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## Working Paper

# The effect of classroom rank on learning throughout elementary school: Experimental evidence from Ecuador

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Inter-American Development Bank  
Labor Markets and Social Security Division

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# The effect of classroom rank on learning throughout elementary school: experimental evidence from Ecuador

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## Abstract

We study the impact on learning of a child's rank in the classroom using a unique experiment from Ecuador. Within each school, students were randomly assigned to classrooms in every grade between kindergarten and 6<sup>th</sup> grade. Therefore, two students with the same ability can have different classroom ranks because of the (random) peer composition of their classroom. To isolate the impact of rank from other peer influences we include classroom fixed effects. Children with higher classroom rank at the beginning of the academic year have significantly higher math test scores at the end of that grade. Classroom rank in math, not language, drives our results. The impact of classroom math rank is larger for younger children, and grows substantially over time. Exogenous changes in classroom rank in math also improve executive function, child happiness, and teacher perceptions of student ability.

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## 1. Introduction

There are many settings in economics where relative rank concerns are important. They emerge naturally in tournaments (Lazear and Rosen 1981), they affect job satisfaction (Card et al. 2012), and can interact in interesting ways with social preferences (Bandiera et al. 2005). Although the idea that rank is important in education dates back to Marsh (1987), recent years have seen a growing literature quantifying its importance for a variety of outcomes (including Elsner and Isphording 2017 and 2018; Tincani 2018; Murphy and Weinhardt 2020; Elsner et al. 2021; Pagani et al. 2021; Comi et al. 2021; Bertoni and Nisticò 2023; Denning et al. 2023; Kiessling and Norris 2023; Delaney and Devereux 2022 provide an excellent survey of the recent literature), and showing medium- and long-term impacts on college major choice and attendance, earnings, and the probability of engaging in risky behaviors.<sup>2</sup>

In this paper, we investigate the impact of classroom rank on learning and other outcomes throughout elementary school. We use a unique experiment in elementary schools in Ecuador where, at the start of every grade, a cohort of students was randomly assigned across classrooms within a given school. Compliance with the random assignment was almost perfect, 98.9 percent on average over the 7 years of the experiment (Elsner et al 2021 also explore random assignment of students to classrooms, although they consider higher education, whereas our focus is on elementary schools). Variation in peer groups resulting from randomly assigning students to classrooms means that two students with the same underlying ability and in the same school and grade will have different classroom ranks.

This experimental design generates substantial variation in classroom rank achievement, which we measure in percentiles (so rank is normalized to be between 0 and 1). For example, for individuals with median ability in their school (i.e., with a rank close to 0.5 within their school), classroom rank can be as low as 0.364 or as high as 0.593 depending on the classroom they are assigned to.<sup>3</sup>

We find that increasing a child's classroom rank at the start of a given grade, keeping own ability constant, raises end-of-grade achievement. Rank, however, is just a particular form of peer effects, which could influence outcomes in various ways. The experimental design we explore generates

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<sup>2</sup> Existing papers show causal effects of rank on many domains. Murphy and Weinhardt (2020) and Cicala et al. (2018) show that rank can have positive effects on test scores in primary and secondary school. Elsner and Isphording (2017) and Elsner et al. (2021) document positive impacts of rank in high school on college enrolment and choice. Delaney and Devereux (2022) show that rank can partly explain the gender gap in the choice of STEM as a college major. In recent work, Denning et al. (2023) show that rank can also have long-term impacts on earnings later in life. Rank has also been shown to affect the likelihood of engaging in risky behaviors, as in Elsner and Isphording (2018). Pagani et al (2021) look at personality traits, Comi et al. (2021) focus on disruptive behaviours, and Kiessling and Norris (2023) study the relationship between rank and mental health.

<sup>3</sup> Even for very high and very low levels of student ability, for which variation in rank is bound to be more limited, there can be rank differences of 0.1 (10 percentile points) or more depending on the classroom one is assigned to. This alleviates the type of concerns raised by Angrist (2014), who shows that experimental assignment of children to peer groups may generate weak variation in peer characteristics, making it difficult to study peer effects in those settings.

variation in other classroom-level characteristics that may affect learning. In order to isolate the impact of classroom rank from other peer influences, we estimate models that control for classroom-level characteristics in a flexible way, i.e. by including classroom fixed effects.

We show that the effects of classroom rank are entirely driven by classroom rank in math, rather than language. Focusing on classroom rank in math, we document that rank effects are concentrated in the upper half of the rank distribution. Consistent with this result, we also find that rank effects are larger for children with higher lagged achievement, for children who started kindergarten with higher levels of vocabulary and for children who attended preschool. Rank effects are lower for children of less educated mothers. We see no evidence that the effect of classroom rank varies with the gender of the child.

A central contribution of our paper is the focus on differences in the effects of classroom rank by grade, and how these evolve over time. For this purpose, we divide our sample into children in “early” grades (1<sup>st</sup> and 2<sup>nd</sup> grades), “middle” grades (3<sup>rd</sup> and 4<sup>th</sup> grades) and “late” grades (5<sup>th</sup> and 6<sup>th</sup> grades) in elementary school. We first show that classroom rank effects are largest in the early grades: moving a child from the 50<sup>th</sup> to the 75<sup>th</sup> percentile of classroom rank in 1<sup>st</sup> or 2<sup>nd</sup> grade increases her end-of-grade achievement by more than 1 percentile point of national achievement, while classroom rank in 5<sup>th</sup> and 6<sup>th</sup> grades has no effect on achievement. We reject the null that rank effects are the same in early, middle, and late grades ( $p$ -value $<0.01$ ).

Our analysis then turns to the evolution of classroom rank effects over time. We show, remarkably, that the effects of early classroom math rank increase substantially as children age. After 4 lags the effect of classroom rank in 1<sup>st</sup> or 2<sup>nd</sup> grade is almost twice as large as it was originally. This result stands in sharp contrast with the effects of many other determinants of achievement, which tend to fade out.<sup>4</sup>

The fact that the medium-term effects of early classroom rank in math are larger than the short-term effects could occur for a number of reasons. One possibility is that students randomly assigned a high classroom rank in 1<sup>st</sup> (or 2<sup>nd</sup>) grade benefit from a virtuous cycle, where a high rank in grade  $t$  leads to higher learning and higher rank in grade  $t + 1$ , which in turn lead to higher learning and higher ranks in the following grades. We find, however, that our estimates are more consistent with an alternative model where, in addition to its direct effect on achievement, early math rank affects an unobserved trait—for example, academic self-concept—which affects learning in later grades, and which does not depreciate over time.

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<sup>4</sup> See, for example, Chetty, Friedman, and Rockoff (2014), and Jacob, Lefgren, and Sims (2010) for estimates of the fade-out of the effects of teacher quality, measured by teacher value added. For a review and discussion of fade-out in education interventions, with a particular focus on early childhood, see Bailey et al. (2020). For a recent example from a setting similar to ours, see Barrera-Osorio et al. (2020) on the fade-out of information on student performance provided to parents in Colombia.

With this insight, we turn to other child outcomes. Between 1<sup>st</sup> and 4<sup>th</sup> grades, we collected data on child executive function (EF). EF refers to a set of skills that allow individuals to plan, focus attention, remember instructions, and juggle multiple tasks. It includes working memory, inhibitory control, and cognitive flexibility (Center for the Developing Child 2019). Executive function in childhood has been shown to predict a variety of outcomes in adulthood, including performance in the labor market, involvement in criminal activities, and health status, even after controlling for socioeconomic status in childhood (Moffitt et al. 2011). We show that randomly assigning a child to a classroom where her rank is higher during the early grades improves her executive function in the medium term.

We also have data on a number of non-cognitive outcomes. At the end of 1<sup>st</sup> grade, we asked children whether they were happy in school, and at the end of 6<sup>th</sup> grade, we collected data on child depression, self-esteem, grit, and growth mindset. We show that children with higher math classroom rank (at the start of 1<sup>st</sup> grade) are more likely to say they are always happy (at the end of that grade). However, we do not find significant impacts of classroom rank on non-cognitive skills measured at the end of 6<sup>th</sup> grade.

Finally, we analyze whether classroom rank affects teacher perceptions of whether each student ranks at the top or bottom of the class, which is another unique feature of our data. These perceptions are correlated with actual student ranks in test scores, but the correlation is not perfect, either because of measurement error, or because teacher perceptions reflect other student attributes beyond their performance on the test. Teacher perceptions of student ability or performance could be important if, for example, teachers pay more attention to children they consider higher-achieving—and if such teacher reactions, in turn, affect future achievement. We show that, controlling for own lagged achievement, students who have a higher classroom rank in one grade are more likely to be perceived to be at the top of the class (and less likely to be thought to be at the bottom of the class) by their teachers in subsequent grades.

In sum, children with higher classroom rank have higher end-of-grade achievement. The effects of early classroom rank in math in 1<sup>st</sup> and 2<sup>nd</sup> grade increase substantially over time. More highly ranked children also have higher executive function, exhibit higher levels of happiness, and are thought to be higher-achieving by their future teachers.

Our paper extends the economics literature on the impacts of rank in important ways. First, ours is the first paper that analyzes rank effects in elementary education using an experiment with multiple rounds of random assignment, with essentially perfect compliance, although Elsner et al. (2021) also explore the naturally occurring random assignment of students to college classes. The fact that we have



data on children randomly assigned to different classrooms in every grade in elementary school means that we can convincingly test whether classroom rank has larger effects in some grades than in others.

Second, because we follow children over the entire elementary school cycle, we can credibly estimate how the effects of classroom rank evolve over time. As we show, both of these considerations—differences across grades and changes in the impact of classroom rank over time—are important, at least in the setting that we study.

In addition, we have unusually rich data on, executive function, happiness, depression, self-esteem, grit, growth mindset and, more uniquely, of teacher perceptions of student ability. This helps us understand how teachers perceive rank, and how these perceptions may mediate the impact of student rank on outcomes.

Finally, unlike most of the literature on this topic, we conduct this study in a developing country context. In our setting, children come from very poor families, and they perform one standard deviation below the norm on a standardized vocabulary score (the TVIP, which is the Spanish language version of the Peabody Picture Vocabulary Test). This setting allows us to examine whether the magnitude of the impacts of rank on student outcomes observed in richer countries are similar in a context such as ours, where the perceptions of rank by students, families and teachers may be different than in much wealthier countries, as well as their reactions to such perceptions.

The rest of the paper proceeds as follows. In section 2 we describe the setting and data, in section 3 we discuss our empirical strategy. Results are in section 4, and we conclude in section 5.

## **2. Setting and data**

### **A. Experimental setting and descriptives**

We study the acquisition of math, language, executive function, and non-cognitive skills in Ecuador, a middle-income country in South America. As is the case in most other Latin American countries, educational achievement of young children in Ecuador is low (Berlinski and Schady 2015).

The data we use come from an experiment in 202 schools.<sup>5</sup> Schools have at least two classrooms per grade (most have exactly two). An incoming cohort of children was randomly assigned to kindergarten classrooms within schools in the 2012 school year. These children were reassigned to 1<sup>st</sup> grade classrooms in 2013, to 2<sup>nd</sup> grade classrooms in 2014, to 3<sup>rd</sup> grade classrooms in 2015, to 4<sup>th</sup> grade classrooms in 2016, to 5<sup>th</sup> grade classrooms in 2017, and to 6<sup>th</sup> grade classrooms in 2018. Compliance with the assignment rules was very high—98.9 percent on average.

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<sup>5</sup> Araujo et al. (2016) discuss in detail the selection of schools in this study. They show that the characteristics of students and teachers in our sample are very similar to those of students and teachers in a nationally representative sample of schools in Ecuador.

Random assignment means that we can effectively deal with concerns about any purposeful matching of students with teachers and peers that often arise in non-experimental settings. We provide further details on the classroom assignment rules and compliance with randomization in Appendix A.

We have baseline data on maternal education, household wealth, whether a child attended preschool, and her vocabulary skills at the beginning of kindergarten. Table 1, Panel A, provides summary statistics for children in our sample. The table shows that children were 5 years of age on the first day of kindergarten, on average, and half of them are girls. Mothers were in their early 30s and fathers in their mid-30s. Both parents had on average just under 9 years of schooling, which corresponds to completed middle school. The average receptive vocabulary score of children in the sample is 1.7 SDs below the level of children that were used to norm the sample for the test.<sup>6</sup>

Table 1, Panel B, summarizes characteristics of classrooms and teachers. Average class size is 36. The average teacher in the sample has 18 years of experience. Eighty-two percent of teachers are women, and 82 percent are tenured. An important consideration in interpreting our results is that there is always one teacher per classroom, without a classroom aide, and that the same teacher is responsible for all academic subjects (all subjects other than physical education and, when they are available, art and music).

We collected data on math and language achievement at the end of each grade between kindergarten and 6<sup>th</sup> grade. For both subjects, tests were a mixture of material that teachers were meant to have covered explicitly in class—for example, in math, addition or subtraction; material that would have been covered, but probably in a somewhat different format—for example, simple word problems; and material that would not have been covered at all in class but that has been shown to predict current and future math achievement—for example, the Siegler number line task.<sup>7</sup> We aggregate responses in math and, separately, language, by Item Response Theory (IRT), and calculate an average achievement score that gives the same weight to math and language.<sup>8</sup>

In every grade between kindergarten and 4<sup>th</sup> grade, we tested child executive function. EF includes a set of basic self-regulatory skills which involve various parts of the brain, but in particular the prefrontal

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<sup>6</sup> To measure baseline receptive vocabulary, we use the *Test de Vocabulario en Imágenes Peabody* (TVIP) (Dunn et al. 1986), the Spanish-speaking version of the much-used Peabody Picture Vocabulary Test (PPVT). The TVIP was normed on samples of Mexican and Puerto Rican children. It has been used widely to measure development among Latin American children. See Paxson and Schady (2007) for a comparison of vocabulary scores between children in Ecuador and the U.S., and Schady et al. (2015) for evidence on levels and socioeconomic gradients in the TVIP in five Latin American countries, including Ecuador.

<sup>7</sup> The number line task works as follows. Children are shown a line with the two clearly marked endpoints—for example, in 1<sup>st</sup> grade, the left end of the line is marked with a 0, and the right end is marked with a 20. They are then asked to place various numbers on the line—for example, the number 2 or the number 18. The accuracy with which children place the numbers has been shown to predict general math achievement (see Siegler and Booth 2004).

<sup>8</sup> Our results are very similar if, instead, we calculate a simple sum of correct responses within blocks of questions on the test, and give equal weight to each of these test sections (as in Araujo et al. 2016).

cortex.<sup>9</sup> Executive function is generally thought of as having three domains: working memory, inhibitory control, and cognitive flexibility. It is an important determinant of how well young children adapt to and learn in school. Basic EF skills are needed to pay attention to a teacher; wait to take a turn or raise one's hand to ask a question; and remember steps in, and shift from one approach to another, when solving a math problem, among many other tasks that children are expected to learn and carry out in the classroom. Children with high EF levels are able to concentrate, stay on task, focus, be goal-directed, and make good use of learning opportunities. Low levels of EF are associated with low levels of self-control and “externalizing” behavior, including disruptive behavior, aggression, and inability to sit still and pay attention, which affects a child's own ability to learn, as well as that of her classmates (Séguin and Zelazo 2005).<sup>10</sup>

At the end of each grade, we asked teachers who were the 5 children with the highest achievement, and 5 with the lowest achievement.<sup>11</sup> In 1<sup>st</sup> grade, we asked children whether they were happy in school and in their classroom (two separate questions). In both cases, children had the option of answering “always”, “sometimes”, or “never”. Most children in the sample answered “always” to both questions, so we use their responses to construct a single variable for children who were always happy, almost always happy, or mostly happy.

In 6<sup>th</sup> grade, we collected data on child depression, self-esteem, growth mindset, and grit. To measure child depression, we used the Patient-Reported Outcomes Measurement Information System (PROMIS) Depression Scale for children aged 11-17 years, developed by the American Psychiatric Association.<sup>12</sup> To measure self-esteem, we selected 5 questions from the National Longitudinal Study of

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<sup>9</sup> Volumetric measures of prefrontal cortex size predict executive function skills; children and adults experiencing traumatic damage to the prefrontal cortex sustain immediate (and frequently irreversible) deficits in EF (Nelson and Sheridan 2011, cited in Obradovic et al. 2012).

<sup>10</sup> Working memory measures the ability to retain and manipulate information; for example, 2<sup>nd</sup> grade child were asked to remember (increasingly long) strings of numbers and repeat them in order and then backwards. Cognitive flexibility measures the ability to shift attention between tasks and adapt to different rules; for example, 1<sup>st</sup> grade children were shown picture cards that had trucks or stars, red or blue, and were asked to first sort cards by *shape* (trucks versus stars), and then by *color* (red versus blue). Inhibitory control refers to the capacity to suppress impulsive responses; for example, kindergarten children were quickly shown a series of flash cards that had either a sun or a moon and were asked to say the word “day” when they saw the moon and “night” when they saw the sun. We calculate scores on each of the three domains in executive function, as well as a measure of overall EF scores that gives one-third of the weight to each individual domain. We do not have data on inhibitory control in 1<sup>st</sup> grade. In this grade, the overall measure of executive function includes only cognitive flexibility and working memory, with equal weight given to both.

<sup>11</sup> Importantly, these data are not disaggregated by subject—that is, we did not ask teachers who they thought were top and bottom performers in math and, separately, in language.

<sup>12</sup> Olinio et al (2013), Klein et al. (2005), and Aylward et al. (2008) argue that the PROMIS depression scale has superior qualities (greater precision, more internal reliability, and more discriminant validity) than other commonly-used depression scales, including the Beck Depression Inventory (BDI), the Children's Depression Inventory (CDI), and the Center for Epidemiologic Studies-Depression (CES-D) scale.

Adolescent to Adult Health (Add Health).<sup>13</sup> To measure Growth Mindset, we selected 10 of the 20 questions on the Dweck “Mindset Quiz”; growth mindset refers to the belief that intelligence is malleable, rather than fixed, and can be increased with effort (Blackwell, Trzesniewski, and Dweck 2007; Dweck 2008). Finally, to measure grit, we adapted 4 questions from the 8-item Grit Scale for children (Duckworth and Quinn 2009); grit refers to the capacity of individuals to persevere at a given task. For each of these 6<sup>th</sup> grade outcomes, we aggregated responses by factor analysis. We also calculate an overall non-cognitive score that gives the same weight to each of the individual tests.

Most of the tests were applied to children individually (as opposed to in a group setting) by specially trained enumerators.<sup>14</sup> All tests, other than the non-cognitive tests applied in 6<sup>th</sup> grade, were applied in school. For all tests, to choose questions, we piloted the test; made changes as necessary; and selected questions that could be understood by children in our context, and which showed reasonable levels of variability in the pilot. Further details on child assessments are provided in Appendix B.

#### B. How much variation is there across classroom rank within school?

A potential concern could arise if there is limited variation in classroom rank across students with the same ability who are assigned to different classrooms as a result of the randomization. As illustrated in Angrist (2014), experimental designs that rely on random assignment of individuals to groups to identify peer effects may suffer from modest variation in peer characteristics, resulting in identification issues that are akin to a weak instruments problem. This empirical concern potentially applies to our setting, as we rely on random assignment of students to classrooms within schools to identify the causal effect of classroom rank on learning.

We show however that, in practice, our empirical work does not suffer from this problem. This problem is more likely to emerge in linear in means models that regress student outcomes on average peer characteristics. It is less important for examining the role of rank, which depends on the whole distribution of student characteristics in the classroom, and not only on its mean. In our dataset, two students with the same ability rank who attend the same school have quite different classroom ranks.

We can graphically illustrate this point by plotting the distribution of classroom rank for students with the same level of ability. Figure 1, Panel A, relies on 1<sup>st</sup> grade data, and shows quantiles of the distribution of classroom rank (y-axis) for students with a given school rank (x-axis). In practice, to construct this figure, we plot different percentiles of classroom rank within each school rank ventile.

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<sup>13</sup> See Harris and Udry 2018 for a description of the Add Health data. The questions on self-esteem in Add Health build on the much-used Rosenberg Self-Esteem Scale (Rosenberg 1989).

<sup>14</sup> The only exception is some of the achievement tests in 4<sup>th</sup> through 6<sup>th</sup> grades, which were applied in a group setting.

The figure shows that two 1<sup>st</sup> grade students with the same ability in the same school (and therefore, the same school rank), can have very different classroom ranks. For example, within the 10<sup>th</sup> ventile of school rank (with median value equal to 0.475), classroom rank varies between 0.364 (5<sup>th</sup> percentile) and 0.593 (95<sup>th</sup> percentile).

Another way to illustrate the same point is to plot percentiles of classroom rank against the residuals of a regression of national percentile rank (our measure of underlying ability) on school fixed effects. This is another way of normalizing ability within school (in Panel A, this was done by using school rank instead of actual test scores). The resulting figure is shown in Figure 1, Panel B, where we plot different percentiles of classroom rank within residualised national rank ventiles. Within the 10<sup>th</sup> ventile of residualised national rank, classroom rank varies between 0.324 (5<sup>th</sup> percentile) and 0.636 (95<sup>th</sup> percentile). A similar pattern is shown in Appendix Figures C1 and C2 for all other grades.

Another potential concern is that the students' perception of rank does not necessarily coincide with the measure of rank we use. This is an issue in this whole literature, and we do not have anything new to add. As argued in Murphy and Weinhardt (2020), the literature in psychology has established that students have accurate perceptions of their rank in a group even if they do not know their absolute ability. Furthermore, if individuals did not know their rank, we would not see an impact of rank on outcomes as we find in our paper, and as has been found in the entire literature on this subject.<sup>15</sup>

### 3. Empirical strategy

#### A. Main model

Our goal is to estimate the impact of child *i*'s classroom rank on her subsequent learning in elementary school. The dataset we use allows us to construct measures of classroom rank, lagged achievement, and achievement at the end of the current grade for children between 1<sup>st</sup> and 6<sup>th</sup> grades. With these data, we can investigate the impact of classroom rank in the short- and medium-run, starting as early as 1<sup>st</sup> grade.<sup>16</sup>

To begin our discussion, we focus on two questions. First, how should we measure rank, and which measure of rank is likely to be more relevant for future learning? Second, how do we identify the causal impact of rank on learning? The two issues are interlinked in our setting, so we discuss them together.

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<sup>15</sup> That said, it is true that the problem is one of measurement error, since objective rank may not correspond to perceived rank by students, families or teachers, which they use to make decisions, so our estimates could be biased. In fact, it is possible that multiple perceptions of rank on the same student by different individuals, families and teachers, are not only different from each other, but also operate in different ways.

<sup>16</sup> We can construct measures of math and language achievement at the end, but not the beginning, of kindergarten. For this reason, our analysis focuses on 1<sup>st</sup> through 6<sup>th</sup> grades.

In the experiment we study, in each school, children were randomly assigned to classrooms at the start of every grade. As a result, every student has a randomly-assigned set of peers in each grade, so two students with the same underlying ability can nevertheless have different classroom ranks. Our study exploits this variation by estimating the impact of rank at the beginning of grade  $t$  on learning at the end of grade  $t$ , as well as on learning in subsequent grades (until the end of 6<sup>th</sup> grade). Achievement at the start of grade  $t$  is measured using tests administered at the end of grade  $t - 1$ . Beginning-of-grade classroom rank for each student is based on her achievement at the end of  $t - 1$  and the achievement of her randomly-assigned classmates. From now on, we refer to this measure simply as classroom rank.<sup>17</sup>

Our approach assumes that students and teachers react to the beginning-of-grade student classroom rank. This makes most sense if they can perceive their rank, and act on it, fairly early during the school year. While we are not able to assess the impact of school rank as convincingly, since random assignment happens within schools, in Appendix G we explore impacts of school rank of achievement.

Throughout the paper we denote  $Y_{i,s,c,t}$  as student  $i$ 's performance (measured by an index of math and language scores), at the end of grade  $t$ , in school  $s$ , and classroom  $c$ . To be consistent with the literature, we define  $Y_{i,s,c,t}$  in terms of percentiles of national rank.<sup>18</sup>  $CR_{i,s,c,t}$  is student  $i$ 's classroom rank at the start of grade  $t$ , when the student is randomly assigned to classroom  $c$ . In our simplest model, we pool observations from all grades and estimate:

$$Y_{i,s,c,t} = \beta CR_{i,s,c,t} + g_t(Y_{i,s,c,t-1}) + \delta_{c,t} + \varepsilon_{i,s,c,t} \quad (3.1)$$

where  $\delta_{c,t}$  is a classroom (by grade) fixed effect and  $\varepsilon_{i,s,c,t}$  is a residual. This specification also allows us to address any concerns related to the fact that our experimental design generates variation in multiple factors (other than classroom rank) that could affect learning. This specification is analogous to the main approach in Murphy and Weinhardt (2020). As explained in their paper, there is variation in rank across individuals assigned to different classrooms that one can exploit even after accounting for classroom fixed effects, due to differences in the distribution of ability across classrooms.<sup>19</sup>

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<sup>17</sup> One issue we face is that we only observe end of grade  $t - 1$  scores for students who were in a school in our sample in grade  $t - 1$ , which means that we do not know what these test scores are for students who arrived at the school in grade  $t$ . Therefore, we cannot compute grade  $t$  ranks for these students, and our measures of ranks for all other children ignore the fact that new entrants are in their classroom. This will introduce random measurement error in rank.

<sup>18</sup> Percentile ranks normalize automatically by classroom (or school) size. The disadvantage of this is that being the first or last in a small or large classroom may mean something very different. In practice, however, we find no evidence that classroom rank measured in percentiles has different effects in smaller and larger classrooms.

<sup>19</sup> Note that this is not equivalent to exploring within-classroom variation. In each classroom, individual ability and classroom rank are perfectly correlated, so one would not be able to estimate this model classroom by classroom (allowing

In this model,  $\beta$  is restricted to be the same across all grades, but all other parameters are allowed to be grade-specific.  $g_t(Y_{i,s,c,t-1})$  is a third-order polynomial in  $Y_{i,s,c,t-1}$ .<sup>20</sup> We also estimate models in which, instead of pooling data for all six grades, we estimate separate coefficients on  $\beta$  for 1<sup>st</sup> and 2<sup>nd</sup> grades (“early” grades), 3<sup>rd</sup> and 4<sup>th</sup> grades (“middle” grades) and 5<sup>th</sup> and 6<sup>th</sup> grades (“late” grades).<sup>21</sup>

Next, we separately analyze the effects of classroom rank in math and language:

$$Y_{i,s,c,t}^k = \beta^k CR_{i,s,c,t}^k + g_t(Y_{i,s,c,t-1}^k) + \delta_{c,t}^k + \varepsilon_{i,s,c,t}^k \quad (3.2)$$

where the  $k$  superscript refers to a subject, math or language.

Up to this point, we have assumed that the effect of classroom rank on learning is linear in classroom rank. It is quite possible that this is not the case—it may be, for example, that rank has a different effect at the top and bottom of the (rank) distribution. Therefore, we also consider a version of equation (3.1) where the effect of classroom rank on learning is not constrained to be linear:

$$Y_{i,s,c,t} = \beta(CR_{i,s,c,t}) + g_t(Y_{i,s,c,t-1}) + \delta_{c,t} + \varepsilon_{i,s,c,t} \quad (3.3)$$

where  $\beta(CR_{i,s,c,t})$  is now a flexible function of  $CR_{i,s,c,t}$ . In our preferred specification we discretize  $CR_{i,s,c,t}$  into  $q$  values (forcing it to take  $q = 10$  values, corresponding to 10 deciles of the distribution), so  $\beta(CR_{i,s,c,t}) = \sum_{q=1}^{10} \beta_q CR_{i,s,c,t,q}$  (where  $CR_{i,s,c,t,q}$  is an indicator variable that takes value 1 if  $CR_{i,s,c,t}$  is in decile  $q$ ).<sup>22</sup>

## B. Alternative models

Other papers in this literature use different measures of rank and different specifications. A leading example is Murphy and Weinhardt (2020), who study the impact of school rank at the end of elementary school on learning in secondary school. In contrast, we study the impact of classroom rank at the beginning of a

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for classroom-specific parameters). The model is identified because it imposes that the impact of classroom rank is the same across classrooms. With this assumption, it would be identified even if we allowed for some restricted forms of interactive fixed effects, as in, for example, Bai (2009).

<sup>20</sup> As Murphy and Weinhardt (2020) emphasize, it is important to use flexible specifications for this function, to avoid the potential problem that  $\beta$  does not capture a true rank effect but is instead an artefact of the misspecification of this function. Our robustness checks show that considering polynomials in lagged scores of order higher than three, or controlling for lagged scores using percentiles of lagged national rank, does not lead to substantial changes in the results. For this reason, in our main empirical specification  $g_t(Y_{i,s,c,t-1})$  is a cubic polynomial in its argument.

<sup>21</sup> In Appendix E we also present estimates where  $\beta$  varies by grade in an unrestricted way, and therefore it is also indexed by  $t$ . Those estimates are noisier than the ones we focus on in the paper.

<sup>22</sup> As pointed out by a referee, when the classroom rank of one student increases in a classroom the classroom rank of another student in the same classroom automatically decreases. This can induce another form of spillovers in the classroom. However, since we observe the classroom rank of every student, this is automatically accounted for in our models.

grade on learning occurring in that grade. Because in Murphy and Weinhardt (2020) rank is measured at the end of elementary school, it is a result of a student’s position relative to her peers, but also of the student’s reaction to her peers and any subsequent feedback, as well as other school shocks occurring between the beginning and the end of elementary school.<sup>23</sup> If we were to use the Murphy and Weinhardt (2020) specification instead of ours (and using classroom rank instead of school rank) we would estimate:

$$Y_{i,s,c,t} = \beta CR'_{i,s,c,t-1} + g_t(Y_{i,s,c,t-1}) + \delta_{c,t} + \varepsilon_{i,s,c,t} \quad (3.4)$$

where  $CR'_{i,s,c,t-1}$  is the classroom rank at the end of grade  $t - 1$ , computed using scores at the end of  $t - 1$  relative to peers in  $t - 1$ .

The main reason why we use equation (3.1), rather than (3.4), as our preferred specification is that it follows directly from our experimental design. Classroom ranks at the start of a grade are randomly assigned and cannot be modified by student effort or other unobserved shocks, whereas classroom ranks at the end of a grade are both a result of random assignment of peers, student effort, peer effort, and potentially even the responses of teachers and parents. Also, our approach estimates the effects of classroom rank experienced in the same year as we measure learning, which is arguably more relevant in the short run for a “big fish little pond” mechanism (as in Marsh 1987).<sup>24</sup> That said, students may not know their rank at the start of the grade and may take some time to learn about it. In contrast, in Murphy and Weinhardt (2020) students are more likely to have a reasonable perception of their rank since it is measured at the end of elementary school. Therefore, we present results from estimating (3.4) as a robustness check.

### C. Dynamics

To estimate the dynamics of rank effects, we begin with specifications of the following form:

$$Y_{i,s,c,t+l} = \beta_{t,l} CR_{i,s,c,t} + g_{t+l}(Y_{i,s,c,t-1}) + \delta_{c,t} + \varepsilon_{i,s,c,t+l} \quad (3.5)$$

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<sup>23</sup> Murphy and Weinhardt (2020) document the impact of this measure on future learning, when a student moves to another school and experiences a different set of peers. In their model students are motivated to work hard in secondary school because of the rank they experienced and perceived in their past school, as opposed to their rank in the current school.

<sup>24</sup> That said, one advantage of using end-of-classroom rank in our setting would be that we can construct rank using everyone in the classroom at the end of the previous grade, while our preferred measure of beginning-of-grade rank excludes new school entrants for whom we do not have test scores at the end of the previous grade, introducing measurement error in our preferred measure of classroom rank (on average, 7.1 percent of students are new entrants in each classroom). However, as we show in the robustness checks below, our results are robust to a standard multiple imputation procedure for missing data.



We estimate these regressions separately for “early”, “middle”, and “late” grades, as discussed above. When  $l = 0$ , equation (3.5) is equivalent to (3.1) and provides estimates of the short-run impact of classroom rank at the start of grade  $t$ ,  $CR_{i,s,c,t}$ , on learning at the end of that same grade,  $Y_{i,s,c,t}$ . We label this effect  $\beta_{t,0}$ . When  $l > 0$ , equation (3.5) provides estimates of the medium-term effect of classroom rank at various lags, which we label  $\beta_{t,l}$ .

$\beta_{t,l}$  (medium-run impact) and  $\beta_{t,0}$  (short-run impact) are related through three main channels: (i) class rank in grade  $t$  affects learning at the end of that grade, and therefore also affects student achievement in grade  $t + 1$  and in subsequent grades, through the function  $g_{t+1}(Y_{i,s,c,t})$  in the  $(t + 1)$  version of equation (3.1); (ii) since learning at the end of grade  $t$  is affected, classroom rank in  $t + 1$  and subsequent grades is also affected, which can have a further impact on learning in those grades, captured by  $\beta_{t+1,0}$  in equation (3.1); (iii) in addition, class rank in grade  $t$  may affect other skills not captured by our tests at the end of that grade (unobserved skills), but which nevertheless can affect learning in grades  $t + 1$  and beyond.

To quantify the importance of these channels, we first estimate an additional equation relating classroom rank at the beginning of grade  $t + 1$  with learning at the end of grade  $t$ , which we will assume can be approximated by the following linear relationship:

$$CR_{i,s,c,t+1} = \delta_{t+1} + \gamma_{t+1}Y_{i,s,c,t} + \tau_{c,t+1} + \eta_{i,s,c,t+1} \quad (3.6)$$

where  $\tau_{c,t+1}$  is a classroom fixed effect, and  $\eta_{i,s,c,t+1}$  is the variation coming from random assignment of students to different peer groups. In practice, as we show below, when  $Y_{i,s,c,t}$  is the national percentile rank,  $\gamma_{t+j} \approx 1$ .

Suppose, for simplicity, that  $g_t(Y_{i,s,c,t-1})$  is also linear:  $g_t(Y_{i,s,c,t-1}) = \lambda_t Y_{i,s,c,t-1}$ . Taking equations (3.1) and (3.6) together, and assuming that all medium-term impacts of rank on learning operate through observed tests scores and observed rank:

$$\begin{aligned} \frac{\partial Y_{i,s,c,t}}{\partial CR_{i,s,c,t}} &= \beta_{t,0} \\ \frac{\partial Y_{i,s,c,t+1}}{\partial CR_{i,s,c,t}} &= \left( \frac{\partial Y_{i,s,c,t+1}}{\partial CR_{i,s,c,t+1}} \frac{\partial CR_{i,s,c,t+1}}{\partial Y_{i,s,c,t}} + \frac{\partial Y_{i,s,c,t+1}}{\partial Y_{i,s,c,t}} \right) \frac{\partial Y_{i,s,c,t}}{\partial CR_{i,s,c,t}} \\ &= (\beta_{t+1,0} \gamma_{t+1} + \lambda_{t+1}) \beta_{t,0} \end{aligned}$$

Similarly:

$$\frac{\partial Y_{i,s,c,t+2}}{\partial CR_{i,s,c,t}} = (\beta_{t+2,0} \gamma_{t+2} + \lambda_{t+2})(\beta_{t+1,0} \gamma_{t+1} + \lambda_{t+1})\beta_{t,0}$$

Substituting these expressions, in subsequent grades we get:

$$\frac{\partial Y_{i,s,c,t+l}}{\partial CR_{i,s,c,t}} = \beta_{t,0} \prod_{j=1}^l (\beta_{t+j,0} \gamma_{t+j} + \lambda_{t+j}) \quad (3.7)$$

Equation (3.7) tells us how the medium-term impact of rank in grade  $t$  on learning in grade  $t + j$  depends on the short-term impact of rank at the beginning of each grade on learning at the end of that grade ( $\beta_{t,0}$ ), the impact of learning in one grade on learning in the subsequent grade ( $\lambda_t$ ), and the impact of learning in one grade on classroom rank in the subsequent grade ( $\gamma_t$ ). We also note that, because (as we show below)  $\gamma_{t+j} \approx 1$ , equation (3.7) indicates that it is possible that we may see little or no decay of rank effects over time. This is because a high classroom rank early in elementary school can in principle lead to a self-fulfilling cycle, where a high rank produces high learning, which in turn leads to a high rank, which in turn leads to high learning. Observed differences between estimates of actual medium-term impacts of rank ( $\beta_{t,l}$ ), and  $\frac{\partial Y_{i,s,c,t+l}}{\partial CR_{i,s,c,t}}$  from equation (3.7) tell us about the importance of unobserved skills as mediators of the medium-term impacts of rank.

#### D. Executive function, non-cognitive skills, and teacher perceptions

To estimate effects of ability classroom rank on happiness in 1<sup>st</sup> grade and non-cognitive skills in 6<sup>th</sup> grade, we run regressions comparable to (3.1), replacing achievement in grade  $t$  with the relevant outcome.<sup>25</sup> To estimate rank effects on executive function, we also use the model in (3.1), but add to this model a third-order polynomial in lagged EF (in addition to the polynomial in lagged achievement). These regressions use information in 1<sup>st</sup> through 4<sup>th</sup> grades, where data on current and lagged EF are available. Finally, as discussed above, we have data on teacher perceptions of students—specifically, a list of the 5 students each teacher thought had the highest, and lowest, achievement in their classrooms. We generate indicator variables for children who are seen to be at the top and, separately, bottom of their classroom by their teachers, and use them as outcomes. To assess medium-term effects of classroom rank on non-cognitive

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<sup>25</sup> Child happiness is only available in 1<sup>st</sup> grade, so we run regressions of child happiness in 1<sup>st</sup> grade on math classroom rank at the beginning of 1<sup>st</sup> grade and the polynomial on math achievement at the end of kindergarten. Non-cognitive skills are only available at the end of 6<sup>th</sup> grade, so it is not obvious whether we should regress these skills on rank in 1<sup>st</sup> grade, 6<sup>th</sup> grade, or any grade in between. Because (as we argue below) we are most interested in possible medium-term effects of early rank (rank in 1<sup>st</sup> and 2<sup>nd</sup> grade), and to be consistent with the results on child happiness, we report the results of 6<sup>th</sup> grade non-cognitive skills on 1<sup>st</sup> grade math classroom rank.

skills, executive function and teacher perceptions we also run regressions analogous to (3.5), replacing achievement in grade  $t + l$  with the relevant outcome.

## 4. Results

### A. Graphical evidence

To motivate our analysis, we start with some simple figures. For this purpose, we first sort children into ventiles on the basis of their test scores in math and language at the end of grade  $t - 1$  (say, end of kindergarten). Then, within each ventile, we calculate average test scores at the end of grade  $t$  (end of 1<sup>st</sup> grade) for two groups of children: those who, relative to other children in that ventile, were randomly assigned to classrooms where their rank at the beginning of  $t$  was “high”—classroom rank in the top 25 percent for that ventile—and those in classrooms where their rank was “low”—in the bottom 25 percent for that ventile. If classroom rank has a positive effect on test scores, we would expect the line that corresponds to high-ranked children to be above that which corresponds to low-ranked children.

Results are shown in Figure 2. Panel A focuses on the short-term effects of classroom rank in 1<sup>st</sup> grade. The figure shows that children with high classroom ranks have higher achievement than those with low classroom ranks, but only above the 40<sup>th</sup> percentile of the distribution of national rank. Panel B compares these two groups of children at the end of 3<sup>rd</sup> grade. The figure shows that the vertical distance between the two lines is larger than in Panel A, indicating that the effect of 1<sup>st</sup> grade classroom rank increases over time.

Panel C focuses on the short-term effects of classroom rank in 4<sup>th</sup> grade. Children with higher classroom ranks appear to have higher achievement at the end of 4<sup>th</sup> grade, but the difference is quite small. Panel D focuses on these same children at the end of 6<sup>th</sup> grade. The lines in this panel are very similar to those in Panel C, suggesting that the (modest) effects of 4<sup>th</sup> grade classroom rank do not grow over time. In sum, Figure 2 suggests that: the effects of classroom rank are larger in 1<sup>st</sup> grade than in 4<sup>th</sup> grade; the 1<sup>st</sup> grade effects are larger in the upper half of the distribution; and these effects increase over time.

### B. Static model

Table 2 reports estimates of the effect of classroom rank on learning, measured by an index of math and language, using equation (3.1) above. Column (1) shows that the coefficient on  $\beta$  is .030, with a standard error of .008.<sup>26</sup> This implies that moving a child from the 50<sup>th</sup> to the 60<sup>th</sup> percentile of classroom rank increases her end-of-grade achievement by 0.30 percentiles of the national distribution. To get a sense of

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<sup>26</sup> Standard errors for all models are clustered at the school level.

magnitude, we take all children who are in the same school and grade, who have the same value of lagged achievement in  $t - 1$ , but are assigned to different classrooms in grade  $t$ . The (absolute value) of the median difference in classroom rank between children in these pairs is 5.5 percentiles (that is, on average, in these pairs of identical children, one child has a classroom rank of 47.25 and the other has a rank of 52.75. At the 75<sup>th</sup> and 90<sup>th</sup> percentiles of the difference, the values are 9.8 and 14.7 percentiles, respectively).

In columns (2) and (3), we report estimates of the effects of classroom rank in math on achievement in math and language. In columns (4) and (5) we report estimates of the effects of classroom rank in language on achievement in language and math. Finally, in columns (6) and (7) we report estimates of the effects of classroom rank in math and classroom rank in language on achievement in language and math. The table shows that classroom rank in math affects math achievement (coefficient of .040, with a standard error of .010) and, to a lesser extent, language achievement; in contrast, classroom rank in language does not affect either math or language achievement. The results are robust to regressing math and language achievement on classroom rank in math and classroom rank in language in a joint fashion.<sup>27</sup>

Appendix Table E1 reports the results from a number of robustness checks. To facilitate comparisons, column (1) reproduces the coefficient and standard error from column (2) in Table 2. Column (2) shows that, as expected given the random assignment, including controls for child gender, as well as age and its square, does not affect our results. Columns (3) to (6) report estimates where  $g_t(Y_{i,s,c,t-1})$  is specified as a polynomial of orders 1, 2, and 4, and using dummies for percentiles of lagged national rank respectively (as opposed to our main estimates, in which we include a cubic in lagged achievement). The estimated classroom rank effects are larger when we include only linear or quadratic terms in lagged achievement. Reassuringly, however, the coefficient on classroom rank is essentially unchanged when we include a polynomial of order 4, or dummies for percentiles in lagged achievement as a control (rather than a polynomial of order 3).

In column (7), of Table E1 we present estimates of equation (3.4). As we discuss above, in this specification—which is similar in character to that used by Murphy and Weinhardt (2020)—classroom rank refers to rank at the end of  $t - 1$ , rather than at the beginning of  $t$ . Table E1 shows that these

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<sup>27</sup> We do not know why math rank, but not language rank, affects achievement. It is in principle possible that classroom rank in math is more visible to students and teachers than is the case with language rank. However, we do not find strong evidence that this is the case in our setting. As we discuss below, teachers appear to place similar weights on math and language achievement in determining which students in their class have the highest and lowest achievement. We note that it is not uncommon in the literature to find larger effects of school-based interventions on math than on language (see the discussion in Fryer 2017). In the U.S., teachers have larger effects on math than on language achievement (Hanushek and Rivkin 2010).

estimates are substantially larger than those from our preferred specification.<sup>28</sup> The approach to classroom rank we take therefore yields conservative estimates of rank effects on learning.

At this point it is important to mention the potential impact of selective attrition on our estimates. There are between 14,322 (kindergarten) and 17,529 (5<sup>th</sup> grade) students per grade in our data. These are not always the same students. Typically, from one year to the next, between 7.5 and 10 percent of students leave our sample of schools (the exception is the transition from kindergarten to 1<sup>st</sup> grade, where this number is 15 percent).<sup>29</sup> Selective attrition out of our sample of schools could generate a correlation between  $CR_{i,s,c,t}$  and  $\varepsilon_{i,s,c,t}$ , which may bias estimates of the effect of classroom rank. To assess whether our estimates are likely to be affected by these considerations, we use a standard multiple imputation procedure, as described, for example, in Little and Rubin (2019). We describe the implementation of the procedure in detail in Appendix D. We find that the main estimates of the effect of contemporaneous classroom rank in math on math achievement are robust to this procedure (estimated coefficient is .040 with a standard error of .015).<sup>30</sup>

### C. Heterogeneity

We begin our analysis of heterogeneity by estimating effects at different points in the distribution of classroom rank, again focusing on the effects of math classroom rank on math achievement. In Figure 2 we graph coefficients and confidence intervals on deciles 1 through 4, and 6 through 10 from equation (3.3) above, with decile 5 as the omitted category. The figure shows that there is essentially no impact of classroom rank in the bottom half of the distribution. For example, we cannot reject the null that being in the lowest decile of classroom math rank has the same effect on achievement as being in the middle of the distribution of rank. The coefficients that correspond to deciles 6 through 10, on the other hand, are all positive, and are generally larger in the higher deciles (so that the coefficient for the 10<sup>th</sup> decile is larger in magnitude than that for the 7<sup>th</sup> decile). In this case, we can reject the null that being in the highest decile of classroom math rank has the same effect on achievement as being in the middle of the distribution.

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<sup>28</sup> It is interesting that classroom rank effects estimated by (3.4) are larger than those estimated by (3.1). As mentioned above, this could happen because children are more aware of end-of-grade rank than beginning-of-grade rank and therefore react more to it, or because end-of-grade rank captures other aspects of the school experience besides rank.

<sup>29</sup> Similarly, in any given grade, between 7 and 13 percent of students are new entrants to the sample (with the exception of 1<sup>st</sup> grade, where this value is 24 percent). New students are randomly assigned to classrooms just like any other students.

<sup>30</sup> Using the baseline data, we can conduct a balancing exercise to test for the presence of correlation between pre-determined child characteristics and classroom rank at the beginning of any given grade, which complement the random assignment tests in Appendix A. As in Denning et al (2023), Elsner et al (2021), or Elsner and Isphording (2018), we regress baseline variables on classroom rank and the remaining controls in our main models, as described above. Given the time discrepancy between the measurement of rank and the measurement of baseline characteristics, selective “attrition” (every year not only existing students move to another school, but new students enter the school and are incorporated in the randomization of students to classrooms) is an issue, so the balancing tests correct for it using multiple imputation. The results of this exercise are presented in Appendix Table D2.

In Table 3, we analyze other possible sources of heterogeneity in the effect of classroom rank. We first focus on gender. In theory, if girls have different levels of self-confidence than boys (as in Bordalo et al. 2019), or react differently to competition than boys (as in Niederle and Vesterlund 2011), it is possible that they react differently to rank. In the first column, we present estimates of a specification where all the coefficients in equation (3.2) for math are interacted with gender. Girls have significantly lower math scores than boys, but the impact of classroom rank on learning is the same for girls and boys.

We also interact classroom rank with vocabulary at the beginning of kindergarten, or with lagged achievement. These results show that classroom rank effects are substantially and significantly larger for children with higher baseline vocabulary levels, as well as for those with higher lagged achievement. In sum, and consistent with the results in Figure 2, Table 3 shows that classroom rank seems to have larger impacts for higher-achieving children. Finally, we interact classroom rank with variables capturing socio-economic status. Specifically, we run regressions in which we interact classroom rank in math with an indicator for mothers having less than secondary education, an indicator for whether the child attended preschool, and an indicator for household wealth being above the median. Table 3 shows that classroom rank effects are stronger among children who attended preschool, and weaker for children of lesser educated mothers.

#### D. Dynamics

We begin our analysis of dynamics by estimating effects of classroom rank separately for children in the “early” grades (1<sup>st</sup> and 2<sup>nd</sup> grade), “middle” grades (3<sup>rd</sup> and 4<sup>th</sup> grade), and “late” grades, both contemporaneously (without lags, as in equation (3.1) above) and at various lags (as in equation (3.5)). These results are in Table 4.<sup>31</sup>

Column (1) shows that the short-term effect of math classroom rank in the early and middle grades are both positive and of a comparable magnitude. On the other hand, classroom rank in 5<sup>th</sup> and 6<sup>th</sup> grades

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<sup>31</sup> In Appendix E we report results from estimating rank effects by grade, rather than by aggregating estimates into “early”, “middle”, and “late” grades.

has no effect on achievement.<sup>32</sup> We test and reject the null that the coefficients on the early, middle, and late grades are the same (p-value: .025).<sup>33</sup>

We next turn to the evolution of rank effects over time. Specifically, in columns (2) through (5), we report estimates of the effect of classroom math rank on math achievement after (up to) 1, 2, 3, and 4 lags, respectively. In the first row, corresponding to rank in the early grades, the coefficients increase monotonically over time—the impact after 4 lags is .105 (with a standard error of .022), almost twice as large as the short-term effect. However, we cannot reject the null that the coefficients for lag 0 and lag 4 are the same (p-value: .106). On the other hand, the coefficients in the second row of the table show that the classroom rank effects in the middle grades decline, although we cannot reject the null that the coefficients for lag=0 and lag=2 are the same (p-value: .630).<sup>34</sup>

Table 4 shows suggestive evidence that the effects of classroom rank in the early grades increase substantially over time.<sup>35</sup> Given these results, we now ask the following question: can we account for the increase in the effect of early classroom rank using estimates of  $\beta_{t,0}$  (short-term impact of classroom rank),  $\gamma_t$  (impact of learning on future rank), and  $\lambda_t$  (impact of lagged skills on current skills)?

Estimates of  $\beta_{t,0}$ ,  $\gamma_t$ , and  $\lambda_t$  for each grade  $t$  show that  $\gamma_t \approx 1$  but  $\beta_t + \lambda_t < 1$  for every grade, which means that, if rank operated primarily through short-term learning gains and the resulting improvement in subsequent rank, we should observe fade-out in the impacts of rank on learning.<sup>36</sup> As we

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<sup>32</sup> This result is perhaps surprising, and not shown before in other papers. It is possible that the impact of rank may depend on how long one has to learn about it and experience it, since what matters for the student's reaction to rank is his or her perception of rank. In our setting, but not in other settings where the effect of rank has been studied, there is annual reshuffling of peers, so it may be more difficult for children to learn about their rank. Therefore, the long-term impact of rank within a stable peer group could differ from the long-term impact of a rank that changes more frequently. In addition, potentially the peer group with which to compare oneself goes from the classroom to the entire school as one experiences more and more peers. One reason all this could dampen the measured impact of objective rank, is that it could blur the connection between perceived and objective rank. However, this also depends on the degree to which rank is perceived to be stable by the individual, and the extent to which they believe they can change rank. We have also estimated a specification where we look at school rather than classroom rank (Table G1 in Appendix G). Our results are analogous to the ones shown here for classroom rank.

<sup>33</sup> Our results show a lack of effects of late classroom rank on achievement. This is somewhat in contrast with other papers in the literature, which find effects of rank on outcomes measured later in life (see Delaney and Devereux (2022) for a review of the literature). The impact of rank may depend on how much time a student has to learn about their rank and experience its effects. Therefore, the long-term impact of rank within a stable peer group may well differ from the long-term impact of a rank that changes more frequently, as is the case in our setting, where random assignment to a classroom happens at the beginning of every grade.

<sup>34</sup> We do not know why the effects of rank in the early grades increase, while those in the middle grades do not. We note, however, that some theories of human capital argue that earlier investments tend to have the largest effects (as in Cunha and Heckman 2007), in part because earlier investments allow children to better take advantage of later investments. It is possible that early classroom rank affects achievement in this way.

<sup>35</sup> Appendix Table D3 shows that the estimated effects of early classroom rank on later grade achievement are robust to a standard multiple imputation procedure (as in Little and Rubin (2019)) which corrects for selective attrition.

<sup>36</sup> One could think that the result that  $\gamma_t \approx 1$  is an automatic consequence of random assignment. In principle this does not necessarily happen because national and classroom rank could have very different scales, especially if there is non-random sorting of students into schools. However, if schools look very similar in the types of students they get, it may

have seen in Table 4, this is not the case. Rather, our results suggest that classroom rank operates at least in part by producing sustainable changes in another unobserved skill, which has independent effects on learning.

A useful way to make this point is in Figure 4, which plots the implied change in learning in grades  $t + l$ , as a response to an exogenous change in early (1<sup>st</sup> or 2<sup>nd</sup> grade) achievement ( $t = 0$ ) percentile rank by 10 points, under two scenarios: (i) using the estimates of  $\beta_{t+l}$  (for  $l = 0, 1 \dots 4$ ) from equation (3.5), labeled *reduced form* in the figure; and (ii) using the estimates of  $\gamma_t$ ,  $\beta_t$  and  $\lambda_t$ , (for several values of  $t$ ) from equations (3.1) and (3.6), and then simulating the response to a particular change in rank using the equations (3.7), labeled *structural* in the figure. The figure shows that, under scenario (i), the effect of rank grows over time, while in scenario (ii) it does not.<sup>37</sup>

In sum, Figure 4 suggests that early classroom rank affects future achievement through channels that are not modelled explicitly in our equations, such as through its impact on other unobservable skills. We examine impacts on non-academic skills in the next section.<sup>38</sup>

In appendix Tables F1 and F2 we report results from additional exercises that exploit the panel nature of our dataset to examine the impacts of changes in classroom rank on national rank in math. The regressions in appendix Table F1 are run for “early” (1<sup>st</sup> and 2<sup>nd</sup>), “middle” (3<sup>rd</sup> and 4<sup>th</sup>) and “late” (5<sup>th</sup> and 6<sup>th</sup>) grades. Panel A of appendix Table F1 shows results from an exercise in which we regress national rank in math on contemporaneous classroom rank in math, lagged classroom rank in math and an interaction between the two, pooling data across grades. These regressions control for third-order polynomials in lagged national rank in math and in national rank in math measured in period  $t - 2$ , as well as classroom-by-grade fixed effects. Although all coefficients are positive, they are imprecisely estimated and we cannot reject that the model is linear in classroom rank.

Panel B of appendix Table F1 shows results from an exercise analogous to that in Elsner et al. (2021), estimating the impacts of changes in classroom rank over time on national rank in math. The results from this exercise show that, for middle grades, a positive change in classroom rank over time has a positive impact on national rank in math, while a negative change in classroom rank over time has a negative impact on national rank in math. Finally, appendix Table F2 shows results from another exercise analogous to

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happen that the distribution of classroom and national rank look similar, and that random assignment leads automatically to  $\gamma_t \approx 1$ .

<sup>37</sup> For the purpose of illustration, we normalize the estimate of  $\beta_{t,0}$  to be the same across the two scenarios.

<sup>38</sup> Throughout this analysis we assume that rank affects actual achievement as opposed to affecting only test-taking skills. Therefore, we interpret our results as saying that there is a virtuous cycle in learning as opposed to a virtuous cycle in test-taking confidence. In reality, we cannot distinguish the two. However, notice that the tests administered in our data are low stakes tests, and students may be relaxed when taking them. Furthermore, in the earlier grades, tests are administered individually by an enumerator, which may also help attenuate any issues related to test-taking confidence.



that in Elsner et al. (2021), estimating the cumulative impacts of changes in classroom rank between first and 6<sup>th</sup> grade on national rank in math at the end of 6<sup>th</sup> grade. We control for a third-order polynomial in national rank in math in kindergarten and classroom fixed effects. The results show positive and increasing cumulative effects of positive changes in classroom rank over time, and increasingly negative cumulative effects of negative changes in classroom rank over time.<sup>39</sup>

Appendix Table G1 reports estimates to a model analogous to equation (3.5), but using school rank as our main regressor of interest (instead of classroom rank), and controlling for school-by-grade fixed effects (instead of classroom-by-grade fixed effects). While we do not have random assignment at the school level, we can estimate the impact of school rank on achievement assuming that conditional on lagged ability and the school fixed effect, a student’s rank within the school is random. As Table G1 shows, here too we find that the impact on achievement of rank in “early” grades is stronger than the impact of rank in “late” grades. In addition, we show that early school rank has persistent and growing impacts on achievement in later grades. These estimates are qualitatively similar to our main dynamic estimates presented in Table 4.<sup>40</sup> The fact that early rank has long-lasting effects on achievement may indicate that children learn about their rank during the first years of school, and their perception of their ranking persists over time.

#### E. Executive function, non-cognitive skills, and teacher perceptions

As discussed above, there is a large literature in child psychology that argues that executive function is a key determinant of learning (Anderson 2002; Espy 2004; Senn et al. 2004). In our data, too, EF in a given grade predicts future achievement.<sup>41</sup> It is therefore of interest to analyze whether classroom rank improves executive function. Table 5 shows estimated effects of classroom rank in math on executive function (standardized to have mean zero and unit standard deviation) separately for children in the “early” and “middle” grades, both contemporaneously and at various lags.<sup>42</sup> Below the standard errors in parentheses

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<sup>39</sup> It is perhaps difficult to reconcile the results in tables F1 (panel B) and F2. However, notice that in F2 we are only controlling for first period ability in the regression, and therefore, changes in classroom rank capture both a change in rank due to a change to randomly assigned peers, and changes in rank due to changes in ability which can occur for several reasons, which could potentially affect the interpretation of these coefficients as measuring exclusively the impact of changes in rank.

<sup>40</sup> They are however larger in absolute value, which could perhaps suggest that school rank has a larger impact on learning than classroom rank.

<sup>41</sup> We do not have the data to identify the causal effect of executive function on learning. Nevertheless, in a regression of achievement in grade  $t$  on executive function in grade  $t - 1$ , including school fixed effects, the coefficient on EF is .538 (with a standard error of .005). If, in addition, we control for achievement at the end of  $t - 1$ , the coefficient on EF is .096 (with a standard error of .002).

<sup>42</sup> We did not apply executive function tests past 4<sup>th</sup> grade.

we report p-values computed based on the Romano-Wolf stepdown procedure using 5,000 bootstrap replications (see Romano and Wolf, 2005 and Clarke, Romano and Wolf, 2020).

The first column of Table 5 shows that the coefficients on contemporaneous classroom math rank are positive for both early and middle grades, albeit not significant. Looking across rows, we find that early classroom rank has a positive and significant effect on executive function after one lag. The estimated effect implies that an increase in early classroom rank from the 50<sup>th</sup> to the 60<sup>th</sup> percentile of the distribution improves executive function by 2.65 percent of a standard deviation. However, the effect of early classroom rank on executive function fades out after two lags (the estimated coefficient is .054 with standard error .077).

Table 6 reports the marginal effects from ordered probit regressions of child happiness on math classroom rank in 1<sup>st</sup> grade. These results show that moving a child from the 50<sup>th</sup> to the 60<sup>th</sup> percentile of the distribution of classroom rank increases the probability that a child says she is always happy in school by 1.4 percentage points.

Appendix Table H1 reports the results from regressions of 6<sup>th</sup> grade non-cognitive skills on math classroom rank in “early” (1<sup>st</sup> and 2<sup>nd</sup>), “middle” (3<sup>rd</sup> and 4<sup>th</sup>) and “late” (5<sup>th</sup> and 6<sup>th</sup>) grades. While all estimated coefficients are positive, they are not significant (with the exception of the effect of late classroom rank on the aggregate non-cognitive skills index, which is estimated to be .267 with standard error .157, and the self-esteem component, which is estimated to be .287 with standard error .149).

Finally, we turn to teacher perceptions. Teacher ratings are informative about student performance. Although the top (bottom) 5 students reported by the teacher are not always the 5 highest (lowest) performing students in the tests we administer, there is some overlap.<sup>43</sup> The fact that these two measures are only imperfectly correlated could in part be a result of measurement error in either one of them. It is also possible, however, that in assessing “achievement” teachers are in fact taking account of a broader or somewhat different construct, i.e., teacher perceptions do not measure exactly the same thing as the tests. Furthermore, if teachers perceive highly-ranked children to be particularly high achieving—even conditional on their actual ability—they may reinforce academic self-concept of highly-ranked children, and thus contribute to the impact of rank on learning outcomes we observe in both the short- and medium-run.

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<sup>43</sup> There is no evidence that teachers make more use of math or language achievement in assessing who are top and bottom students: The correlations between being in the top 5 by measured math achievement and language achievement, on the one hand, and having a teacher report a student as being in the top 5 are 0.38 and 0.33, respectively, while the correlations between being in the bottom 5 by measured math achievement and language achievement, on the one hand, and having a teacher report a student as being in the bottom 5 are 0.38 and 0.41, respectively.

In Table 7 we report the results of regressions of teacher perceptions on (randomly assigned) classroom rank in math, conditional on achievement in math. We pool across “early” (1<sup>st</sup> and 2<sup>nd</sup>), “middle” (3<sup>rd</sup> and 4<sup>th</sup>), and “late” (5<sup>th</sup> and 6<sup>th</sup>) grades. Here too we report p-values computed based on the Romano-Wolf stepdown procedure using 5,000 bootstrap replications. In column 1 of Table 7, we show that, in a regression that controls for achievement at the end of  $t - 1$ , higher classroom rank at the start of grade  $t$  increases the probability that a child is seen as a top student by her teacher in  $t + 1$  for early, middle and late grades. Moreover, in column (5) we show that higher classroom rank at the start of grade  $t$  in early grades reduces the probability that the student is seen as a bottom student by her teacher in  $t + 1$ . Looking across rows, we find that the effect of early and middle classroom rank on the probability of being seen as a top student by a teacher is positive and significant after several lags, but tends to fade out for early rank. Interestingly, a higher classroom rank in early grades significantly reduces the probability of being seen as a bottom student after 3 lags.<sup>44</sup>

In sum, we show that children who, as a result of random assignment, have higher math classroom rank have higher levels of executive function, are more likely to be happy, and are also perceived to be higher-achieving by their future teachers.

## 5. Conclusion

This paper analyzes the impact of classroom ability rank measured at the start of the academic year on learning during that year and learning in subsequent years. In our data, which comes from a longitudinal study of students in elementary schools in Ecuador, two students with the same underlying ability and attending the same school can have different classroom ranks because they are randomly assigned to different classrooms, with slightly different peers.

We measure classroom rank and learning in math and language. Beginning-of-grade classroom rank and end-of-grade achievement are available for all grades from 1<sup>st</sup> to 6<sup>th</sup> grade. We also observe executive function in kindergarten through 4<sup>th</sup> grade, self-reported child happiness in 1<sup>st</sup> grade, and non-cognitive skills at the end of 6<sup>th</sup> grade. In addition, we have data on teacher perceptions of student ability in every grade between kindergarten and 6<sup>th</sup> grade.

We show that classroom rank has modest positive short-term effects on achievement. Estimated effects of classroom rank can be confounded by the effects of peer quality. Students randomly assigned to classrooms with better peers will in general have lower classroom ability rank, but potentially benefit from

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<sup>44</sup> In table H2 in the appendix we include teacher perceptions of rank and student rank simultaneously in our model of learning. Both variables are important predictors of learning, and interestingly, the coefficient on student rank is only slightly affected by the inclusion of teacher perceptions of rank.

better peers. For this reason, our main model specification controls for classroom-by-grade fixed effects. The conflation of rank and peer quality effects is a feature of any study where both change at the same time, such as studies of the impact of selective schools, affirmative action, or neighbourhood effects.

We also show that classroom rank in math, but not language, affects achievement. The impact of classroom rank in math is larger for younger children and grows substantially over time. Moving a child from the 50<sup>th</sup> to the 60<sup>th</sup> percentile of classroom rank in 1<sup>st</sup> (2<sup>nd</sup>) grade increases her achievement in 5<sup>th</sup> (6<sup>th</sup>) grade by 1 percentile of the national distribution. The increase in the magnitude of rank effects is remarkable given the evidence that impacts of many other interventions in elementary school fade out over time. Exogenous changes in classroom math rank also improve executive function, happiness, and teacher perceptions of students. Changes in these skills, or others that we do not observe, are likely to be important in explaining how classroom rank raises child learning.

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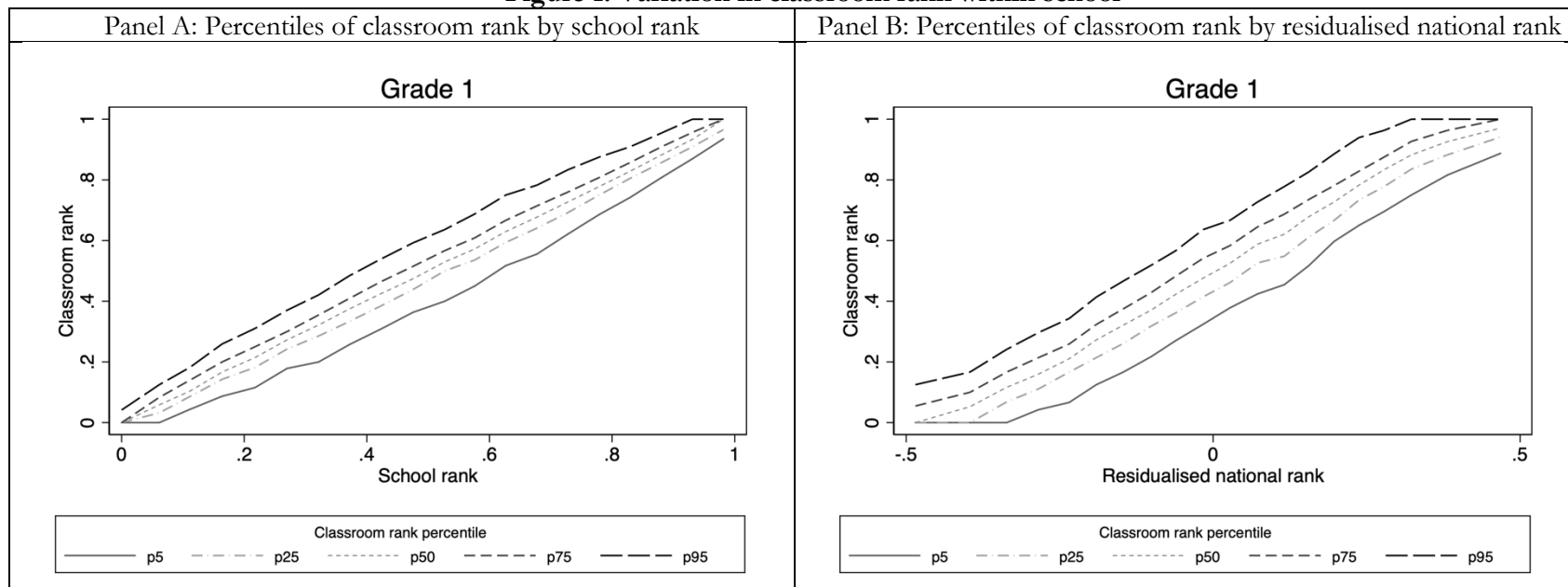
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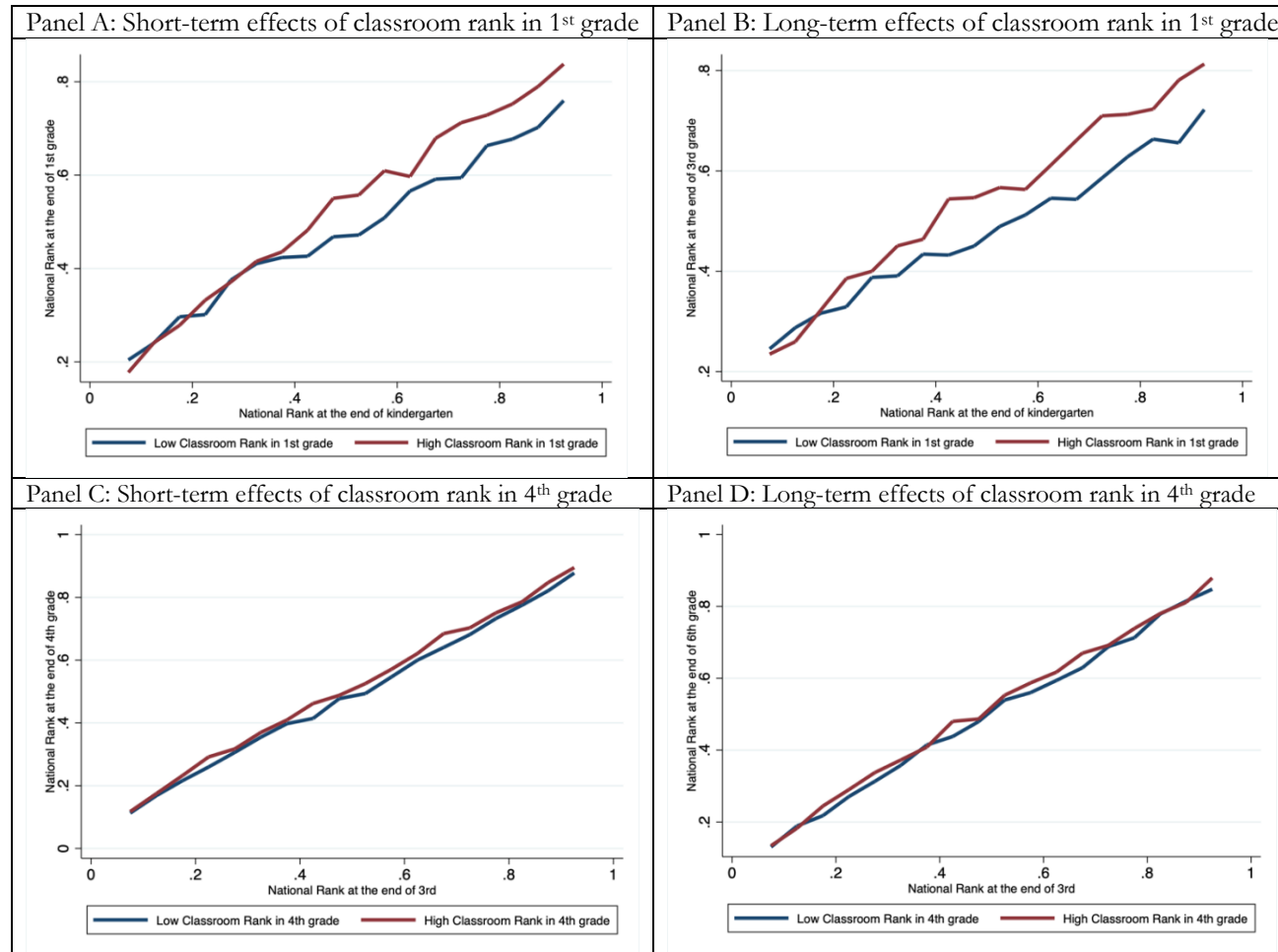


**Figure 1: Variation in classroom rank within school**



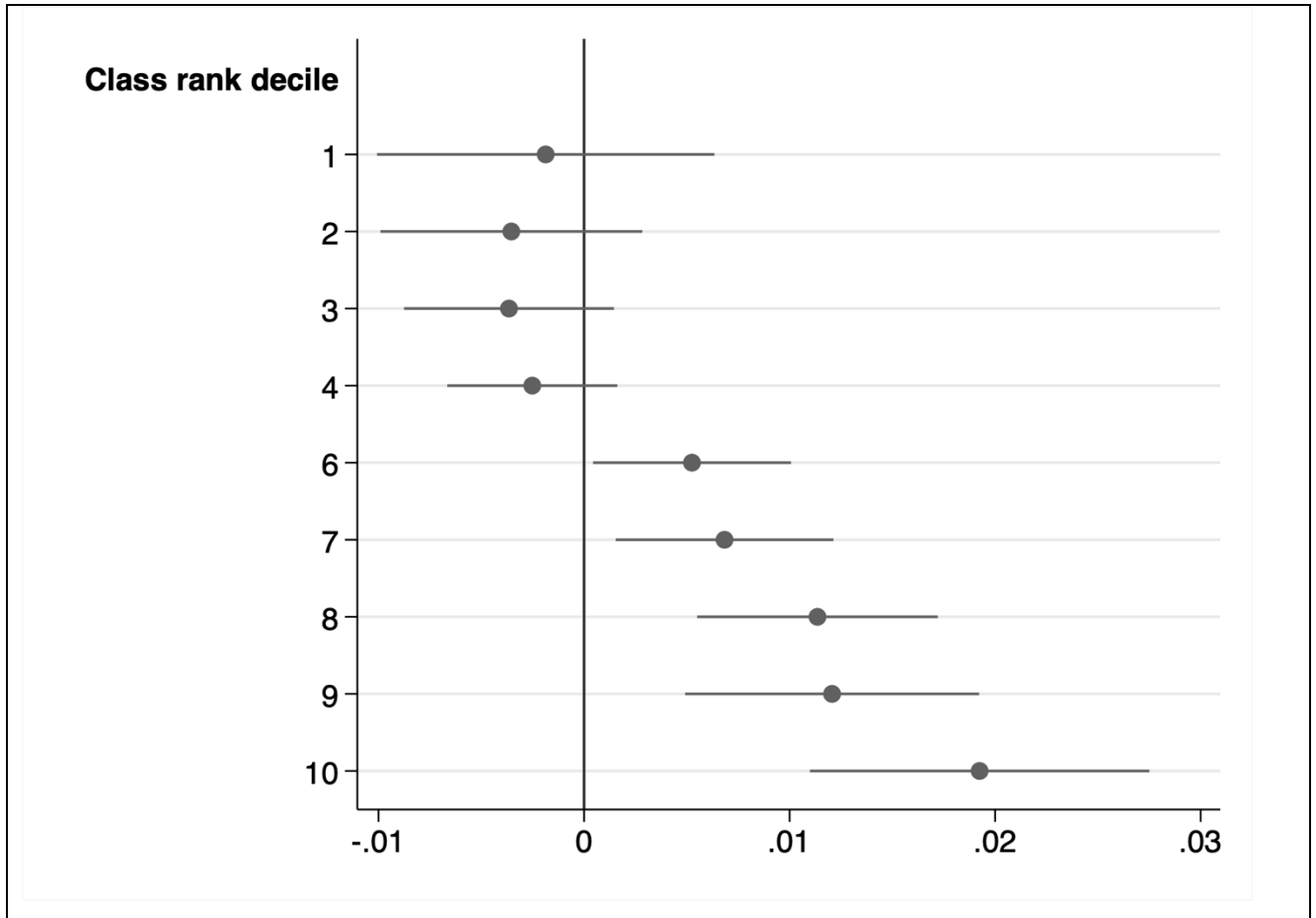
*Notes:* To construct the figure in Panel A we plot the 5<sup>th</sup>, 25<sup>th</sup>, 50<sup>th</sup>, 75<sup>th</sup> and 95<sup>th</sup> percentile of classroom rank within each ventile of school rank, against the median school rank in that ventile. To construct the figure in Panel B, we first regress national ability rank on school fixed effects and derive residuals. Then we plot percentiles of classroom rank within each ventile of residualised national ability rank, against the median residualised national rank in that ventile.

**Figure 2: Visual evidence of classroom rank effects on achievement**



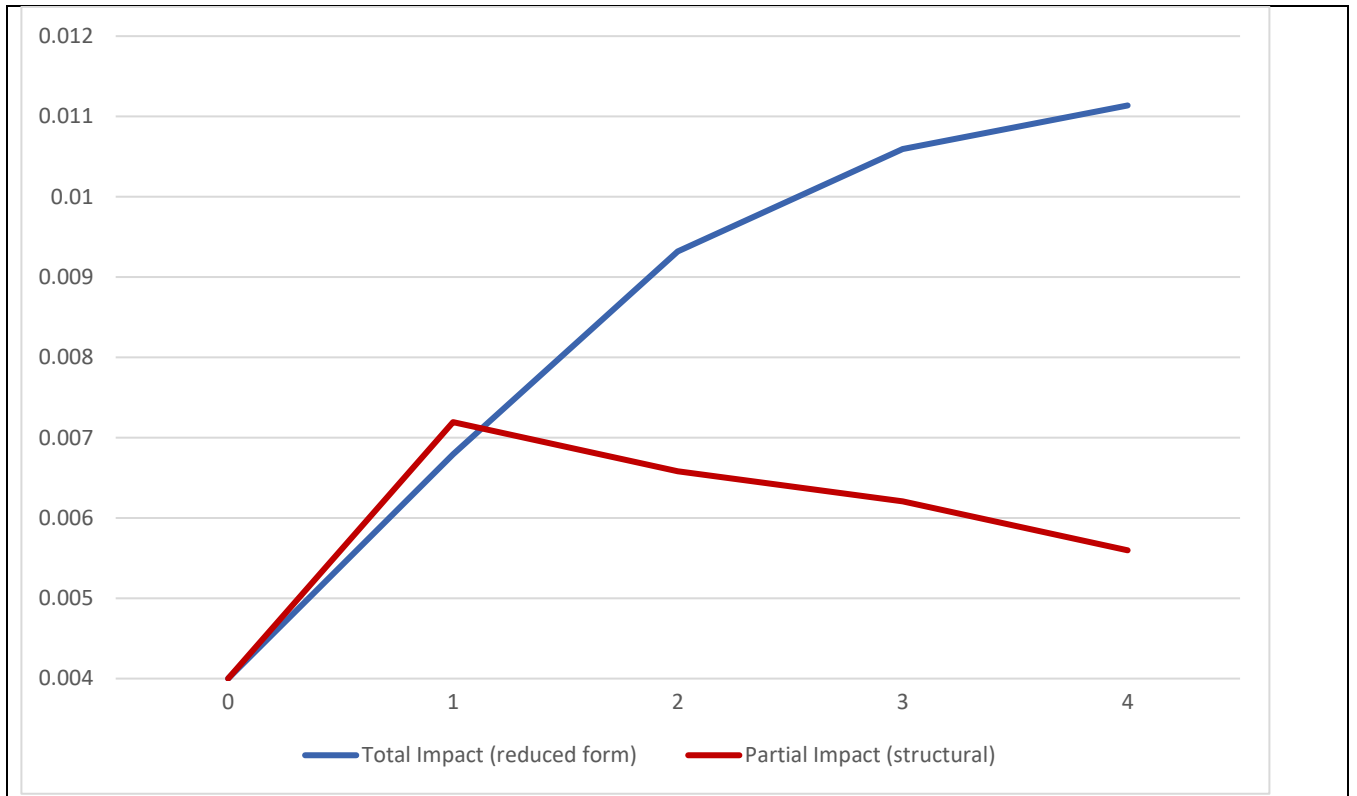
*Notes:* To generate this figure, we first sort children into ventiles on the basis of their test scores at the end of grade  $t - 1$ . Then, for each ventile, we calculate average test scores at the end of grade  $t$  for two groups of children: those who, relative to other children in that ventile, were randomly assigned to classrooms where their rank at the beginning of  $t$  was “high”—classroom rank in the top 25 percent for that ventile—and those in classrooms where their rank was “low”—in the bottom 25 percent for that ventile. Panel A focuses on the short-term effects of classroom rank in 1<sup>st</sup> grade, and Panel B compares these two groups of children at the end of 3<sup>rd</sup> grade. Panel C focuses on the short-term effects of classroom rank in 4<sup>th</sup> grade, and Panel D focuses on these same children at the end of 6<sup>th</sup> grade.

Figure 3: Classroom rank effects at different deciles of the distribution of achievement



*Notes:* To produce this figure we discretize classroom rank in math into 10 deciles and run regressions of math achievement on classroom rank deciles. We graph coefficients and 90 percent confidence intervals on deciles 1 through 4, and 6 through 10, with decile 5 as the omitted category. All regressions include a third order polynomial in lagged national rank and classroom-by-grade fixed effects. All regressions are limited to schools in which there are at least two classrooms per grade. Standard errors are clustered at the school level throughout.

Figure 4: Total and partial effects of rank on achievement



Notes: The figure plots the implied change in learning in grades  $t + l$ , as a response to an exogenous change in early (1<sup>st</sup> or 2<sup>nd</sup> grade) achievement ( $t = 0$ ) percentile rank by 10 points, under two scenarios: (i) using the estimates of  $\beta_{t+l}$  (for  $l=0, 1 \dots 4$ ) from equation (3.7) in the main text (“total impact”); and (ii) using the estimates of  $\gamma_t, \beta_t$  and  $\lambda_t$ , (for several values of  $t$ ) from equations (3.1) and (3.8) in the main text, and then simulating the response to a particular change in rank using the equations (3.9) (“partial impact”). We normalize the estimate of  $\beta_{t,0}$  to be the same across the two scenarios.

**Table 1: Child, teacher, and classroom characteristics**

	Mean	Standard deviation	N
<b>Child and household characteristics</b>			
Age of child (months)	60.3	4.9	13,858
Gender of child	0.49	0.50	14,477
Receptive vocabulary score (TVIP)	83.3	16.9	13,733
Mother's years of completed schooling	8.8	3.8	13,627
Father's years of completed schooling	8.5	3.8	10,594
Mother's age	30.2	6.6	13,637
Father's age	34.6	7.9	10,620
Proportion who attended preschool	0.61	0.49	14,472
Household has piped water in home	0.83	0.38	14,407
Household has flush toilet in home	0.46	0.50	14,407
<b>Teacher and classroom characteristics</b>			
Proportion female	0.82	0.38	2830
Proportion tenured	0.82	0.38	2818
Years of experience	18.1	10.5	2820
Class size	36.2	6.4	2838

*Notes:* The table reports means and standard deviations of the characteristics of children entering kindergarten in 2012, measured at the beginning of the school year, and of the teachers they had between kindergarten and 6<sup>th</sup> grade. The TVIP is the *Test de Vocabulario en Imágenes Peabody*, the Spanish version of the Peabody Picture Vocabulary Test (PPVT). The test is standardized using the tables provided by the test developers which set the mean at 100 and the standard deviation at 15 at each age.

**Table 2: Classroom rank effects, math-language index and separating math and language**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Math+Language index	Math	Language	Math	Language	Math	Language
Classroom rank in math+language index	.030*** (.008)						
Classroom rank in math		.040*** (.010)	.030*** (.011)			.037*** (.009)	.024*** (.009)
Classroom rank in language				.017 (.012)	.003 (.009)	-.000 (.009)	-.006 (.009)
N	87,706	87,713	87,760	87,740	87,789	87,709	87,756

*Notes:* The table reports estimates from regressions of achievement national rank on classroom rank, pooling observations across grades. For the regression in Column (1), national rank and classroom rank are calculated on the basis of the index averaging across math and language scores, and a third-order polynomial in lagged national rank in the index averaging across math and language achievement is included as a control. For Columns (2) and (3), classroom rank is calculated on the basis of math test scores, and a third-order polynomial in lagged math achievement is included as a control. For the regressions in Columns (4) and (5), classroom rank is calculated on the basis of language test scores, and a third-order polynomial in lagged language achievement is included as a control. For the regressions in Columns (6) and (7), we include classroom rank calculated on the basis of math and language achievement, and a third-order polynomial in lagged math and language achievement is included as a control. All regressions control for classroom-by-grade fixed effects. All regressions are limited to schools in which there are at least two classrooms per grade. Standard errors are clustered at the school level throughout. \*Significant at 10%, \*\*significant at 5%, \*\*\*significant at 1%.

**Table 3: Heterogeneity of math classroom rank effects**

	(1)	(2)	(3)	(4)	(5)	(6)
	Gender	Baseline Vocabulary	Lagged national rank	Mother has less than secondary education	Attended preschool	Household wealth above median
Classroom rank	.039*** (.010)	.036*** (.012)	.010 (.016)	.045*** (.012)	.042*** (.012)	.046*** (.012)
Main covariate effect	-.006*** (.002)	.020*** (.001)	1.02*** (.034)	-.013*** (.003)	-.021*** (.008)	.017*** (.002)
Interaction (rank*covariate)	.002 (.003)	.007*** (.003)	.060** (.026)	-.008* (.005)	.027** (.013)	-.007 (.004)
N	87,700	61,178	87,713	63,270	63,270	62,433

*Notes:* The table reports estimates from regressions of national rank in math on classroom math rank and interactions. Observations are pooled across grades. Column (1) shows the results from a regression of national rank on classroom rank interacted with an indicator variable for girls. Column (2) shows the results from a regression of national rank on classroom rank interacted with baseline vocabulary. Column (3) shows the results from a regression of national rank on classroom rank interacted with lagged national rank. Column (4) shows the results from a regression of national rank on classroom rank interacted with an indicator variable for mother having less than secondary education. Column (5) shows the results from a regression of national rank on classroom rank interacted with an indicator variable for the child having attended preschool. Column (6) shows the results from a regression of national rank on classroom rank interacted with an indicator variable for the child belonging to a family with household wealth above the median. All regressions are limited to schools in which there are at least two classrooms. All regressions include a third order polynomial in lagged national rank in math and classroom-by-grade fixed effects. Standard errors are clustered at the school level. N is lower for columns (2), (4), (5) and (6) because we collected this information at the beginning of kindergarten, so it is not available for children who joined schools in our sample after the beginning of kindergarten. \*Significant at 10%, \*\*significant at 5%, \*\*\*significant at 1%.

**Table 4: Effects of math classroom rank on achievement, by grade and lag**

	Lags					F-test 1	F-test 2
	0	1	2	3	4		
Classroom rank "early" (1st & 2nd grades)	.064*** (.017)	.077*** (.019)	.090*** (.021)	.096*** (.022)	.105*** (.022)	[.584]	[.106]
Classroom rank "middle" (3rd & 4th grades)	.048*** (.017)	.038** (.019)	.038* (.020)			[.845]	[.630]
Classroom rank "late" (5th & 6th grades)	.005 (.015)						
F-test 3	[.025]	[.160]	[.091]				
F-test 4	[.011]						

*Notes:* The table reports estimates from regressions of national rank in math on classroom math rank for different lags of classroom rank, separately for children in the “early” (1st and 2nd), “middle” (3rd and 4th), and “late” grades (5th and 6th) grades. All regressions are limited to schools in which there are at least two classes. All regressions include a third order polynomial in lagged national rank in math and classroom-by-grade fixed effects. Standard errors are clustered at the school level. P-values for joint-tests of equality of coefficients (in square brackets) are calculated after running estimates by Seemingly Unrelated Regressions. Joint-test 1 is a test that the coefficient for all lags is the same, and Joint-test 2 is a test that the coefficient on lag=0 is the same as that on lag=4 (for the “early” grades) or lag=2 (for the “middle” grades). Joint-test 3 is a test that the coefficients on “early”, “middle”, and “late” grades are the same, and Joint-test 4 is a test that the “early” and “late” effects are the same. Standard errors are clustered at the school level throughout. N lag 0, “early”: 26,695. N lag 0, “middle”: 30,078. N lag 0, “late”: 30,940. N lag 1, “early”: 24,648. N lag 1, “middle”: 28,194. N lag 2, “early”: 23,199. N lag 2, “middle”: 26,706. N lag 3, “early”: 22,140. N lag 4, “early”: 21,210. \*Significant at 10%, \*\*significant at 5%, \*\*\*significant at 1%.

**Table 5: Math classroom rank effects on executive function**

	Lags		
	0	1	2
Classroom rank “early” (1 <sup>st</sup> & 2 <sup>nd</sup> grades)	.096 (.067)	.265*** (.078)	.054 (.077)
Classroom rank “middle” (3 <sup>rd</sup> & 4 <sup>th</sup> grades)	.096 (.071)	.000	.338 (.080)

*Notes:* The table reports estimates from regressions of executive function, in SDs, on classroom achievement rank in math, for different lags of classroom rank, separately for children in the “early” (1<sup>st</sup> and 2<sup>nd</sup>) and “middle” (3<sup>rd</sup> and 4<sup>th</sup>) grades. All regressions include third order polynomials in lagged national rank in math, a third-order polynomial in lagged executive function, and classroom-by-grade fixed effects. Below the standard errors in brackets we report p-values computed according to the Romano and Wolf stepdown procedure (see Romano and Wolf, 2005 and Clarke, Romano and Wolf, 2020), using 5000 bootstrap replications. All regressions are limited to schools in which there are at least two classrooms per grade. We cannot assess the impact of classroom rank past 4<sup>th</sup> grade, as we did not apply executive function tests after that grade. Standard errors are clustered at the school level throughout. N lag 0, “early”: 26,696. N lag 0, “middle”: 30,065. N lag 1, “early”: 24,649. N lag 2, “early”: 23,199. \*Significant at 10%, \*\*significant at 5%, \*\*\*significant at 1%.

**Table 6: Math classroom rank effects on happiness**

	Child happiness (1 <sup>st</sup> grade)		
	Mostly happy	Almost always happy	Always happy
Classroom rank	-.076*** (.022)	-.062** (.018)	.138*** (.040)

*Notes:* The table reports estimates from a regressions of child happiness in 1<sup>st</sup> grade on classroom achievement rank in math in 1<sup>st</sup> grade, estimated by ordered probit. All regressions include third order polynomials in kindergarten national rank in math and classroom fixed effects. All regressions are limited to schools in which there are at least two classrooms per grade. Standard errors are clustered at the student level throughout. N is 12,062. Standard errors are clustered at the school level throughout. \*Significant at 10%, \*\*significant at 5%, \*\*\*significant at 1%.

**Table 7: Math classroom rank effects on teacher perceptions**

	Top 5 student				Bottom 5 student			
	Lags							
	0	1	2	3	0	1	2	3
Rank “early” (1 <sup>st</sup> & 2 <sup>nd</sup> grade)	.099*** (.029)	.092*** (.026)	.060** (.025)	.027 (.027)	-.079*** (.028)	-.047 (.029)	-.021 (.026)	-.058** (.025)
Rank “middle” (3 <sup>rd</sup> & 4 <sup>th</sup> grade)	.000	.000	.021	.534	.004	.162	.617	.021
Rank “late” (5 <sup>th</sup> & 6 <sup>th</sup> grade)	.074** (.031)	.088*** (.031)			-.007 (.031)	-.019 (.032)		
	.021	.004			.774	.640		
	.083* (.042)				-.056 (.042)			
	.065				.321			

*Notes:* The table reports the results from regressions of a child being reported to be among the top 5 (bottom 5) by achievement by her teachers in grade  $t+1$  on classroom rank in grade  $t$ , controlling for a third-order polynomial in national achievement in math in grade  $t-1$ , and school-by-grade fixed effects, pooling across “early” (1<sup>st</sup> and 2<sup>nd</sup>), “middle” (3<sup>rd</sup> and 4<sup>th</sup>) and “late” (5<sup>th</sup> and 6<sup>th</sup>) grades. Below the standard errors in brackets we report p-values computed according to the Romano and Wolf stepdown procedure (see Romano and Wolf, 2005 and Clarke, Romano and Wolf, 2020), using 5000 bootstrap replications. All regressions are limited to schools in which there are at least two classrooms per grade. Standard errors are clustered at the school level throughout. N “early”: 27,794. N “middle”: 30,842. N “late”: 15,552. \*Significant at 10%, \*\*significant at 5%, \*\*\*significant at 1%.