village after controlling for baseline test scores and background characteristics. The difference in θ_{slv} across sectors allows us to identify the private premium at the level of schooling l and village v:

$$\beta_{lv} = \theta_{1lv} - \theta_{0lv} \tag{3}$$

To interpret β_{lv} causally, we require selection into the private relative to the public sector in *each* village to be accounted for by our set of included controls. This is stronger than the assumption in Section 3.2 only in that this conditional ignorability assumption is imposed for each village individually (rather than the sample as a whole); it is weaker than the standard assumption underlying school value-added models wherein the choice of *each* school in the sample is assumed to be conditionally ignorable.

Our principal aim in this section is to compare the productivity of available options in the same market and estimate the distribution of the private premium across markets and levels. This goal is similar to Andrabi et al. (2025), who establish the validity of school-level value-added using alternative sources of (within-village) identification. Like other applications of value-added models across many dispersed markets (see, e.g., Andrabi et al. (2025); Einav et al. (2025), we will focus on reallocations only within the same market.[23] When we correlate estimated productivity across sectors in the same villages, we shall only interpret these as associations: positive correlations may be informative about spatial inequality but could arise from institutional effects (e.g., through school competition) or through the effect of village-level unobservables not proxied for by our covariates.

The value-added estimates in θ_{slv} will be measured with uncertainty, introducing measurement error in both the individual parameters and, potentially, the private premium β_{lv}. This presents a challenge for several parts of our analysis. First, individual value-added estimates may be severely biased even under the assumption that our approach yields unbiased estimates *on average*. Second, the estimated slope in a regression that has value-added estimates in the right-hand side (e.g., private on public value-added, a core object of interest in our analysis) will be subject to attenuation bias due to measurement error.

We address this in two ways. We rely on Empirical Bayes estimates of value-added when investigating individual productivity parameters. Our Empirical Bayes procedure shrinks value-added estimates toward their level and sector averages proportionally to the uncertainty with which they are estimated (for details and the impact of shrinkage, see Appendix E.1). This shrinkage procedure is suitable when the goal is to improve unit-specific forecasts. However, it does not allow us to appropriately investigate moments of the value-added distribution — such as the covariance between private and public

[23]Comparing value-added estimates *across* villages is, however, much more complicated due to the possibility of spatial unobservables (e.g., neighbourhood effects, as in Chetty & Hendren (2018)). The relevant thought experiment here would be to move a child from, say, a private school in village A to a public school in village B; this would only be valid if village A or B themselves did not have an independent effect on student outcomes, beyond that proxied by covariates. We are not aware of results in any setting that validate value-added models against experimental or regression discontinuity estimates for this purpose.

value-added (Walters, 2024).[24] Denoting the within-village covariance matrix of θ_{sljv} across sectors s, levels l and subjects j as $\mathbf{\Sigma}_\theta$, we estimate $\mathbf{\Sigma}_\theta$ directly following Angrist et al. (2025), which corrects for bias due to estimation noise (for details, see Appendix E.2). In the results, we visually illustrate the relationships between our raw value-added estimates, and report both the unadjusted and bias-corrected slopes throughout.

Finally, by pooling children aged 4–10 in the same specification, we also assume that (age-varying) lagged scores and background characteristics can address selection into early primary schooling (i.e., the *timing* of the transition, not just the sector). This is consequential for 5-year-olds, around half of whom are enrolled in primary school (Table 1). The alternative strategy is to omit 5-year-olds (Figure E.3), to which our results are robust.

4.2 Value-added across markets, sectors and levels

We now present the results of estimating the specification from Equation (2), which yields four measures of test score gains for each village: the average value-added in private and public options at the preschool and primary levels.[25] The variation in test-score gains is slightly larger in the private sector compared to the public one, both at the pre- and primary school levels. We focus, first, on the difference in value-added between the public and private sectors (e.g., the private premium) across different markets, and then turn to within-market correlations of these estimates.

Figure 2 orders villages by their average value-added in the public sector, as estimated by the Empirical Bayes method, along the horizontal axis. Red (blue) dots denote the average value-added in the public (private) sector for each village. The top panels show preschool results, separately by subject, and the bottom panels show primary school results.

No village has a public preschool sector that, on average, performs better than its private sector in math — with few exceptions, the same is true for Tamil.[26] The average private premium is 0.72σ in math and 0.57σ in Tamil, which is very similar to the results in Section 3.3. While productivity differences vary across markets, they are substantial almost everywhere. This pattern is very different at the primary school level. While the average

[24] While the variance of the raw value-added estimates will be inflated due to estimation error, the shrunken Empirical Bayes estimates will, in general, understate the variance of the true value-added parameters (Walters, 2024).

[25] Figure A.5 presents density plots of these estimates, together with bias-corrected variances of the value-added distributions. For 15 villages, we are unable to estimate village-level value-added in all combinations of subjects, sectors and levels. In most cases, this is because too few children are enrolled in private preschools. This leaves us with 200 villages for which we have complete value-added estimates.

[26] This is not primarily a consequence of Empirical Bayes shrinkage. Focusing on raw (unshrunk) value-added estimates, only 7% (14%) of villages have a public preschool sector that outperforms its private sector on value-added in math (Tamil), despite raw value-added being estimated with a significant degree of noise (Figure E.4).

private productivity premium is essentially zero in math and negative in Tamil, as noted previously, the magnitude of these differences remains modest across all villages.

Next, we investigate the association between value-added estimates across sectors and levels within villages. We show binned scatter plots of raw value-added estimates in Figure 3. As mentioned above, these relationships are likely subject to attenuation bias due to estimation noise in the value-added estimates. Hence, we report both raw and bias-corrected slopes throughout (see Appendix E.2 for details on the bias correction approach).

We identify a positive correlation between the value-added of the private and public sectors. Regressing raw private on public preschool value-added yields a coefficient of 0.44 in math and 0.34 in Tamil (the top panel of Figure 3). These slope coefficients are biased towards zero: correcting for estimation error increases these slopes to 0.79 in math and 0.58 in Tamil. These sectoral correlations are higher in primary school, with slopes of 0.89 in math and 0.99 in Tamil (after correcting for bias). However, the difference in slopes at the pre- and primary levels are only statistically significant in Tamil ($p = 0.48$ in math, $p = 0.02$ in Tamil). These associations in productivity across sectors are consistent with potential "multiplier effects" in which, due to market-level incentives and competition, an increase in public sector quality also leads to improvements in the private sector — Andrabi et al. (2024) present experimental evidence of such a mechanism at work in primary schooling markets in Pakistan, although we are not aware of similar evidence at the preschool level. In short, villages with particularly low-quality public schools also tend to have weaker-performing *private* schools than other villages, and this pattern is stronger in primary relative to preschool markets, at least in Tamil.

We also investigate the within-sector correlation of value-added between pre- and primary schools within the same markets (bottom panel of Figure 3). In both the public and private sectors, productivity is clearly correlated across levels of schooling. On average, an increase of one standard deviation of *preschool* value-added predicts an increase of roughly half a standard deviation of *primary school* value-added. This correlation is not surprising in the private sector since private preschools are often vertically integrated with private schools. Government preschools, on the other hand, are managed by a parallel administrative set-up separate from the School Education Department (which oversees primary schools), but appear to display similar correlations.[27] As such, productivity differences across markets appear to be relatively persistent throughout early childhood and adolescence.

[27]Public preschools are run by the Ministry of Women and Child Welfare at the national level, not the Ministry of Education (which runs primary schools). Staffing, pay, management, and overall capacity all differ between these two structures.

Turning to market shares at the preschool level, we find that private preschool enrolment does *not* increase with the size of the private premium (see Figure A.6). At the primary school level, in contrast, we *do* find that market shares reflect differences in the private premium. In math, a one standard deviation increase in the village-level private premium is associated with a 15 percentage point increase in private enrolment. In Tamil, the correlation is essentially flat, which is not unsurprising given that many households opt for private primary schools precisely because of their focus on English rather than the local language.[28]

Importantly, market shares are equilibrium outcomes. Thus, the lack of association between private preschool premia and market shares is consistent with distinct explanations. For example, it is possible that (i) households do not value cognitive skill production for very young children and/or, (ii), higher-quality preschools also charge higher prices (which we do not measure), or (iii) higher-quality preschools want to keep enrolment low to maintain quality. Since market shares appear to respond at the primary level to value-added in math (which is emphasized in both public and government schools), explanations for this pattern are likely to be specific to preschools.

We provide further analyses of the correlates of village-level value-added in the Appendix, which reveal three additional findings. First, value-added is highly correlated across subjects (Figure A.7): within a village, private and public preschools that provide high value-added in math also tend to do so in Tamil. Second, private premia across markets are largely uncorrelated with market size, measured as the number of children aged 4–6 in each village, and village-level SES at any level of schooling (Table A.6). If anything, villages with weaker socio-economic composition tend to have larger private premia compared to those with stronger composition.

5 Conclusion

This paper provides new insights to the study of public-private differences in education systems in LMICs. In nearly all villages in our sample, private preschools exhibit a substantial advantage in test score value-added over public options, which accounts for nearly two-thirds of socioeconomic inequality at the school entry age. This is in stark contrast to the (small) differences in productivity between public and private schools at the primary school level. Within villages, we document a

[28] As shown in Chen (2024) and discussed in Walters (2024), using shrunken Empirical Bayes estimates when correlating value-added with market shares may be inappropriate if the precision and magnitude of the value-added estimates are correlated. We address this issue by adopting an alternative Empirical Bayes procedure that is robust to such precision dependence (Chen, 2024) and find similar results (see Appendix E.5).

positive correlation between the value-added of educational options, suggesting spatial inequality in access to educational opportunity (akin to neighbourhood effects in other settings, see e.g., Chetty & Hendren (2018)).

Why does the public sector underperform private alternatives so starkly at the preschool level, even while producing similar value-added at the primary school level? Our household-based dataset lacks the information on time use, pedagogical practices, or other facility-based inputs needed for a structured analysis of this question. A likely explanation is that private preschools dedicate more instructional time to cognitive stimulation. In their control group, in the same districts, Ganimian et al. (2024) document only 38 minutes of preschool instruction per day in anganwadis. Doubling this, using a part-time worker, led to gains of 0.28σ for children who attended the treatment centres. It is likely that, at the preschool level, private institutions effectively provide more instructional time than even the treatment group of Ganimian et al. (2024), which rationalizes the large private preschool premia we find.[29] This limited instructional time in *anganwadis*, potentially combined with low productivity of such instruction, is a problem across Indian states.[30]

A potential explanation for our results is that, despite the *stated* importance given to early childhood cognition in policy documents, preschool instruction has been less central to anganwadi centres than their role as daycare centres or supplementary nutrition centres. Our results acknowledge this possibility but also highlight that (i) the ICDS system is the principal government intervention at the preschool stage, (ii) it is the largest such system globally and (iii) it is explicitly intended to be part of the system-wide pivot towards achieving universal literacy and numeracy by Grade 3. Providing *quality* preschool to all children, as targeted by the Sustainable Development Goals and the National Education Policy, is unlikely to be feasible without improving the value-added of *anganwadis* in the skills related to functional literacy and numeracy.

Our results suggest that scaling effective interventions targeting learning in public preschools, beyond effects on achievement, could also improve socioeconomic equality. Promising anganwadi-based interventions include, for example, additional staffing in Tamil Nadu (Ganimian et al., 2024) and WhatsApp-based instructional materials for anganwadi

[29] Put differently, it is possible that public preschools could achieve similar gains as private preschools if they provided instruction of equivalent duration. That they are unable to do so reflects, at least in part, inadequate resources and staffing (Ganimian et al., 2024). See Singh et al. (2024) for an example of sharp learning gains in response to government investments after COVID-19 in this setting.

[30] See, for instance, descriptive findings reported in the India Early Childhood Education Impact Study (Kaul & Bhattacharjea, 2019) and the FOCUS report (CIRCUS, 2006). Indeed, in the FOCUS Report, anganwadis in Tamil Nadu were highlighted as being better-functioning than in other states, suggesting our results might understate the differential with private sector preschools. *De facto*, preschool education has not been prioritized by the government: this is evident, for example, also in budgetary allocations, staffing, and the social status of preschool workers compared to primary school teachers.

workers and parents (Keskar et al., 2025). That we find positive correlations between private and public value-added also potentially suggests that these improvements might even have multiplier effects through market-level interactions (such as demonstrated by Andrabi et al. (2024) at the primary school level). Developing, validating and scaling such interventions should, given our results, be an important priority for research and policy.

Our results also suggest that substantial gains may be possible from vouchers that enable children to attend private preschools. Dean & Jayachandran (2019) show the effects of such a policy in India, finding gains of as much as 0.8σ for students induced to attend a particular private preschool provider using a randomized voucher. Our value-added estimates are remarkably similar on average; however, we also observe significant differences in the private premium across villages. Voucher policies to move students to private preschools are also, *de facto*, already at scale: Romero & Singh (2022) show that the principal effect of private sector quotas in India's Right to Education Act 2009 on enrolment is to move some students from public preschools or no enrolment into private preschools. Understanding the effects of these policy-induced moves on skill acquisition, which has not been done outside of the COVID-19 pandemic, as well as ways to target the policies to make them more effective, are clearly avenues where further research would be productive.[31]

More generally, both in India and elsewhere among LMICs, there is remarkably little evidence on the organization of preschool markets. Understanding the distribution of productivity among individual providers, the preferences and information sets of parents, constraints on the provision of quality instruction, and the nature of competition and market interactions between providers are all topics of substantial importance for academic research. We hope that the observational results presented in this paper will spur further work in this area, including interventions to improve the functioning of these markets.

[31] These voucher-led policies could also be substantially cost-effective. Ganimian et al. (2024) document gains of 0.11σ incurred with a per-child cost of $\sim$3500 INR per year. Private preschool fees in our study districts are $\sim$7000 INR ($\sim$95 USD) per year on average. Taking our value-added estimates of private preschool premia of 0.55–0.7σ at face value, even if only 20% of voucher recipients could be induced to shift from public preschools (with the rest of voucher spending being inframarginal), this would be as or more cost-effective. These calculations do not account for (unmeasured) potential treatment effects of private institutions on English, which are likely to be large (Muralidharan & Sundararaman, 2015; Singh, 2015). Incorporating these would further improve the measured cost-effectiveness.

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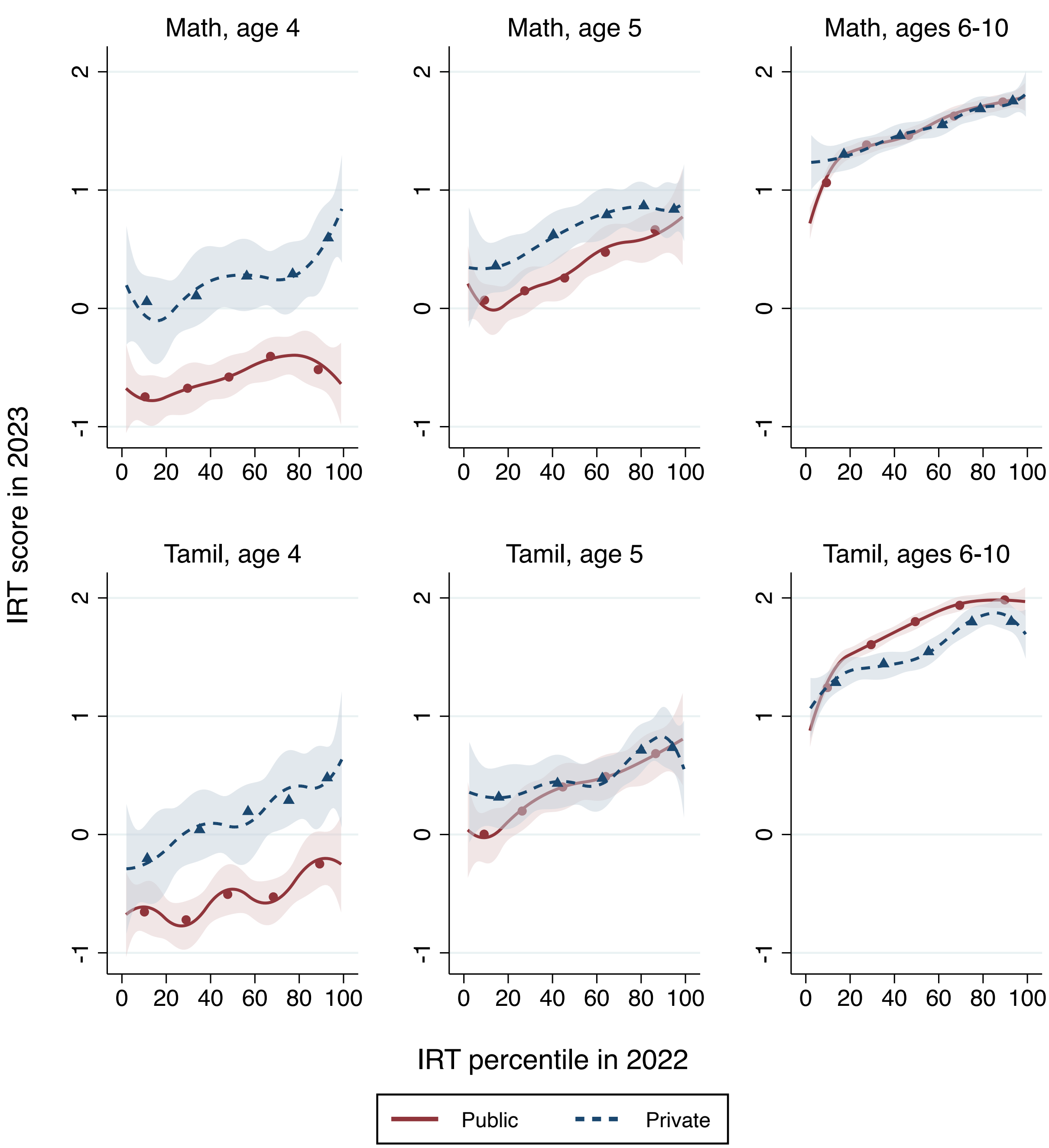
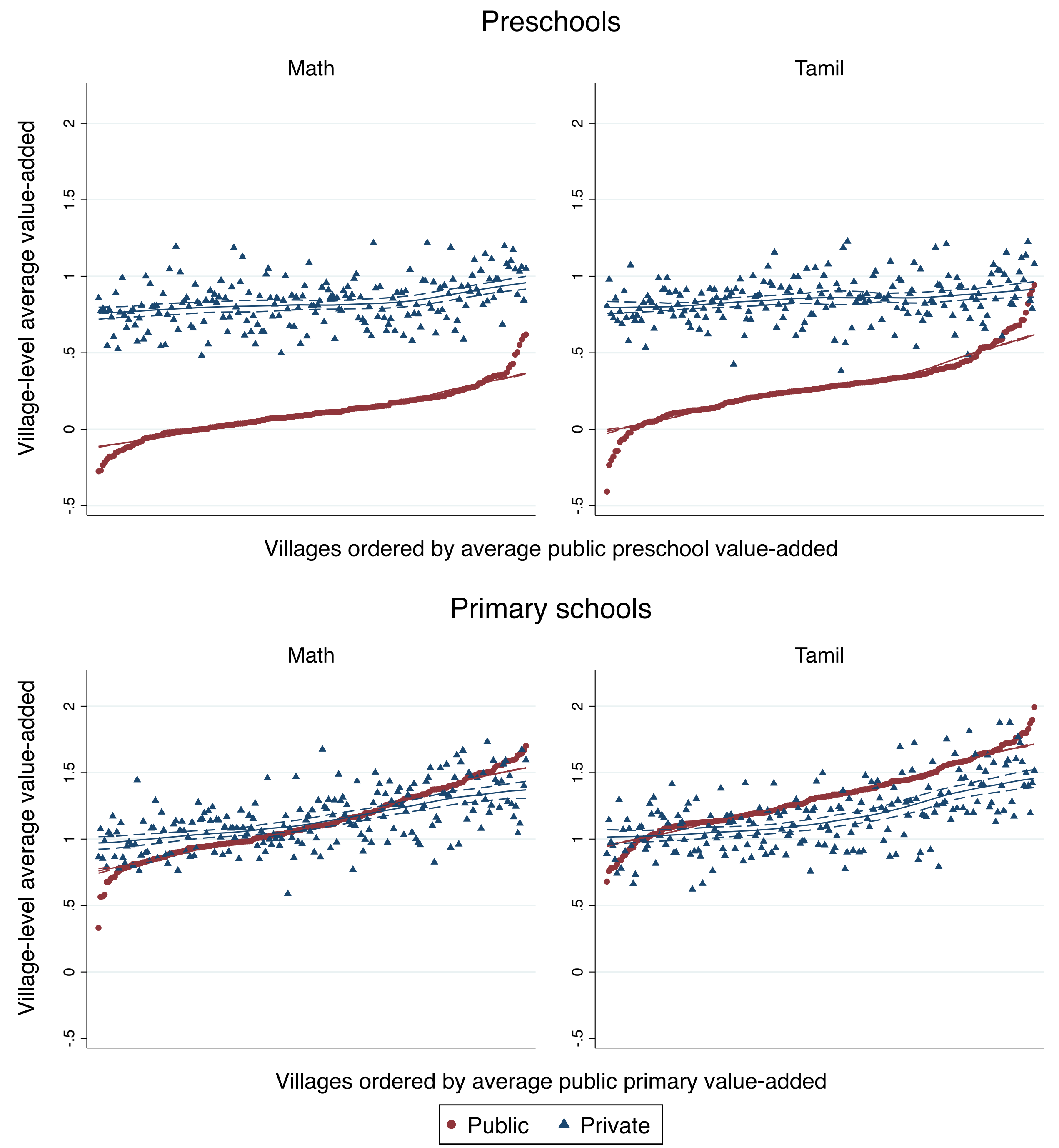
6 Figures

Figure 1: IRT scores in 2023 by percentile scores in 2022 and sector



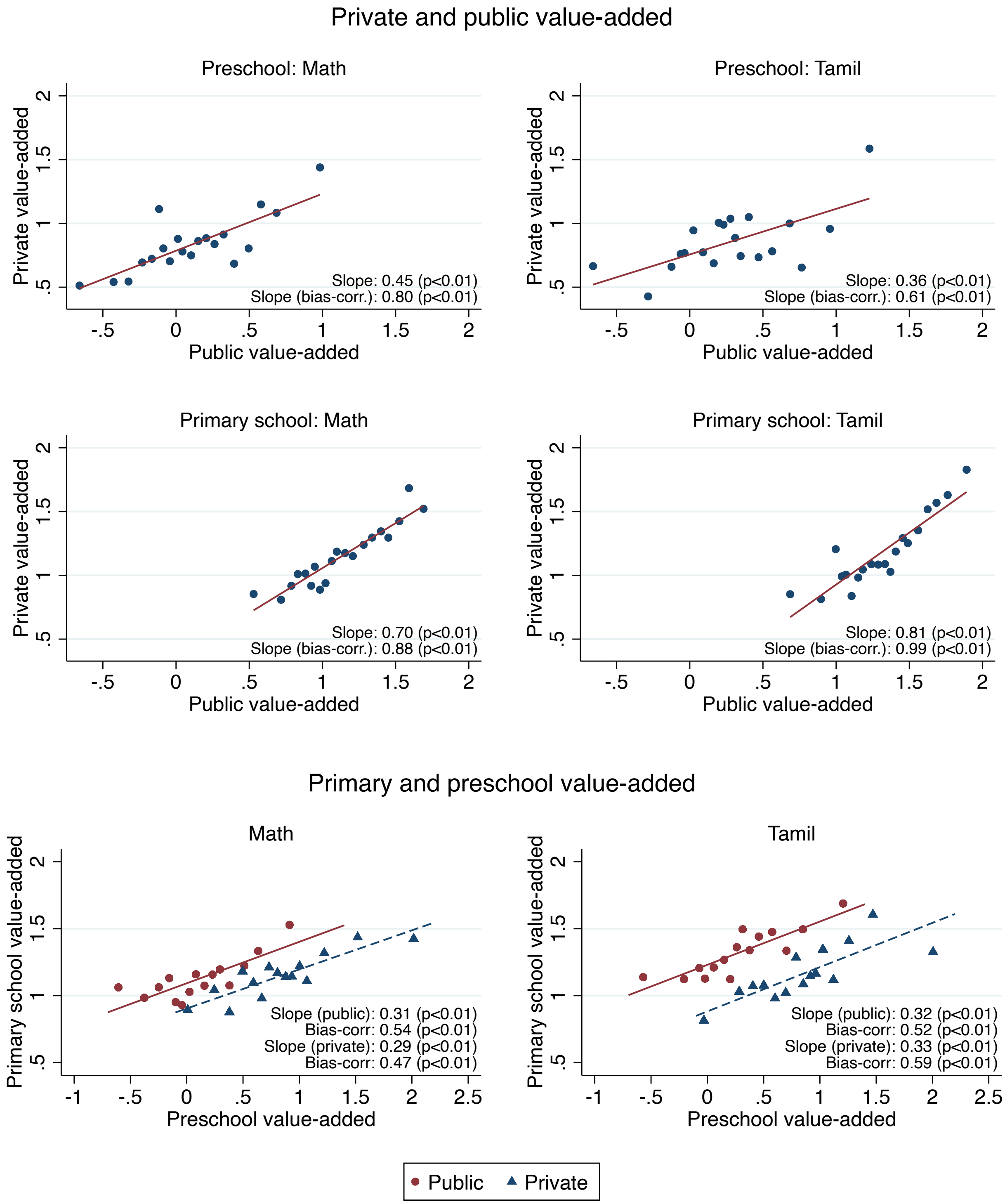
Notes: These figures relate the IRT score of a student in the 2023 assessment, to that of his or her within-age percentile rank in the 2022 assessment, separately by private/public enrolment. These semiparametric estimations condition on village FEs, deciles of the SES index, child gender, and parental education, using the approach of Cattaneo et al. (2024). The data is shown in 5 equally sized bins, and fitted lines are piecewise, quadratic polynomials with a smoothness parameter of 2, generated using the `binsreg` package in Stata.

Figure 2: Village-level average value-added of private and public options



Notes: These figures show village-level average school value-added by sector (public/private) and level (preschool/primary) using Empirical Bayes measures as described in Appendix E.1. Villages are ordered along the x-axis by their average value-added in government schools. The regression specification generating these estimates is given by Equation 2.

Figure 3: Correlations of village value-added across sectors and levels



Notes: These figures show binned scatter plots of village-level Empirical Bayes value-added estimates. In the top panel, we show the correlation between private and public sector value-added, separately by subject and level (preschool/primary school). In the bottom panel, we show correlations between pre- and primary school value-added, separately by subject and sector. Raw and bias-corrected slopes and p-values associated with a test of zero slopes are shown in the figures; robust standard errors are used for inference (see Appendix E.2 for details on the bias-correction procedure).

7 Tables

Table 1: Child and household characteristics by age and enrolment status

	No	Preschool			Primary school		
	school	Public	Private	Diff.	Public	Private	Diff.
Panel A: Age 4							
Female	0.55	0.48	0.45	-0.03	0.43	0.65	0.21
Mother educ.: $<$ Gr. 9	0.26	0.24	0.16	-0.08***	0.27	0.12	-0.15
Mother educ.: $\geq$ Gr. 12	0.29	0.35	0.60	0.25***	0.46	0.53	0.07
SES percentile	49.11	47.20	65.60	18.40***	55.78	66.18	10.39
Math IRT score in 2022	-1.34	-1.27	-1.10	0.17***	-0.73	-0.65	0.07
Tamil IRT score in 2022	-1.55	-1.36	-1.18	0.17***	-0.50	-0.67	-0.17
Share of students	0.04	0.61	0.33		0.02	0.01	
Observations	87	1349	726		37	17	
Panel B: Age 5							
Female	0.46	0.51	0.46	-0.05	0.51	0.50	-0.01
Mother educ.: $<$ Gr. 9	0.28	0.23	0.16	-0.07***	0.25	0.15	-0.10***
Mother educ.: $\geq$ Gr. 12	0.40	0.34	0.57	0.24***	0.31	0.56	0.25***
SES percentile	43.98	45.25	67.74	22.49***	43.87	66.58	22.71***
Math IRT score in 2022	-0.99	-1.04	-0.66	0.38***	-0.81	-0.36	0.45***
Tamil IRT score in 2022	-1.07	-1.08	-0.68	0.41***	-0.85	-0.36	0.49***
Share of students	0.02	0.24	0.20		0.39	0.15	
Observations	50	683	592		1132	434	
Panel C: Ages 6–10							
Female	0.44	0.40	0.34	-0.06	0.51	0.44	-0.07***
Mother educ.: $<$ Gr. 9	0.26	0.00	0.12	0.12	0.31	0.15	-0.16***
Mother educ.: $\geq$ Gr. 12	0.21	0.20	0.52	0.32**	0.24	0.51	0.27***
SES percentile	46.87	44.47	68.45	23.98***	42.66	63.25	20.60***
Math IRT score in 2022	0.29	-0.30	-0.44	-0.14	0.62	0.92	0.30***
Tamil IRT score in 2022	0.19	-0.17	-0.48	-0.32	0.71	0.83	0.12***
Share of students	0.00	0.00	0.00		0.73	0.26	
Observations	39	15	67		10465	3797	

Notes: This table reports average differences in child and household characteristics by type of enrolment, separately by age, in three panels. The types of enrolment are no school and private/public pre-/primary school. Columns 4 and 7 show the difference between children in the private and public sectors, respectively. Virtually all children attend preschool at age 4. At age 5, children start transitioning into primary school. Between the ages of 6 and 10, virtually all are enrolled in primary school.

Table 2: Private school value-added in preschool and primary school

	Age 4		Age 5		Ages 6–10	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Math						
Private school	0.807***	0.737***	0.322***	0.180***	0.125***	-0.005
	(0.047)	(0.051)	(0.044)	(0.048)	(0.022)	(0.019)
Math IRT score in 2022		0.177***		0.242***		0.306***
		(0.028)		(0.026)		(0.010)
Panel B: Tamil						
Private school	0.640***	0.588***	0.160***	0.053	-0.098***	-0.172***
	(0.052)	(0.054)	(0.045)	(0.047)	(0.023)	(0.022)
Tamil IRT score in 2022		0.199***		0.207***		0.338***
		(0.027)		(0.024)		(0.011)
Controls	Village FE	All	Village FE	All	Village FE	All
Observations	1,839	1,839	2,841	2,841	14,344	14,344

Notes: Robust standard errors, clustered at the village level, in parentheses. Columns 1, 3, and 5 show raw means by private school attendance within villages. Columns 2, 4, and 6 include lagged scores, village fixed effects, and controls for deciles of the SES wealth index, paternal and maternal education, and child gender. Test scores refer to equated IRT scores, standardized with respect to children aged 5 in the 2022 assessments. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

Table 3: Robustness of the private school premium

	Math				Tamil			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Age 4								
Private school	0.737***	0.736***	0.724***	0.724***	0.588***	0.585***	0.565***	0.573***
	(0.051)	(0.051)	(0.051)	(0.051)	(0.054)	(0.054)	(0.054)	(0.056)
RV	0.31	0.31	0.31	0.30	0.24	0.24	0.23	0.24
$RV_{\alpha=0.05}$	0.28	0.28	0.27	0.27	0.20	0.20	0.20	0.20
$R^2_{Y\sim D\|X}$	0.12	0.12	0.12	0.12	0.07	0.07	0.07	0.07
$R^2_{Y\sim Z\|D,X}$	0.06	0.02	0.02	0.02	0.07	0.02	0.02	0.02
$R^2_{D\sim Z\|X}$	0.16	0.15	0.15	0.13	0.16	0.15	0.15	0.13
Observations	1839	1839	1839	1802	1839	1839	1839	1802
Panel B: Ages 6–10								
Private school	-0.005	0.012	0.012	0.014	-0.172***	-0.192***	-0.192***	-0.178***
	(0.019)	(0.019)	(0.019)	(0.020)	(0.022)	(0.022)	(0.022)	(0.023)
RV	0.00	0.01	0.01	0.01	0.08	0.09	0.09	0.08
$RV_{\alpha=0.05}$	.	.	.	.	0.06	0.07	0.07	0.06
$R^2_{Y\sim D\|X}$	0.00	0.00	0.00	0.00	0.01	0.01	0.01	0.01
$R^2_{Y\sim Z\|D,X}$	0.25	0.03	0.03	0.02	0.32	0.07	0.07	0.06
$R^2_{D\sim Z\|X}$	0.18	0.17	0.18	0.15	0.17	0.17	0.17	0.14
Observations	14344	14344	14344	13932	14344	14344	14344	13932
Core controls	✓	✓	✓	✓	✓	✓	✓	✓
Lagged, squared Tamil & Math scores		✓	✓	✓		✓	✓	✓
Survey month 2022×2023 FEs			✓	✓			✓	✓
Caste & home inputs in 2022				✓				✓

Notes: Robust standard errors, clustered at the village level, in parentheses. This table shows regression of IRT scores in 2023 on having attended a private rather than public preschool or primary school during the previous school year. Column 1 reports estimates of this private premium in our core specification (Equation 1). Column 2 adds controls for baseline (2022) scores quadratically in both math and Tamil. Column 3 further includes fixed effects for the month in which the baseline and endline surveys were conducted, as well as their interactions. Finally, Column 4 adds controls for caste and several educational inputs measured at baseline: whether the child had recently received educational content via 1) internet, 2) TV, or 3) books at home. These measures are missing 37 (412) children at age 4 (6 to 10). Robustness values and partial R-squared under extreme scenarios of potential confounders are computed as described in Appendix D.1. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

Table 4: Decomposition of SES gap (top/bottom 25%) in 2023 test scores, preschool and primary level

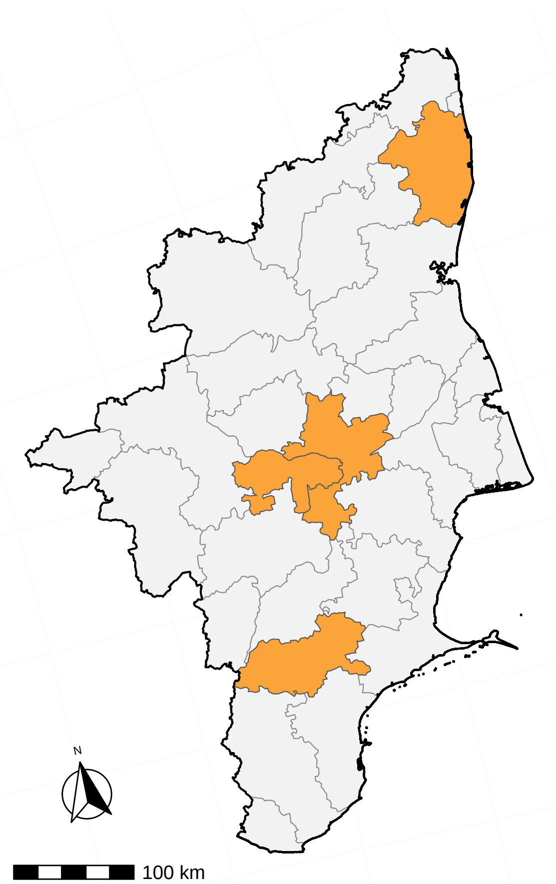
	Age 4			Age 5			Ages 6–10		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Math									
Top 25% SES	0.434***	0.412***	0.169*	0.307***	0.250***	0.143*	0.238***	0.164***	0.155***
	(0.072)	(0.072)	(0.081)	(0.056)	(0.055)	(0.065)	(0.029)	(0.026)	(0.026)
Private school			0.680***			0.249**			0.024
			(0.080)			(0.075)			(0.026)
Panel B: Tamil									
Top 25% SES	0.356***	0.329***	0.135	0.147*	0.086	0.054	0.099**	0.053	0.105***
	(0.068)	(0.067)	(0.069)	(0.061)	(0.063)	(0.067)	(0.030)	(0.027)	(0.030)
Private school			0.541***			0.074			-0.142***
			(0.081)			(0.073)			(0.031)
Lagged score control		✓	✓		✓	✓		✓	✓
Observations	914	914	914	1,360	1,360	1,360	6,873	6,873	6,873

Notes: Robust standard errors, clustered at the village level, in parentheses. Village fixed effects and controls for child gender are included in all regressions. Test scores refer to the IRT EAP scores, standardized with respect to children aged 5 in the 2022 assessments. The SES index is based on questions regarding the availability of household amenities and computed with PCA. The omitted category contains students in households with an SES index below the 25th percentile. Households with an SES index between the 25th and 75th percentiles are excluded from the regressions. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

Online Appendix for "The productivity of public and private preschools (and schools): Evidence from India" by Berg, Romero, and Singh

A Additional tables and figures

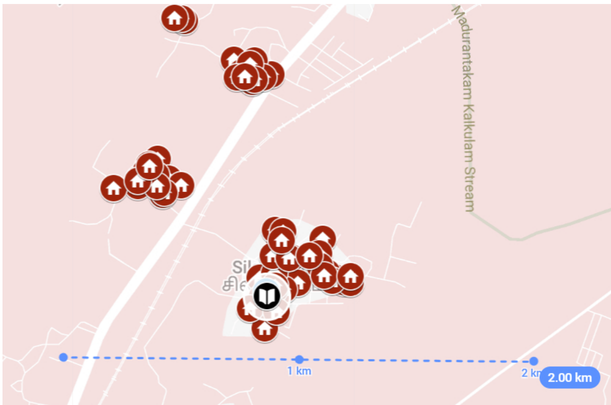
Figure A.1: Map of sample districts in Tamil Nadu



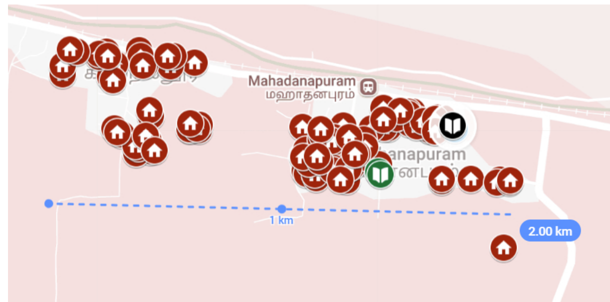
Note: This figure shows the four sample districts (Kancheepuram, Karur, Tiruchchirappalli, and Virudhunagar) included in the data collection.

Figure A.2: Villages and the households we sample (some examples)

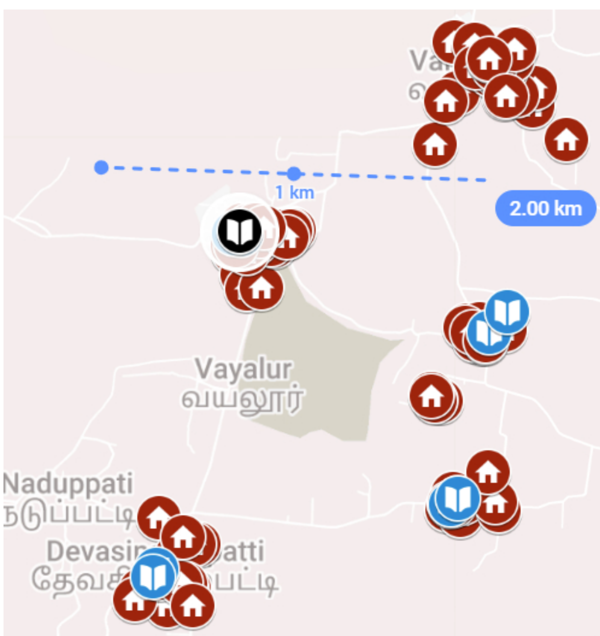
(a) Village 1



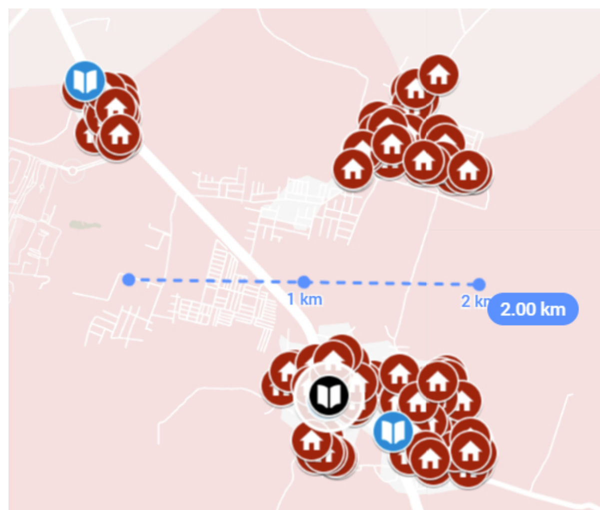
(b) Village 2



(c) Village 3

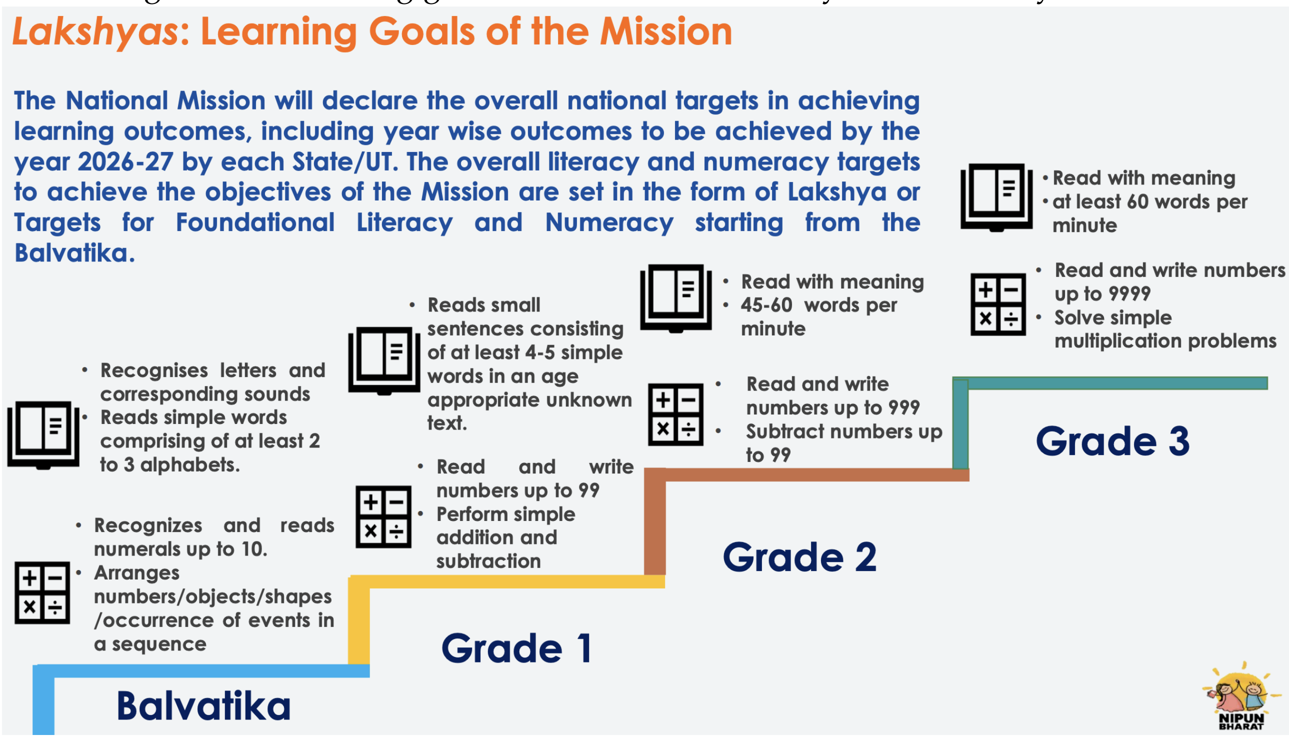


(d) Village 4



Note: This figure shows 4 villages in our sample. The black point with a "book" icon represents the "reference point" (i.e., the anganwadi). Blue "book" icons represent other public schools (across all levels), and green "book" icons represent other private schools (across all levels). The red "house" icons represent households in our sample (i.e., with children). The villages were selected based on their position at the 20th, 40th, 60th, and 80th percentiles of the maximum distance between the reference point and a household in our sample. As can be seen, in most cases, 'villages' encompass several clusters of households that are located near each other. The 2-kilometer rule was not strictly enforced, and in most cases, either all households in a cluster were included or none.

Figure A.3: Learning goals in foundational literacy and numeracy in India



Note: This figure shows official learning goals for early childhood education as presented in the National Initiative for Proficiency in reading with Understanding and Numeracy (NIPUN) by the Ministry of Education in India. Retrieved from https://static.pib.gov.in/WriteReadData/specificdocs/documents/2021/jul/doc20217531.pdf

Table A.1: Comparing baseline sample to NFHS

	NFHS-V (1)	Baseline (2)	Difference (3)
Panel A: Assets and household characteristics			
Internet	0.59	0.48	-0.11***
	(0.49)	(0.50)	p=0.00
Washing machine	0.14	0.09	-0.06***
	(0.35)	(0.28)	p=0.00
Fridge	0.56	0.46	-0.10***
	(0.50)	(0.50)	p=0.00
Computer	0.09	0.08	-0.01
	(0.28)	(0.27)	p=0.20
Television	0.94	0.93	-0.02***
	(0.23)	(0.26)	p=0.01
Fan	0.97	0.97	-0.00
	(0.16)	(0.17)	p=0.83
Electricity	0.99	0.94	-0.05***
	(0.08)	(0.23)	p=0.00
Car	0.05	0.05	-0.01
	(0.22)	(0.21)	p=0.34
Tractor	0.02	0.03	0.00
	(0.15)	(0.16)	p=0.35
Bike	0.77	0.75	-0.02
	(0.42)	(0.43)	p=0.14
Bicycle	0.46	0.36	-0.10***
	(0.50)	(0.48)	p=0.00
Number of children (3-10 yrs old)	1.62	1.55	-0.07***
	(0.68)	(0.62)	p=0.00
Scheduled caste	0.36	0.33	-0.03
	(0.48)	(0.47)	p=0.16
Owns land	0.31	0.25	-0.06***
	(0.46)	(0.43)	p=0.00
Observations	2,561	17,486	
Panel B: Maternal education			
Mother education: at least some primary	0.96	0.96	-0.00
	(0.20)	(0.21)	p=0.38
Mother education: at least some secondary	0.87	0.93	0.06***
	(0.33)	(0.25)	p=0.00
Observations	2,542	16,280	

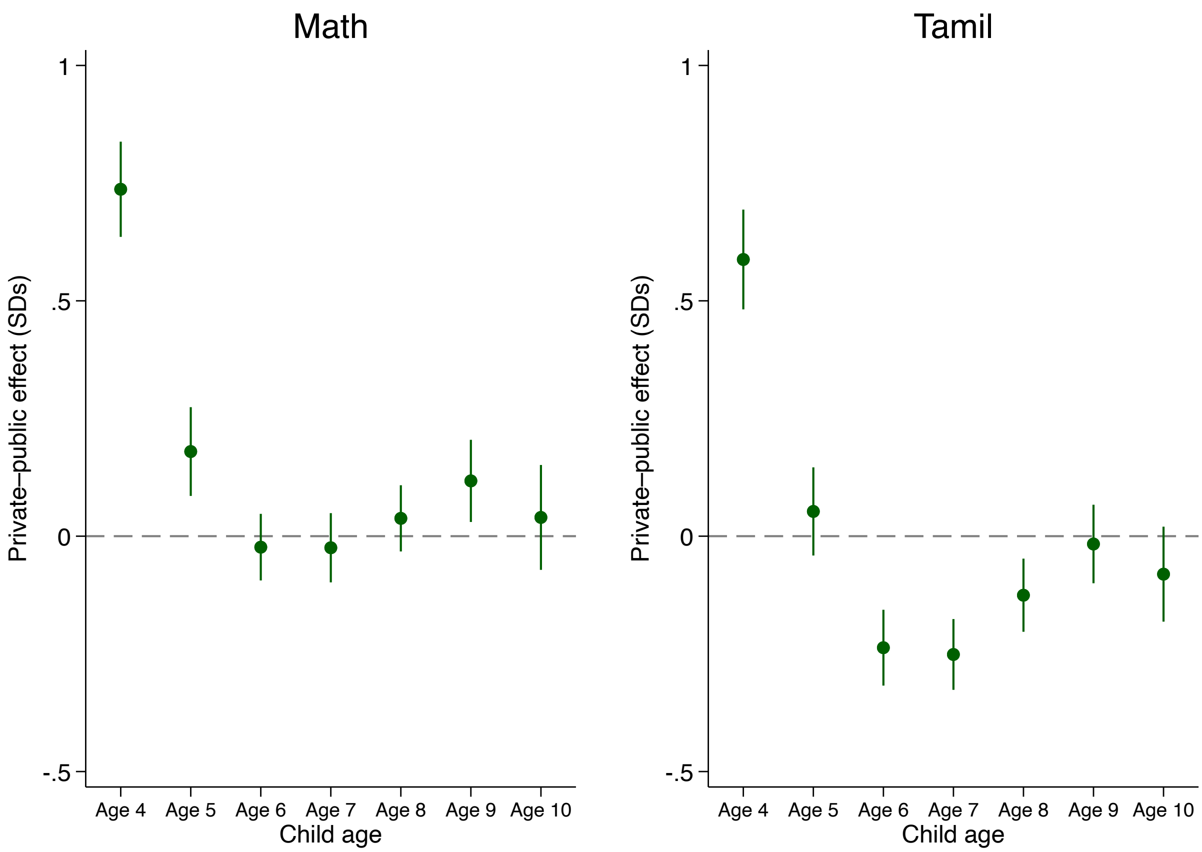
Notes: The table presents means and standard deviations for households in Tamil Nadu with children aged 3–10 in the NFHS-V survey (Column 1) and households in our baseline sample in 2022 (Column 2). Column 3 shows differences and statistical significance (clustering standard errors at the sampling cluster level for NFHS-V and the village level in our sample). $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

Table A.2: Private school value-added in preschool and primary school for children aged 5

	Math (1)	Tamil (2)
Private school	0.715***	0.590***
	(0.0587)	(0.0562)
Primary school	1.051***	1.072***
	(0.0487)	(0.0475)
Private school × Primary school	-0.698***	-0.698***
	(0.0693)	(0.0672)
Math IRT score in 2022	0.171***	
	(0.0236)	
Tamil IRT score in 2022		0.148***
		(0.0216)
Constant	-0.146***	-0.164***
	(0.0400)	(0.0405)
Controls	All	All
Observations	2,841	2,841

Notes: Robust standard errors, clustered at the village level, in parentheses. The regressions include only children aged 5, around half of whom are already enrolled in primary school. The coefficient on the private school dummy captures the private premium in preschools. The sum of this coefficient and that of the interaction between private and primary school captures the private premium in primary school. Lagged scores, village fixed effects, and controls for deciles of the SES wealth index, paternal and maternal education, as well as child gender are included in both regressions. Test scores refer to the IRT Expected A Posteriori (EAP) scores, standardized with respect to children aged 5 in the 2022 assessments. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

Figure A.4: Private school value-added by age



Notes: These figures show the private premium by age (as opposed to estimating the premium for 6- to 10-year-olds together). The coefficients are analogous to those in Table 2, but they are estimated separately for children of different ages.

Table A.3: Private school value-added in math competencies

	Dependent variable: Proportion correct on math items							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Age 4	*Addition*	*Subtraction*	*Number identification*	*Quantitative comparison*				
Private school	0.251***	0.248***	0.326***	0.188***				
	(0.026)	(0.027)	(0.019)	(0.020)				
Math IRT score in 2022	0.072***	0.081***	0.059***	0.054***				
	(0.013)	(0.015)	(0.011)	(0.010)				
Constant	0.516***	0.414***	0.270***	0.651***				
	(0.019)	(0.021)	(0.016)	(0.015)				
Observations	1,839	1,839	1,839	1,839				
Number of items	2	1	7	5				
Public sector avg.	0.414	0.317	0.189	0.577				
Panel B: Ages 6–10	*Addition*	*Subtraction*	*Number identification*	*Quantitative comparison*	*Multiplication & division*	*problems Applied*	*Geometry*	*Measurement*
Private school	0.012*	0.010	-0.011	0.022*	0.014	0.051***	-0.023	0.005
	(0.005)	(0.006)	(0.008)	(0.011)	(0.013)	(0.009)	(0.032)	(0.011)
Math IRT score in 2022	0.047***	0.044***	0.057***	-0.043***	0.059***	0.058***	0.049**	0.076***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.004)	(0.015)	(0.005)
Constant	0.756***	0.661***	0.893***	0.699***	0.402***	0.709***	0.311***	0.630***
	(0.002)	(0.002)	(0.002)	(0.004)	(0.007)	(0.003)	(0.022)	(0.006)
Observations	14,344	14,344	5,201	14,344	9,142	14,344	805	9,142
Number of items	12	6	4	6	4	5	3	2
Public sector avg.	0.782	0.684	0.890	0.665	0.457	0.740	0.377	0.700

Notes: Robust standard errors, clustered at the village level, in parentheses. Lagged scores, village fixed effects, and controls for deciles of the SES wealth index, paternal and maternal education, and child gender included. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

Table A.4: Private school value-added in Tamil competencies

	Dependent variable: Proportion correct on Tamil items				
	(1)	(2)	(3)	(4)	(5)
Panel A: Age 4	*Letter & word recognition*	*Oral comprehension*			
Private school	0.221***	0.096***			
	(0.020)	(0.017)			
Tamil IRT score in 2022	0.059***	0.057***			
	(0.009)	(0.009)			
Constant	0.450***	0.583***			
	(0.014)	(0.013)			
Observations	1,839	1,839			
Number of items	7	5			
Public sector avg.	0.364	0.504			
Panel B: Ages 6–10	*Letter & word recognition*	*Oral comprehension*	*Word & sentence comprehension*	*Sentence & story comprehension*	*Spelling*
Private school	-0.058***	-0.024	-0.003	-0.024*	0.001
	(0.006)	(0.014)	(0.004)	(0.010)	(0.034)
Tamil IRT score in 2022	0.075***	0.042***	0.034***	0.085***	0.049**
	(0.003)	(0.007)	(0.002)	(0.005)	(0.019)
Constant	0.797***	0.818***	0.872***	0.412***	0.624***
	(0.002)	(0.005)	(0.002)	(0.006)	(0.033)
Observations	13,525	1,837	14,344	9,142	805
Number of items	14	5	12	16	2
Public sector avg.	0.846	0.800	0.894	0.504	0.713

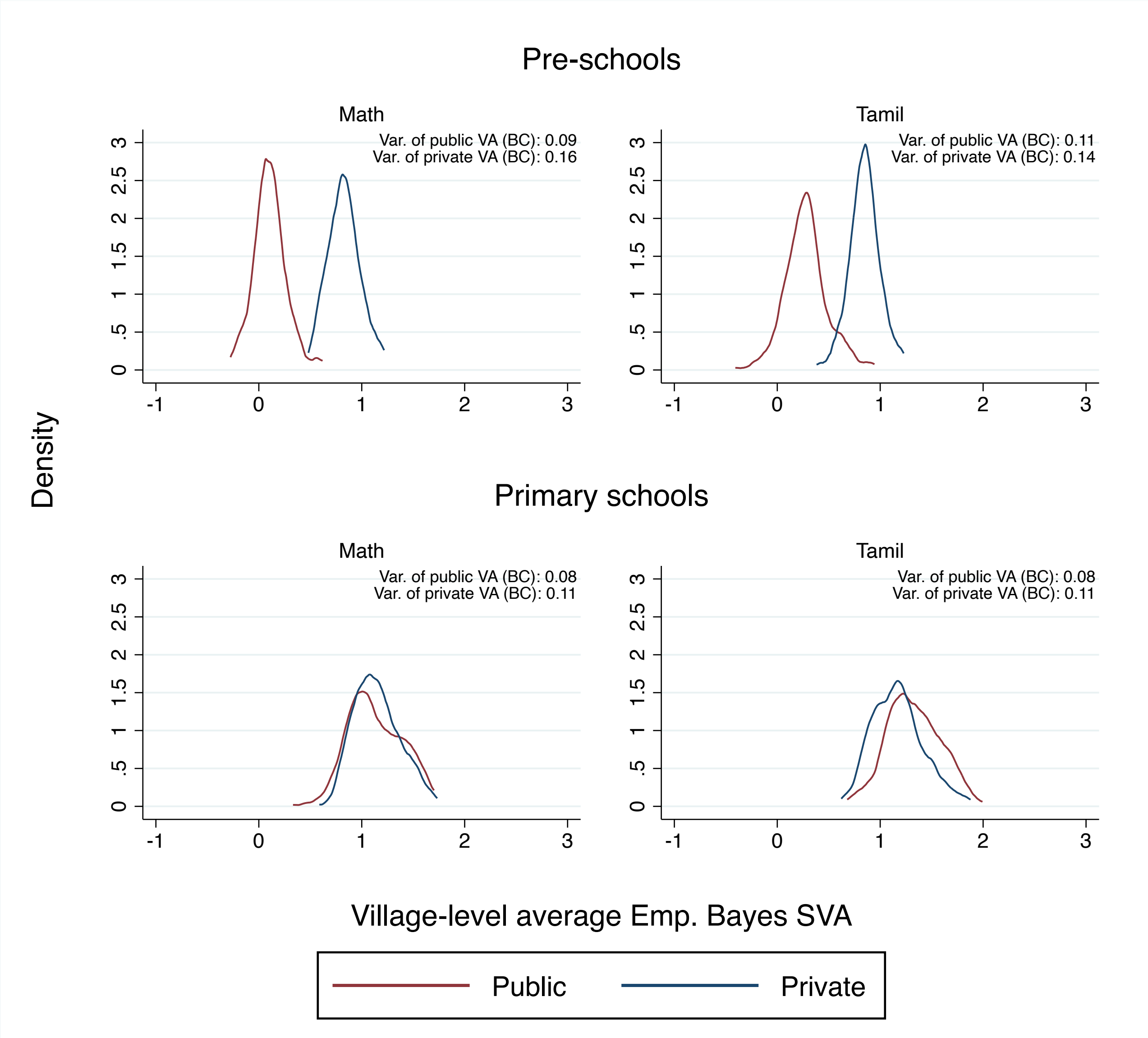
Notes: Robust standard errors, clustered at the village level, in parentheses. Lagged scores, village fixed effects, and controls for deciles of the SES wealth index, paternal and maternal education, and child gender included. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

Table A.5: Value-added of public/private preschools relative to no enrolment, age 4

	Math		Tamil	
	(1)	(2)	(3)	(4)
Public	0.229	0.244	0.302*	0.291*
	(0.126)	(0.125)	(0.147)	(0.146)
Private	1.039***	0.982***	0.946***	0.882***
	(0.126)	(0.127)	(0.146)	(0.146)
Math IRT score in 2022		0.176***		
		(0.0275)		
Tamil IRT score in 2022				0.198***
				(0.0266)
Constant	-0.801***	-0.582***	-0.819***	-0.541***
	(0.120)	(0.127)	(0.140)	(0.146)
Controls	Village FE	All	Village FE	All
Observations	1,906	1,906	1,906	1,906

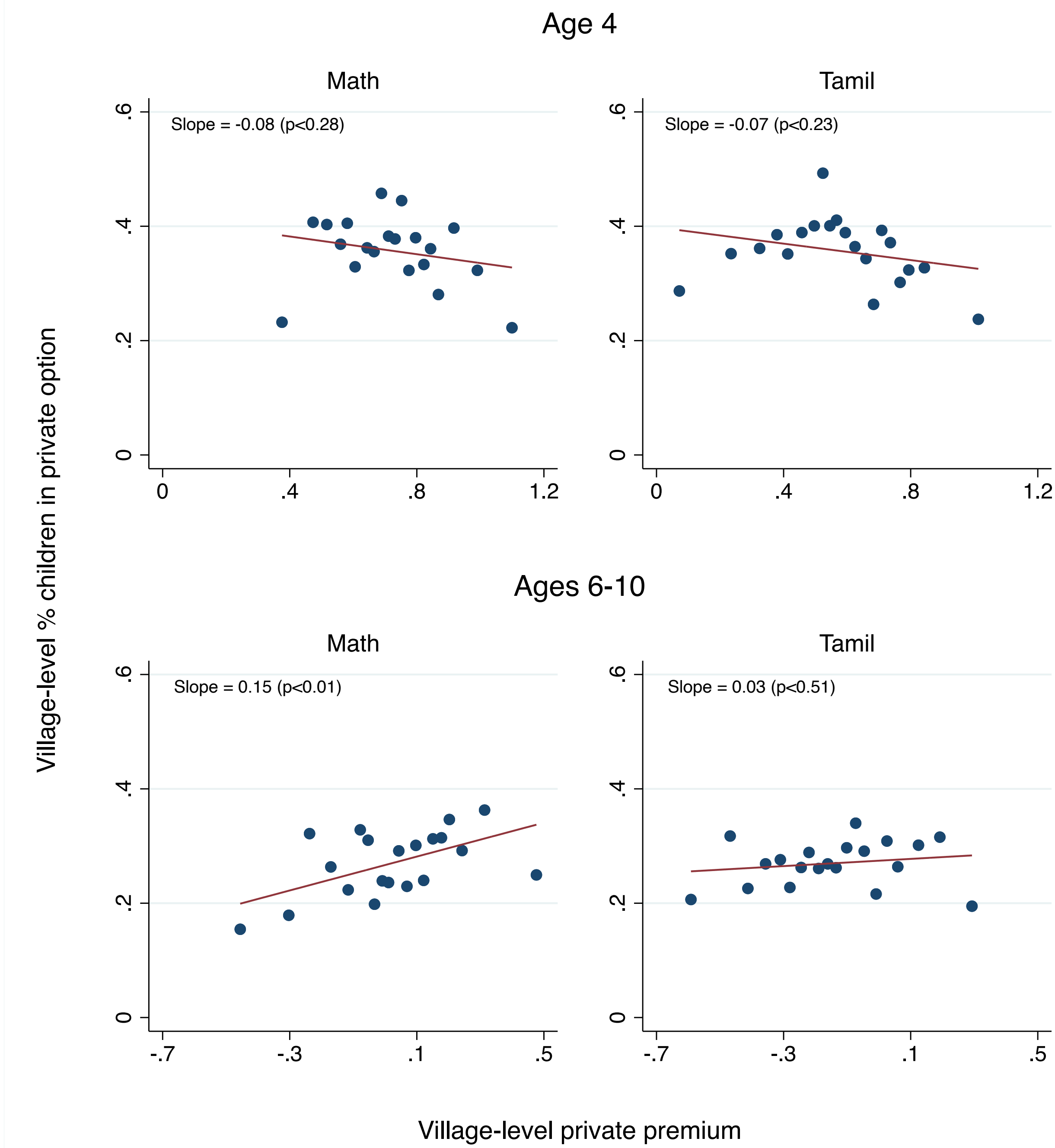
Notes: Robust standard errors, clustered at the village level, in parentheses. The omitted category is children not enrolled in any type of education (67 children). Columns 1 and 3 report raw test score differences by type of school attended, within villages. Columns 2 and 4 include village fixed effects and controls for lagged scores, deciles of the SES wealth index, paternal and maternal education, as well as child gender. Test scores refer to the IRT Expected A Posteriori (EAP) scores, standardized with respect to children aged 5 in the 2022 assessments. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

Figure A.5: Distributions of village-level average value-added



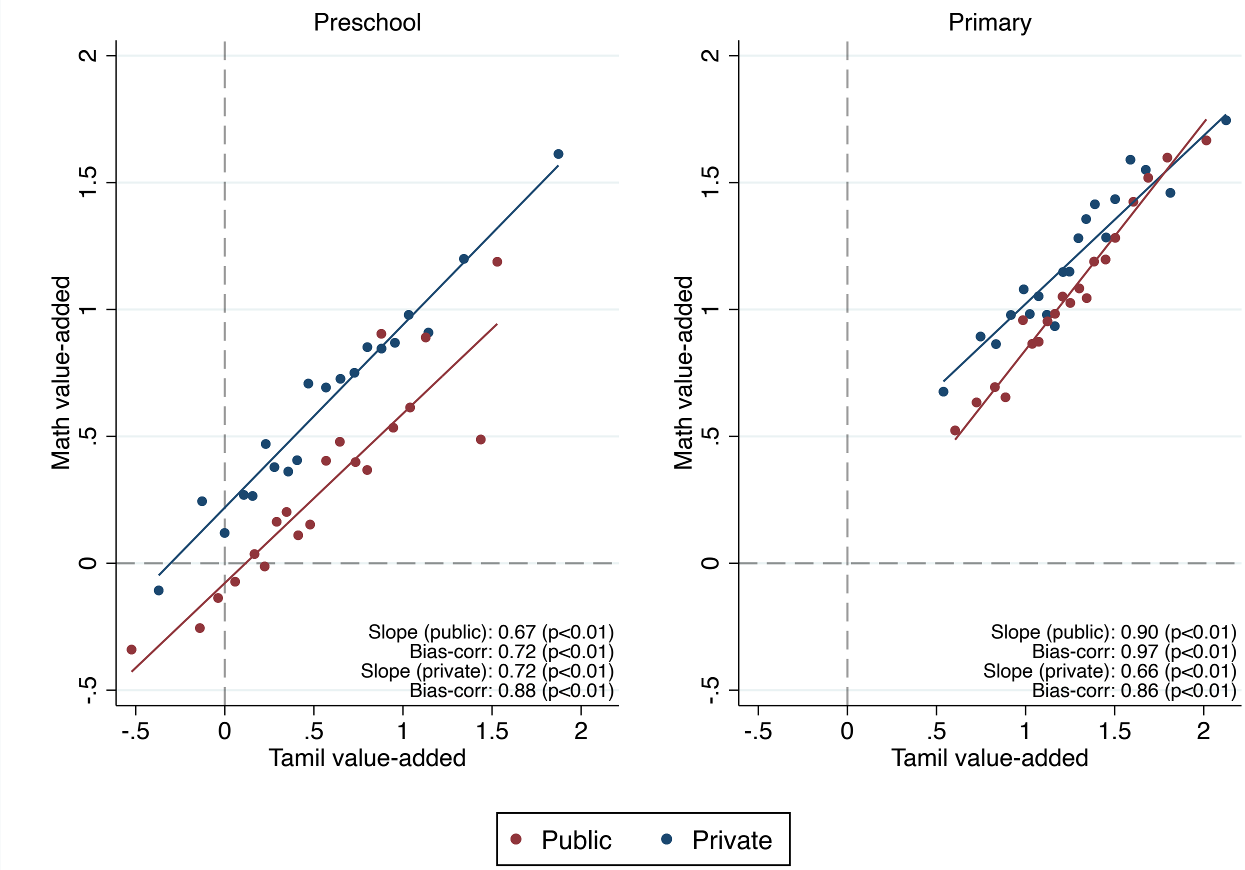
Notes: These figures show kernel density plots of village-level average school value-added by sector (public/private) and level (preschool/primary) using Empirical Bayes measures as described in Section E.1. These are generated in a regression that includes children aged 4–10, as described in Section 4.1. Bias-corrected variances of the underlying value-added parameters are computed as described in Appendix E.2.

Figure A.6: Village-level private premium and share of private enrolment



Notes: These figures depict the correlation between (1) the village-level share of children in a private school and (2) the difference between the average *private* and *public* school value-added in the same village (i.e., the private premium). These value-added measures are Empirical Bayes estimates, estimated as described in Appendix E.1.

Figure A.7: Cross-subject correlations of village-level value-added



Notes: These figures show local polynomial fits of village-level value-added in math vs. Tamil at the preschool/primary school level, respectively. Both raw and bias-corrected slopes are shown in the bottom right of the figures (see Appendix E.2 for details on bias-correction).

Table A.6: Regressions of private premia on market size and village average SES

	Preschool private premium		Primary school private premium	
	Math	Tamil	Math	Tamil
	(1)	(2)	(3)	(4)
Number of children in market (std)	0.008	0.010	-0.002	-0.002
	(0.013)	(0.017)	(0.017)	(0.015)
Village average SES (std)	-0.021	-0.046*	0.016	-0.022
	(0.014)	(0.018)	(0.015)	(0.018)
Constant	0.720***	0.571***	0.020	-0.147***
	(0.012)	(0.015)	(0.015)	(0.015)
Observations	200	200	211	211

Notes: Robust standard errors in parentheses. This table shows village-level regressions of private premia, across sectors and levels, on the standardized number of children aged 6–10 and the standardized average SES percentile in the village. One standard deviation of number of children corresponds to 45 children; for village-level average SES, it corresponds to 9.7 percentiles of the SES distribution. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

B Attrition between 2022 and 2023 survey waves

This section investigates whether attrition between the 2022 and 2023 survey waves correlates with socioeconomic status, age, or baseline ability.

B.1 Age distribution in the main sample

In 2022, children from age 3 were surveyed and assessed. The 2023 survey was administered just over a year after the 2022 wave, which means that children aged slightly more than one year between the waves (1.2 years, on average, in our main sample). Hence, some children who were 3 years old at baseline had turned 5 by the endline. Likewise, children aged 2 at baseline — some of whom would be 4 by the endline — were not assessed in 2022 and are therefore excluded. This results in a slightly skewed age distribution in the main sample, such that children aged 4 at the endline are underrepresented. The main sample contains 1,837 children with endline age 4, 2,840 with endline age 5, and 14,344 with endline age 6–10.

B.2 Attrition between 2022 and 2023

Since the main sample focuses on children aged 4–10 at the time of the endline survey, most of these children were aged 3–9 at baseline. However, some children who were 9 years old at baseline will have aged out of the sample frame (e.g., turned 11) by the endline. This makes it non-trivial to specify exactly which children at baseline should be considered for possible attrition.

To address this issue, we look at attrition for *all* children aged 3–9 at baseline. While a subset of these children will not be included in the main sample (i.e., those who turned 11 by the endline), this gives a fair representation of attrition in the relevant age span. Table B.1 shows the results of this analysis.

Table B.1: Attrition of children aged 3–9 in the 2022 survey wave

	Re-surveyed	Attrited	Difference (village FE)
	(1)	(2)	(3)
Child age (years)	5.79	6.04	0.30***
	(1.82)	(2.08)	p=0.00
SES percentile	49.24	51.86	0.27
	(28.42)	(29.79)	p=0.62
Mother Edu: $<$ Gr.9	0.25	0.24	0.00
	(0.43)	(0.43)	p=0.64
Mother Edu: Gr. 9-11	0.41	0.40	-0.01
	(0.49)	(0.49)	p=0.13
Mother Edu: Gr. 12+	0.34	0.36	0.01
	(0.47)	(0.48)	p=0.26
Father Edu: $<$ Gr.9	0.38	0.38	0.01
	(0.49)	(0.48)	p=0.28
Father Edu: Gr. 9-11	0.42	0.41	-0.02**
	(0.49)	(0.49)	p=0.02
Father Edu: Gr. 12+	0.20	0.22	0.01
	(0.40)	(0.41)	p=0.11
Enrolled in private	0.21	0.24	0.01
	(0.41)	(0.43)	p=0.12
Math (2022)†	-0.01	0.03	0.02
	(0.90)	(0.91)	p=0.16
Tamil (2022)†	-0.00	0.01	0.01
	(0.94)	(0.96)	p=0.49
Observations	19,200	6,161	

Notes: This table presents means and standard deviations (in parentheses) for children aged 3–9 in the 2022 survey wave along a number of characteristics measured in 2022. The first column displays this information for children who were successfully re-surveyed in 2023, and the second for those who were not. The third column displays the differences between these groups, along with the p-value of the difference, controlling for village fixed effects. Standard errors of differences are clustered at the village level. † Math and Tamil (2022) baseline scores correspond to the residuals after regressing the original IRT scores on age brackets in years. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

B.3 Inverse probability weighting

Tables B.2 and B.3 reproduce the main results on the average private premium (Table 2) and the SES decomposition (Table 4) using inverse probability weighting. In particular, we predict the probability of being observed in 2023, conditional on being observed in 2022, using data on child age, gender, SES, parental education, baseline test scores, and village indicators in a probit regression. We then reproduce the main results, weighing each child inversely by the probability of being observed. The results change little under this alternative strategy.

Table B.2: Private school value-added in preschool and primary school: IPW

	Age 4		Age 5		Ages 6–10	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Math						
Private school	0.805***	0.735***	0.341***	0.192***	0.133***	-0.002
	(0.046)	(0.050)	(0.046)	(0.049)	(0.023)	(0.019)
Math IRT score in 2022		0.185***		0.244***		0.310***
		(0.028)		(0.026)		(0.010)
Panel B: Tamil						
Private school	0.631***	0.576***	0.161***	0.049	-0.106***	-0.180***
	(0.051)	(0.053)	(0.046)	(0.048)	(0.024)	(0.024)
Tamil IRT score in 2022		0.198***		0.210***		0.339***
		(0.027)		(0.023)		(0.011)

Notes: Robust standard errors, clustered at the village level, in parentheses. Columns 1, 3 and 5 show raw means by private school attendance within villages. Columns 2, 4 and 6 include lagged scores, village fixed effects, and controls for deciles of the SES wealth index, paternal and maternal education, and child gender. Test scores refer to equated IRT scores, standardized with respect to children aged 5 in the 2022 assessments. All regressions are weighted inversely proportional to their estimated probability of being observed in 2023, conditional on being observed in 2022. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

Table B.3: Decomposition of SES gap (top/bottom 25%) in 2023 test scores, preschool and primary level: IPW

	Age 4			Age 5			Ages 6–10		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Math									
Top 25% SES	0.441***	0.420***	0.172*	0.324***	0.266***	0.152*	0.248***	0.174***	0.161***
	(0.069)	(0.069)	(0.078)	(0.057)	(0.055)	(0.067)	(0.032)	(0.028)	(0.026)
Private school			0.667***			0.263**			0.034
			(0.078)			(0.080)			(0.028)
Panel B: Tamil									
Top 25% SES	0.378***	0.351***	0.154*	0.148*	0.085	0.054	0.098**	0.052	0.110***
	(0.070)	(0.069)	(0.072)	(0.061)	(0.062)	(0.068)	(0.031)	(0.027)	(0.030)
Private school			0.527***			0.074			-0.155***
			(0.082)			(0.078)			(0.031)

Notes: Robust standard errors, clustered at the village level, in parentheses. Village fixed effects and controls for child gender are included in all regressions. Test scores refer to the IRT EAP scores, standardized with respect to children aged 5 in the 2022 assessments. The SES index is based on questions regarding the availability of household amenities and computed with PCA. The omitted category contains students in households with an SES index below the 25th percentile. Households with an SES index between the 25th and 75th percentiles are excluded from the regressions. All regressions are weighted inversely proportional to their estimated probability of being observed in 2023, conditional on being observed in 2022. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

Village-level results robustness to attrition

In this section, we show that our village-level results are robust to attrition. Specifically, we present the most important village-level results separately for villages with above- and below-median attrition rates, and show that they are remarkably similar. In Table B.4, we show the average private premium at the pre- and primary school levels, both with and without Empirical Bayes shrinkage, as well as correlations in value-added across sectors and levels. We do this separately for all villages, both below- and above-median attrition. Average private premiums are very similar across low- and high-attrition villages, both with and without Empirical Bayes shrinkage. While the sector correlations are more imprecisely estimated, the qualitative pattern of stronger cross-sector correlation at the primary, relative to preschool level, still holds. The correlations in value-added across levels appear to differ between villages with high and low attrition rates. In villages with low attrition, the correlation between primary and preschool value-added appears to be stronger in the public sector than in the private sector.

Table B.4: Results for above- and below median attrition villages

	Math			Tamil		
	All (1)	Below (2)	Above (3)	All (4)	Below (5)	Above (6)
Panel A: Private premia						
Private premium (preschool, EB)	0.72	0.73	0.71	0.57	0.58	0.56
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Private premium (primary, EB)	0.02	-0.01	0.05	-0.15	-0.14	-0.16
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Private premium (preschool)	0.73	0.73	0.72	0.58	0.62	0.53
	(0.04)	(0.05)	(0.05)	(0.04)	(0.06)	(0.05)
Private premium (primary)	0.02	-0.00	0.04	-0.13	-0.09	-0.17
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Panel B: Sector correlations						
Private/public BC corr. (preschool)	0.80	0.84	0.69	0.61	0.61	0.51
	(0.13)	(0.17)	(0.20)	(0.11)	(0.14)	(0.18)
Private/public BC corr. (primary)	0.88	0.86	0.95	0.99	0.96	0.85
	(0.08)	(0.13)	(0.14)	(0.10)	(0.14)	(0.16)
Panel C: Level correlations						
Primary/preschool BC corr. (public)	0.54	0.56	0.31	0.52	0.48	0.47
	(0.09)	(0.10)	(0.10)	(0.08)	(0.09)	(0.11)
Primary/preschool BC corr. (private)	0.47	0.26	0.55	0.59	0.40	0.62
	(0.06)	(0.08)	(0.08)	(0.08)	(0.10)	(0.11)
Number of villages	215	107	108	215	107	108
Share of attrition	0.26	0.35	0.16	0.26	0.35	0.16

Notes: This table reproduces the main findings on village value-added separately for villages above and below the median in terms of share of attriting children from 2022 to 2023. Panel A shows the average private premia, both using Empirical Bayes and raw value-added estimates, at the pre- and primary school levels. Panel B shows the slope coefficient of private on public value-added, using the bias correction procedure described in Appendix E.2. Panel C shows the corresponding slope coefficients across levels (pre- and primary).

C Measuring socioeconomic status

We construct a household-level socioeconomic status (SES) index based on ownership of a set of assets in the 2022 survey round. Households were asked whether they own a washing machine, refrigerator, grinder, mixer, computer, TV, fan, electric lights, car, tractor, motorbike/scooter, bicycle, and a telephone. Furthermore, we recorded whether the household owns agricultural land and the house in which they reside, as well as whether they have access to running water. These responses are coded as binary variables, and combined into a single index using principal component analysis (PCA): the first eigenvector constitutes our SES index. Finally, this index is transformed into percentiles. Table C.1 reports descriptive statistics of household asset ownership and maternal education by quartiles of our constructed SES index, as well as for the full sample.

Table C.1: Household characteristics by quartiles of the constructed SES index

	SES quartiles				
	First (1)	Second (2)	Third (3)	Fourth (4)	All (5)
Panel A: Household assets					
Internet	0.20	0.21	0.60	0.87	0.46
	(0.40)	(0.41)	(0.49)	(0.34)	(0.50)
Washing machine	0.01	0.01	0.02	0.28	0.07
	(0.09)	(0.09)	(0.15)	(0.45)	(0.26)
Refrigerator	0.09	0.22	0.58	0.91	0.44
	(0.29)	(0.41)	(0.49)	(0.28)	(0.50)
Grinder	0.56	0.97	1.00	1.00	0.88
	(0.50)	(0.16)	(0.07)	(0.03)	(0.33)
Mixer	0.62	0.99	1.00	1.00	0.90
	(0.49)	(0.10)	(0.05)	(0.00)	(0.30)
Computer	0.02	0.03	0.05	0.21	0.07
	(0.14)	(0.18)	(0.22)	(0.41)	(0.26)
TV	0.75	0.97	1.00	1.00	0.93
	(0.43)	(0.16)	(0.07)	(0.03)	(0.26)
Fan	0.89	1.00	1.00	1.00	0.97
	(0.31)	(0.04)	(0.02)	(0.02)	(0.17)
Electric lights	0.90	0.96	0.97	0.98	0.95
	(0.30)	(0.20)	(0.17)	(0.14)	(0.22)
Car	0.01	0.01	0.03	0.14	0.04
	(0.08)	(0.11)	(0.16)	(0.35)	(0.20)
Tractor	0.01	0.01	0.01	0.08	0.03
	(0.10)	(0.11)	(0.12)	(0.27)	(0.16)
Motorbike	0.38	0.71	0.94	0.99	0.75
	(0.49)	(0.45)	(0.24)	(0.12)	(0.43)
Bicycle	0.27	0.39	0.30	0.48	0.36
	(0.44)	(0.49)	(0.46)	(0.50)	(0.48)
Telephone	0.92	0.99	1.00	1.00	0.98
	(0.27)	(0.10)	(0.01)	(0.02)	(0.15)
Owns land	0.16	0.23	0.24	0.43	0.26
	(0.36)	(0.42)	(0.43)	(0.50)	(0.44)
Owns house	0.83	0.86	0.93	0.96	0.89
	(0.38)	(0.35)	(0.25)	(0.19)	(0.31)
Running water	0.16	0.22	0.27	0.48	0.27
	(0.36)	(0.42)	(0.44)	(0.50)	(0.45)
Panel B: Maternal education					
Mother's education: $<$ Grade 9	0.35	0.28	0.20	0.16	0.25
	(0.48)	(0.45)	(0.40)	(0.37)	(0.43)
Mother's education: $\geq$ Grade 12	0.21	0.28	0.36	0.52	0.34
	(0.41)	(0.45)	(0.48)	(0.50)	(0.47)
Observations	5,013	4,781	5,216	4,304	19,314

Notes: This table shows means and standard deviations (in parentheses) of household asset ownership that forms the basis of our SES index (Panel A) and maternal education (Panel B). The sample is split along quartiles of the SES index in Columns 2–5, and Column 6 shows descriptives for the full sample.

D Robustness of the average private premium

D.1 Details on robustness values and bounds

For robustness values and bounds, we follow closely the approach suggested in Cinelli & Hazlett (2019), and implement the procedure using the provided STATA software `sensemakr`.

We provide an outline of this procedure below. Consider a notation where test scores are denoted as Y, private enrolment as D, the set of control variables included as $\mathbf{X}$, and the hypothetical confounder as Z. Under a scenario where Z is equally powerful in predicting Y and D, the robustness value is defined as:

$$R^2_{Y\sim Z|\mathbf{X},D} = R^2_{D\sim Z|\mathbf{X}} = RV_q, \tag{4}$$

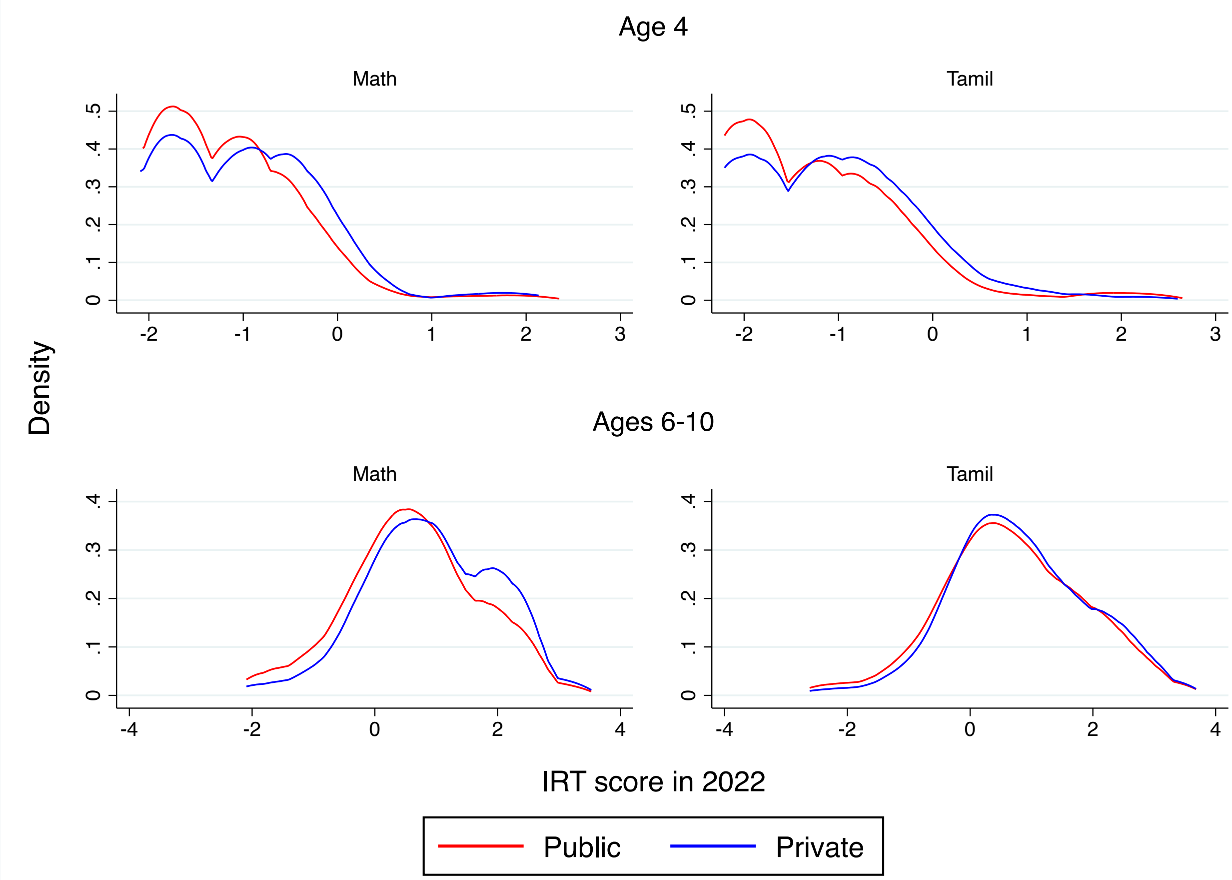
where RV_q is the value of these partial R^2's strong enough to reduce the estimated treatment effect by $100q\%$. In our analysis, we report two robustness values: RV, which sets $q = 1$ and hence asks how strong a predictor Z would have to be to completely nullify the treatment effect of D, and $RV_{\alpha=0.05}$ which asks how much is necessary to render the treatment effect statistically insignificant at the 5% level.

The strength of this procedure is that we can reason about bounds for $R^2_{Y\sim Z|\mathbf{X},D}$ and $R^2_{D\sim Z|\mathbf{X}}$ in a transparent way. We consider a scenario under which the confounder Z is as predictive of the residual variation in Y and D, as a set of benchmarking covariates $\mathbf{B}$. In our analysis, this includes the core covariates of the value-added analysis: baseline test score, deciles of our SES index, parental education, and child gender. Cinelli & Hazlett (2019) shows that under this assumption, we can recover bounds for the relevant partial R^2's and compare them to the robustness values to evaluate how problematic such a confounder Z would be.

We perform this exercise separately for children aged 4 and 6–10, as in our main analysis. Results are shown in Table 3. We find robustness values around 27–31% in math, and 20–24% in Tamil. This means that a problematic confounder would have to explain roughly a quarter of the residual variation in both test scores and private enrolment to nullify the estimated private premia. For the partial R^2's in our baseline specification, we find that (conditional on private enrolment) the benchmarking covariates $\mathbf{B}$ explain a nontrivial share of the variation in private enrolment ($\sim$ 16%), but fairly little of the variation in test scores ($\sim$ 7%). These values fall in the more extensive specifications in columns (2) through (4) — this is because the partial R^2 of the baseline test score and SES controls become less predictive once we control flexibly for baseline test score in the other subject and other proxies for SES, such as caste. Hence, the most informative values are found in column (1). Importantly, these values are well below the robustness values, suggesting that a confounder Z as strong as $\mathbf{B}$ would not nullify our private premia.

D.2 Common support of covariates

Figure D.1: Distributions of baseline test scores



Notes: These figures show the kernel densities of IRT scores in 2022 (baseline) for children enrolling in private and public institutions over the following year, separately by subject and age groups. While children in private institutions are generally positively selected on baseline scores, the distributions broadly overlap.

We estimate the probability of attending private school separately by age using a probit regression. The predictors used are the full set of value-added controls, lagged scores in both subjects, as well as village fixed effects, deciles of the SES index, paternal and maternal education, and child gender. In Tables D.1-D.3, showing the average impact of private enrolment, we restrict the sample to children with an estimated probability of attending private school within 5, 10, and 15 percentage points of the age-specific median. This excludes a significant share of the main analysis sample: one standard deviation of the estimated probability of private enrolment is similar across ages at around 28 percentage points. The results are similar to those in the main analysis.

Table D.1: Private school value-added in preschool and primary school (probit restriction: 5 percentage points)

	Age 4		Age 5		Ages 6–10	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Math						
Private school	0.967***	0.789***	0.291*	0.267*	-0.083	-0.060
	(0.167)	(0.185)	(0.131)	(0.131)	(0.051)	(0.048)
Math IRT score in 2022		0.449**		0.434		0.315***
		(0.147)		(0.234)		(0.023)
Panel B: Tamil						
Private school	1.131***	0.997***	0.005	-0.011	-0.209***	-0.186***
	(0.157)	(0.214)	(0.133)	(0.140)	(0.054)	(0.052)
Tamil IRT score in 2022		0.245		-0.264		0.288***
		(0.133)		(0.169)		(0.024)
Controls	Village FE	All	Village FE	All	Village FE	All
Observations	148	148	272	272	2,007	2,007

Notes: Robust standard errors, clustered at the village level, in parentheses. Columns 1, 3 and 5 show raw means by private school attendance within villages. Columns 2, 4 and 6 include lagged scores, village fixed effects, and controls for deciles of the SES wealth index, paternal and maternal education, and child gender. Test scores refer to equated IRT scores, standardized with respect to children aged 5 in the 2022 assessments. The sample is restricted to children within 5 percentage points of the age-specific median probability of private enrolment. These probabilities are estimated using age-specific probit regression of private enrolment on lagged test scores, SES, parental education, gender, and village fixed effects. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

Table D.2: Private school value-added in preschool and primary school (probit restriction: 10 percentage points)

	Age 4		Age 5		Ages 6–10	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Math						
Private school	0.750***	0.695***	0.196	0.204*	-0.019	-0.018
	(0.111)	(0.107)	(0.101)	(0.097)	(0.033)	(0.032)
Math IRT score in 2022		0.270***		0.299***		0.325***
		(0.075)		(0.088)		(0.017)
Panel B: Tamil						
Private school	0.670***	0.641***	0.058	0.067	-0.193***	-0.183***
	(0.102)	(0.100)	(0.089)	(0.088)	(0.038)	(0.036)
Tamil IRT score in 2022		0.273***		0.050		0.316***
		(0.060)		(0.083)		(0.017)
Controls	Village FE	All	Village FE	All	Village FE	All
Observations	373	373	565	565	4,036	4,036

Notes: Robust standard errors, clustered at the village level, in parentheses. Columns 1, 3 and 5 show raw means by private school attendance within villages. Columns 2, 4 and 6 include lagged scores, village fixed effects, and controls for deciles of the SES wealth index, paternal and maternal education, and child gender. Test scores refer to equated IRT scores, standardized with respect to children aged 5 in the 2022 assessments. The sample is restricted to children within 10 percentage points of the age-specific median probability of private enrolment. These probabilities are estimated using age-specific probit regression of private enrolment on lagged test scores, SES, parental education, gender and village fixed effects. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

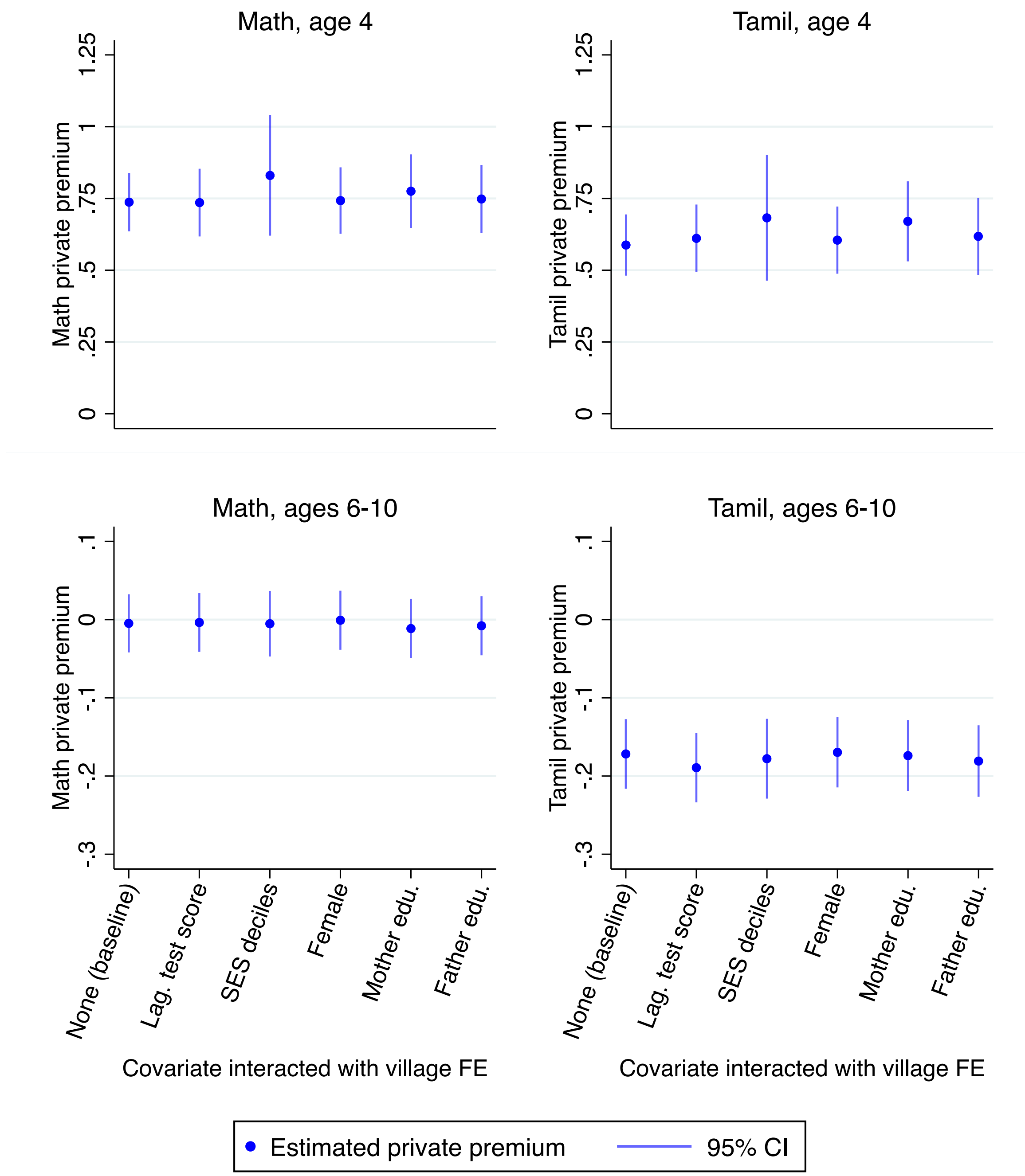
Table D.3: Private school value-added in preschool and primary school (probit restriction: 15 percentage points)

	Age 4		Age 5		Ages 6–10	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Math						
Private school	0.740***	0.709***	0.117	0.123	0.018	0.000
	(0.077)	(0.077)	(0.076)	(0.075)	(0.029)	(0.028)
Math IRT score in 2022		0.197***		0.228***		0.310***
		(0.055)		(0.055)		(0.014)
Panel B: Tamil						
Private school	0.622***	0.610***	-0.034	-0.014	-0.179***	-0.179***
	(0.081)	(0.082)	(0.073)	(0.072)	(0.032)	(0.030)
Tamil IRT score in 2022		0.261***		0.126*		0.316***
		(0.053)		(0.056)		(0.015)
Controls	Village FE	All	Village FE	All	Village FE	All
Observations	607	607	926	926	6,243	6,243

Notes: Robust standard errors, clustered at the village level, in parentheses. Columns 1, 3 and 5 show raw means by private school attendance within villages. Columns 2, 4 and 6 include lagged scores, village fixed effects, and controls for deciles of the SES wealth index, paternal and maternal education, and child gender. Test scores refer to equated IRT scores, standardized with respect to children aged 5 in the 2022 assessments. The sample is restricted to children within 15 percentage points of the age-specific median probability of private enrolment. These probabilities are estimated using age-specific probit regression of private enrolment on lagged test scores, SES, parental education, gender and village fixed effects. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

D.3 Interacting covariates with village indicators

Figure D.2: Robustness of private premium to interacting controls with village FEs at different ages



Notes: These figures show the estimated average private premium under alternative specifications that allow each of the control variables specified along the horizontal axis to vary across villages. In practice, this amounts to interacting the covariate with village-level indicators.

E Empirical Bayes shrinkage and bias-corrected value-added covariances

E.1 Empirical Bayes shrinkage

This section details the construction of the Empirical Bayes estimates used throughout the paper. We follow a simplified version of the approach used by Andrabi et al. (2025). Let

$$y_{islv} = \theta_{slv} + \mathbf{\Gamma} \mathbf{X}_{islv} + \epsilon_{islv} \tag{5}$$

where y_{islv} is the test score of child i in the private/public sector s and pre-school/primary level l, in village v. θ_{slv} is the village-level average value-added in a given sector, at a given level. $\mathbf{X}_{islv}$ is a vector of controls (lagged test scores, SES index deciles, maternal and paternal education, gender, and age), and ϵ_{islv} is an idiosyncratic error term. The variance of value-added, denoted σ^2_{sl}, is common across villages but allowed to differ between sectors. The variance of the error term is denoted by σ^2_{ϵ}. Both are assumed to be independent and homoskedastic. We denote the number of children in a given village-level-sector cell as N_{slv}.

Our estimate of θ_{slv} (i.e., the village-level-sector fixed effect) is

$$\hat{\theta}_{slv} = \theta_{slv} + \frac{1}{N_{slv}} \sum_{i \in slv} \epsilon_{islv} \tag{6}$$

The variance of this estimate is equal to

$$\begin{aligned} Var(\hat{\theta}_{slv}) &= E\Big[\Big(\theta_{slv} + \frac{1}{N_{slv}} \sum_{i \in slv} \epsilon_{islv}\Big)^2\Big] && (7) \\ &= E(\theta^2_{slv}) + E\Big(\frac{1}{N^2_{slv}} \sum_{i \in slv} \epsilon^2_{islv}\Big) && (8) \\ &= \sigma^2_{sl} + E\Big(\frac{1}{N_{slv}} \sigma^2_{\epsilon}\Big) && (9) \end{aligned}$$

The second equality follows from the assumption that ϵ_{islv} is independent and identically distributed at the child level. Rearranging terms, the variance of value-added purged of estimation error is equal to

$$\sigma^2_{sl} = Var(\hat{\theta}_{slv}) - E\Big(\frac{1}{N_{slv}} \sigma^2_{\epsilon}\Big) \tag{10}$$

We can obtain an estimator of the left-hand side by plugging in moment estimators on the right-hand side. $Var(\hat{\theta}_{slv})$ is estimated as the sample variance of the fixed effects.[32] The variance of the error term, σ^2_{ϵ}, is estimated using residuals from Equation (5). An estimate of σ^2_{sl} in Equation (10) is obtained by taking the average of the right-hand side.

[32] V denotes the number of villages: $Var(\hat{\theta}_{slv}) = \frac{1}{V} \sum_{v=1}^{V} (\hat{\theta}_{slv} - \hat{\mu}_{sl})^2$, where $\hat{\mu}_{sl}$ is equal to $\frac{1}{V} \sum_{v=1}^{V} \hat{\theta}_{slv}$.

Given a standard hierarchical model with normal priors, the Empirical Bayes scaling term is then given by

$$h_{slv} = \frac{\sigma_{sl}^2}{\sigma_{sl}^2 + \frac{1}{N_{slv}}\sigma_\epsilon^2} \tag{11}$$

We shrink each fixed effect toward its level-sector mean $\hat{\mu}_{sl} = \frac{1}{V}\sum_{v=1}^{V}\hat{\theta}_{slv}$, where V is the number of villages. The Empirical Bayes estimate of average value-added in a village-level-sector cell is therefore given by:

$$\hat{\theta}_{slv}^{EB} = h_{slv} \cdot \hat{\theta}_{slv} + (1 - h_{slv}) \cdot \hat{\mu}_{sl} \tag{12}$$

Intuitively, as the sample size of a given cell (N_{slv}) approaches infinity, h_{slv} tends to 1 such that the Empirical Bayes estimate is simply equal to the fixed effect. At the other extreme, the Empirical Bayes estimate shrinks the coefficient completely to the level-sector mean $\hat{\mu}_{sl}$.

E.2 Bias-corrected estimation of $\boldsymbol{\Sigma}_\theta$

Let θ_{sljv} be the estimate of value-added in sector s, level l, subject j and village v. To simplify notation, let k denote the combinations of levels, sectors and subjects, so that:

$$k = \begin{cases} 1 & \text{public, preschool, math} \\ 2 & \text{private, preschool, math} \\ 3 & \text{public, primary school, math} \\ 4 & \text{private, primary school, math} \\ ... & \end{cases}$$

and so on for Tamil. Hence, θ_{1v} is the math test score gains in public preschools in village v. Denote the 8×8 covariance matrix of these value-added estimates across sectors, levels and subjects as $\boldsymbol{\Sigma}_\theta$, where element k, m is given by:

$$\sigma_{km} = Cov(\theta_{kv}, \theta_{mv}). \tag{13}$$

Sample moments of our estimated value-added parameters will not, in general, provide unbiased estimators of $\boldsymbol{\Sigma}_\theta$ due to excess estimation noise. Instead, we closely follow the approach of Angrist et al. (2025) in estimating this covariance matrix. We can write the covariance in Equation (13) as quadratic forms of the underlying (true) value-added parameters:

$$\sigma_{km} = \boldsymbol{\theta}' \mathbf{A}_{km} \boldsymbol{\theta}, \tag{14}$$

where $\boldsymbol{\theta} = (\theta_{11}, \theta_{12}, ..., \theta_{1V}, \theta_{21}, ..., \theta_{8V})'$ is a $8V \times 1$ vector of value-added parameters: eight for each of a total of V villages. $\mathbf{A}_{km}$ is a $8V \times 8V$ matrix of $V \times V$ blocks of zeros, except for the (k, m)th block which is a centering matrix: $(V-1)^{-1}(\mathbf{I}_V - V^{-1} l_V l_V')$. Here, $\mathbf{I}_V$ is the $V \times V$ identity matrix, and l_V is a column vector of 1's with length V.

Even if our estimates $\hat{\boldsymbol{\theta}}$ are unbiased (i.e., $E[\hat{\boldsymbol{\theta}}] = \boldsymbol{\theta}$), each of its elements contains some degree of noise. Let the sampling variance matrix of $\boldsymbol{\theta}$ be denoted by $8V \times 8V$ matrix $\mathbf{V} = E[(\hat{\boldsymbol{\theta}} - \boldsymbol{\theta})(\hat{\boldsymbol{\theta}} - \boldsymbol{\theta})]$. As shown in Walters (2024), the bias in sample analogues of σ_{km} can be written as:

$$E[\hat{\boldsymbol{\theta}}'\mathbf{A}_{km}\boldsymbol{\theta}] = \boldsymbol{\theta}'\mathbf{A}_{km}\boldsymbol{\theta} + tr(\mathbf{A}_{km}\mathbf{V}). \tag{15}$$

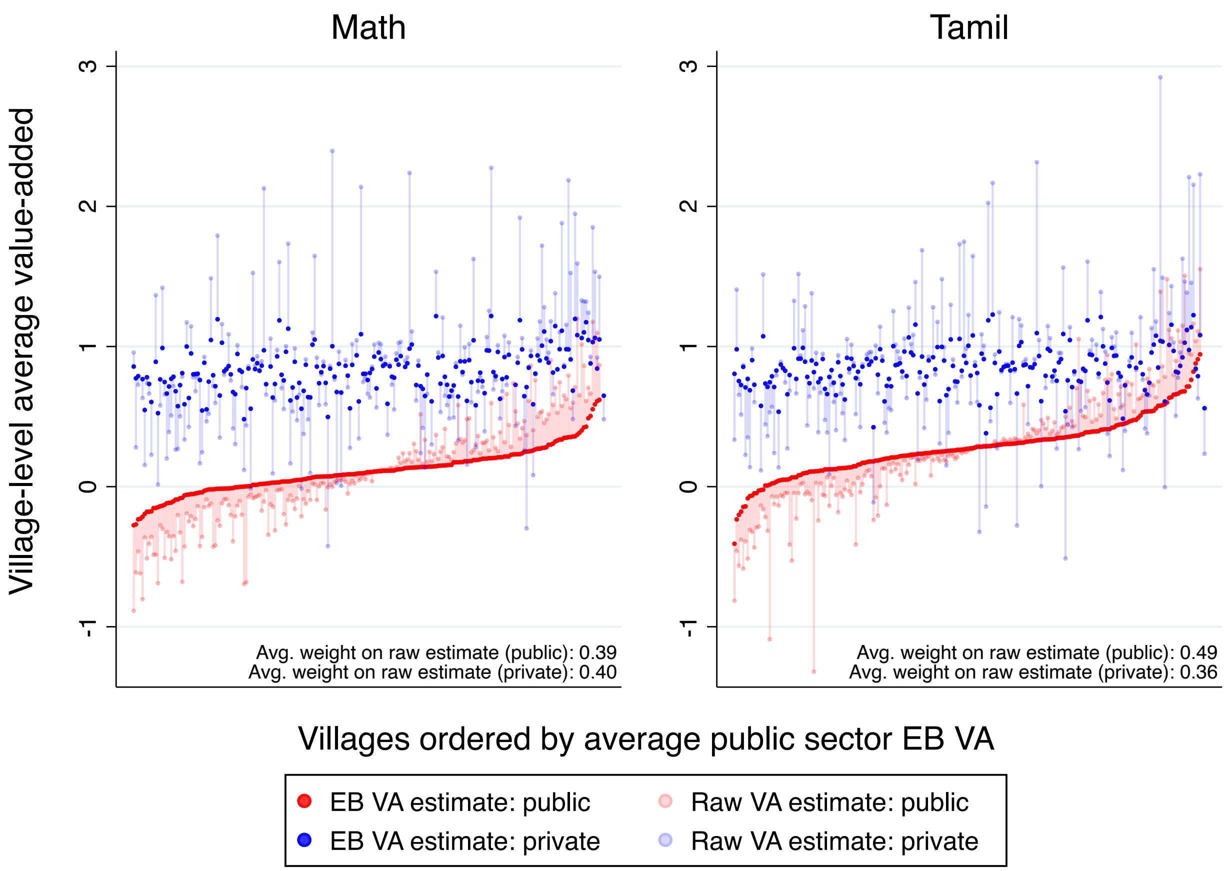
Hence, estimation error gives rise to an additional term on the left-hand side. The formula for bias-corrected estimates of σ_{km} subtracts this bias term directly:

$$\hat{\sigma}_{km} = \hat{\boldsymbol{\theta}}'\mathbf{A}_{km}\hat{\boldsymbol{\theta}} - tr(\mathbf{A}_{km}\hat{\mathbf{V}})$$

where $\hat{\mathbf{V}}$ is the heteroskedasticity-robust covariance matrix obtained from estimation of $\hat{\boldsymbol{\theta}}$ in seemingly unrelated regressions, stacking regressions on each of the outcomes on level-sector-village indicators and controls. A bias-corrected estimate of the slope of a regression of $\hat{\theta}_{kv}$ on $\hat{\theta}_{mv}$ is simply given by $\hat{\gamma}_{km} = \hat{\sigma}_{km}/\hat{\sigma}_{mm}$, i.e., the ratio of the bias-corrected covariance and the bias-corrected variance of the "left-hand side" variable. For instance, $\hat{\gamma}_{21}$ would denote the slope of a regression of private on public preschool math value-added across villages.

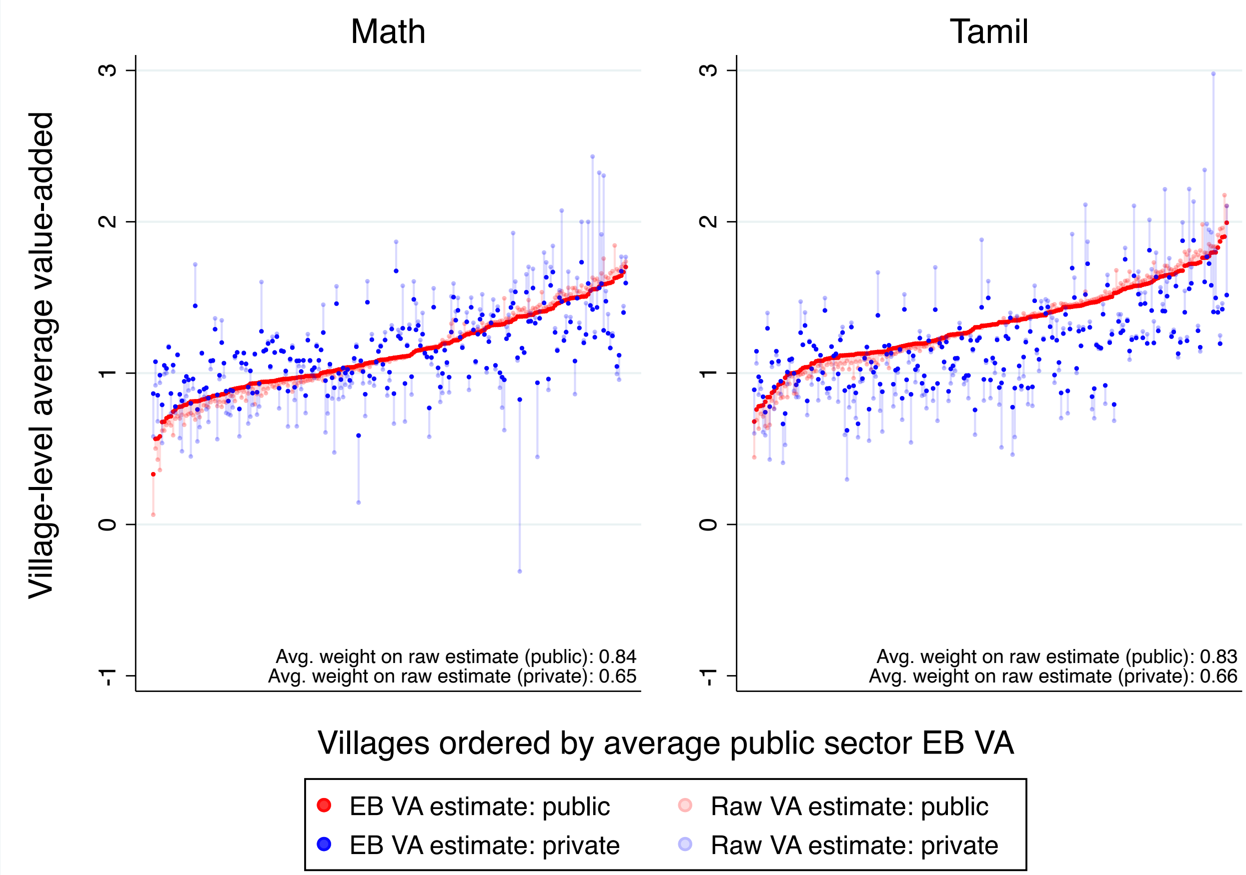
E.3 Impact of Empirical Bayes shrinkage

Figure E.1: Raw and shrunken village value-added estimates: preschool



Notes: These figures show Empirical Bayes estimates (solid colors) and raw value-added estimates (faded colors) separately for public and private preschools. Villages are ordered, along the horizontal axis, by their Empirical Bayes estimates of public sector productivity.

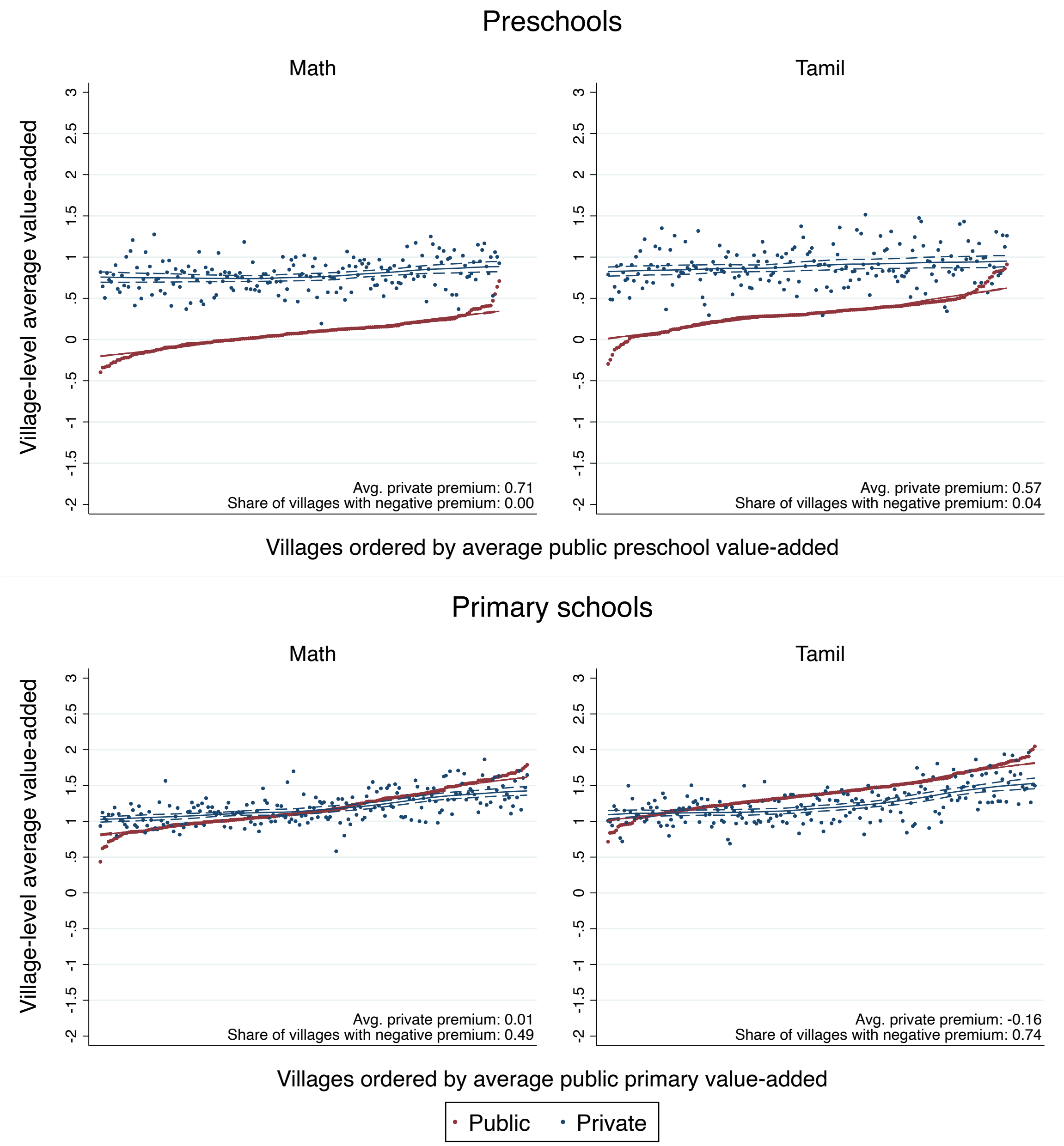
Figure E.2: Raw and shrunken village value-added estimates: primary school



Notes: These figures show Empirical Bayes estimates (solid colors) and raw value-added estimates (faded colors) separately for public and private primary schools. Villages are ordered, along the horizontal axis, by their Empirical Bayes estimates of public sector productivity.

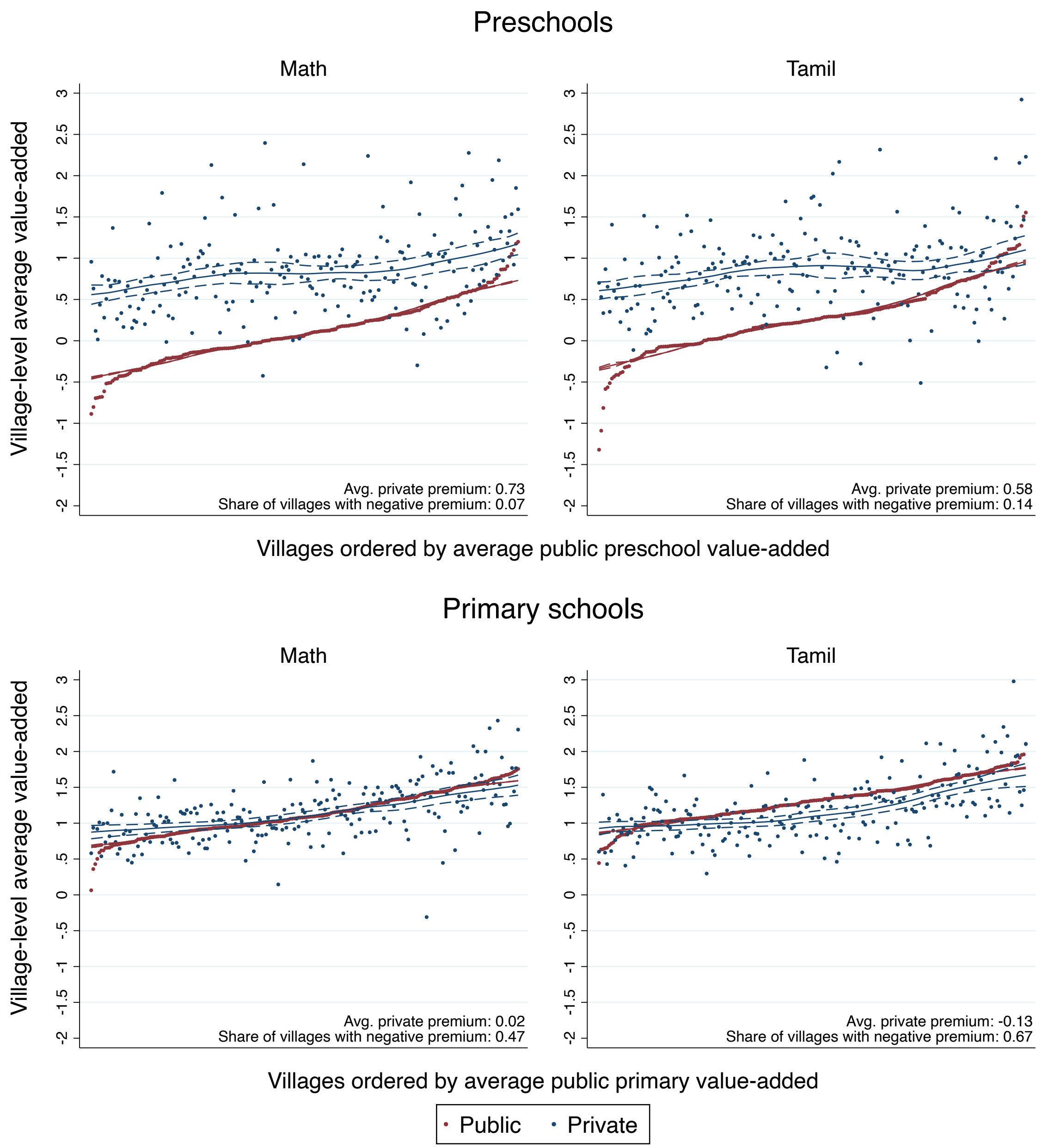
E.4 Village private premia without Empirical Bayes, 5-year-olds, and both

Figure E.3: Village-level average value-added: without 5-year-olds



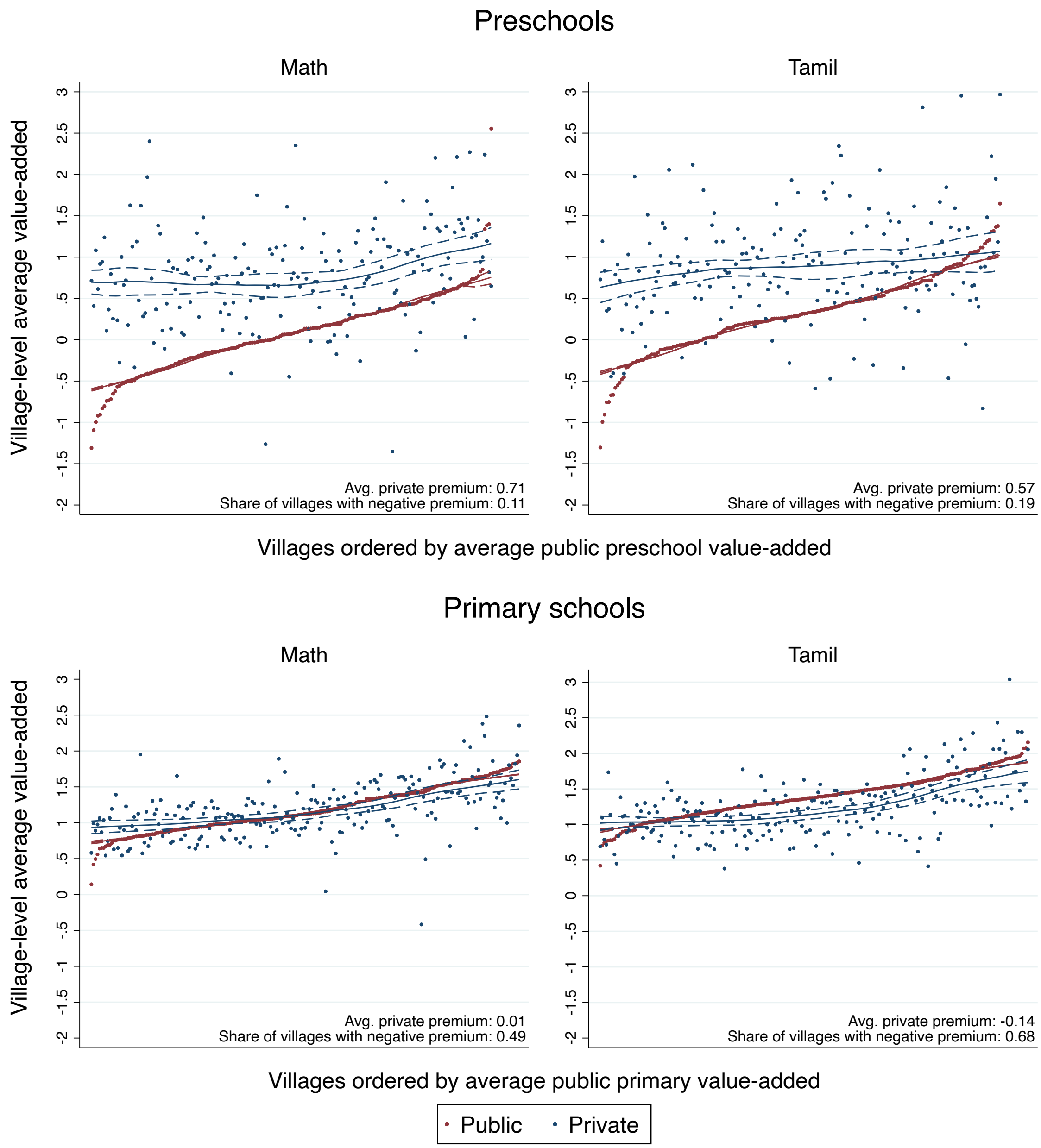
Notes: These figures show village-level average school value-added by sector (public/private) and level (preschool/primary) using Empirical Bayes estimates, excluding children of age 5. Villages are ordered along the x-axis by their average value-added in government schools. The regression specification generating these estimates is given by Equation 2.

Figure E.4: Village-level average value-added: without Empirical Bayes



Notes: These figures show village-level average school value-added by sector (public/private) and level (preschool/primary) using raw value-added estimates (rather than Empirical Bayes estimates). Villages are ordered along the x-axis by their average value-added in government schools. The regression specification generating these estimates is given by Equation 2.

Figure E.5: Village-level average value-added: without Empirical Bayes and 5-year-olds



Notes: These figures show village-level average school value-added by sector (public/private) and level (preschool/primary) using raw value-added estimates (rather than Empirical Bayes estimates), excluding children of age 5. Villages are ordered along the x-axis by their average value-added in government schools. The regression specification generating these estimates is given by Equation 2.

E.5 Precision dependence and Empirical Bayes shrinkage

This procedure may be problematic in settings where effect sizes (i.e., value-added) are correlated with precision. In our context, such correlation could arise if villages with higher private-sector value-added also have greater private school enrolment, reflecting parental demand for test score gains. In that case, lower value-added estimates would tend to come from villages with fewer children enrolled in private options, resulting in less precise estimates that are disproportionately shrunk toward the sectoral mean. This would be problematic for our investigation of the relationship between private premia and village-level market shares of the private sector.

We address this by adopting an alternative Empirical Bayes shrinkage approach that allows for precision dependence (Chen 2024; Walters 2024). This approach models value-added in a particular sector and pre-/primary school level — for simplicity denoted as θ_v — as a non-linear function of its standard error s_v, plus a constant:

$$\theta_v = \psi_0 + \psi_1 \log s_v + s_v^{\psi_2} r_v,$$

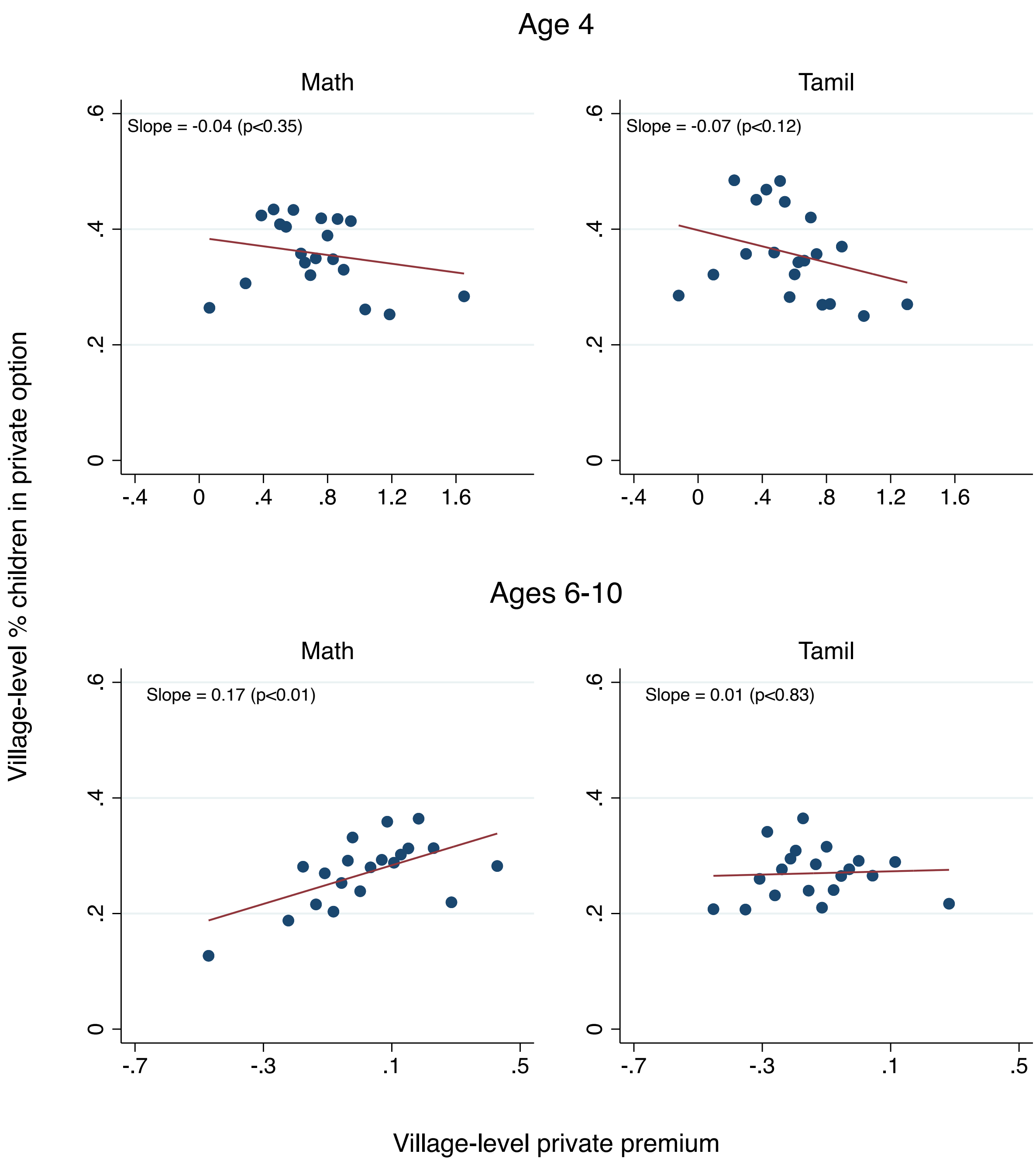
where $r_v | s_v$ is drawn from a normal distribution with mean zero and variance σ_r^2. It can be shown that the Empirical Bayes posterior prediction of θ_v can be written as:

$$\theta_v^{EB} = \hat{\psi}_0 + \hat{\psi}_1 \log s_v + s_v^{\hat{\psi}_2} \hat{r}_v^*,$$

where $\hat{r}_v^* = [\hat{\sigma}_r^2 / (\hat{\sigma}_r^2 + s_v^{2(1-\hat{\psi}_2)})] \cdot [s_v^{\hat{\psi}_2} / (\hat{\theta}_v - \hat{\psi}_0 - \hat{\psi}_1 \log s_v)]$. The estimates $\hat{\psi}_0$ and $\hat{\psi}_1$ are coefficients from a regression of value-added $\hat{\theta}_v$ on $\log s_v$ plus a constant. Estimates $\hat{\psi}_2$ and $\hat{\sigma}_r^2$ come from a nonlinear least squares regression of $[(\hat{\theta}_v - \hat{\psi}_0 - \hat{\psi}_1 \log s_v)^2 - s_v^2]$ on $s_v^{2\psi_2} \sigma_r^2$.

Figure E.6 shows the correlation between private sector market shares and the private premium across villages. In practice, the alternative Empirical Bayes approach gives similar results as in the main analysis.

Figure E.6: Village-level private premium and share of private enrolment under precision-dependent Empirical Bayes shrinkage

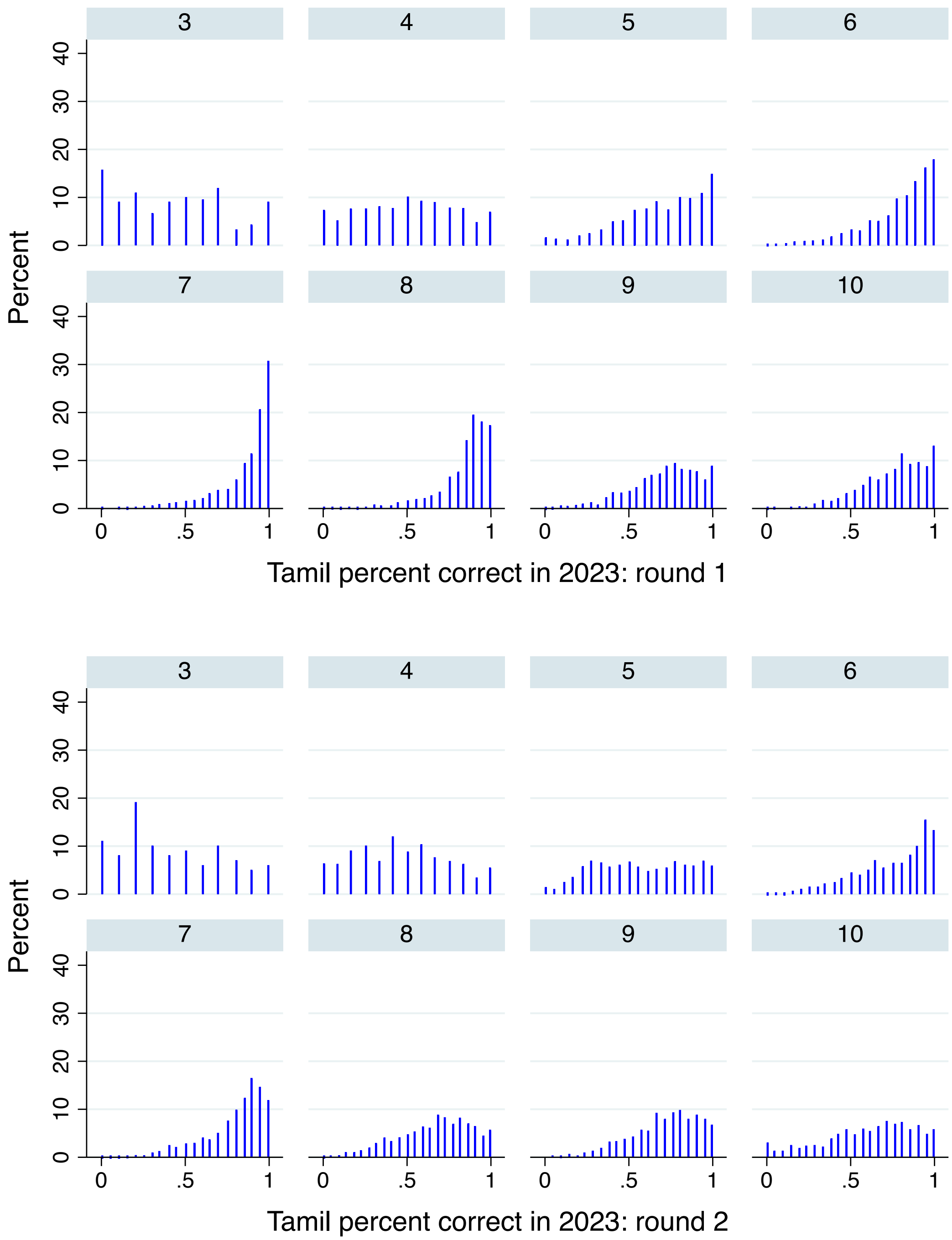


Notes: These figures depict the correlation between (1) the village-level share of children in a private school and (2) the difference between the average *private* and *public* school value-added in the same village (i.e., the private premium). These value-added measures are Empirical Bayes estimates allowing for value-added estimates to be correlated with the size of their standard errors, estimated as described in Appendix E.5.

F Main value-added results using round 2 data in 2023

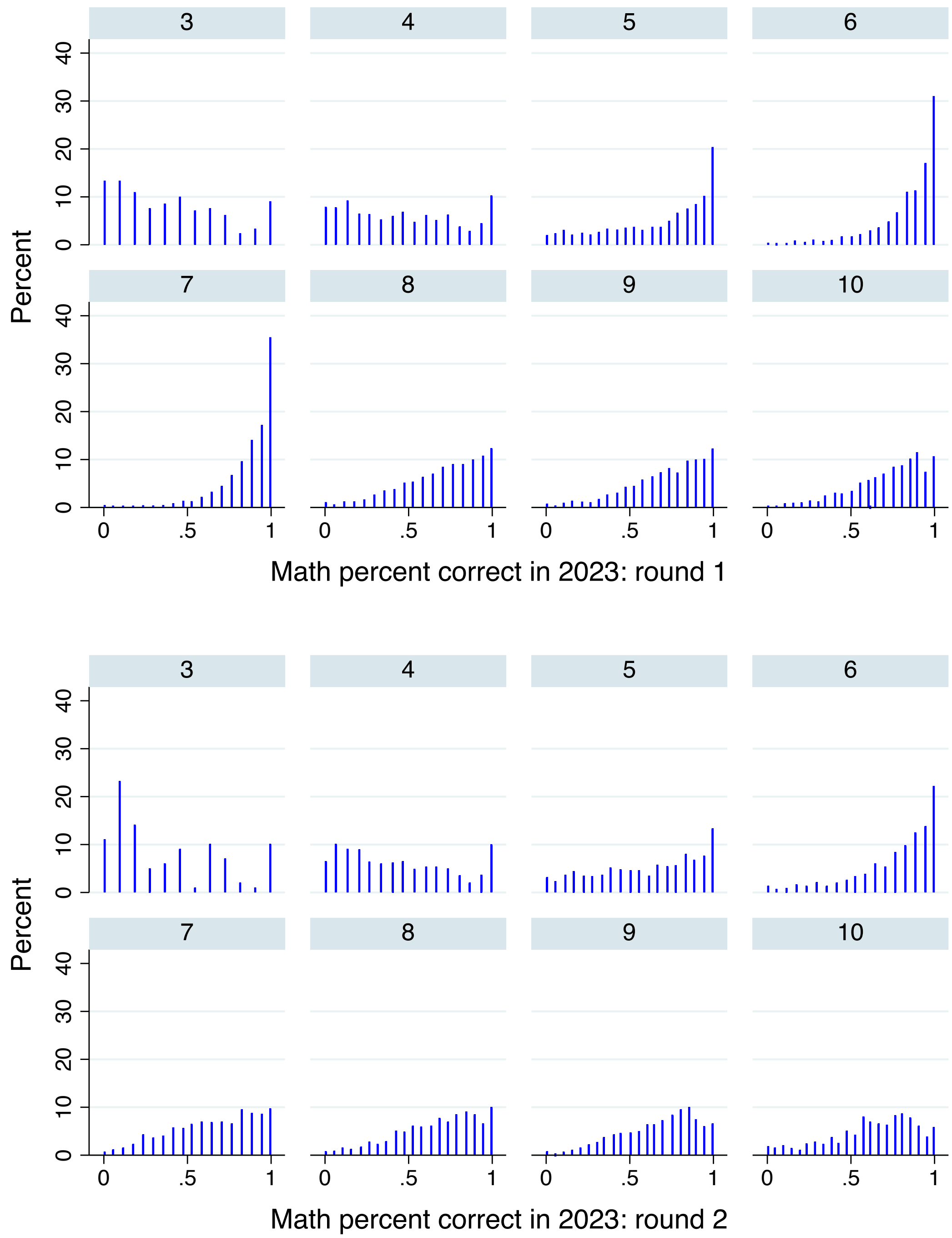
F.1 Test score distributions in 2023 by round

Figure F.1: Histograms of percentage correctly answered Tamil items by age



Notes: These figures show histograms of percent correctly answered Tamil items, separately by age, in the two assessment rounds of 2023. Each bin captures the density by steps of 1 percentage point.

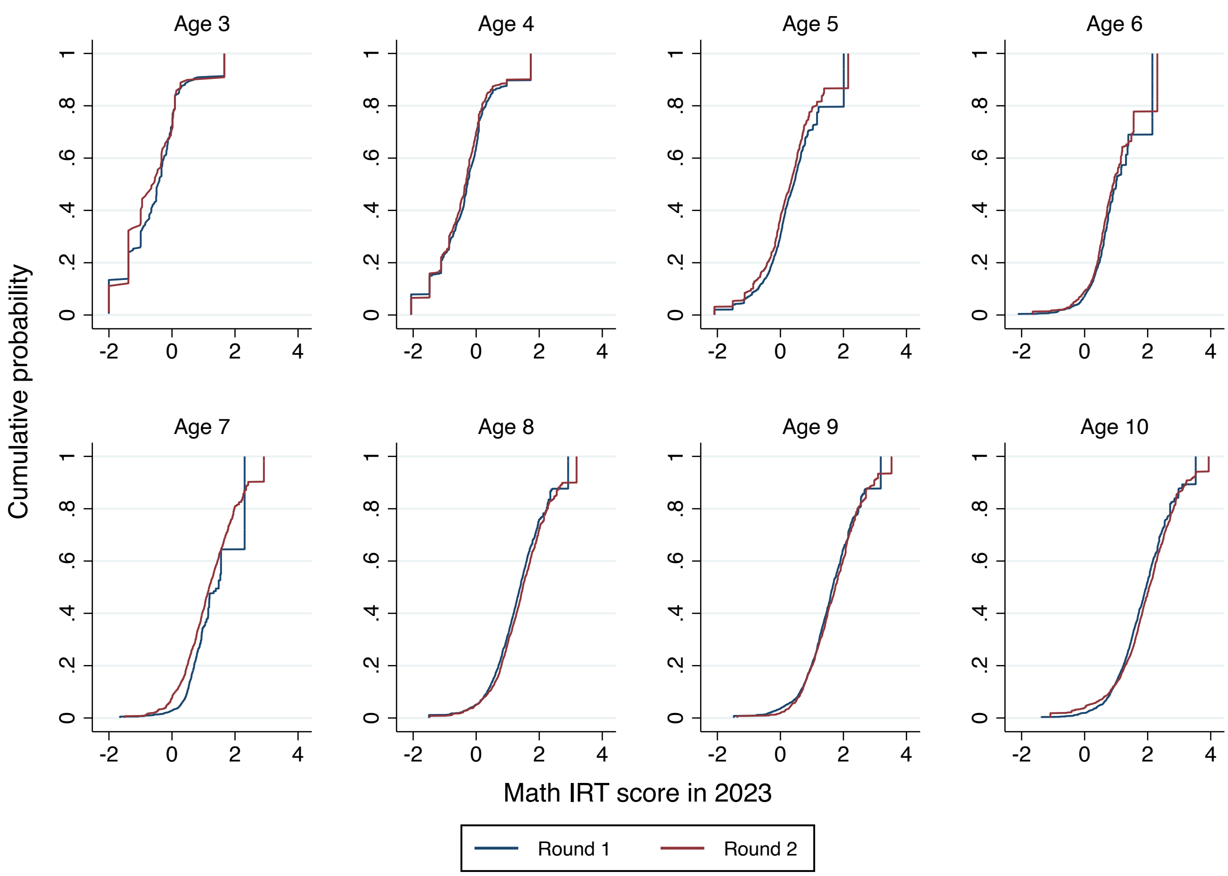
Figure F.2: Histograms of percentage correctly answered math items by age



Notes: These figures show histograms of percent correctly answered math items, separately by age, in the two assessment rounds of 2023. Each bin captures the density by steps of 1 percentage point.

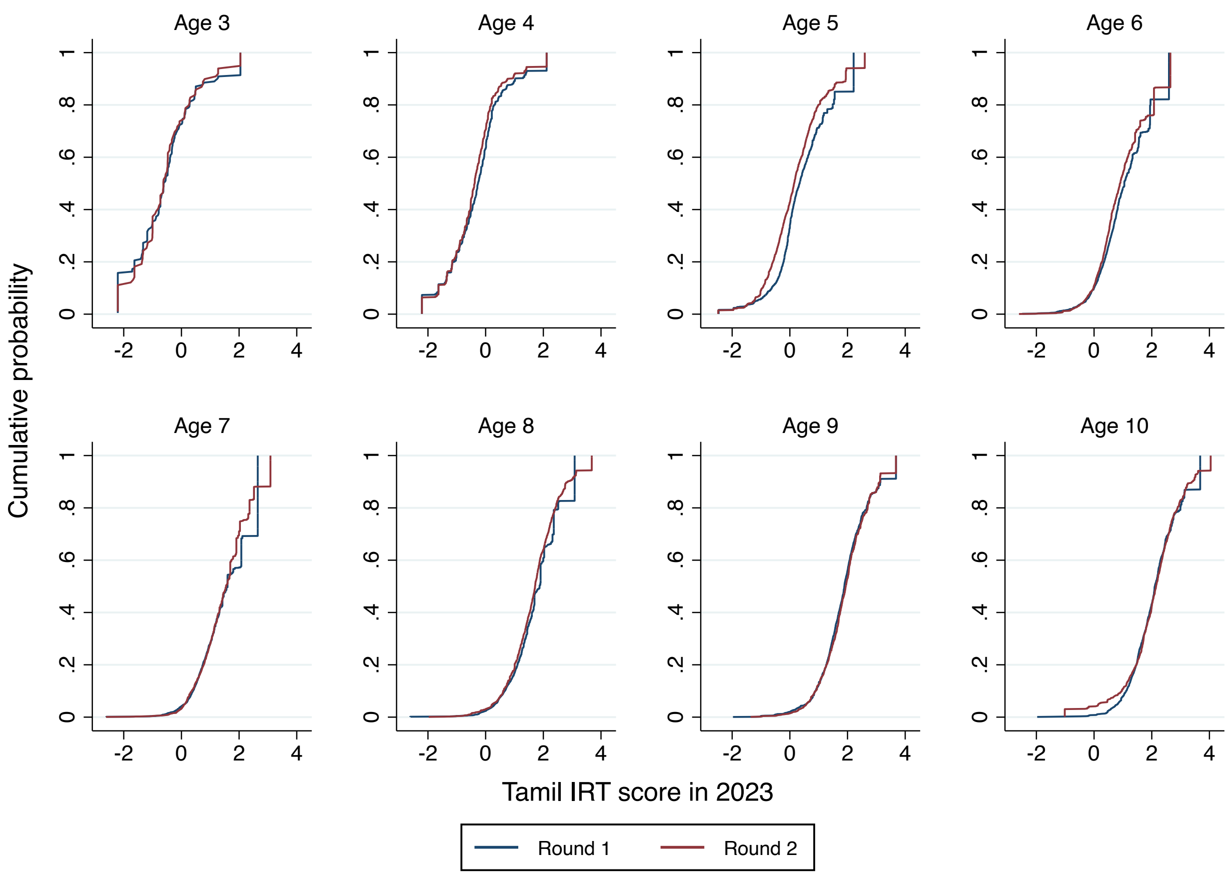
F.2 IRT score distributions in 2023 by age and round

Figure F.3: CDFs of math IRT scores in 2023 by age and round



Notes: These figures show cumulative distribution functions (CDFs) of IRT EAP scores in math in 2023, by child age and assessment round.

Figure F.4: CDFs of Tamil IRT scores in 2023 by age and round



Notes: These figures show cumulative distribution functions (CDFs) of IRT EAP scores in Tamil in 2023, by child age and assessment round.

F.3 The private premium using only round 2 assessments in 2023

Table F.1: Private school value-added in preschool and primary school, round 2 assessments in 2023

	Age 4		Age 5		Ages 6–10	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Math						
Private school	0.873***	0.817***	0.395***	0.254**	0.094**	0.003
	(0.082)	(0.087)	(0.071)	(0.076)	(0.034)	(0.031)
Math IRT score in 2022		0.142***		0.213***		0.313***
		(0.040)		(0.038)		(0.015)
Constant	-0.619***	-0.443***	0.218***	0.414***	1.440***	1.194***
	(0.028)	(0.052)	(0.027)	(0.043)	(0.009)	(0.014)
Panel B: Tamil						
Private school	0.742***	0.690***	0.230***	0.096	-0.098**	-0.145***
	(0.089)	(0.088)	(0.066)	(0.076)	(0.034)	(0.030)
Tamil IRT score in 2022		0.163***		0.198***		0.321***
		(0.035)		(0.037)		(0.015)
Constant	-0.608***	-0.398***	0.189***	0.382***	1.659***	1.401***
	(0.030)	(0.051)	(0.025)	(0.044)	(0.009)	(0.014)
Controls	Village FE	All	Village FE	All	Village FE	All
Observations	748	748	1,114	1,114	5,528	5,528

Notes: Robust standard errors, clustered at the village level, in parentheses. Columns 1, 3, and 5 show raw means by private school attendance within villages. Columns 2, 4, and 6 include lagged scores, village fixed effects, and controls for deciles of the SES wealth index, paternal and maternal education, and child gender. Test scores refer to equated IRT scores, standardized with respect to children aged 5 in the 2022 assessments. The sample is restricted to children assessed in the second round of 2023. $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *.

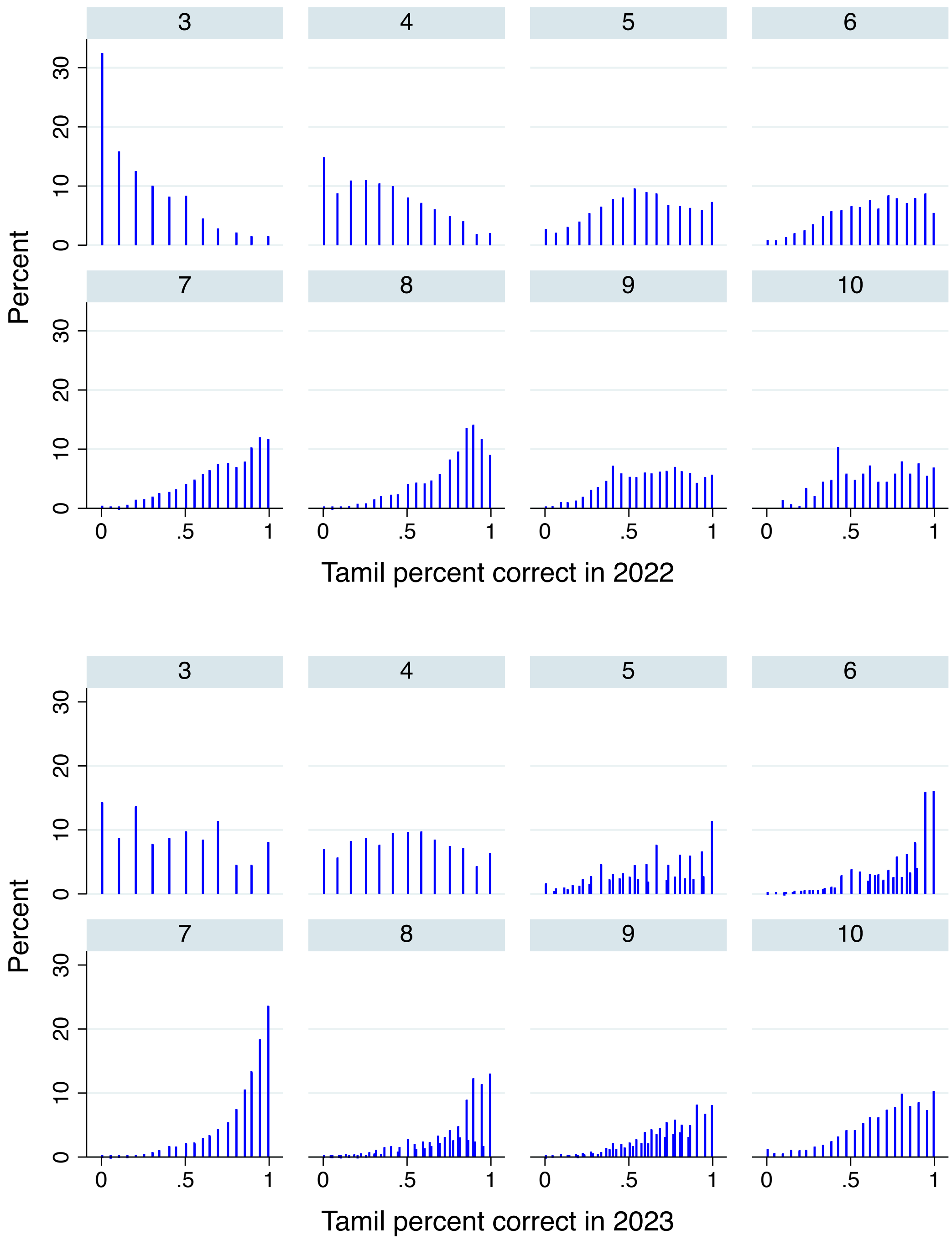
G Details on assessment

Since our data are similar to those used by Singh et al. (2024), details on test content and vertical linking across rounds and ages can be found in the Online Appendix of that paper. A key difference between the data sets used in Singh et al. (2024) and in this paper is that we have an additional round of data collection. Moreover, unlike Singh et al. (2024), we do not use any pre-pandemic data, focusing exclusively on the period after schools reopened.

We provide an overview of the basic details of the test here. Tests were individually administered in Tamil by enumerators during household visits. All tests in both rounds demonstrated strong internal consistency, with Cronbach's alpha exceeding 0.85 in most cases, except for Math and Tamil for 3-year-olds in 2022, which had Cronbach's alpha values of 0.8263 and 0.8070, respectively.

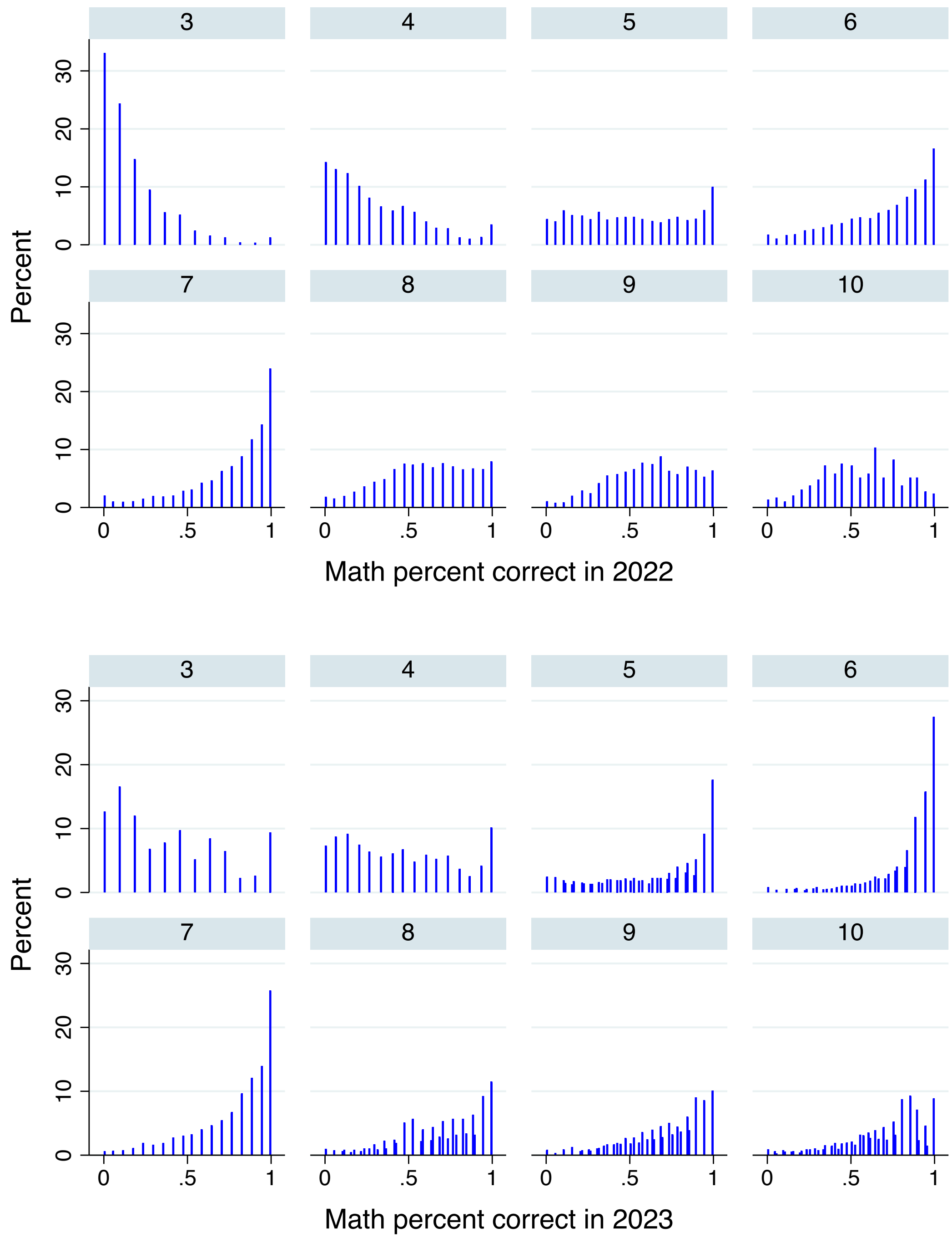
Figures G.1-G.2 show the distribution of correct items by age and year for each subject. While there are some ceiling and floor effects, as mentioned in the main text, the order in which students were tested each year was randomized. In 2023, during the second round of testing, we adapted the tests to prevent ceiling and floor effects (see Appendix F).

Figure G.1: Histograms of percentage correctly answered Tamil items by age



Notes: These figures show histograms of percent correctly answered Tamil items, separately by age, in the assessment waves of 2023 (top) and 2022 (bottom). Each bin captures the density by steps of 1 percentage point.

Figure G.2: Histograms of percentage correctly answered math items by age



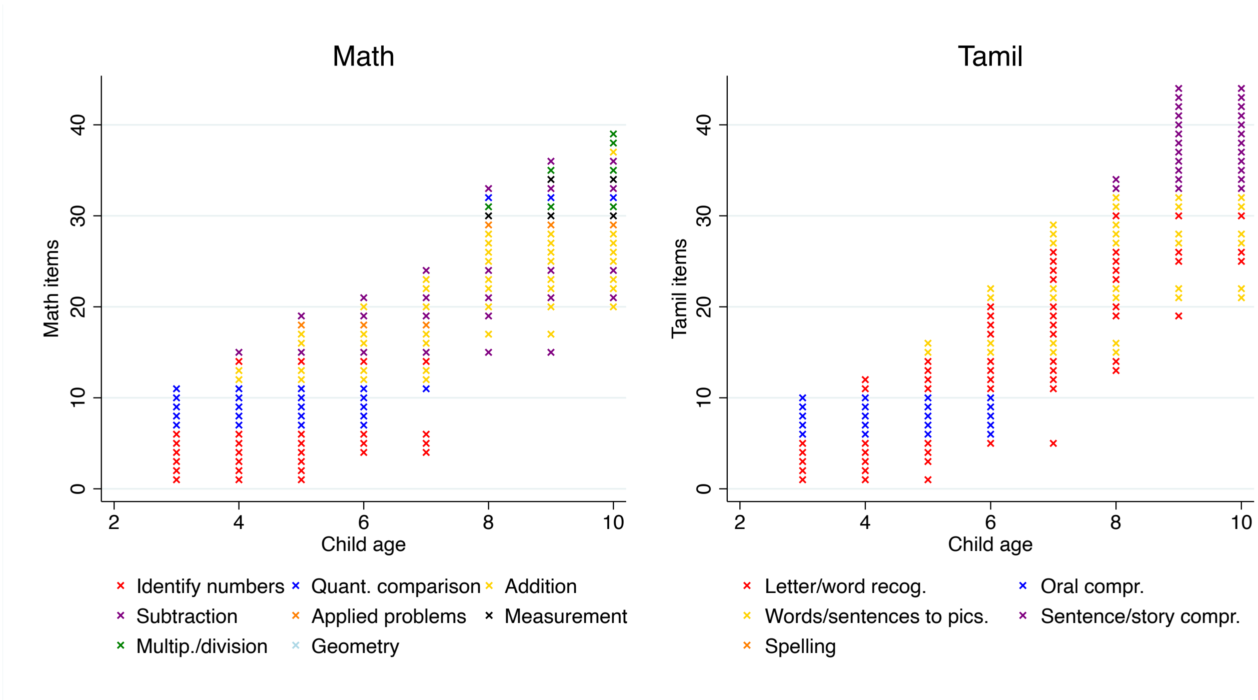
Notes: These figures show histograms of percent correctly answered math items, separately by age, in the assessment waves of 2022 (top) and 2023 (bottom). Each bin captures the density by steps of 1 percentage point.

As a summary measure of the competencies tested and the overlap across ages and rounds, Figure G.3 displays the items asked for each age across years and the competency to which they belong. We order the items by the younger age group to which they are administered.

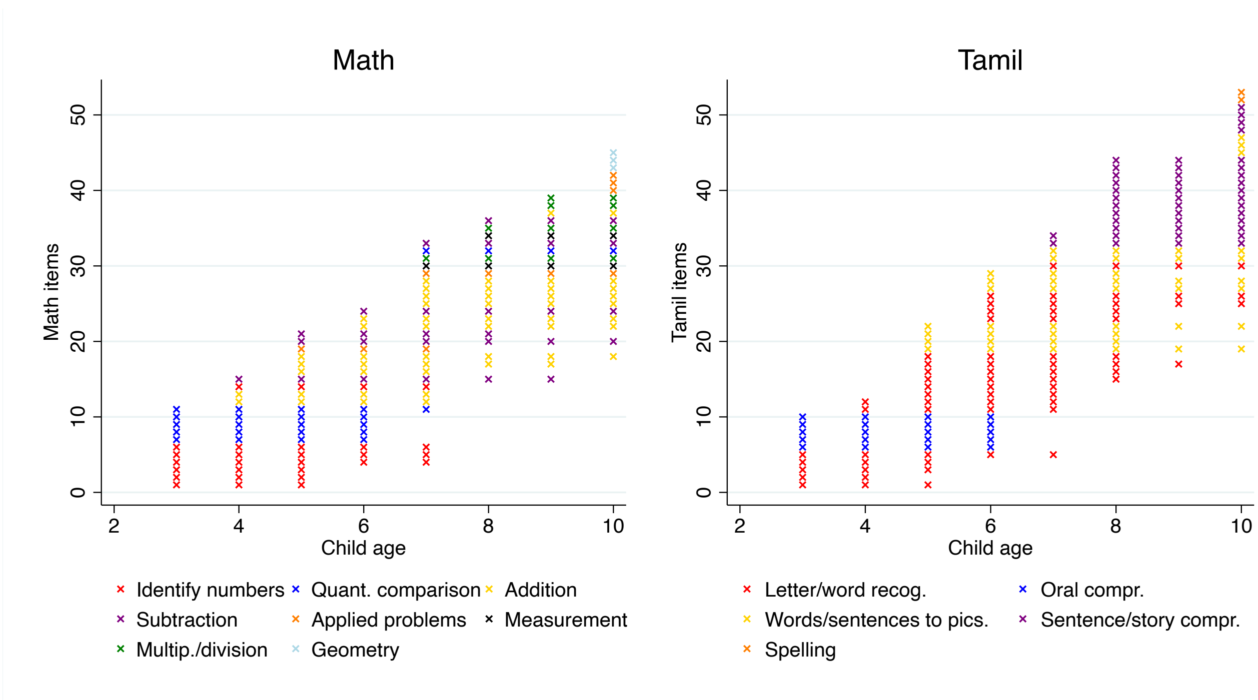
There is a high degree of overlap across years and ages to ensure the vertical linking is robust. Figure G.4 shows the items by age and year of inclusion in the assessments. Most items were included in both years; however, some were added in 2023 to mitigate floor and ceiling effects, as discussed above.

Figure G.3: Assessment item maps

(a) 2022

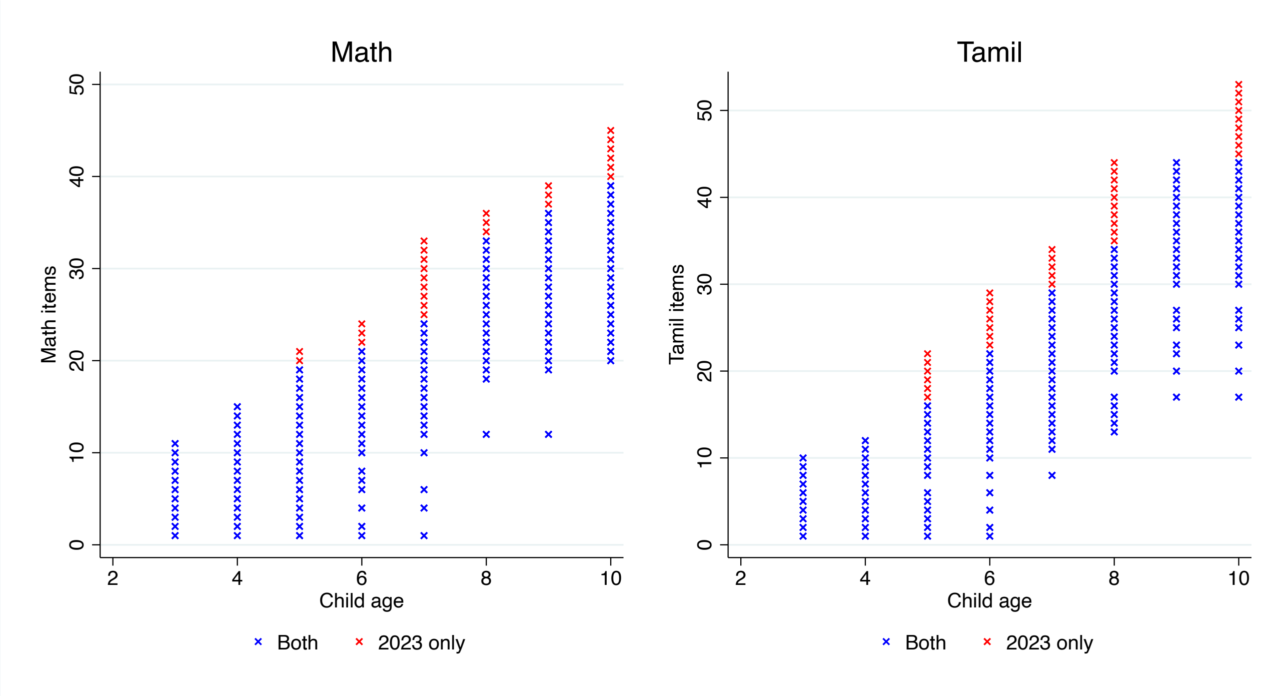


(b) 2023



Notes: These figures show the items administered to children across ages, by competency of the item. The first panel displays the assessment from 2022, while the second panel shows the same assessment from 2023.

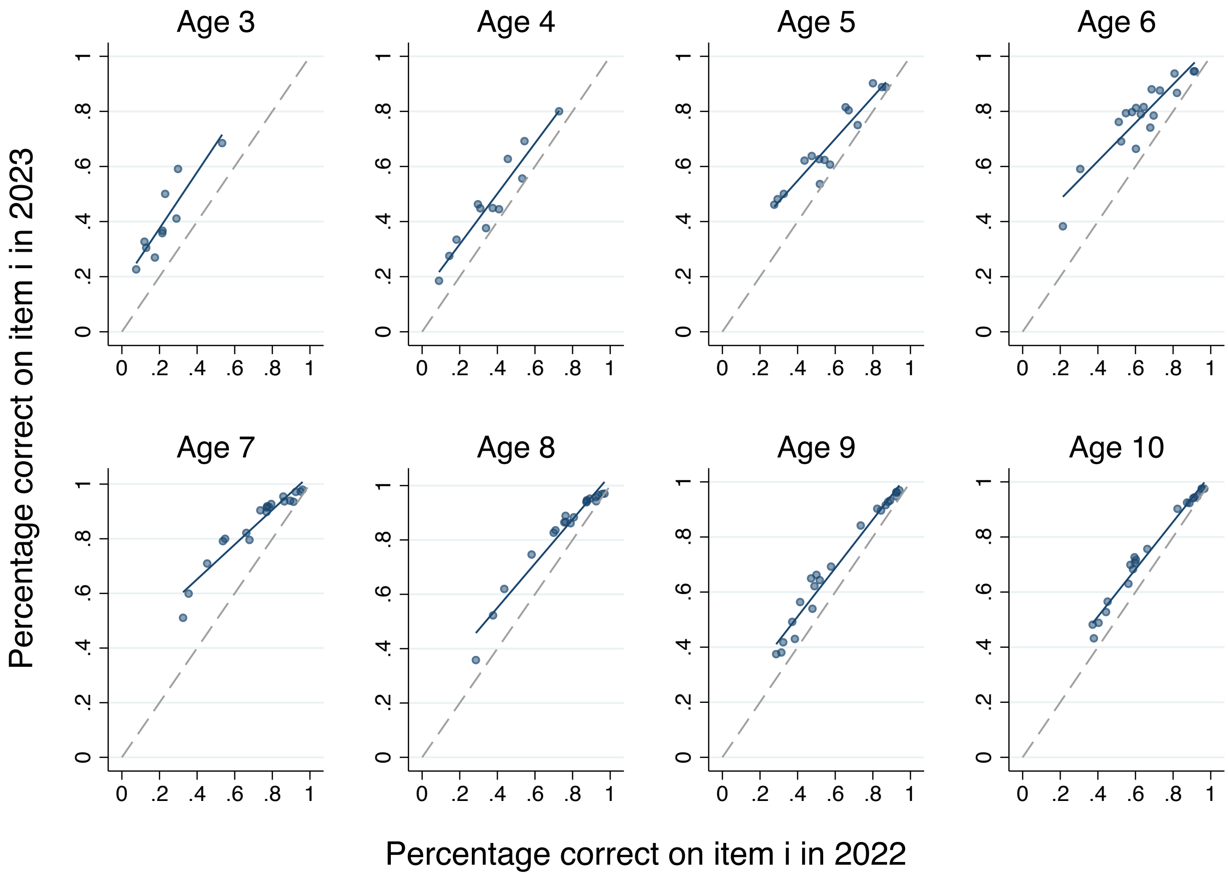
Figure G.4: Assessment items by year



Notes: These figures show the items administered to children across ages, by year of inclusion in the assessments.

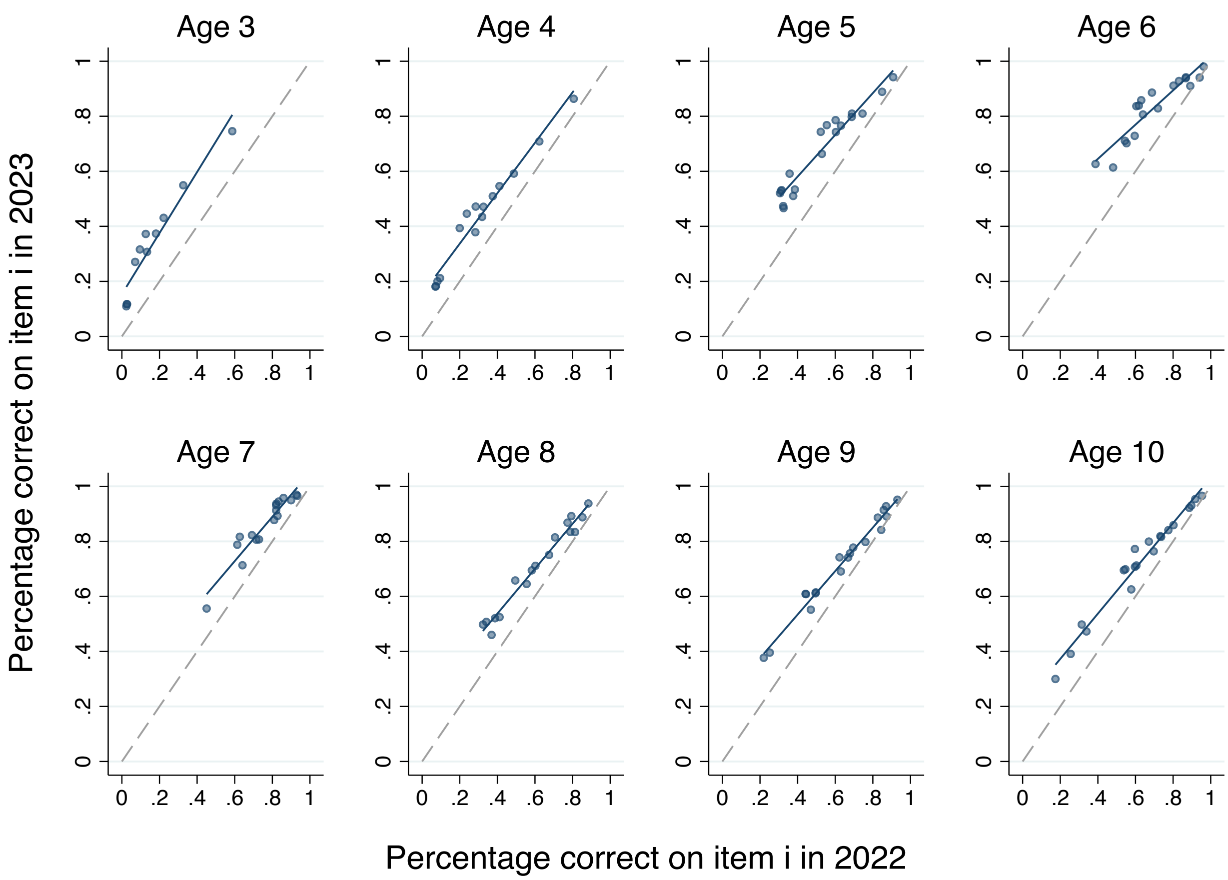
Reassuringly, we find a strong correlation in the proportion of students answering common items correctly across rounds within each age group, indicating stable item-level response patterns (see Figures G.5-G.6). However, performance in 2023 is consistently higher, shifting results vertically above the 45-degree line. This upward shift likely reflects either general improvements in educational outcomes over time.

Figure G.5: Percentage correct on Tamil items across years by age



Notes: These figures show scatter plots of percentage correct on specific Tamil test items, separately by age, in 2022 and 2023. Each dot in the figure corresponds to one item, administered in both rounds.

Figure G.6: Percentage correct on math items across years by age

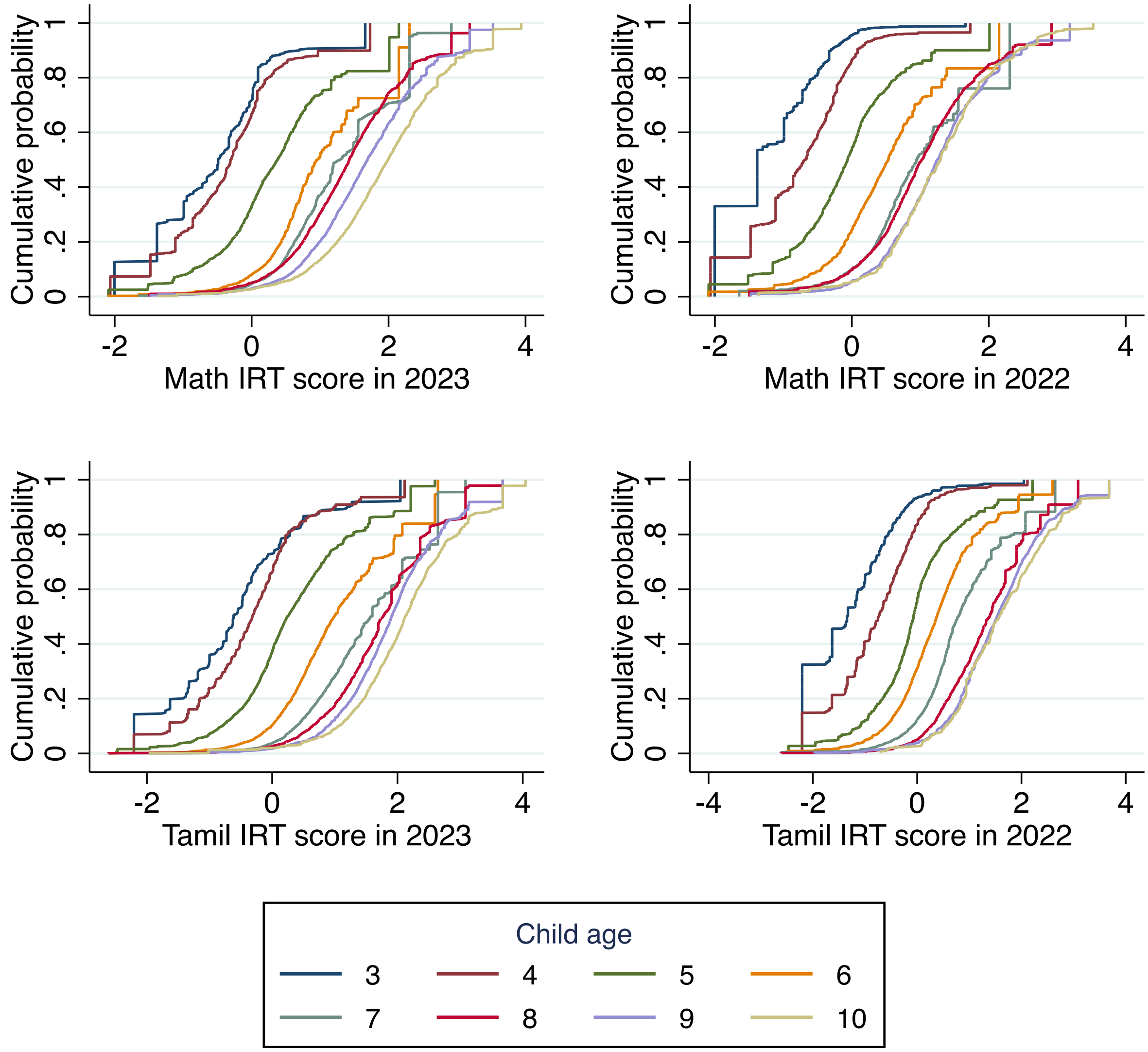


Notes: These figures show scatter plots of percentage correct on specific math test items, separately by age, in 2022 and 2023. Each dot in the figure corresponds to one item, administered in both rounds.

We create comparable, vertically-linked test scores across ages and testing rounds (2022 and 2023) by pooling all responses and estimating Item Response Theory (IRT) scores. Each item was scored dichotomously (correct or incorrect). We employ a two-parameter logistic (2PL) item response theory (IRT) model, which is appropriate for our predominantly open-ended questions, using the `OpenIRT` package in Stata (see https://github.com/tristanz/OpenIRT).

We conduct some basic tests to ensure the vertical linking is adequate. For example, Figure G.7 shows that the distribution of IRT scores increases with age, as expected, across both rounds.

Figure G.7: Cumulative distributions of IRT scores by age

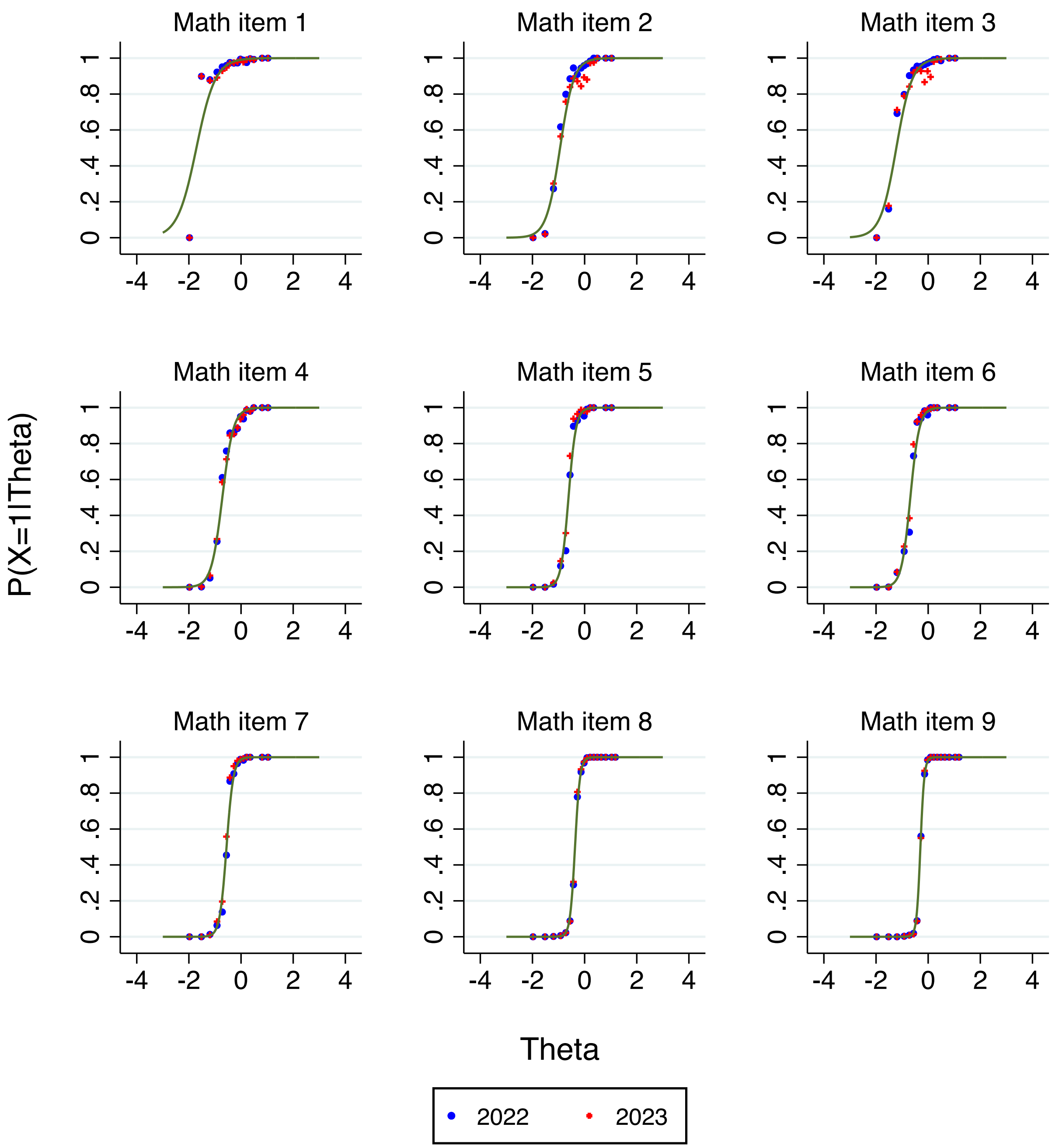


Notes: These figures show the cumulative distribution functions of IRT scores, estimated jointly using both assessment waves, by age and year.

We also show empirical fit to the estimated 2PL Item Characteristic Curve (ICC) for each round in Figures G.8 to G.18, and for children above/below 6 in Figures G.19 to G.29. Overall, questions are able to discriminate between students with different achievement levels (i.e., the ICC monotonically increases, meaning higher ability students are more likely to answer the question correctly), and there is no differential item functioning across rounds or age groups (i.e., students do not have an advantage in answering the question given by the timing of the survey round, and thus the likelihood of answering the question correctly depends on the ability and not the timing of the survey nor age of the child.).

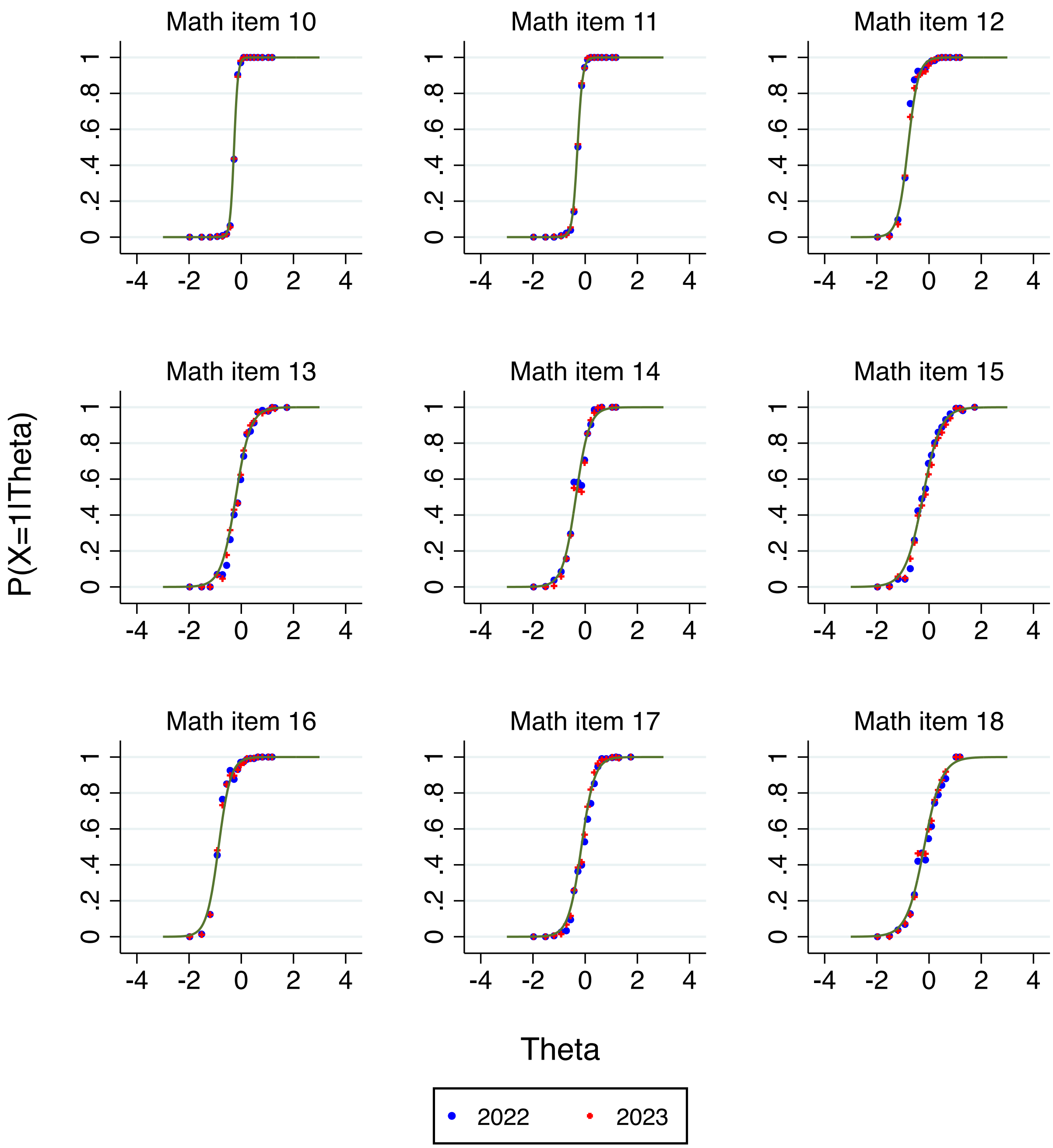
ICC curves and empirical fit by round

Figure G.8: Empirical fit to the estimated item characteristic curve (ICC) for math items by round



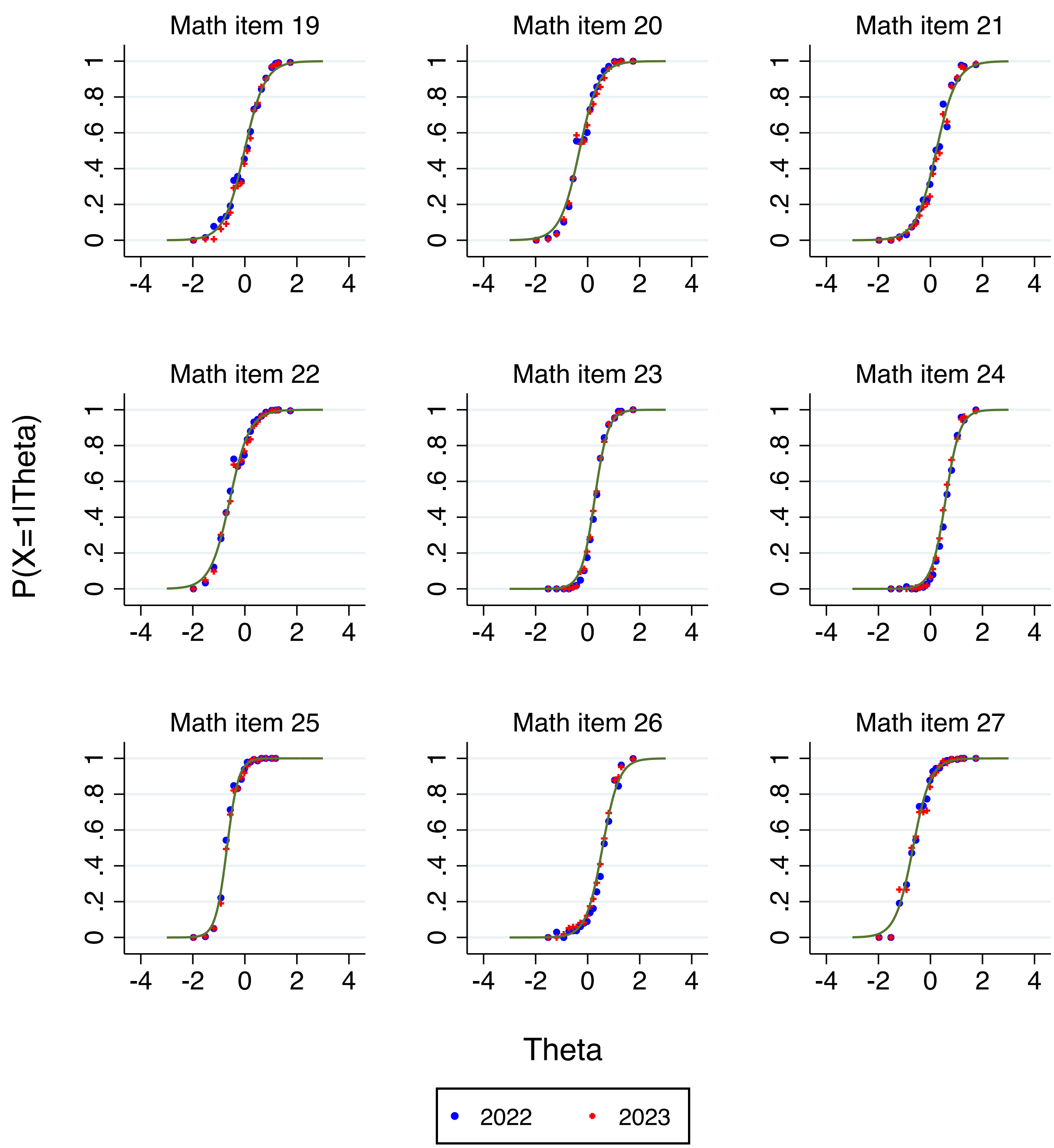
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately by assessment round.

Figure G.9: Empirical fit to the estimated item characteristic curve (ICC) for math items by round



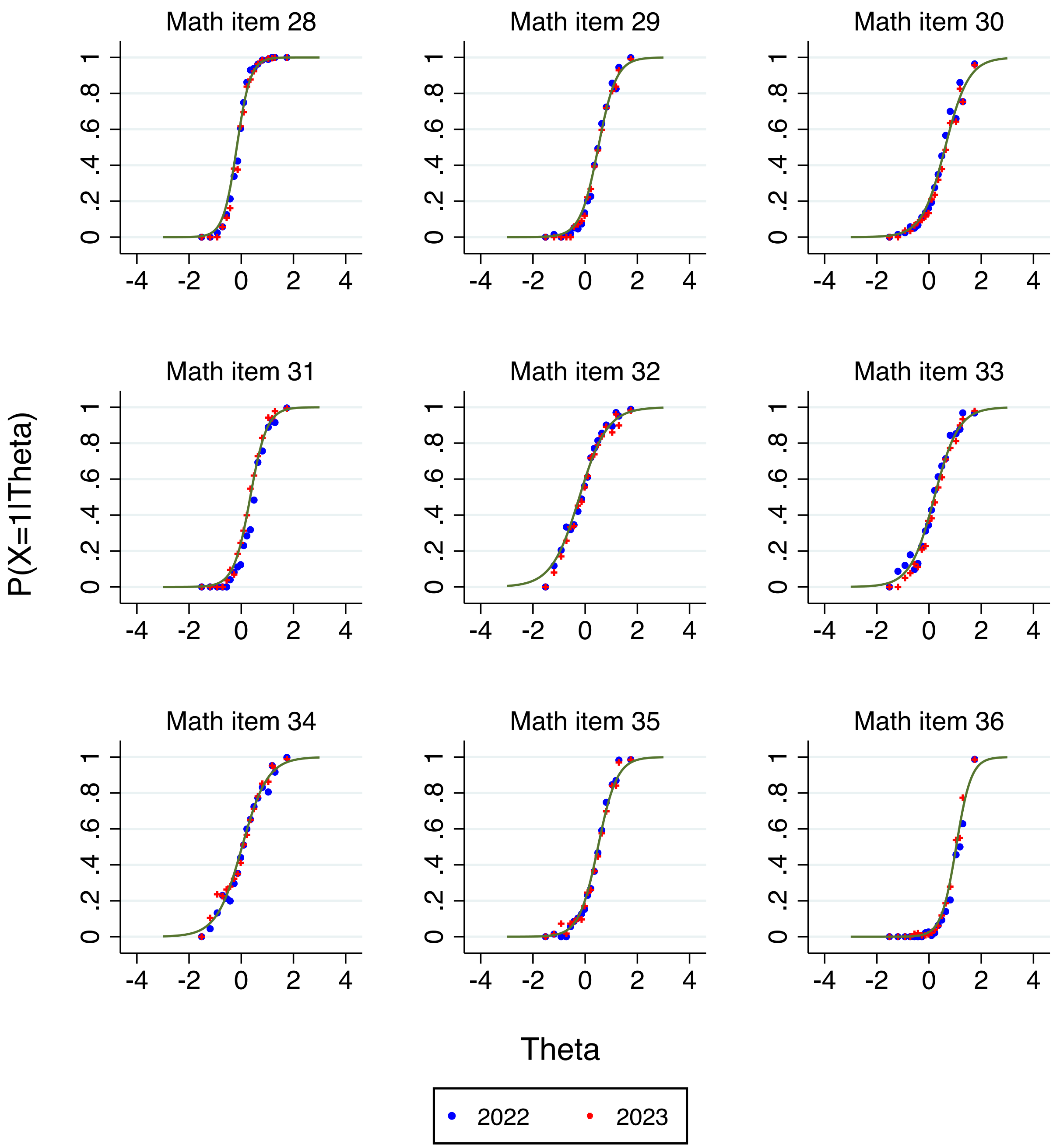
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately by assessment round.

Figure G.10: Empirical fit to the estimated item characteristic curve (ICC) for math items by round



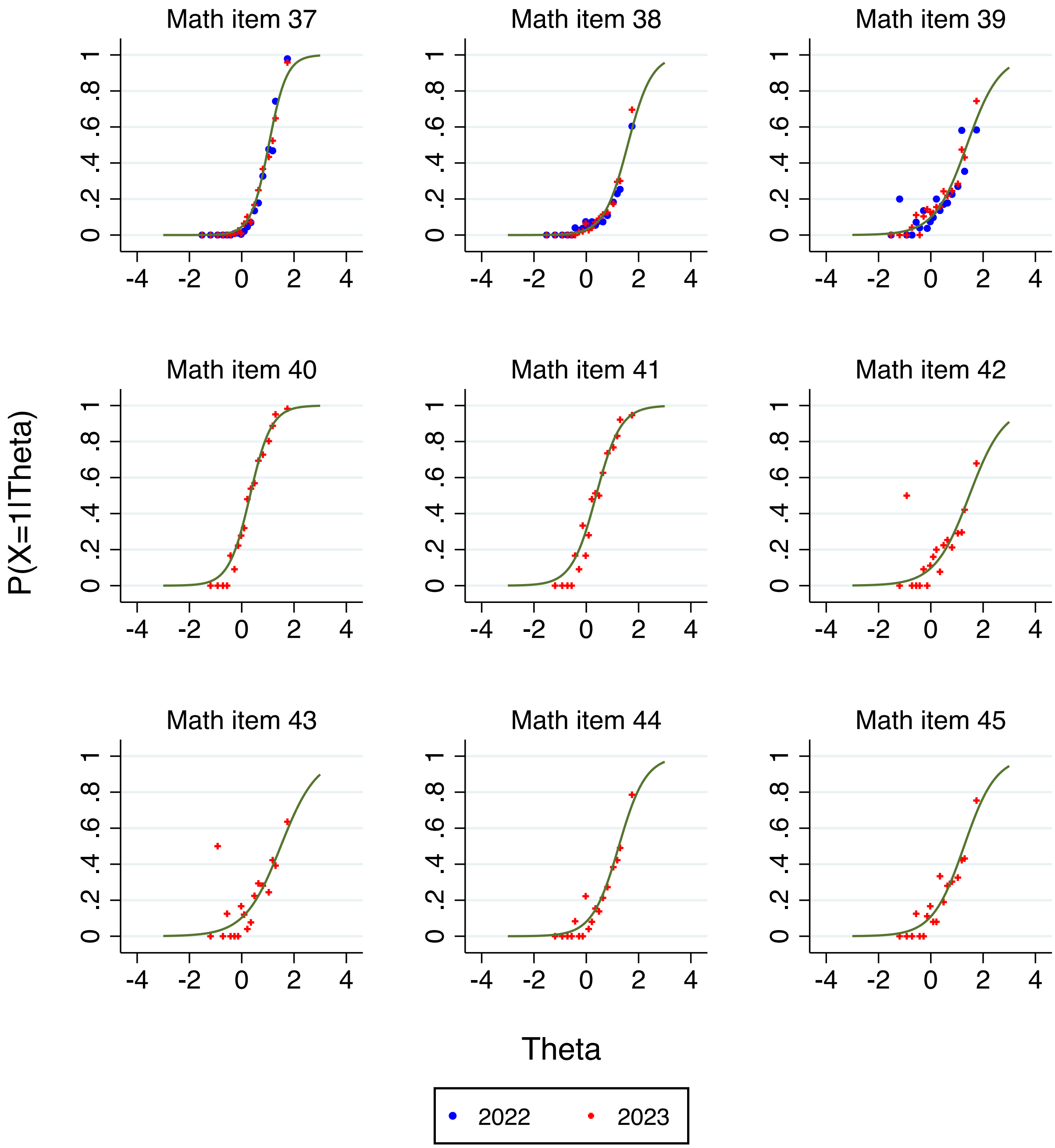
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately by assessment round.

Figure G.11: Empirical fit to the estimated item characteristic curve (ICC) for math items by round



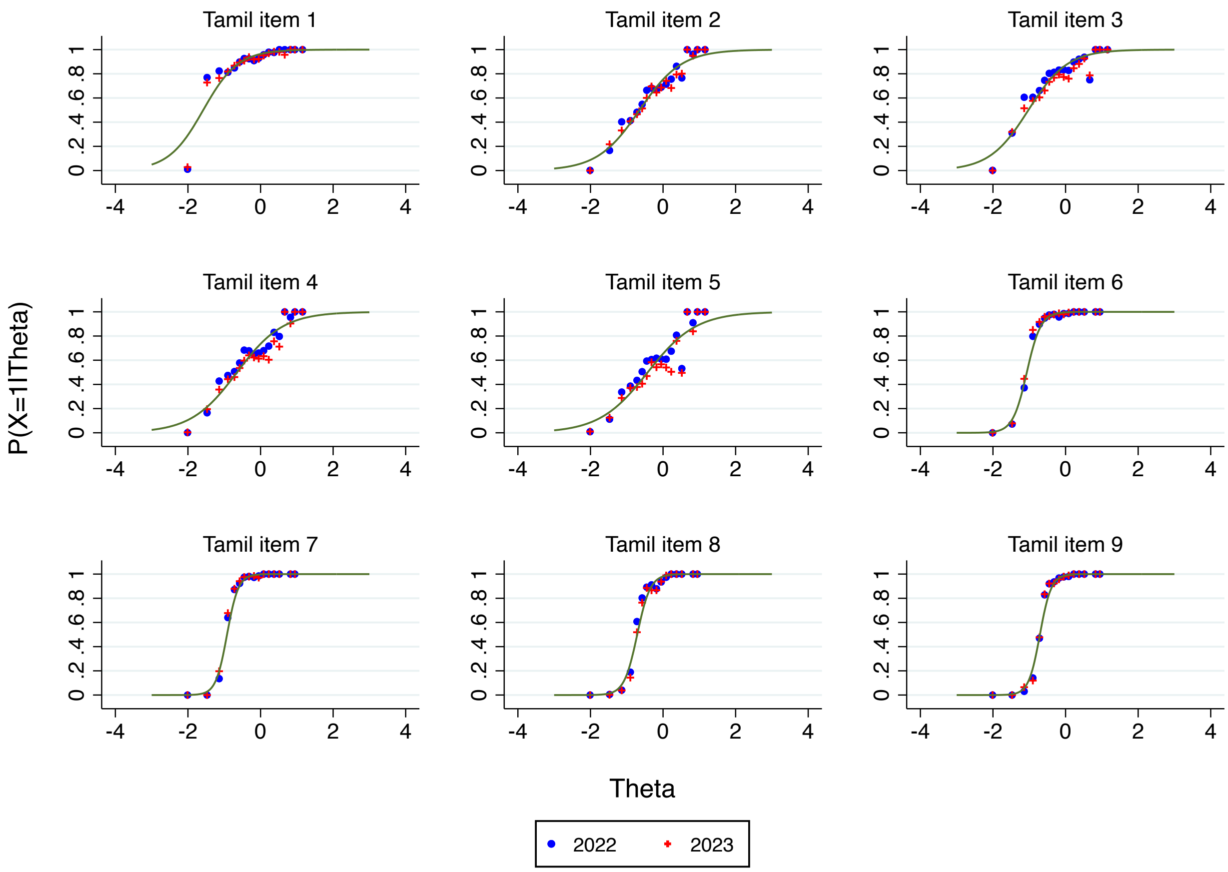
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately by assessment round.

Figure G.12: Empirical fit to the estimated item characteristic curve (ICC) for math items by round



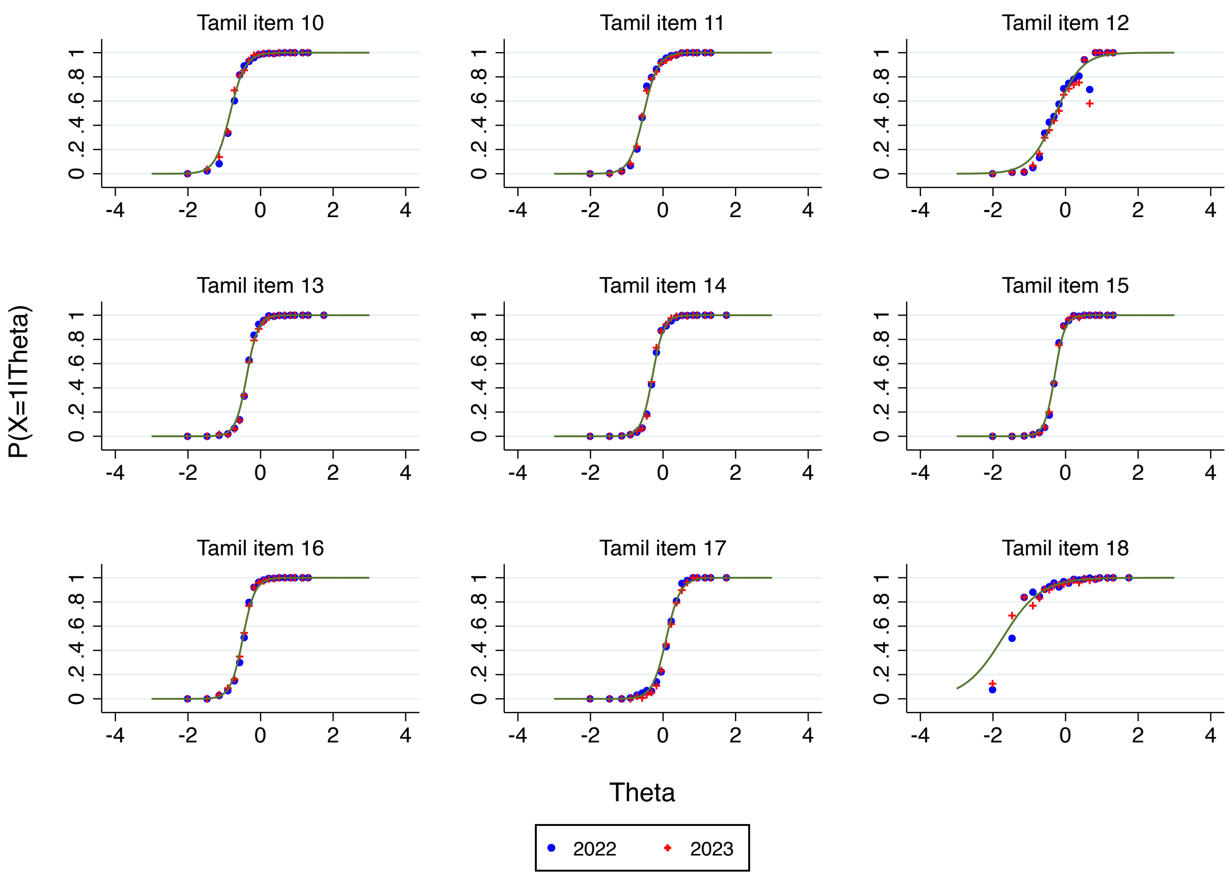
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately by assessment round.

Figure G.13: Empirical fit to the estimated item characteristic curve (ICC) for Tamil items by round



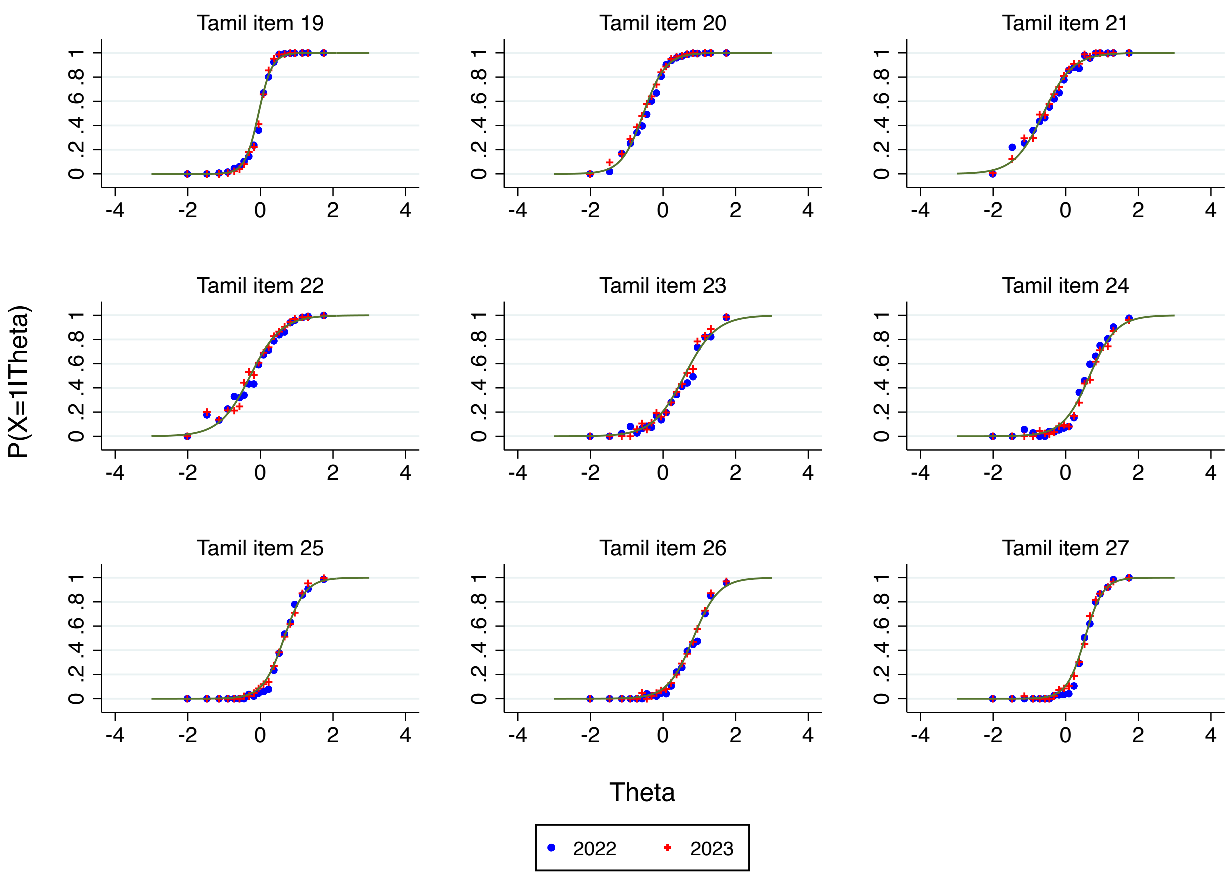
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately by assessment round.

Figure G.14: Empirical fit to the estimated item characteristic curve (ICC) for Tamil items by round



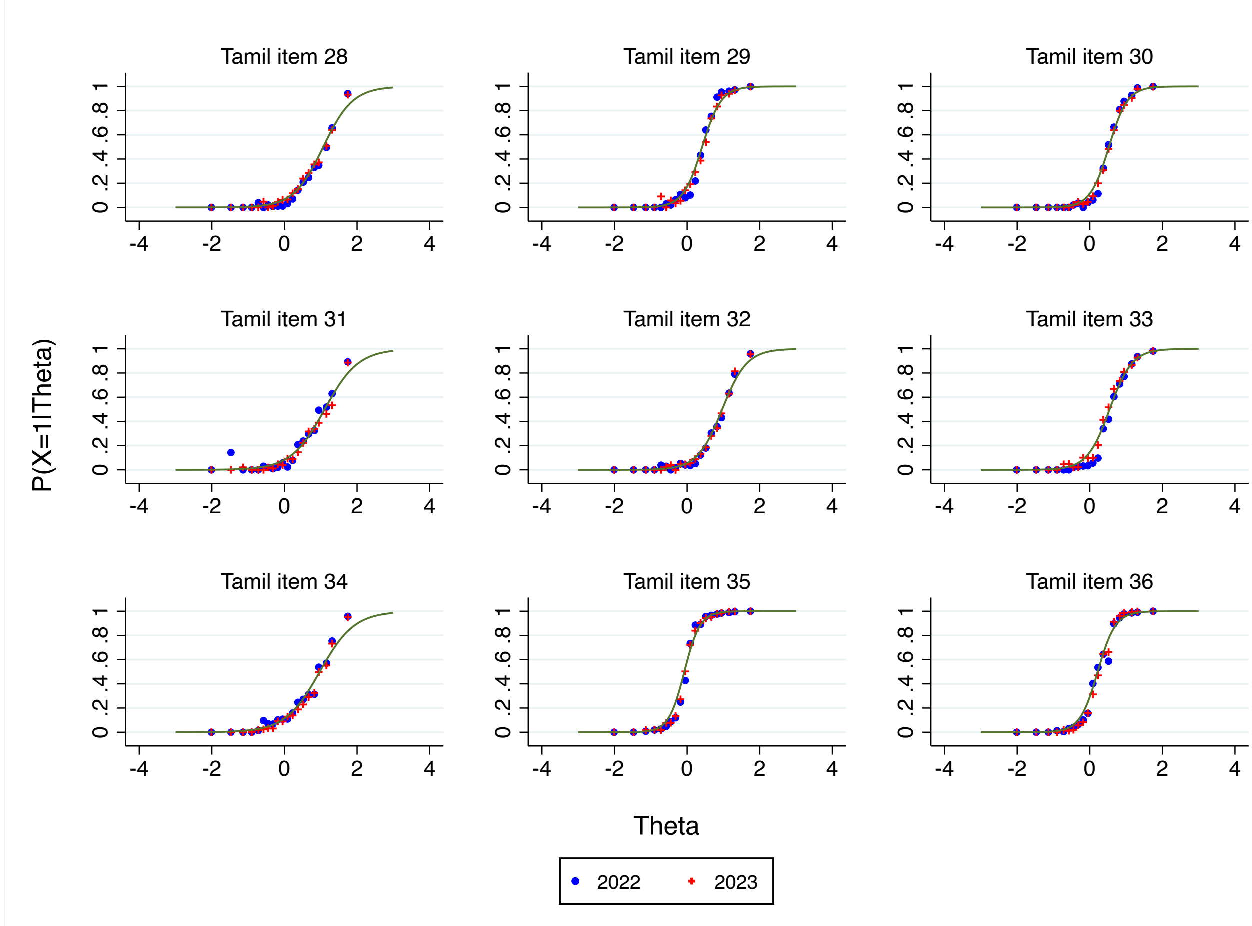
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately by assessment round.

Figure G.15: Empirical fit to the estimated item characteristic curve (ICC) for Tamil items by round



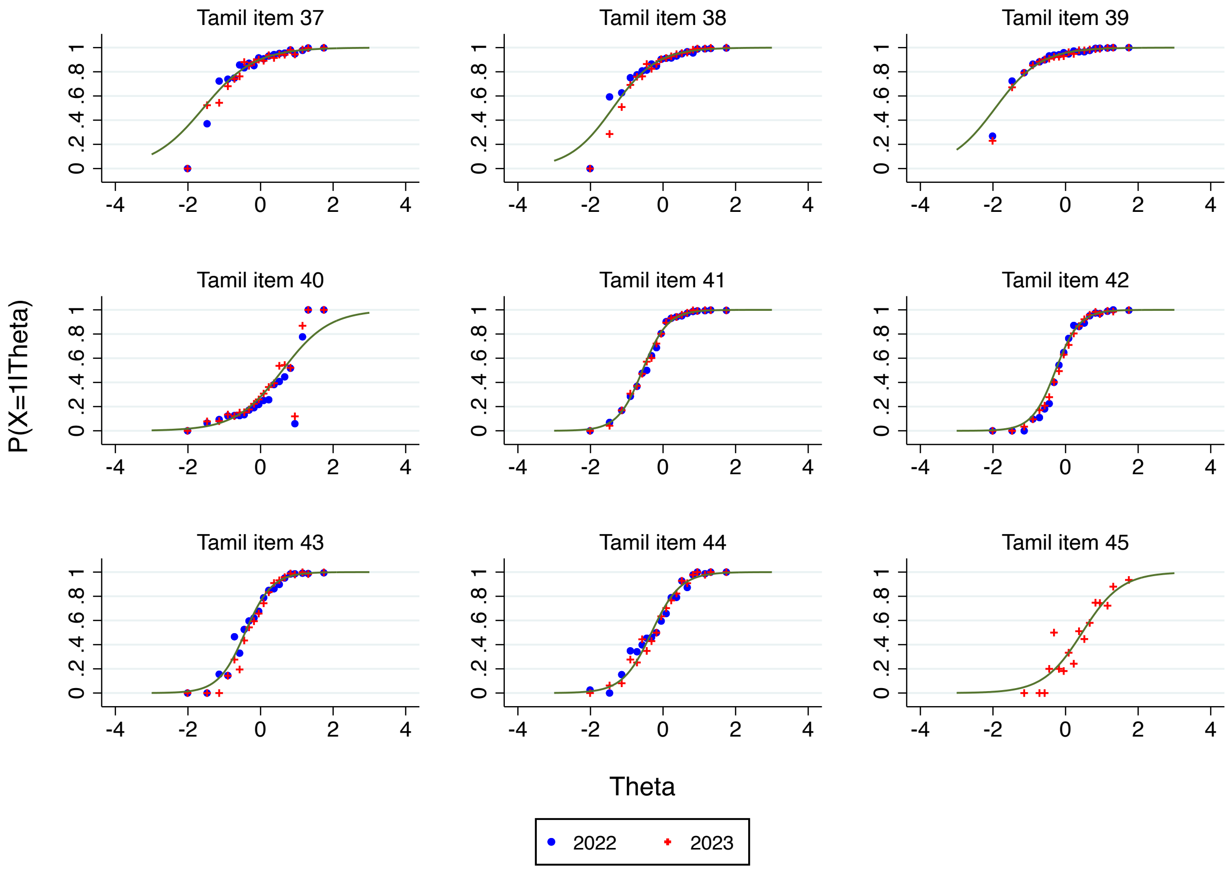
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately by assessment round.

Figure G.16: Empirical fit to the estimated item characteristic curve (ICC) for Tamil items by round



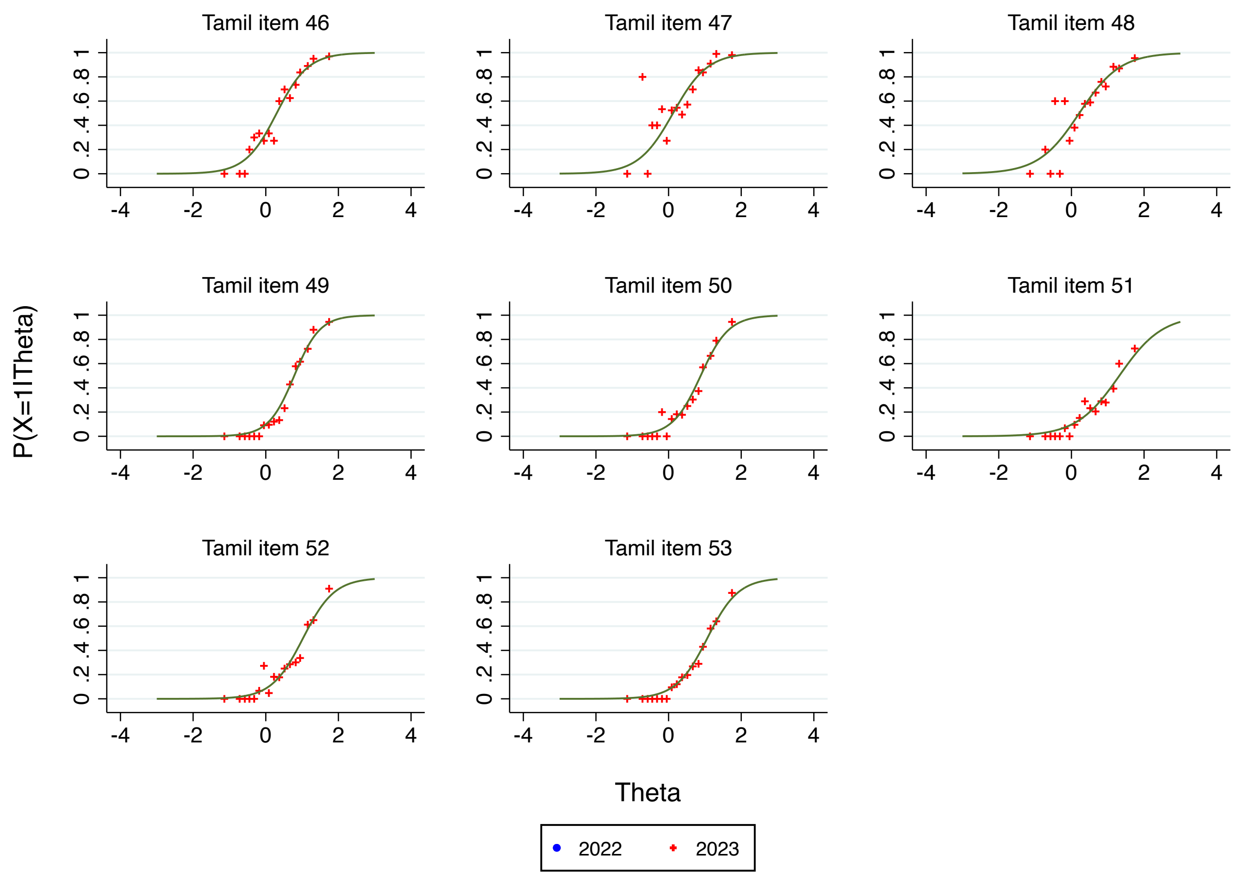
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately by assessment round.

Figure G.17: Empirical fit to the estimated item characteristic curve (ICC) for Tamil items by round



Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately by assessment round.

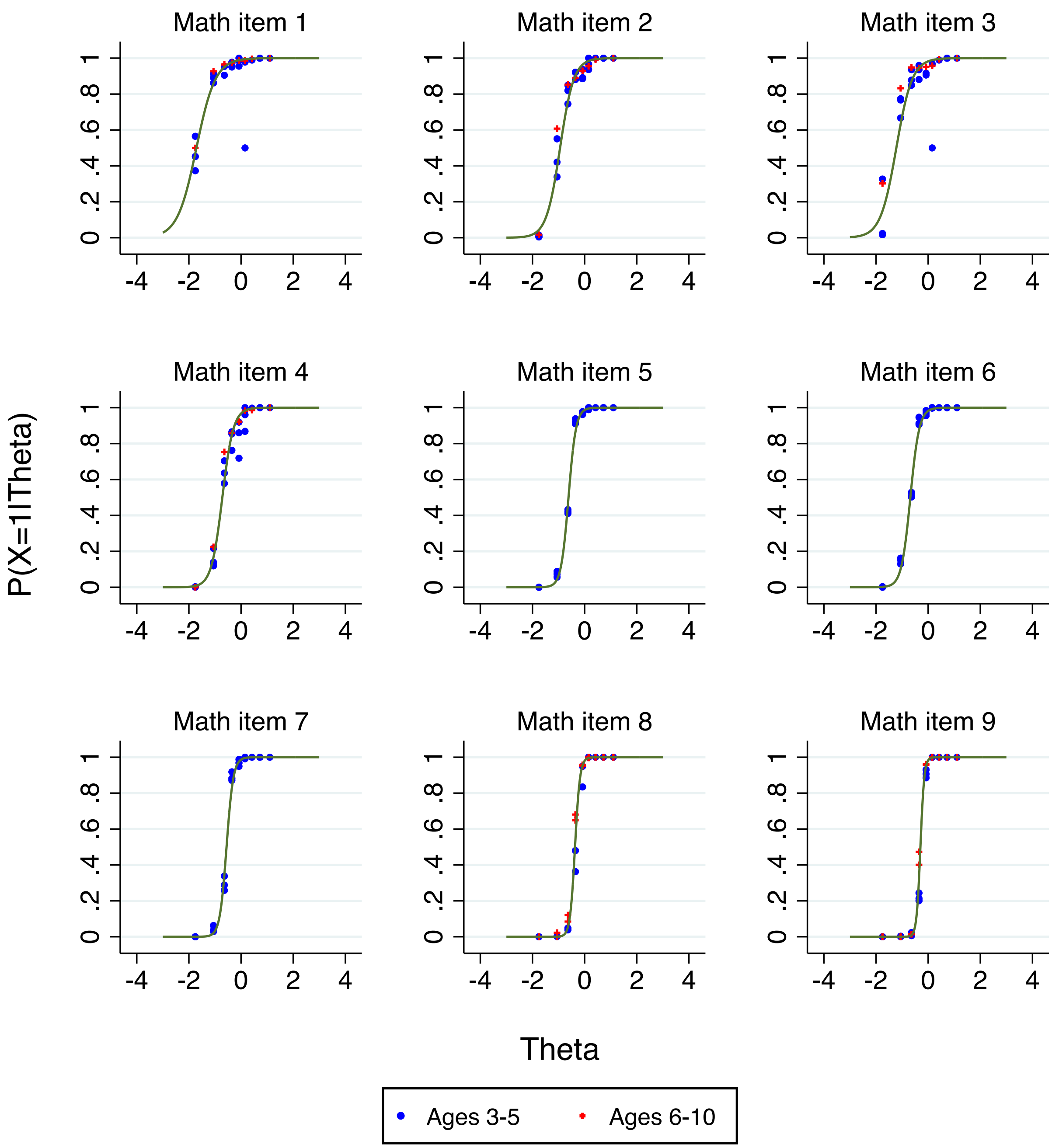
Figure G.18: Empirical fit to the estimated item characteristic curve (ICC) for Tamil items by round



Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately by assessment round.

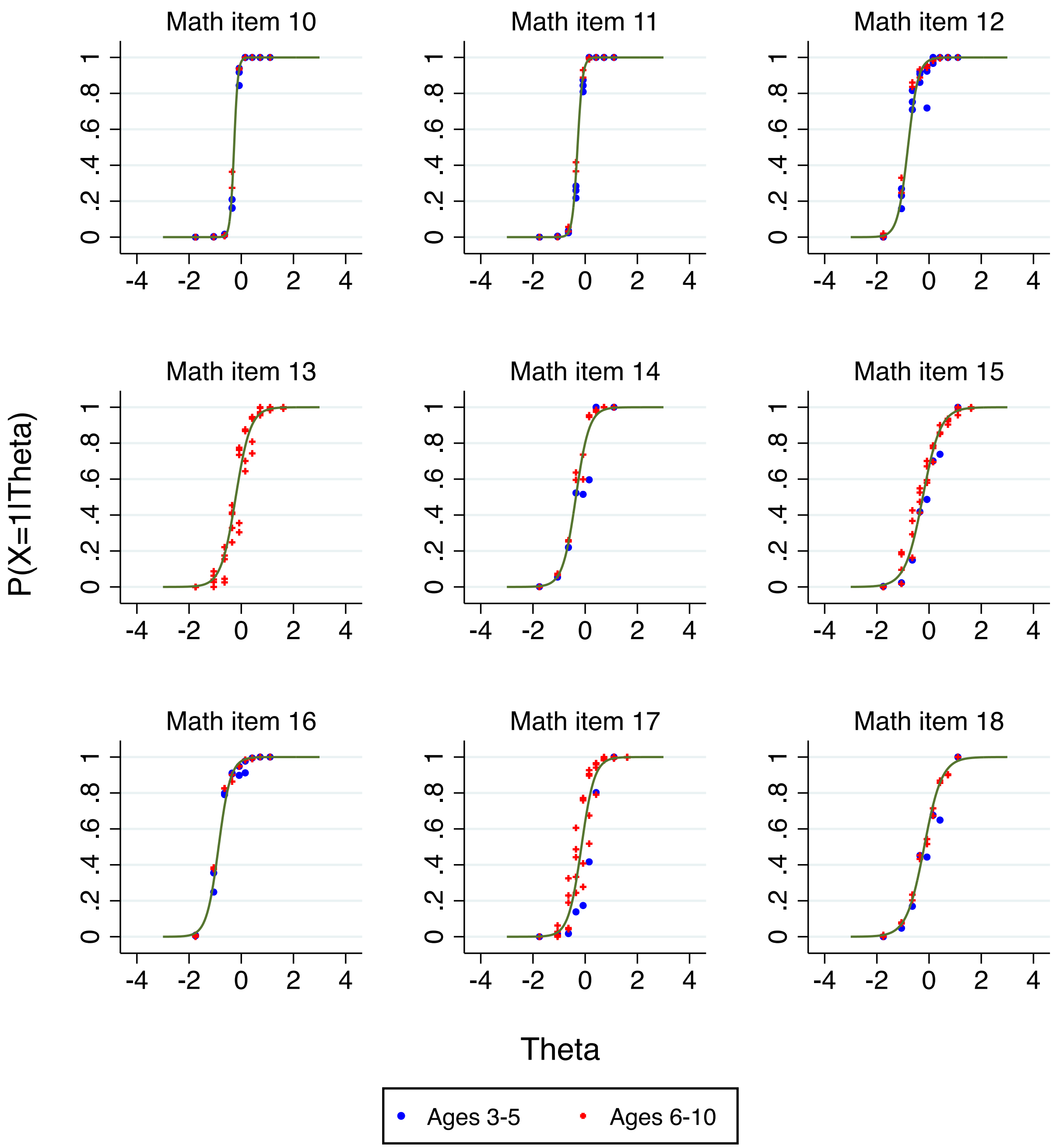
ICC curves and empirical fit by age

Figure G.19: Empirical fit to the estimated item characteristic curve (ICC) for math items by age



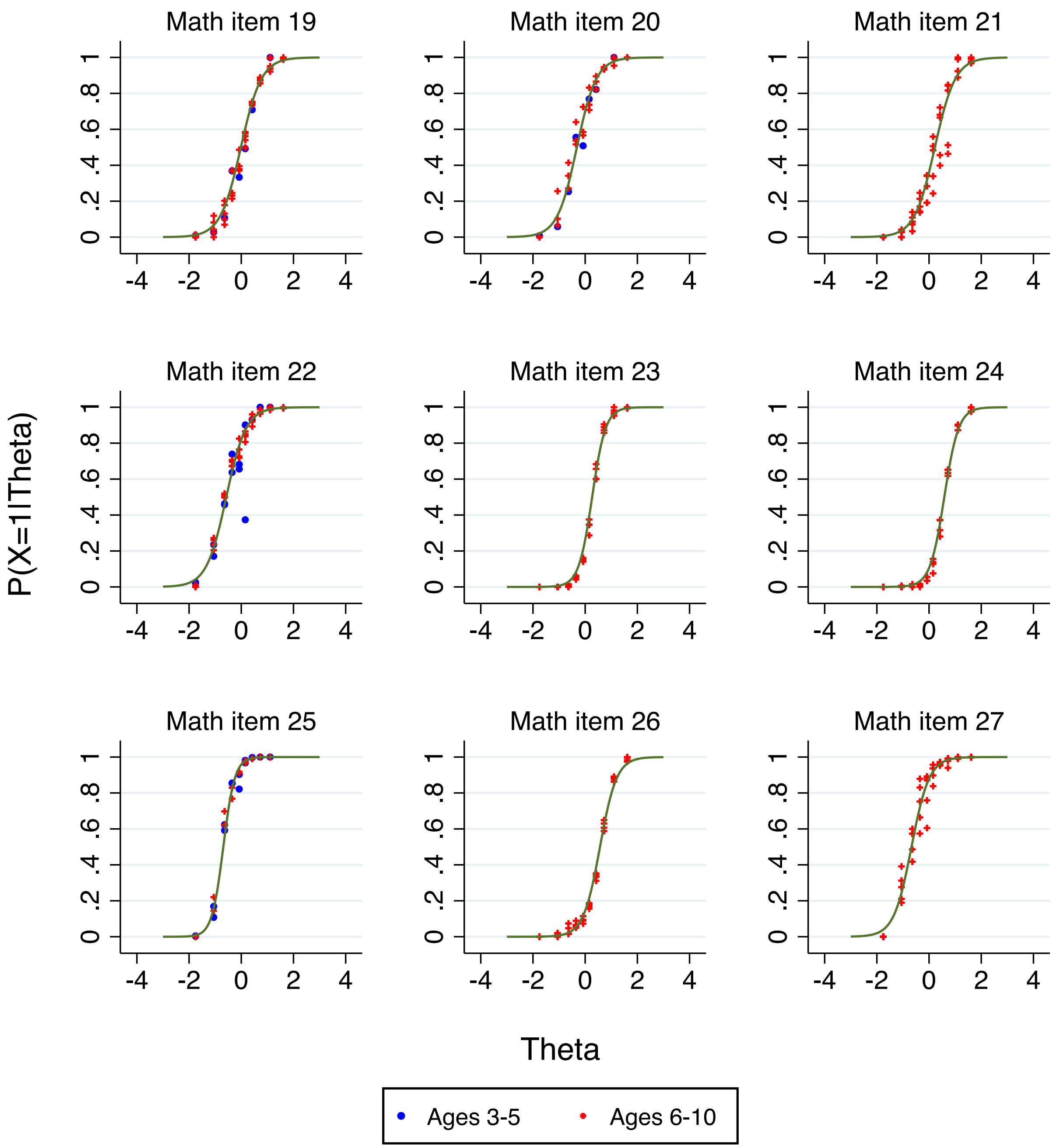
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately for children aged 3-5 and 6-10.

Figure G.20: Empirical fit to the estimated item characteristic curve (ICC) for math items by age



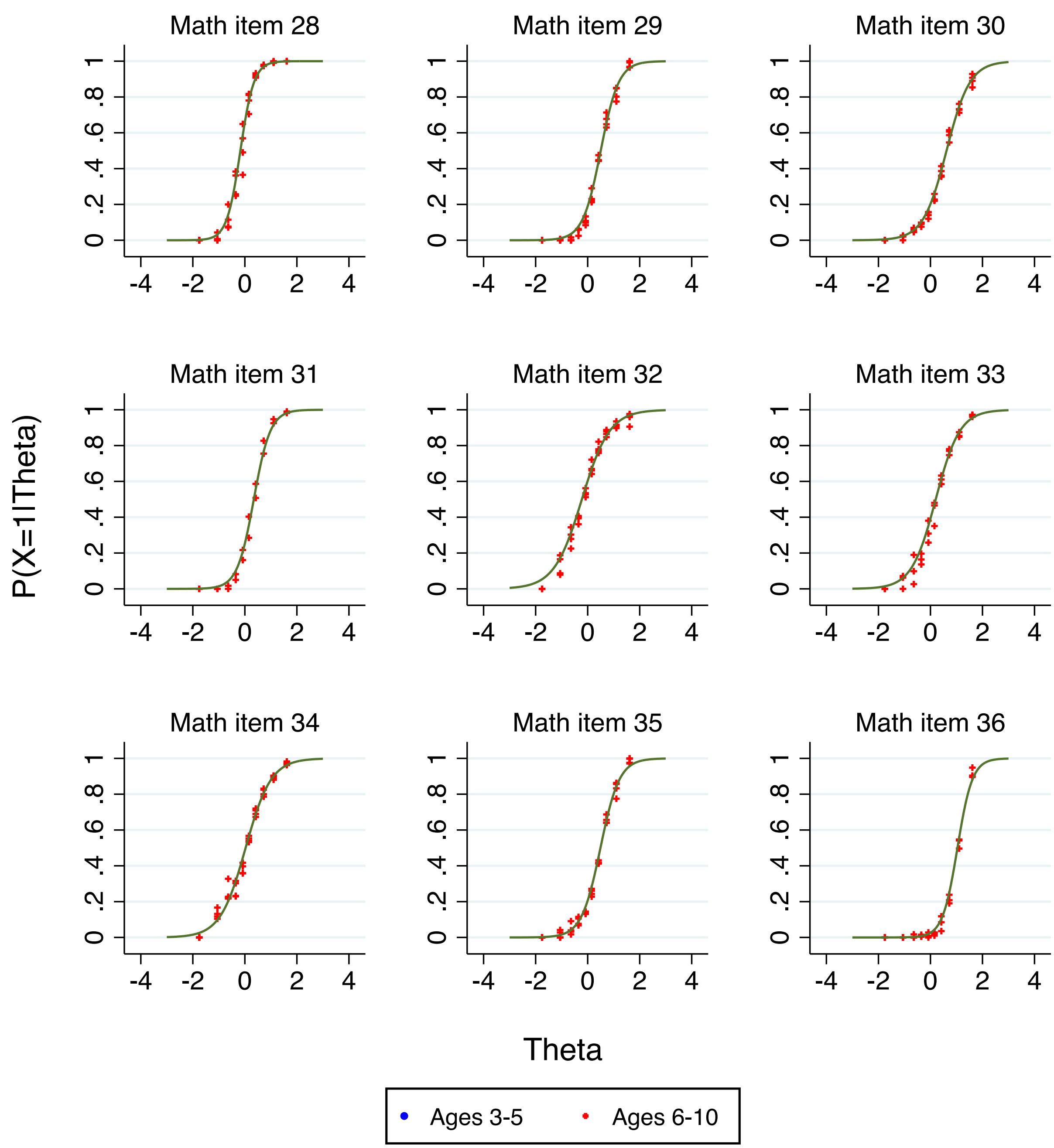
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately for children aged 3-5 and 6-10.

Figure G.21: Empirical fit to the estimated item characteristic curve (ICC) for math items by age



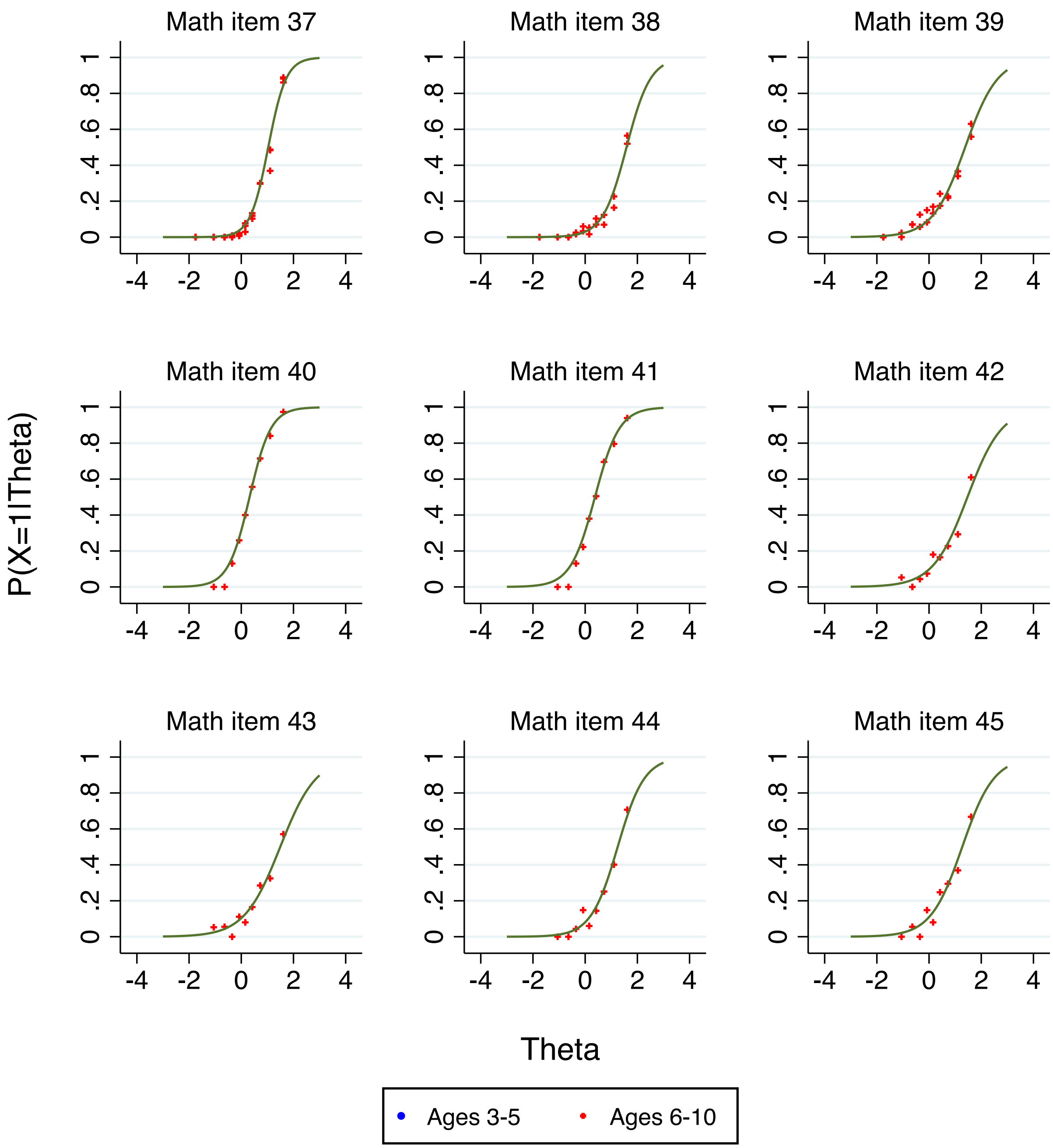
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately for children aged 3-5 and 6-10.

Figure G.22: Empirical fit to the estimated item characteristic curve (ICC) for math items by age



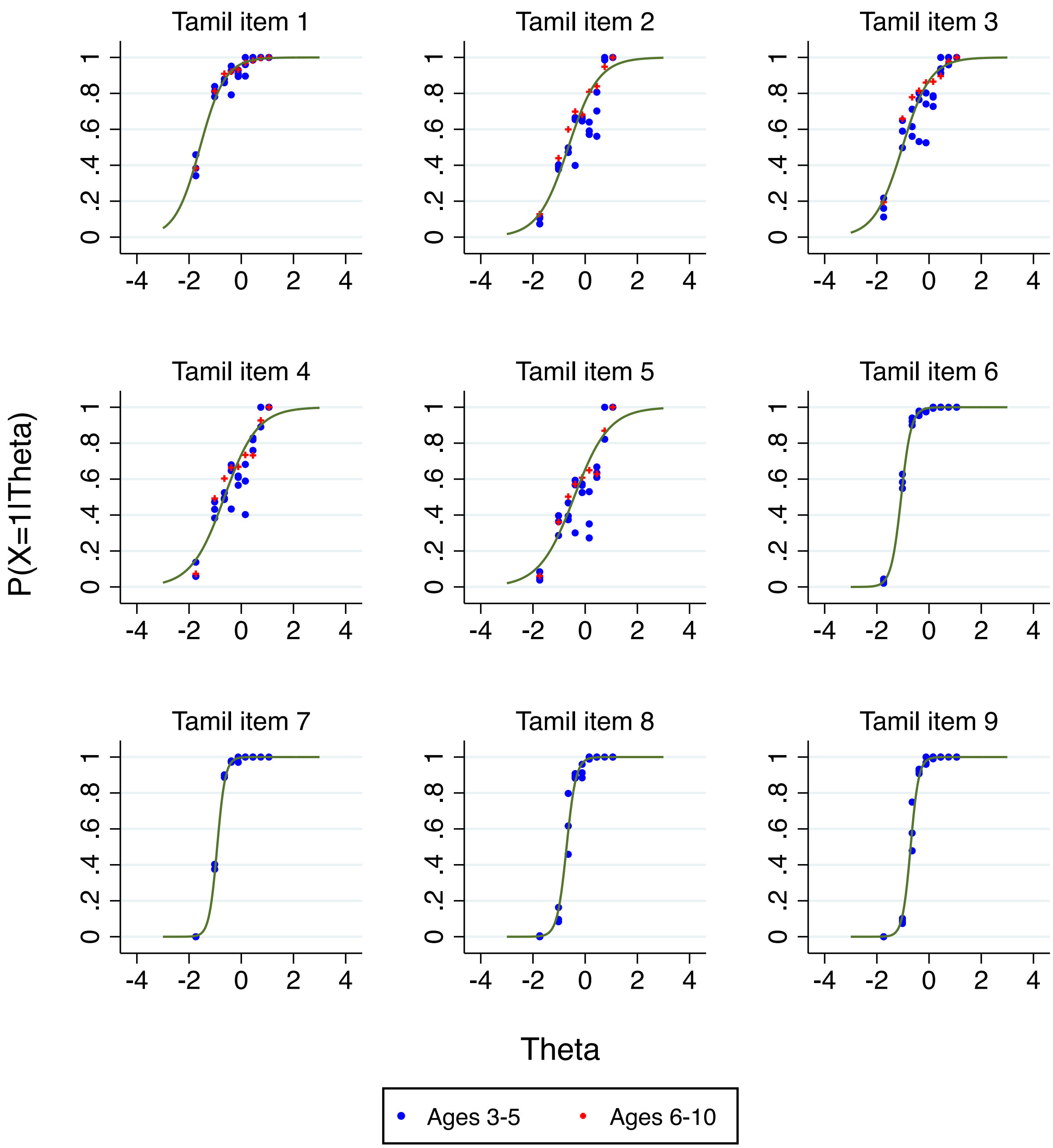
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately for children aged 3-5 and 6-10.

Figure G.23: Empirical fit to the estimated item characteristic curve (ICC) for math items by age



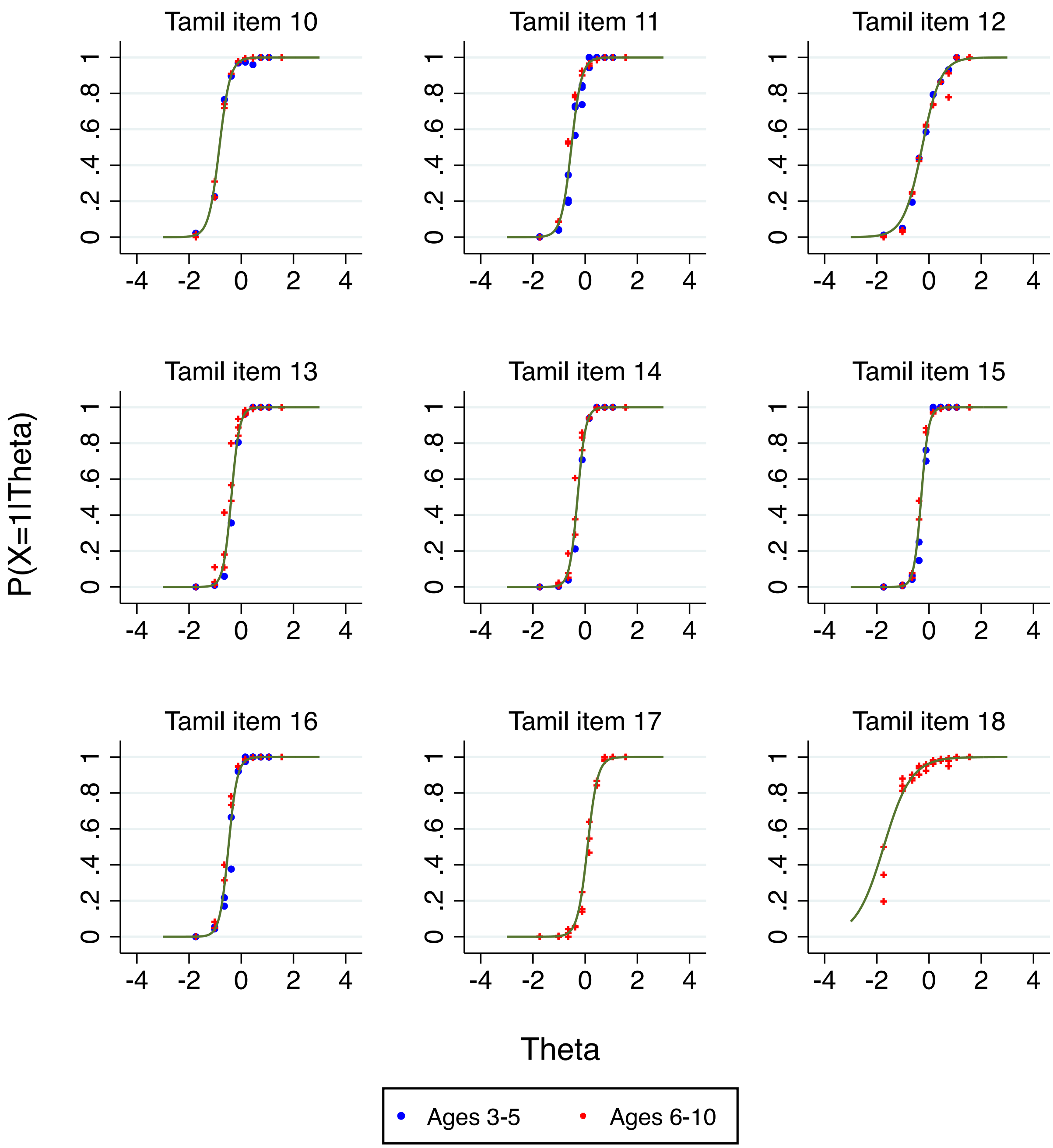
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately for children aged 3-5 and 6-10.

Figure G.24: Empirical fit to the estimated item characteristic curve (ICC) for Tamil items by age



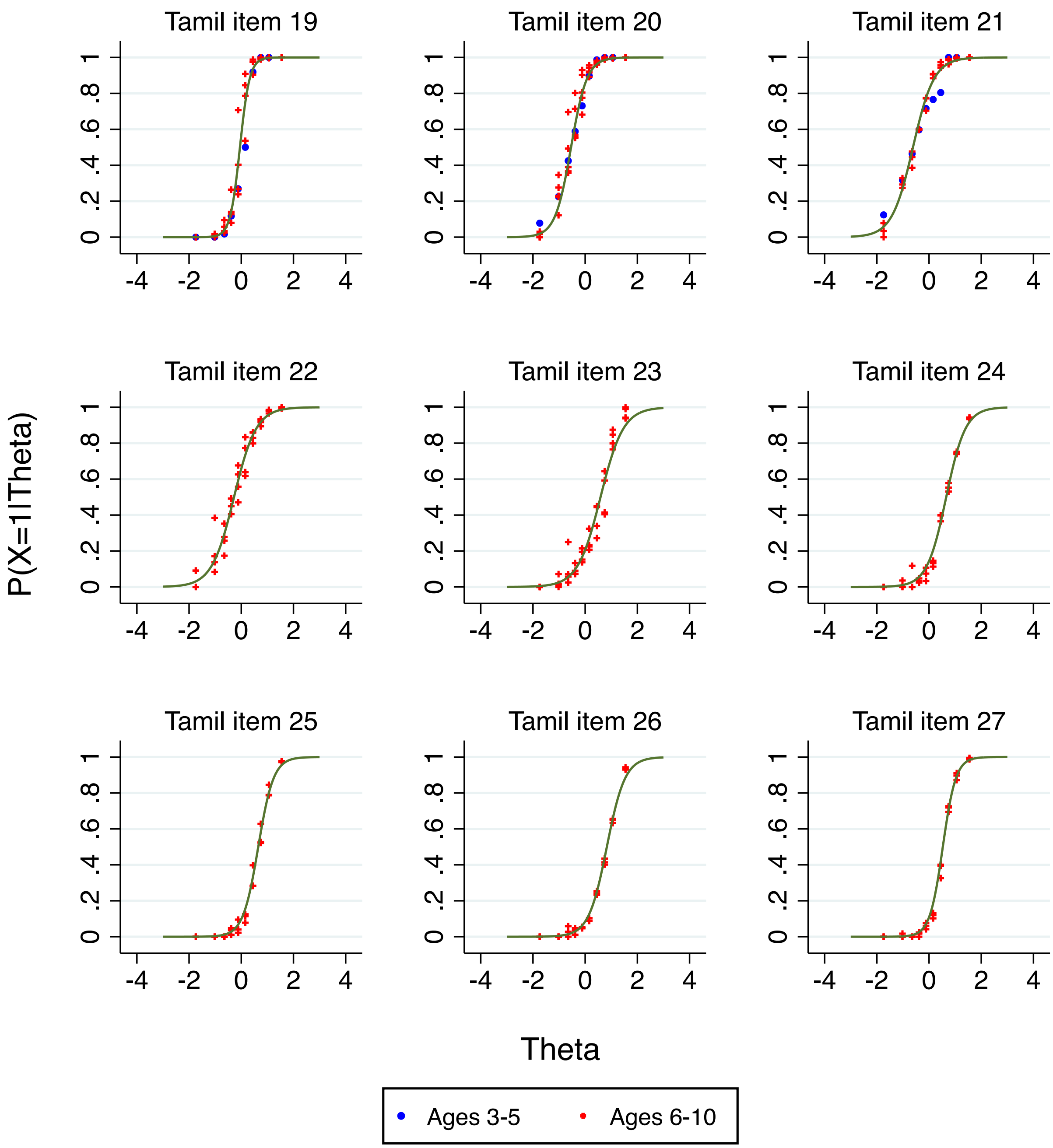
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately for children aged 3-5 and 6-10.

Figure G.25: Empirical fit to the estimated item characteristic curve (ICC) for Tamil items by age



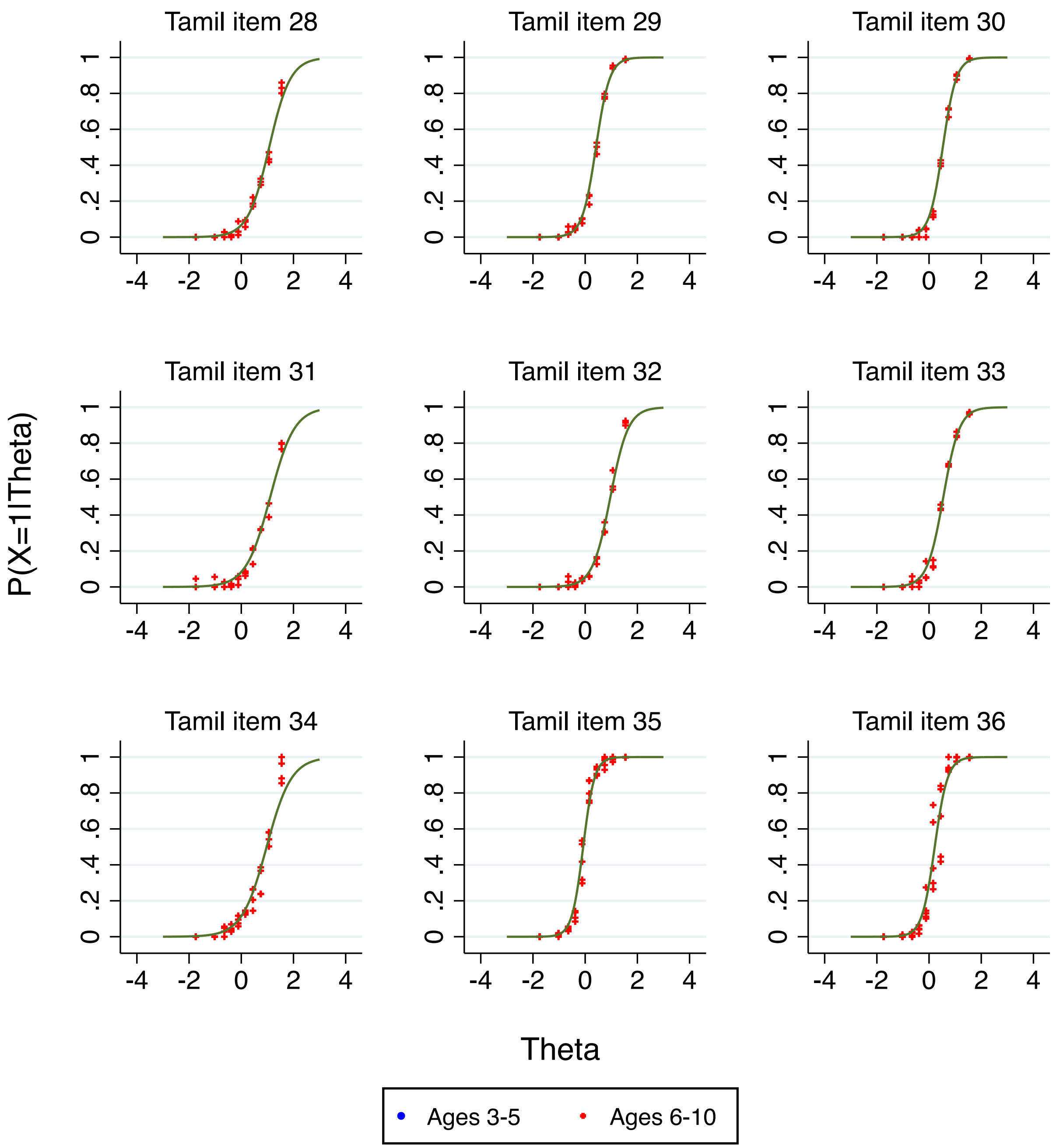
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately for children aged 3-5 and 6-10.

Figure G.26: Empirical fit to the estimated item characteristic curve (ICC) for Tamil items by age



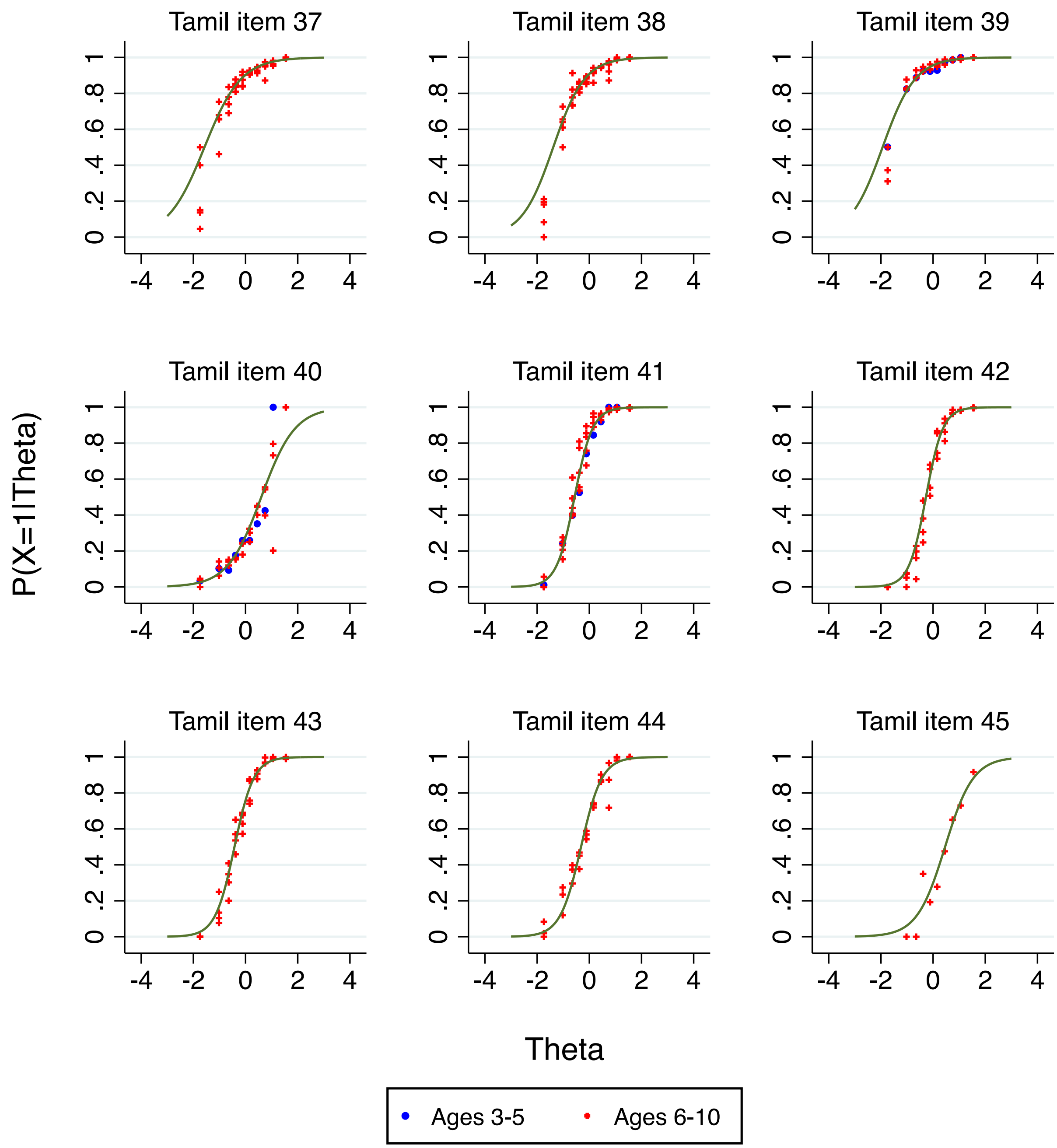
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately for children aged 3-5 and 6-10.

Figure G.27: Empirical fit to the estimated item characteristic curve (ICC) for Tamil items by age



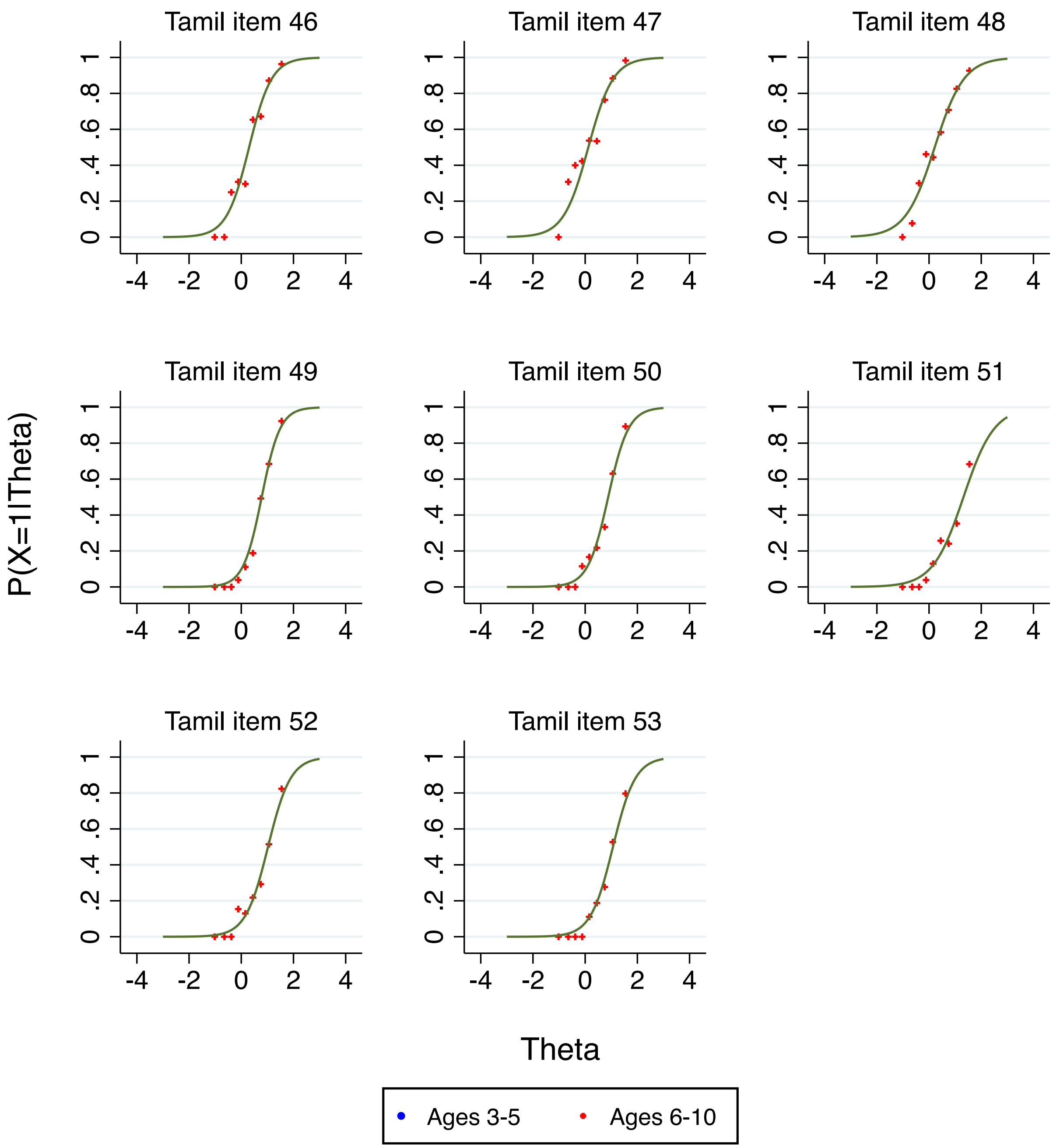
Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately for children aged 3-5 and 6-10.

Figure G.28: Empirical fit to the estimated item characteristic curve (ICC) for Tamil items by age



Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately for children aged 3-5 and 6-10.

Figure G.29: Empirical fit to the estimated item characteristic curve (ICC) for Tamil items by age



Notes: This figure presents the likelihood that students with different IRT scores answer different questions correctly, as well as the item characteristic curve for each question, separately for children aged 3-5 and 6-10.