Social Promotion and Learning Outcomes: Evidence from India

Md Nazmul Ahsan * Rakesh Banerjee † Siddharth Hari‡

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Abstract

We study the effects of a social promotion policy on learning outcomes in India - a context characterized by extremely low learning levels. We exploit geographic variation induced by the "No Detention Policy", introduced as part of India's Right to Education Act, to set up a differences-in-differences design. Using data from seven years of a large scale education survey, we find that as a consequence of social promotion, reading scores improved by 2.5 percent, and math scores by 5 percent. These gains are broad-based, with both boys and girls showing improvements. These effects, however, are strongest in the lowest quartile of the distribution. We test for alternative mechanisms which could be driving our results.

Keywords: Social Promotion, Learning outcomes, Economic growth
JEL Classification: I2, I28, O10, O15

*Saint Louis University, Department of Economics, 3674 Lindell Blvd, DS Hall 346. Email: ahsanm@slu.edu

†Baker Institute for Public Policy, Rice University, Email :Rakesh.Banerjee@rice.edu

‡Corresponding Author. Department of Economics, Virginia Tech. Email: siddharth.hari@vt.edu.
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1 Introduction

Despite the near universalization of primary school enrollment, learning levels in much of the developing world remains poor and progress remains very slow, giving rise to a learning crisis (World Bank, 2018). This is likely to be a critical impediment for economic development as learning outcomes matter more than enrollment or years of schooling *per se* for economic growth (Hanushek and Woessmann, 2012)[1].

Several factors both on the demand and supply side - such as low perceived returns to education and poor quality of teachers - have been highlighted as potential causes for these trends. A relatively understudied question is the effect of grade repetition on learning. This, despite the fact that there is a strong association between grade repetition rates and lower educational attainment on the one hand and economic development on the other. On average, less developed countries have significantly higher repetition rates than developed ones (Manacorda, 2012). In some countries, an often used policy to reduce repetition rates is to allow for mandatory promotion till a certain grade. However, the theoretical predictions of the effects of such social promotion policies on learning outcomes is unclear.

There are several channels through which social promotion (or eliminating grade repetition) could improve learning. First, being forced to repeat a grade stigmatizes a child, breaks established peer relationships leading to worsening of learning outcomes, or in extreme cases, dropping out of school. Second, by creating a "fear-free" learning environment, social promotion can directly lead to improvements in learning (Anderson et al., 2002; Jimerson et al., 2002; Pierson and Connell, 1992). Third, a student repeating a grade costs the same amount of resources as enrolling an additional student at the same grade. This increases class size and further increases the stress on other school inputs like classrooms and desks, which can potentially impede learning (Krueger, 1999). On the other hand, there are several channels through which social promotion policies may impede learning. First, absence of the threat of repetition reduces the incentives of students to exert effort (Koppensteiner, 2011). Second, repeating a grade could allow a student more time to catch up with the curriculum, and by ensuring that all students in a particular grade are at roughly the same learning level, make teaching easier and more effective.

The empirical evidence on the effects of social promotion policies is also limited. The main challenge is to disentangle the effect of retention from other factors that cause retention. Most studies that estimate causal effects of grade repetition use a discontinuity in the rule for promotion, and estimate the effect of repetition on repeaters by comparing

[1] In the regressions, test scores remain a strong predictor of annual per capita growth even after controlling for years of schooling.

students who cleared the test by a small margin with students who failed by a small margin (Jacob and Lefgren, 2004, 2009a; Manacorda, 2012). However, the threat of possible repetition (or the lack of it due to social promotion) can also have an effect on the learning outcomes of non-repeaters, as well as those repeaters who fail by a significant margin. Regression discontinuity designs, by construction, cannot capture these effects. Further, most of the existing evidence is based on provincial or regional changes in policies, limiting generalizability. Finally, most of these studies are based in the context of developed countries where failing students are often provided remedial classes to help them come up to speed with the learning material. In addition to lack of infrastructure and poor teacher quality, developing countries lack such special provisions for repeaters, therefore making the effects of social promotion policies ambiguous.

In this paper we study the effect of a social promotion policy on learning outcomes in India. In particular, we study the effects of the "No-Detention Policy" (NDP), a national social promotion policy introduced by the government across all schools in 2010. The policy was introduced as part of the Right to Education (RTE) Act, a national law aimed at overhauling primary education in India. The NDP mandated that schools (both government and private) could no longer hold students back in a grade, till the student had reached grade 9 (Bandyopadhyay et al., 2011; KPMG, 2016; Nawani, 2015). Prior to the introduction of the NDP, different states in India, followed different grade retention policies (Bhukkal, 2013). For example, the state of West Bengal had a policy of social promotion up to (and including) grade 4, Jharkhand up to and including grade 5 and Gujarat up to and including grade 2. The NDP mandated that all states introduce social promotion up to grade 8. This induced a variation in exposure to the policy at a state-grade level, with some state-grades affected by the policy while some others remain unaffected. We use this to set up a differences-in-differences strategy. In particular, we compare learning outcomes of students in government schools in affected state-grades before and after the policy and contrast this with unaffected grades, after accounting for state-grade specific trends [2]. This method allows us to causally estimate the average effects of the policy on learning outcomes of all students enrolled in a grade - not just the effects on repeaters.

Large scale and frequently conducted national level surveys of learning outcomes are scarce, particularly in less developed countries. In this paper, we use data from the Annual Status of Education Report (ASER), a large scale national survey conducted each year from the period 2007 to 2014. This survey covers all states of India and interviews approximately 320,000 rural households each year. In addition to a rich set of demographic variables, as part of the survey, ASER tests every child in a household between age 5-16 in basic reading (in the local language), as well as on basic math. Crucially for us the ASER

[2] In this setup, some state-grade pairs are *always treated*, whereas other state-grade pairs are *sometimes treated*

test is not grade-dependent - each child is asked exactly the same set of questions. We use the results of these tests as our main outcome variable. While the NDP was applicable to all schools - government and private - social promotion policies prior to the NDP were mandatory only for government schools. In this paper, therefore, we focus exclusively on government schools [3]

We first establish that the policy was indeed implemented. Overall repetition rates fell drastically after the introduction of the policy, driven almost entirely by grades which did not have social promotion prior to the introduction of NDP. Looking at our main dependent variable, learning outcomes, we find that the overall effect of the policy was positive. Test scores improved by 2.5% in reading and 5% in math as a consequence of the NDP. An event study analysis confirms that our results are not confounded by pre-existing trends. We look into heterogeneity of the results to understand the effect of this policy on different sub-groups of the population. We find both boys and girls benefit from the program with girls benefiting marginally more than boys. However, there are no differential effects by parental education. Finally we also find that districts with higher baseline repetition rates benefitted the most from the policy, further allaying concerns that other policy changes might be driving our results.

We perform several robustness checks. One factor which could potentially contaminate the reduced form effects is what we refer to as the *composition effect*. To understand this, consider the following example: Grade 6 in West Bengal did not have social promotion prior to the introduction of the NDP. However, after the policy was introduced, students who would have otherwise (in absence of social promotion) repeated grade 6 move on to grade 7. Everything else held constant this will lead to higher average test scores in grade 6. On the other hand, students who would have otherwise repeated grade 5 now enter grade 6 after the introduction of NDP. Everything else held constant this will lower average test scores in grade 6. The net effect of these two changes is ambiguous. This is different from the *treatment effect* of interest which is the direct effect of the NDP on learning conditional on being in a certain grade. Given the absence of a large scale panel data on learning outcomes in India, we address this concern in two ways. First, we consider the first year this policy was introduced (2010). In the first year, there should be *no* composition effect, since the current composition of all grades was determined prior to the introduction of NDP. However, students are aware that starting this year they cannot be asked to repeat a grade, and therefore any effects on learning captures the true treatment effect of social promotion. We find that even after restricting attention to the first year of the implementation of NDP, we get remarkably similar effects on both reading and math

[3] It is of course possible that some private schools followed pre-NDP social promotion policies. However, since it was not mandatory, and we do not observe whether any given private school followed this policy, we exclude these from our analysis

scores. Second, if we make the assumption of monotonicity of reading and math skills with respect to the repeated grade, conditional on repeating a grade - i.e would be repeaters in grade 6 have higher average test scores than would be repeaters in grade 5 - the composition effect described above will be negative.[4] Therefore the overall effects we find are likely to be a lower bound of the true treatment effect.

There are two additional sources of change in grade composition. First, we find a reduction in dropout rates, which could influence the composition of grades. However, if we assume that students most likely to drop out of school come from the lower end of the learning distribution, the reduced form effects will, once again, be an underestimate of the true treatment effect. Second, since we focus on government schools, another potential threat to our identification could come from students switching between government and private schools in response to this policy. We test for, and rule out such a switch.

Finally, another potential concern with our identification strategy is that the Right to Education Act (RTE) contained several other provisions, such as introducing limits on student-teacher ratios and mandating improvements in school infrastructure. These provisions might directly influence learning outcomes. However, none of these other provisions varied at the state-grade level, unlike the variation introduced by the NDP. To further allay these concerns, we control for a set of factors such as share of education expenditure in the state budget, student-teacher ratios etc. at the state level, and allow for its effects to vary across grades. Our results are unchanged even after controlling for these factors.

We explore three mechanisms which could be driving our results. First, it is possible that parental investments respond to the introduction of this policy. For example, parents might enroll children in private tuitions to compensate for the decreased incentives to study. However, we find no effect of the policy on private tuition.[5] Second, It is possible that students attend school more often because of the "fear-free environment" created by the no repetition policy. We find that attendance in school does not respond to the change in policy. Finally, we study the effect of the policy by each quartile of the test score distribution. While the effects of the policy are positive for all quartiles, we find that the largest effects are for the bottom two quartiles, suggesting that these gains are driven largely by a motivational effect which is likely to benefit would be repeaters.

This paper makes three main contributions. First, a growing literature has focused on policy measures which can improve learning outcomes in less developed countries. Recent papers have studied the impact on learning outcomes of both demand and supply side interventions such as changes in teacher salaries (Muralidharan and Sundararaman,

[4]See Figure A5 for validation of the assumption

[5]We do not have data on any other investment measure

2011; Ree et al., 2017), teacher attendance (Muralidharan et al., 2017), parental investment (Das et al., 2013), information provision (Andrabi et al., 2017) and educational technology (Muralidharan et al., 2016). However, less attention has been paid to the effects of pedagogy and classroom environment. Our paper aims to fill this gap.

Second, we add to the existing literature on the effects of social promotion and grade retention. The debate surrounding such policies is far from being settled, and the existing empirical evidence from a variety of quasi-experimental methods, is mixed. Some studies find a negative effect of grade retention (Gary-Bobo et al., 2016; Holmes and Matthews, 1984; Jacob and Lefgren, 2009b; Koppensteiner, 2011; Ozkan et al., 2017) while others find a positive or null effects of grade retention (Bélot and Vandenberghe, 2014; Schwerdt and West, 2017). Most of the causal estimates are identified as effects on repeaters. We add to this literature by estimating the effects of a social promotion policy on average learning levels and not just on repeaters. Moreover, most of this literature has focussed on understanding the effect of such policies in developed countries. Most developed countries have remedial measures and other support for repeaters. We contribute to the literature by providing estimates of social promotion policies from a developing country country where such resources are typically not available. In addition, most of the existing evidence is from regional level changes in policies. We provide evidence from a national policy change, and bring to bear a large data set on learning outcomes to obtain precise estimates.

Finally, we contribute to the ongoing public policy debate in India about the merits of the NDP. This policy has generated attention in the media and among policymakers. While anecdotal evidence abounds, this is the first paper which rigorously evaluates the impact of the NDP [6]. This paper therefore makes an important contribution to the ongoing policy debate.

The rest of this paper is organized as follows: section 2 provides the context and policy background; section 3 describes the data sets used; section 4 describes our empirical strategy, while results and possible mechanisms are discussed in section 5; section 6 concludes.

2 Policy Background

From its independence, the constitution of India has deemed education to be a right for its citizens (KPMG, 2016). In the constitution of India, article 45 states: *The State shall endeavor to provide, within a period of ten years from the commencement of this Constitution, for free and compulsory education for all children until they complete the age of fourteen years*. However, this

[6] The government is seriously considering rolling back the NDP. See https://www.thehindu.com/education/schools/new-bill-to-allow-states-to-drop-no-detention-policy/article19429977.ece

article is a *directive principle of state policy* which implies that the government is not legally bound to implement this mandate.

Although the government is not legally obliged to follow the directive principles in the Constitution, fundamental rights specified in the same constitution are binding in nature and violation of these rights could imply facing penalties (Mehrotra, 2012). In 2002, the Parliament of India made a significant change by passing the 86th Amendment Act to the Constitution. It mandated free and compulsory education by adding the article 21A in the list of Fundamental Right. However, this amendment did not become active immediately because the 86th Amendment Act also stated :*It shall come into force from such date as the central government may notify the Official Gazette*. This notification did not come in place for eight years, but finally, the act came into effect April 1, 2010 (KPMG, 2016).

In addition to making free and compulsory education a fundamental right, the RTE act also laid down clear guidelines for schools to follow. These policies include: reservation of 25% of seats in private schools for children from economically weaker sections of society; introduction of minimum qualifications for teachers; minimum infrastructure requirements; and limits on teacher-student ratios.

The provision of the RTE that we focus on in this paper is the "No Detention Policy", which mandated that students cannot be asked to repeat a grade, until they clear grade 8. The government's rationale for introducing this policy was to create a "stress-free" learning environment, and also cited the lack of evidence that grade retention improves learning outcomes (Bhukkal, 2013). Though the policy prohibited schools from failing students, it encouraged schools to conduct regular evaluations of students through "Continuous and Comprehensive Evaluations" (CCE). These evaluations were supposed to provide teachers and parents with information on the progress students were making. However, in practice many schools did not implement a regular evaluation process, giving rise to concerns that the NDP will discourage students from exerting effort. This policy has been one of the most controversial provisions of the RTE, and has given rise to serious debates among policymakers and educationists. After several years of debates, the federal government has scrapped the policy in July 2018, leaving it upto individual states to decide if they want to continue with the provision of No Detention. [7]

The main challenge in evaluating the effects of the NDP is that it is a national policy applicable to all states, starting at the same time. To obtain causal estimates, we either need temporal or spatial variation in exposure to social promotion. Education in India

[7] See the article `https://www.livemint.com/Politics/fK5E9oG8rJCifJgEkY7yoI/Lok-Sabha-passes-bill-allowing-detention-of-children-in-Clas.html`. The bill passed in the parliament amended the act.

is part of a "Concurrent List", implying both state governments and federal governments can legislate in this area. Prior to the introduction of NDP by the federal government in 2010, different Indian states followed a policy of social promotion up till different grades. Figure 1 shows the prevalence of social promotion policies different states prior to the introduction of the NDP in 2010. The figure indicates two facts. First, most states followed social promotion at least up till certain grades even prior to 2010. Second, there is considerable variation across states in the grades up to which social promotion existed. [8] For example West Bengal, a state in eastern India, had a social promotion policy till grade 4 in government schools even before the federal policy came into effect in 2010, so students in grade 1 to grade 4 remain unaffected by the federal policy. However, students of grade 5 to grade 8 in West Bengal are affected by the federal policy as they can no longer be detained until they complete grade 8. We use this variation within a state-grade over time to estimate the causal effects of NDP. [9]

3 Data

To carry out the empirical analysis, we use two main sources of data, which are described below.

3.1 Annual Status of Education Report

The Annual Status of Education Report (ASER) is a data set collected by a non governmental organization, *Pratham*. The main purpose of the survey is to measure educational achievement across the country. The survey covers almost all rural districts in India, but urban centers are excluded. This is the largest survey measuring learning outcomes in India, covering approximately $320,000$ households every year. The ASER survey started in 2005, and there has been a survey every year since, with the exception of 2015. In this paper we use ASER surveys covering the period 2007-2014 [10]. The survey consists of two modules - a school survey and a household survey. One government primary school in each village which is part of the survey, is selected and visited by enumerators. The school survey collects information on school level characteristics such as school infrastructure, student enrollment, as well as teacher and student attendance. The household survey collects demographic information, such as household size, as well as the age and education

[8] All these policies in different states were in place well before the 2007, the first year for which we have learning data

[9] It is important to note however, that the pre-NDP social promotion rules pre-NDP were mandatory only for government schools. However, it is possible that some private schools followed these policies as well. Since we do not observe which private schools followed these policies, and which ones didn't, we exclude private schools from our analysis.

[10] We exclude the years 2005 and 2006, because starting in 2007, ASER changed the methodology of measuring learning outcomes. However, these measures are comparable across all surveys between 2007 and 2014

level of each household member. In addition, for each child currently enrolled in school, it collects information on the type of school (public or private), current grade and additional information such as amount of money spent on private tuition. Finally, for children who have dropped out of school, information is collected on the grade at which the child dropped out. In our main specification, we restrict attention to children currently enrolled in government schools in grades 3 through 8, excluding children in grades 1 and 2. We do this for three reasons. First, since we use the state of residence and enrolled grade to determine "treatment" status, we exclude those who are not enrolled in any grade. Second, almost all states had social promotion policies in place at least till grade 2 prior to the NDP, therefore there is very little variation across states till this grade. [11]

Further, as part of the survey, tests in reading (in the local language) and math are conducted. The aim of the test is to understand whether the child has a basic level of learning, and therefore the same test is administered to all children in household, irrespective of their age, enrollment status or the grade in which they are currently enrolled. The reading component consists of four questions - whether the child can recognize a letter, recognize a word, read a paragraph or read a story. The test proceeds sequentially, and stops as soon as an incorrect answer is provided. Following, Shah and Steinberg (2017), we use this test to create a reading score, out of 4 for each child - 0 if the child is not able to recognize a letter, 1 if the child can recognize a letter but not a word and so on. Similarly, the math component of the test has four questions - whether a child can recognize a number between 0 and 9, recognize a number between 10 and 99, perform a simple subtraction and perform a simple division. We use a similar method as the reading score to construct a math score out of 4 for each child. The score is zero when a child cannot recognize a number between 0 and 9 and takes the value 4 when a child is able to perform simple division.

Figure A2 plots mean reading and math scores. As the figure shows both reading and maths score have been declining over time. Figure A3 plots average scores by grade. As the figure shows even students in grade 8 on average do not have perfect test scores despite the test being based on very basic skills. Finally, Figure A4 plots the proportion of students across grades who have a perfect score (read a story and perform simple division). In 2014, only 35 percent of students could read a story and only 15 percent could divide. One concern with using learning outcomes measured in ASER is these tests are very basic and hence may not be able to capture the effects of the policy on more advanced learning. However as these figures indicate, despite the tests being very basic, a significant proportion of students perform very poorly. This indicates there is a significant scope of improvement on these tests. Summary statistics are provided in Table 1

[11] We also show that our results are robust to including grades 1 and grade 2

3.2 District Information System for Education

The District Information System for Education (DISE) is a census of all schools in India. While the data is collected by the government at the school level, we use the data aggregated at the district level. This data set includes information on school infrastructure, teacher-student ratios, etc. Crucially for us, it reports data on enrollment and repetition rates at the grade level. Figure A1 tracks the decline in the proportion of repeaters over time. As of 2014, repetition rates were around 0.5%, a sharp fall from 6% in 2006. [12] However there are two concerns with the DISE data. First, the data is self-reported by schools to the government and it is not cross-verified by an audit. Thus it may suffer from measurement errors, as well as systematic over or underreporting. Second, in the district level aggregates we cannot identify repetition rates separately by government and private schools.

4 Empirical Strategy

In this section we describe in detail the empirical strategy used to causally identify the effects of social promotion. A simple comparison of average learning outcomes before and after the introduction of the NDP would not give us the causal effects of the policy for at least two reasons. First, NDP was introduced as part of the RTE - a policy which included several other changes such as limits on student-teacher ratios and an improvement in school infrastructure. Any changes in learning outcomes before and after the introduction of the policy could therefore be because of the introduction of any of these other policies. Second, as discussed in the previous section, there was a significant downward trend in learning outcomes (as well as repetition rates) even prior to the NDP. Any strategy which tries to provide causal estimates has to control for these pre-existing trends.

Our strategy exploits the fact that there was significant variation in social promotion policies in government schools across states prior to the implementation of the NDP. However, after the introduction of the NDP, all states needed to have social promotion up till grade 8. We use this policy variation to set up a difference in differences empirical design. In this setup, state-grade pairs which already had social promotion before the NDP form the "control" group, whereas state-grade pairs which *did not* have social promotion prior to NDP, but had to introduce it after NDP, form the "treatment" group [13]. Formally, we estimate the following regression:

$$Y_{isgt} = \alpha + \beta\ Treatment_{sgt} + \theta\ X_{it} + \delta_{sg} + \gamma_{st} + \pi * t_{sg} + \epsilon_{isgt}$$

[12] As the figure indicates, repetition rates were declining even prior to the implementation of the NDP. Our empirical specification, which includes state-grade specific time trends, allows us to control for the pre-existing trends

[13] Our "control" group, therefore, is *always treated*, whereas our "treatment" group is *sometimes treated*

Here Y_{isgt} is an outcome variable of interest, for a child i living in state s, grade g and surveyed in year t [14]. $Treatment_{sgt}$ takes the value one if grade g in state s in survey year t had a social promotion policy in place. For example, consider children from grade 3 in two states West Bengal and Madhya Pradesh. In West Bengal social promotion was in place in grade 3 even prior to the introduction of NDP, while in Madhya Pradesh there was no social promotion prior to 2010. So, for a child in grade 3 in West Bengal prior to the introduction of the NDP, *Treatment* takes the value 1, whereas for a child in grade 3 in Madhya Pradesh, prior to NDP, *Treatment* takes the value zero. After the introduction of NDP in 2010, *Treatment* takes the value one for all grades and all states, since the NDP was a nation wide policy.

X_{it} includes a set of child-specific controls such as gender, age and household size. δ_{sg} are state-grade specific fixed effects. Several time invariant factors which vary across state grade pairs might affect learning outcomes. For example, the curriculum followed in schools often varies across states for a same grade. State-grade fixed effects allow us to control for all such factors. γ_{st} are state-year specific fixed effects. These control for time varying differences across states, such as changes in funding for school infrastructure which may influence learning outcomes directly. Finally, $\pi * t_{sg}$ are state-grade specific linear time trends. Learning outcomes may be evolving differently across state-grade pairs over the time period of our sample. State grade specific time trends control for differential trends in the outcome variable which might otherwise be misinterpreted as the treatment effect.

Thus, in effect, our empirical strategy compares average learning outcomes of students enrolled in a particular grade in a state before and after the introduction of NDP, after controlling for state level time varying factors, individual demographics as well a linear trend in learning outcomes for each state-grade.

5 Results

5.1 Repetition Rates

First, we show that the introduction of NDP resulted in a reduction in repetition rates. To establish this, we use the district level DISE data and compare repetition rates of affected state-grades before and after the introduction of the policy with unaffected state-grades acting as the control group. As described above, we control for state-grade fixed effects,

[14]Some of our outcome variables of interest, such as proportion of repeaters and attendance in school are not at the child level. For these outcomes, we use the same estimating strategy as above, with appropriate modifications

state-year fixed effects, district fixed effects and state-grade specific linear time trends. Table 2 shows the results. In column 1 the dependent variable is the absolute number of repeaters in a district-grade-year and in column 2 the dependent variable is proportion of repeaters (calculated as total number of repeaters divided by total number of students enrolled). As the results indicate we find that the both the absolute number as well as the proportion of repeaters goes down as a result of this policy. In an average district number of repeaters decrease by 503, which is 45 percent of the mean number of repeaters in our sample from 2007-2014. The effect is of similar size when we look at proportion of repeaters. Proportion of repeaters declines by 1.4 percent in an average district, which is 41 percent of the average repetition rate in our sample period.

Another consequence of the NDP is that enrollment levels in the lowest grade affected by the NDPy in a state should decline. For example, grade 5 in West Bengal is lowest grade affected in the state of West Bengal. This is because prior to the introduction of NDP in 2010, there was already a social promotion policy in West Bengal till grade 4. Thus for the lowest affected grade, intake from the lower grade (grade 4 in case of West Bengal) remains unchanged as social promotion was already in place at the lower grade, but the number of students exiting the lowest *affected* grade goes up as students can no longer repeat this grade because of the NDP. At the same time the class size (i.e the total number of enrolled students) in unaffected grades should remain unchanged. Column 3 of Table 2 shows the effect of the policy on class size. Here the dependent variable is the class size at the district-grade-year level, and for each state, we only include the lowest affected grade and all grades below that.[15] As the results indicate, there is a decline in class size of 500 for the lowest affected grade, though the coefficient is not statistically significant. The effect size is strikingly similar to number of repeaters in column 1, suggesting this decrease in class size is almost entirely due to a decrease in repeaters. These results confirm the "first stage" effects of the policy, i.e the introduction of NDP indeed lead to a reduction in repetition rates.

5.2 The Effects of NDP on Learning Outcomes

Next, we show the main results, i.e the effects of the NDP on learning outcomes, in reading and math. Students were tested on four questions each in reading and maths. As described above, each child was given a score between 0 and 4. The estimates are shown in Table 3. Column 1 shows the estimates for reading and column 2 for math. As the results indicate, we find that average learning outcomes, both for reading and math, improves as a consequence of the NDP. Compared with the mean scores the magnitudes imply a 2.5% increase in reading scores, and a 5% increase in math. To put these effects in perspective, we compare our results to other papers using ASER data. Using the same measures of

[15] Again if we take the example of West Bengal we include grades from 3-5

reading and maths test from ASER data, Shah and Steinberg (2017) finds a positive effect 0.02 of a drought year on math and reading scores. Compared to these effects, the effect of NDP is twice as large on reading scores and five times larger on math scores .

A central concern in any study using differences-in-differences is the presence of pre-existing trends. To rule out the presence of such trends driving our results, we also present our main results in an event study setting. To do this we use 2007 as a base year and sequentially run the following regression, taking every other year in our sample (2008-2014), one at a time:

$$Y_{isgt} = \alpha + \beta\ Treatment_{sgt} + \theta\ X_i + \delta_{sg} + \gamma_{sy} + \epsilon_{idsgt}$$

We are essentially re-estimating our main regression, using only two years of data at a time, with 2007 serving as the base year. Of course, since we are using only two years of data we cannot include state-grade specific time trends. How is *Treatment* defined? For the years 2010 and beyond, treatment is defined the same way as we did in the main specification. For the years 2008 and 2009 - the years before NDP was introduced - we define treatment by assuming that it was implemented earlier. The regressions with the years 2008 and 2009 can therefore be thought of as placebo tests. Figure 2 and Figure 3 plot the coefficients from each of these regressions. If pre-existing trends, or other policies introduced prior to 2010 were driving our results, we should observe positive coefficients for the years 2008 and 2009 as well. As these figures indicate, however, this is not the case. Prior to 2010, the coefficients for both reading and math are zero. We then start observing positive effects from 2010. As can be seen from the figure, the effect on math scores does not persist 4 years after the policy. This highlights a possible fade out of the effects over years. One possible reason can be that the motivational effect of removing the threat of detention perhaps lasts only in the short run, and over time the disincentive effect might start to dominate. However, we since we only have data for 4 years after the program we are not able to fully test for fade out effects.

5.3 Treatment vs. Composition Effects

One challenge for our identification strategy is to disentangle the *treatment effect* from the *composition effect*. To see the difference between the two, consider the state of West Bengal, which had social promotion in place up till grade 4, prior to the introduction of NDP in 2010. Now consider grade 5 in West Bengal in 2008. It would be composed of all students who were in grade 4 the previous year (i.e in the academic year of 2009), as well as students who failed in grade 5 the previous year. Now consider what happens to the composition of grade 5 post-NDP. It now consists *only* of students who were in grade 4 the previous year, since the introduction of the NDP implies no student can fail in grade 5. Therefore, the composition of grade 5 has now changed, which will influence the average test scores. We call this the composition effect. What about the treatment effect? This is the direct effect on students resulting from the removal of grade retention. This treatment effect of the NDP is what we would like to capture. Similar composition and treatment effects exist for other grades as well. For instance, continuing with our West Bengal example, for any pre-NDP year, grade 6 consisted of students who cleared grade 5 exams the previous year and students who failed grade 6 the previous year. Post-NDP, grade 6 in West Bengal would consist of all students who were in grade 5 the previous year and no grade 6 student the previous regardless of their academic performance. Effectively, therefore, the would-be-repeaters of grade 5 are replacing the would-be-repeaters of grade 6, which in turn affects grade composition.

We follow two approaches to separate the treatment and composition effects. First, we focus on the first academic year in which the NDP was implemented, 2010-11. This year, the composition of grades remains unchanged: for example grade 5 in West Bengal consists of students who were in grade 4 in 2009-10, and students who failed grade 5 in 2009-10. However, in 2010-11, the NDP was announced and students in each grade knew they would not be retained in that grade in the next academic year. Thus this test helps us to directly isolate the treatment effects of the program. Table 4 reports the results from this test. In this regressions we follow the same specification as before but limit our sample to exclude years beyond 2010. We find that the pure treatment effect is positive, significant and of a similar magnitude to the effects found in Table 3 for reading, and slightly smaller for maths. The results highlight, the main effects are not entirely an artifact of change in composition of students in each grade. This is not surprising, given that the average repetition rates in India were around 6%, and the composition effect, driven entirely by repeaters is likely to be small.

Second, under the assumption that conditional on repetition, the grade of repetition is positively associated with learning levels, the composition effect will be unambiguously negative. Continuing with previous example, if we assume the would-be-repeaters of grade 5 (i.e students who would have failed in absence of the NDP) are at a "lesser learning level" than the would-be-repeaters of grade 6 (who they would be replacing after NDP), the direct composition effect of NDP on grade 6 should be negative. This assumption can partly be verified by comparing students with the lowest score in each grade before the policy. In Figure A5 we show the proportion of students who scored zero or one in the test, by each grade before the policy was implemented. As the figure indicates at any grade the proportion of student with a score of zero or one, is more than the grade succeeding it. This provides suggestive evidence in support of the assumption. However, our assumption is valid for all the grades except for the lowest grade in a state affected by NDP. [16]This is because there is no change in the incoming cohort, but the students who would have otherwise failed are now being promoted and therefore are leaving the grade. In this test, therefore, we exclude the lowest grade affected by the policy, and estimate our treatment effects. These results are shown in Table 5, and as can be seen, the effect sizes are remarkably similar to the effects reported in Table 3. Based on these two tests it is very unlikely that our results are being driven by changes in grade composition, and if anything, the true treatment effects are likely to be higher.

There are two other sources through which NDP can affect grade composition. The first is by influencing dropout rates. In Table A1 we present the effect of NDP on dropout rates and on enrollment in private school. As the result in column 1 indicate, the policy lead to a reduction in dropout rates in government schools. [17] A reduction in dropout rates will certainly affect the composition at each grade. However, if we assume that students who dropout are drawn from the lower end of the learning distribution, once again this composition effect will be negative. The second channel through which grade composition might change is if the NDP induced a switch between government and private schools. Since we restrict our sample to government schools, a selected switch towards (or away from) private schools could affect the interpretation of our estimates. In column 2 of Table A1, we show results on the effects of the policy on migration away from government schools. Here the dependent variable is binary, indicating whether or not a student is enrolled in a private school, and we use the same specification as in our main regressions. As the results indicate we do not find any evidence of the policy leading to migration towards private schools.

5.4 Robustness

Another concern with regards to interpreting our coefficients is that along with the NDP, the Right to Education Act mandated several other provisions which might directly influence learning outcomes. We follow two approaches to address this concern. First, while there were indeed several other important provisions of the RTE, none of them varied at the state-grade level, unlike the NDP. Additionally, we control for state-year fixed effects in all regressions which take care of all provisions which varied at the state-year. However, there may be some provisions such as limits on student-teacher ratios which varied at the state-year level and impacted different grades differently. Our second approach, to account for this possibility is to include in our regressions the budget share spent on edu-

[16]For example grade 5 in West Bengal

[17]ASER data contains information on the grade at which a child dropped out of school, but not the year. However, to assign treatment status, we need to know the year as well. To back out the year of drop out, we assume an age grade mapping: a six year old child is assumed to be in grade 1, a seven year old in grade 2 and so on. We then use this information to estimate the effects of the NDP on dropout rates.

cation by each state government in each year and allow the effects of such expenditures to vary be grade.[18] In column 1 and column 2 of Table A2 we show the results of this regression after controlling for budget share on education. As the results indicate, our effects are very similar to those in Table 3. Increased provision of school inputs may also be a result of better management, rather than increased spending. To account for this, we further control for other changes in school inputs. We use the ASER school level data and include average student teacher ratio, proportion of schools with a toilet, proportion of schools with tap water and average number of rooms in a classroom for each state year allow the effects of these inputs to vary by grade. As the results in column 3 and 4 show the effects remain very similar to those in Table 3 [19]. In addition, as discussed previously in Table 4 even we restrict our sample to include only the first year of the policy, our results are unchanged. It is plausible that changes to school infrastructure, student-teacher ratios etc. take many years to get implemented, whereas the NDP was implemented immediately. This further reduces the threat of other components of the RTE driving our results.

Finally, we perform a battery of additional robustness checks. We present these results in Table A3 . First, we include state-grade specific quadratic time trends in addition to linear trends. This helps control for pre-existing trends in a more flexible manner. As the results indicate, introduction of quadratic trends leaves our main results unchanged. Second, we control for district level rainfall shocks. Since we are using only 3 years of pre-treatment data, rainfall shocks in those years can have an effect on learning outcomes. Following, Shah and Steinberg (2017) we include district-specific rainfall shocks of the year of survey and the preceding year and interact these shocks with grades, after controlling for district-year fixed effects. Thus we let the affect of rainfall shocks vary by grade. [20] The results remain very similar to Table 3. Finally, we run the regressions including grade 1 and grade 2 in our sample. Column 5 and column 6 of Table A3 presents these results which are of comparable magnitude to our main specification.

5.5 Heterogeneity

Are the benefits of the NDP broad based? We first look at heterogeneity in outcomes by individual characteristics, such as gender and mother's education.To do this, we re-estimate our main specification, but separately for different sub-groups of the population. As can

[18]The data on budget share is obtained from Reserve Bank of India. The data can be accessed `https://rbidocs.rbi.org.in/rdocs/Publications/PDFs/26ST_120720188B03C920F2684136A715C9639097939A.PDF`

[19]ASER does not have school level data for 2008, so 2008 is excluded in these regressions with school inputs

[20]We use the same definition of rainfall shock as Shah and Steinberg (2017) where the shock variable takes the value 1 if rainfall in that district-year is more than the 80^{th} percentile of the historic (district specific) distribution and -1 if rainfall in that district-year is less than the 20^{th} percentile of the historic distribution. We also use the same data from University of Delaware. The results exclude Sikkim and Meghalaya as they were not covered in ASER 2007

be seen from Table 6 both boys and girls benefitted from the program, however we find girls benefitted more from the program.[21] This may be because girls face more discouragement in response to poor academic performance in general, and from repeating a grade in particular. As a result the gains from a "fear-free" learning environment created by the NDP might be higher for them. Next, we look at heterogeneity by mother's education by estimating separate regressions for children of mothers with less than primary education and for those whose mother have at least a primary education. These results reported in table Table A4, show that these effect sizes are similar for both sub-groups, indicating broad based gains from the policy. [22]

Next we turn our attention to heterogeneity by geography. First, we look at heterogeneity by baseline district level repetition rates. We calculate district-grade level repetition rates for pre NDP years combining DISE data between 2007 and 2009. We then create two categories - districts-grades with repetition rates above and below the media, and test for whether the effects of NDP differ across these two set of districts. The results in Table 7 show that the effects are largely driven by districts with above median repetition rates. This is exactly what we would expect, since removal of grade repetition in districts where repetition was low to begin with should have limited (or no) effects. Second, we look at heterogeneity of the effect of the policy by baseline learning levels. Here we calculate average district-grade level test scores from 2007 to 2009 and use this to classify district-grades as being above or below median. We then examine the effects of NDP separately for districts above or below the median. These results are shown in tables Table A5. As the results show, while both sets of districts benefit from the policy, the effects are larger for districts with above-median baseline test scores.

5.6 Possible Mechanisms

The results so far indicate a positive effect of NDP on learning outcomes, and seem to be benefitting all sub-groups of the population. There can be several possible mechanisms driving the result. Students may be investing more effort and time after the introduction of NDP. This may be because of increased motivation a result of "fear-free" learning environment the policy creates. Parents may be undertaking additional investments in response to the policy. These increased investments can come from increased time, effort or more investments on tuition, books etc. Unfortunately, data restrictions do not permit a comprehensive analysis of all possible mechanisms. However, we use the available data to test for, and rule out, certain channels.

One possibility is that parents respond to this policy by increasing their investment

[21] These differences are statistically significant at 1 percent level.
[22] These differences are statistically insignificant

in children's education by hiring more private tutors. Column 1 of table Table 8 reports treatment effects of the policy on the probability that a child is enrolled in private tuitions, and finds no effects [23]. Next, we test for changes in student effort. While there are several dimensions of student effort, we only have data on attendance at the school level. For this we use the school level survey of ASER. Column 2 of Table 8 shows the results of a regression using attendance as the dependent variable. We find no changes in attendance in response to the policy. Thus, though we cannot identify the exact channel through which the NDP is increasing learning, we can rule out increased parental investments and increased attendance as the key drivers.

Finally, we examine the possibility that the NDP improves learning outcomes by improving student motivation. Lack of motivation is likely to effect students with the lowest scores, i.e at the bottom end of the score distribution. Large positive effects of the NDP among the lowest performing students, would be consistent with the hypothesis of improved motivation driving our findings. In order to achieve this we re-estimate our main specification separately for each quartile of the test score distribution. The results in Table 9 show that overall, the effects for both reading and math are strongest for the bottom two quartiles, and weakest for the top quartile. This provides suggestive evidence that the NDP improves student performances by influencing motivation levels.

6 Conclusion

Despite the wide prevalence of social promotion policies across schools in several countries, its effects are not well understood. One the one hand it can create a stress free environment and on the other it might disincentives learning. Most causal empirical studies find effects of such policies on repeaters, by comparing students who failed by a small margin to those who cleared the test by a small margin. This approach fails to capture the effect of a threat of detention on non-repeaters and on those who fail by a large margin. Moreover, most of the existing evidence is from developing countries. In this paper, we evaluate the impact of a national social promotion policy in India on learning outcomes. Before the policy was implemented in 2010, different states followed social promotion till different grades, while the national policy mandated "no detention" in all schools till grade 8. We use this variation to identify the effects of the policy on average learning outcomes of a grade.

First, we use administrative data and document that the policy was indeed implemented, and establish a sharp decline in repetition rates. We then use 8 waves of a large

[23] We also tested for, and found no effects on the intensive margin - the amount of money spent on private tuitions does not change in response to this policy.

scale household survey on learning outcomes and find the NDP increased reading scores by 2.5 percent and math scores by 5 percent. We validate our finding by event study analysis and also conduct several robustness checks. We argue the effects are not a artifact of a compositional changes. We also rule out other changes in school inputs driving the results. The results indicate that while both boys and girls benefited from the policy, girls benefited more. However, the effects are not different by the level of parents' education. Finally, the districts which had the highest baseline repetition rates benefited the most. Given data constraints, we were restricted in our ability to pinpoint the exact underlying mechanism behind the results, although we were able to rule out a few possible channels such as increased private tutoring and increased attendance in school. Understanding how teachers, parents and students changed their behavior in response to the policy is crucial, which can be an important area for future research. We also find that the effects are concentrated in the lowest quartile of the learning distribution. We use this to argue that the effects are a likely outcome of improved motivation and the creation of a "fear free" learning environment.

It is important to note that our study only evaluates the short term impacts of the policy. The long term impacts could be very different, and could depend on a lot of other factors such as changes in parental and school inputs. Moreover, our study only could measure the impact of the program on basic learning outcomes. It is possible that the program has a different impact on advanced learning, even in the short run, and this is also an important policy-relevant question for future research.

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7 Tables and Figures

FIGURE 1

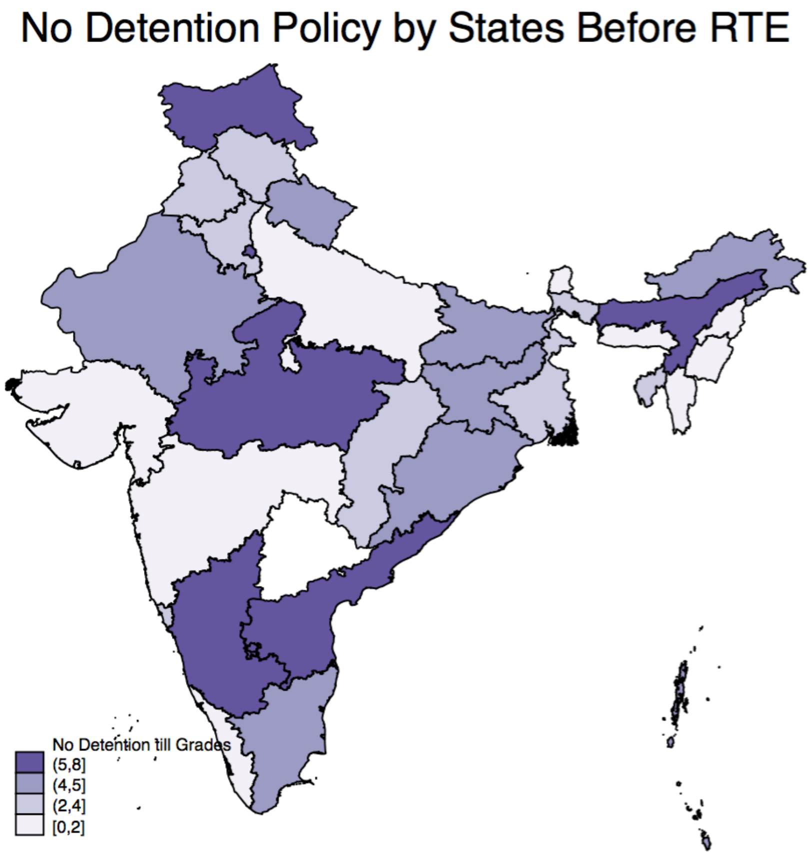
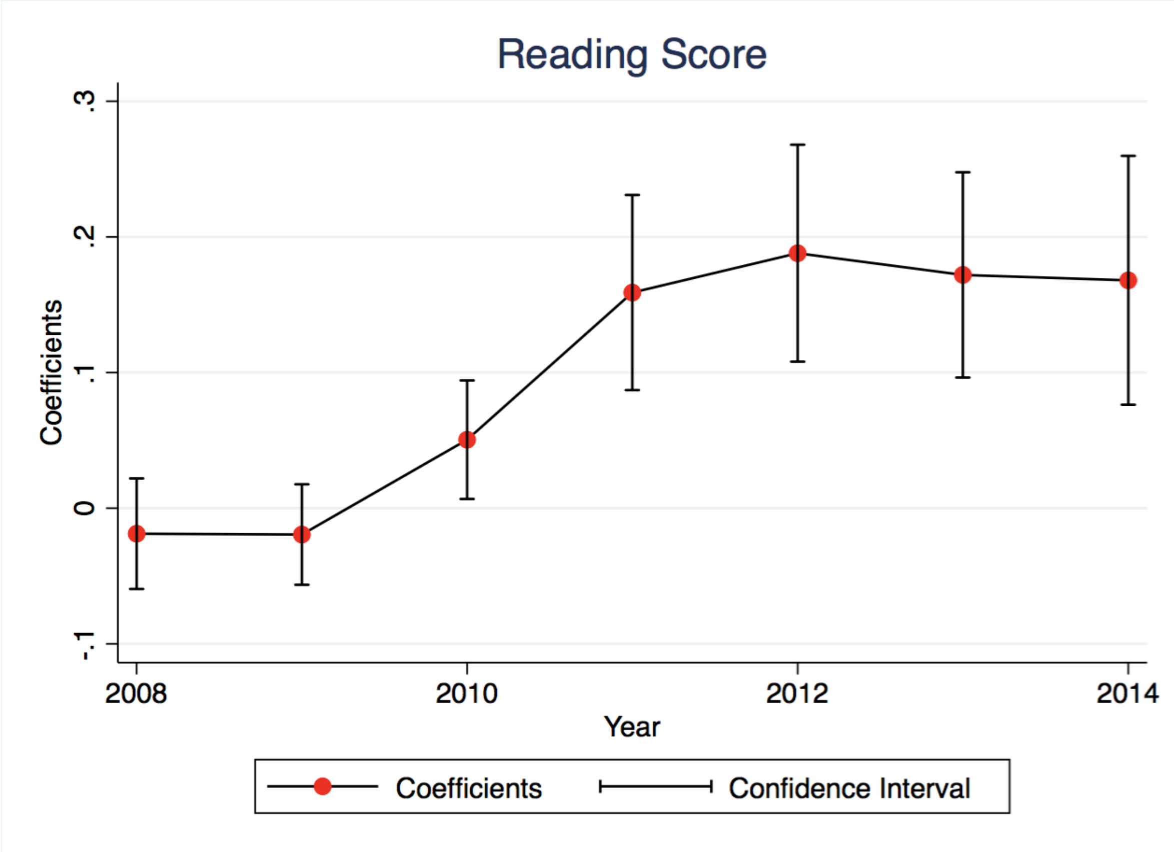
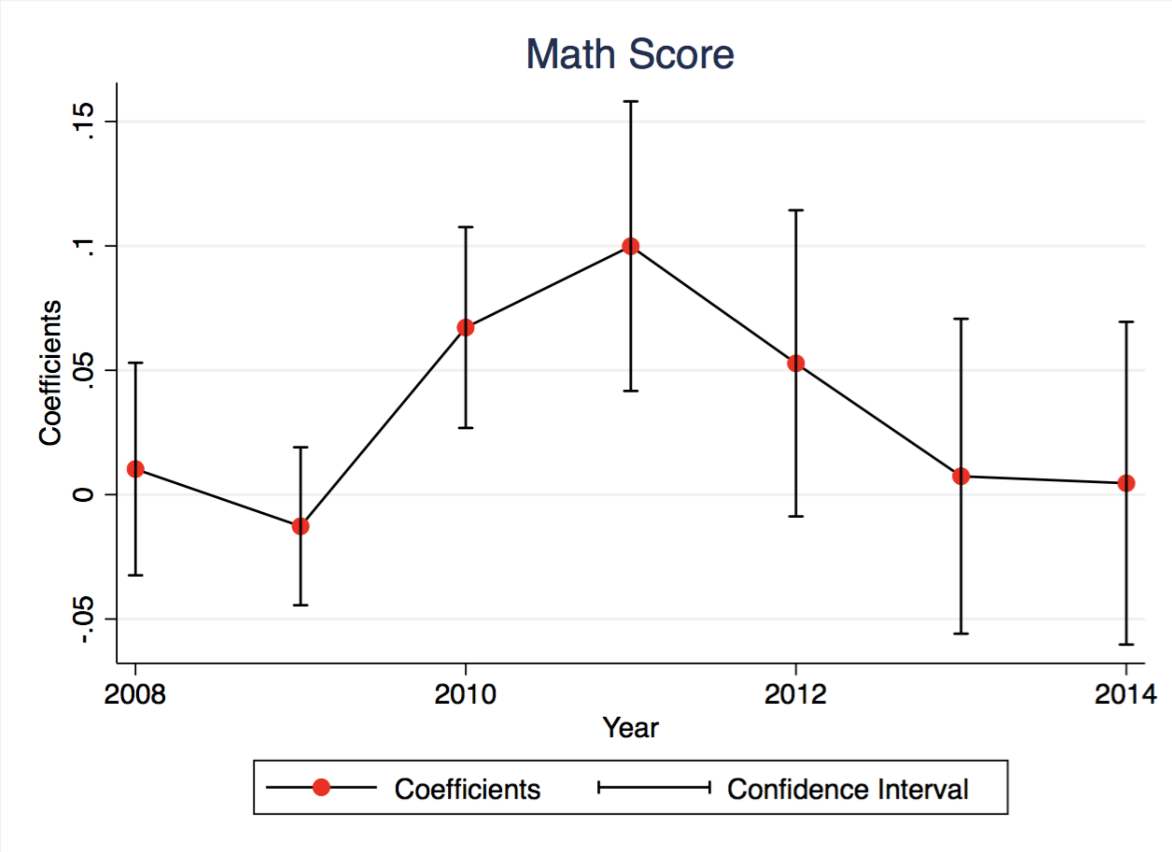


FIGURE 2



Notes: Coefficients for each year is the coefficient of a dummy representing affected state-grade (i,e state-grade which had no social promotion policy in place before NDP) interacted with a dummy for that year obtained by regressing reading score on the interaction dummy mentioned above after controlling for state-grade fixed effects, grade-year fixed effects. The regressions are done taking two years at a time with **2007 as a base year**. Confidence interval indicated are 95 percent confidence intervals. Standard errors are clustered at the state-grade level.

FIGURE 3



Notes: Coefficients for each year is the coefficient of a dummy representing affected state-grade (i,e state-grade which had no social promotion policy in place before NDP) interacted with a dummy for that year obtained by regressing math score on the interaction dummy mentioned above after controlling for state-grade fixed effects, grade-year fixed effects. The regressions are done taking two years at a time with **2007 as a base year**. Confidence interval indicated are 95 percent confidence intervals. Standard errors are clustered at the state-grade level.

TABLE 1: SUMMARY STATISTICS

	ASER Household Data		
	Mean	Standard Deviation	N
Reading Score	2.61	1.38	2208255
Math Score	2.38	1.27	2208255
% Perfect Reading Score	0.39	0.49	2208255
% with Perfect Math Score	0.25	0.44	2208255
% using private tutors	0.21	0.40	2208255
% Female	0.48	0.50	2208255
% with Mothers < Primary Education	0.73	0.44	2208255

	ASER School Data		
	Mean	Standard Deviation	N
Average class size	30.42	27.91	592,225
Average Attendance Rate	0.73	0.23	592,225

	DISE Data		
	Mean	Standard Deviation	N
% of Repeaters	0.04	0.05	49,777

This table provides summary statistics from ASER data, with the sample being restricted to government schools (see main text for details). Proportion of repeaters comes from DISE data.

TABLE 2: REPETITION AND CLASS SIZE

VARIABLES	(1) Repeaters	(2) Proportion Repeaters	(3) Enrollment
Treatment	-503.0***	-0.0134***	-500.6
	(159.5)	(0.00357)	(1055.9)
Observations	37,506	37,285	19,813
R-squared	0.768	0.739	0.954
Dependent Variable Mean	1100	0.0322	36894

The dependent variable in column 1 is number of repeaters in a district-grade-year, the dependent variable in column 2 is proportion of repeaters in each district-grade-year, and the dependent variable in column 3 is the number of students enrolled in a district-grade-year. In column 3, grades greater than the first affected grades are dropped (see main text for details).Treatment takes the value 1 if the state-grade-year had a no detention policy in place. The regressions include grade 3 to grade 8 and control for state-grade fixed effects, state-year fixed effects, district fixed effects and state-grade specific linear time trends. Standard errors are clustered at state-grade level.

TABLE 3: LEARNING OUTCOMES

VARIABLES	(1) Reading	(2) Math
Treatment	0.0806***	0.124***
	(0.0256)	(0.0270)
Observations	1,648,308	1,648,308
R-squared	0.279	0.310
Dependent Variable Mean	3.030	2.752

The dependent variable in column 1 is reading test scores, and in column 2 are math test scores. Both reading and math test scores takes values 0-4. Treatment takes the value 1 if the stage-grade-year had a no detention policy in place. The sample includes only children older than age of 5, currently enrolled in government schools between the grades of 3 and 8. The regressions control for state-grade fixed effects, state-year fixed effects and state-grade specific linear time trends. Controls for family size, child gender and age are also included. Standard are clustered at state-grade level.

TABLE 4: LEARNING OUTCOMES -ISOLATING TREATMENT EFFECTS

	(1)	(2)
VARIABLES	Reading	Math
Treatment	0.0822**	0.0836***
	(0.0348)	(0.0316)
Observations	965,998	965,998
R-squared	0.283	0.300
Dependent Variable Mean	3.154	2.929

The dependent variable in column 1 is reading test scores, and in column 2 are math test scores.Treatment takes the value 1 if the stage-grade-year had a no detention policy in place.The sample includes only children older than age 5, currently enrolled in government schools between the grades of 3 and 8, and does not include data for years beyond 2010. The regressions control for state-grade fixed effects, state-year fixed effects and state-grade specific linear time trends. Controls for family size, child gender and age are also included. Standard are clustered at state-grade level.

TABLE 5: ALTERNATIVE TEST FOR ISOLATING TREATMENT EFFECTS

VARIABLES	(1) Reading	(2) Math
Treatment	0.0743**	0.128***
	(0.0319)	(0.0365)
Observations	1,397,646	1,397,646
R-squared	0.266	0.294
Dependent Variable Mean	3.044	2.768

The dependent variable in column 1 is reading test scores, and in column 2 are math test scores. Treatment takes the value 1 if the stage-grade-year had a no detention policy in place. The sample includes only children older than age 5, currently enrolled in government schools between the grades of 3 and 8, and does not include the first affected grade in each state (see main text for details). The regressions control for state-grade fixed effects, state-year fixed effects, district fixed effects and state-grade specific linear time trends. Controls for family size, child gender and age are also included. Standard are clustered at state-grade level.

TABLE 6: HETEROGENEITY BY GENDER

	Reading		Math	
	Boys	Girls	Boys	Girls
Treatment	0.0532**	0.112***	0.101***	0.147***
	(0.0269)	(0.0264)	(0.0272)	(0.0279)
Observations	856,312	791,996	856,312	791,996
R-squared	0.275	0.286	0.306	0.315
Dependent Variable Mean	3.043	3.015	2.797	2.702

The dependent variable in columns 1 and 2 are reading scores, and in column 3 and 4 are math scores. Treatment takes the value 1 if the stage-grade-year had a no detention policy in place. The sample includes only children older than age 5, currently enrolled in government schools between the grades of 3 and 8. The regressions control for state-grade fixed effects, state-year fixed effects and state-grade specific linear time trends. Controls for family size and child age are also included. Standard errors are clustered at state-grade level.

TABLE 7: HETEROGENEITY BY BASELINE REPETITION RATES

	Reading		Math	
	Low Repetition	High Repetition	Low Repetition	High Repetition
Treatment	0.0283	0.130***	0.0561***	0.184***
	(0.0186)	(0.0381)	(0.0214)	(0.0415)
Observations	755,053	851,364	755,053	851,364
R-squared	0.329	0.326	0.362	0.375
Dependent Variable Mean	2.978	3.068	2.706	2.786

The dependent variable in columns 1 and 2 are reading scores, and in column 3 and 4 are math scores. Treatment takes the value 1 if the stage-grade-year had a no detention policy in place. The sample includes only children older than age 5, currently enrolled in government schools between the grades of 3 and 8. District with below median repetition rates from 2007-2009 form the "low repetition" sample, whereas "high repetition" sample includes districts with above median repetition rates from 2007-2009. The regressions control for state-grade fixed effects, district-year fixed effects and state-grade specific linear time trends. Controls for family size and child age and gender are also included. Standard are clustered at state-grade level.

TABLE 8: MECHANISMS

	Tuition	Attendance
Treatment	0.00260	0.000684
	(0.0102)	(0.00306)
Observations	930,831	393,418
R-squared	0.338	0.315
Dependent Variable Mean	0.221	0.734

The dependent variable in column 1 takes the value 1, if a student attended private tuition, and 0 otherwise. Column 2, is a school level regression, where the dependent variable is the proportion of enrolled students who attended school on the day of the ASER survey. Treatment takes the value one if the stage-grade-year had a no detention policy in place. Data on tuition is not available for the years 2008 and 2009. The regressions control for state-grade fixed effects, state-year fixed effects and state-grade level linear trends. Grades 3 to 8 are included. For the student level regressions, controls for family size, child gender and age are included. Standard errors in parenthesis are clustered at state-grade level.

TABLE 9: TREATMENT EFFECT BY QUARTILE

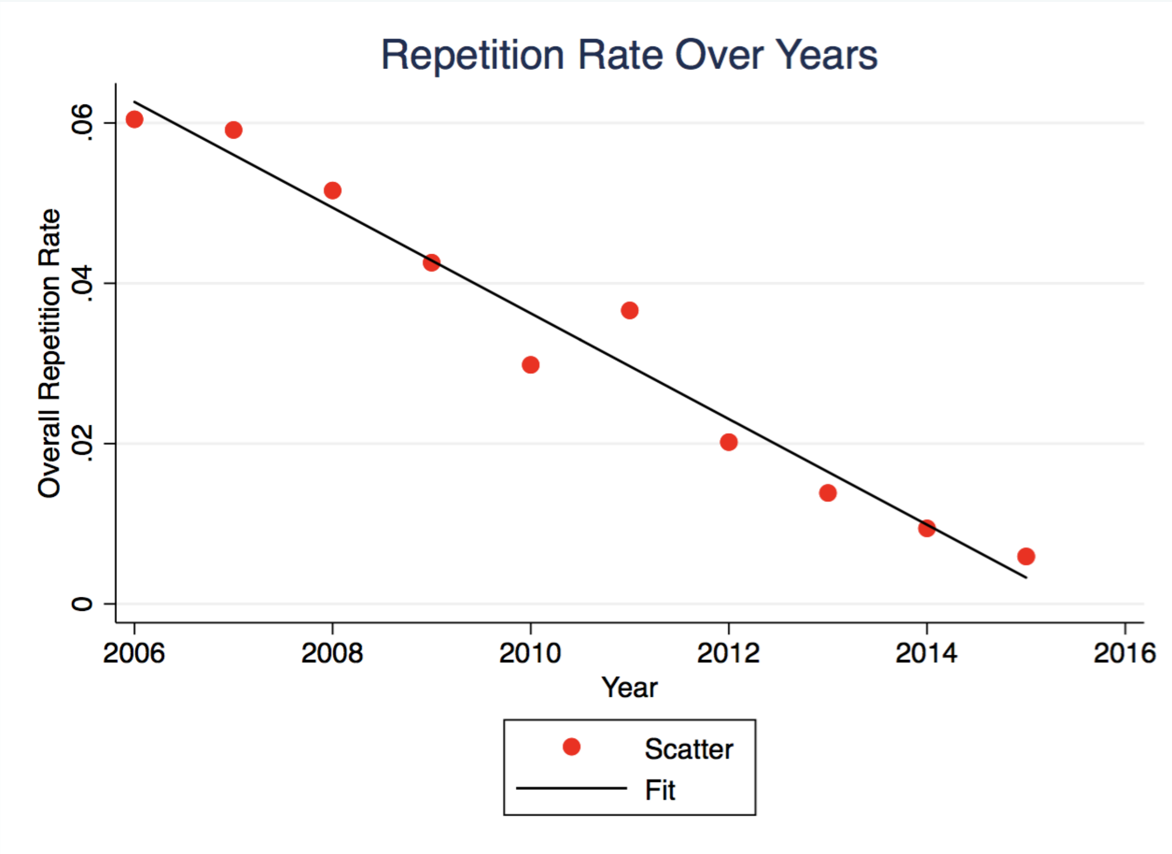
	Reading			
	(1)	(2)	(3)	(4)
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Treatment	0.110***	0.0962	0.0656*	0.0417*
	(0.0355)	(0.0601)	(0.0338)	(0.0235)
Observations	411,215	411,878	412,221	411,564
R-squared	0.594	0.858	0.830	0.468
Dependent Variable Mean	1.734	2.944	3.524	3.912
	Math			
	(1)	(2)	(3)	(4)
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Treatment	0.0899***	0.260***	0.130**	0.00691
	(0.0200)	(0.0530)	(0.0604)	(0.0268)
Observations	411,309	412,104	412,190	411,278
R-squared	0.570	0.845	0.825	0.588
Dependent Variable Mean	1.561	2.552	3.160	3.728

Treatment takes the value 1 if the state-grade-year had a no detention policy in place. The sample includes only children older than age 5, currently enrolled in government schools between the grades of 3 and 8. The regressions control for state-grade fixed effects, state-year fixed effects and state-grade specific linear time trends. Controls for family size and child age and gender are also included. Standard errors are clustered at state-grade level.

8 Appendix

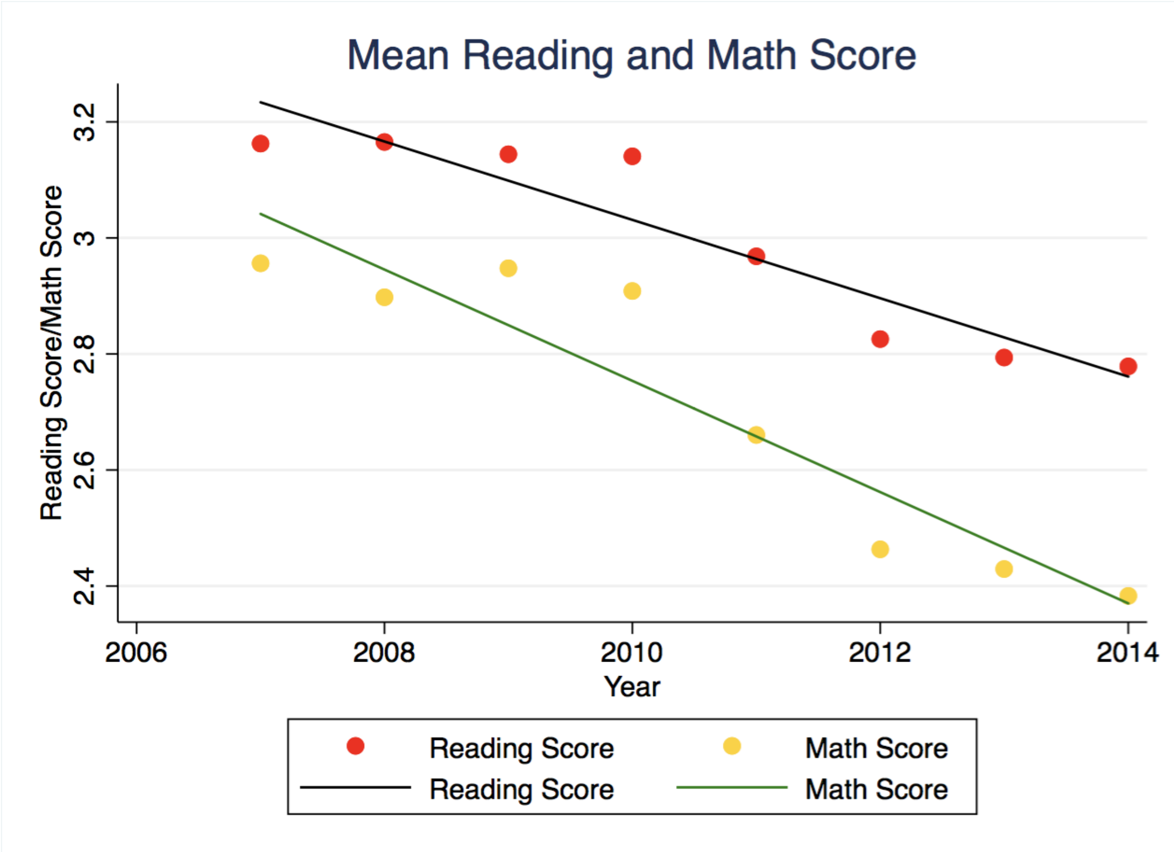
Figures

FIGURE A1



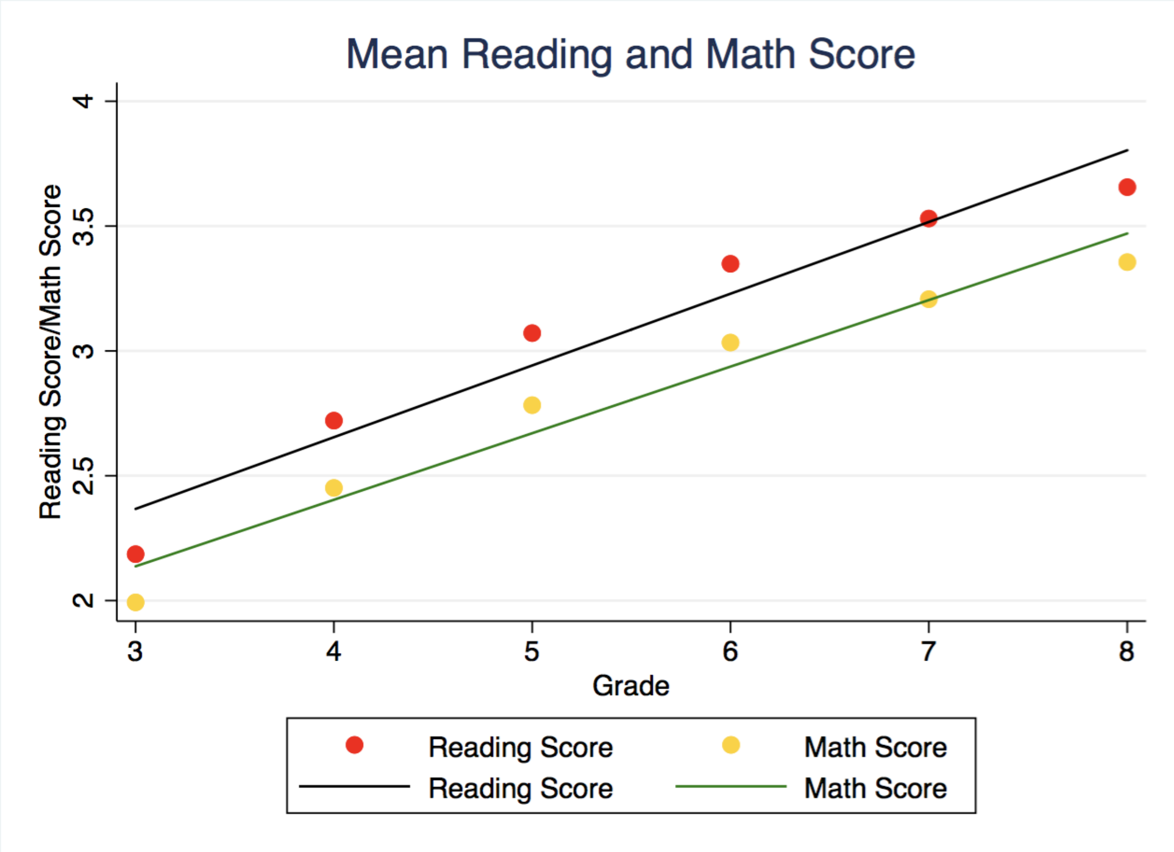
Notes: Proportion of repeaters in a district is calculated as total repeaters at a grade/total enrollment at a grade from the DISE data. This is then averaged across districts in India for each year. Grades 3 to 8 are included.

FIGURE A2



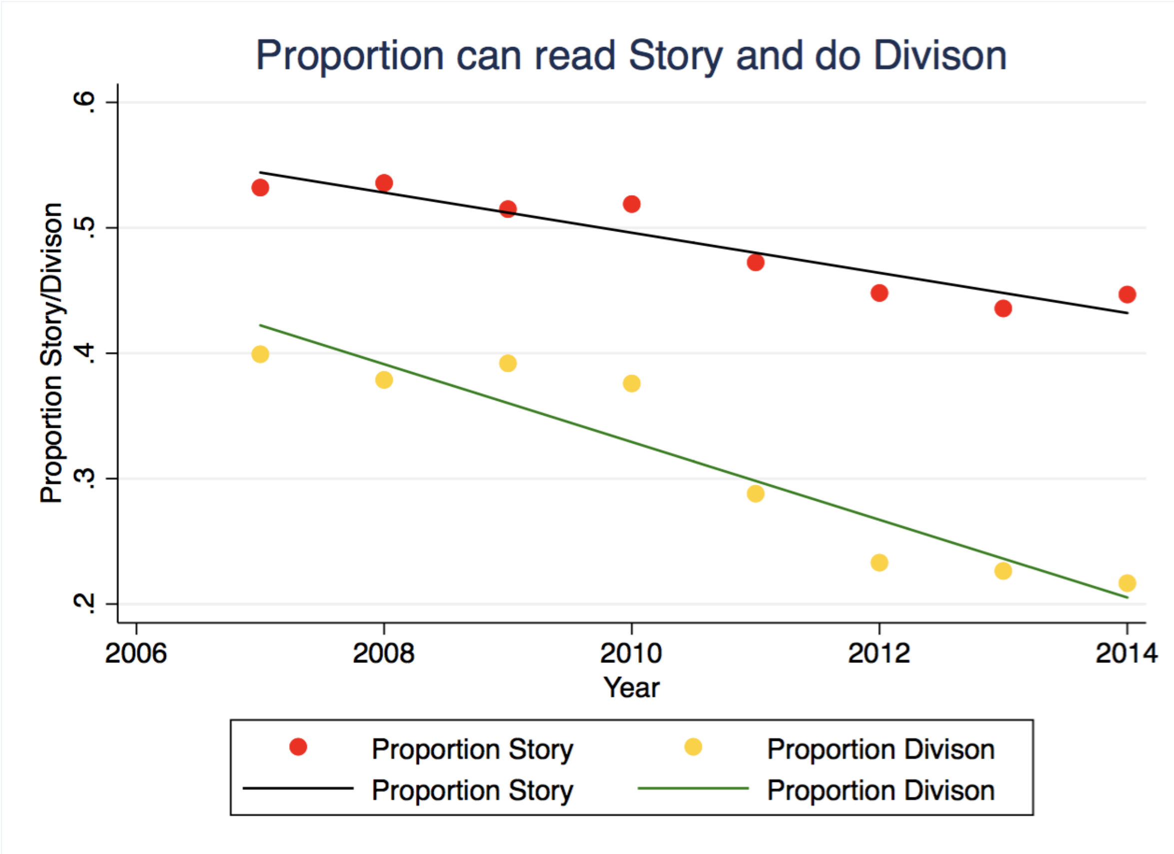
Notes: Mean unweighted scores are calculated for students in government schools from grade 3 to 8 across India by each year from the ASER data.

FIGURE A3



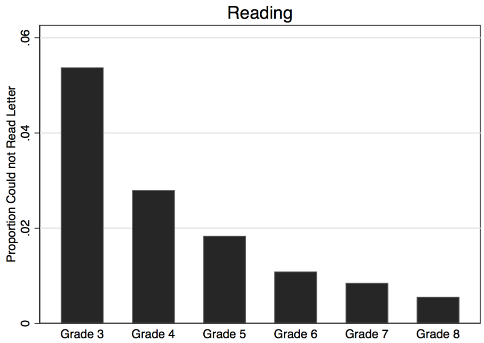
Notes: Mean unweighted scores combining all years are calculated for students in government schools for each grade across India from the ASER data.

FIGURE A4

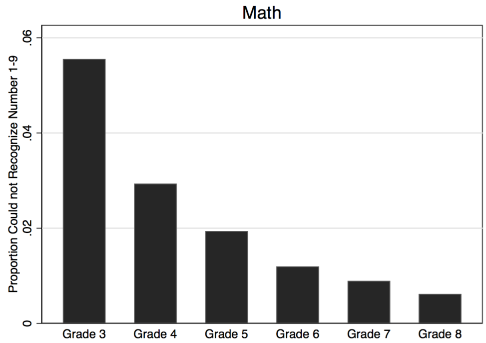


Notes: Proportions are calculated for students in government schools from grade 3 to 8 across India by each year from the ASER data.

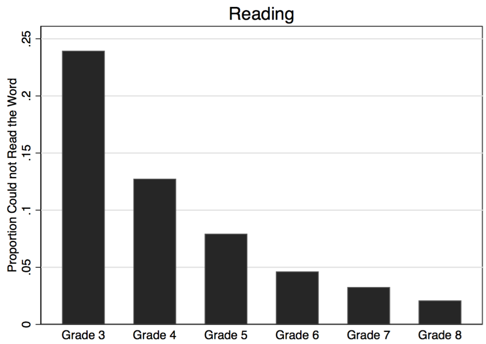
FIGURE A5: SCORES BY GRADE



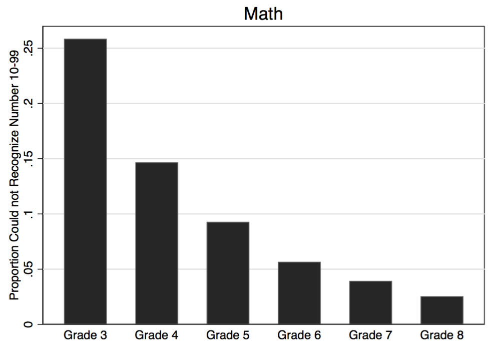
(A) SCORED ZERO IN READING



(B) SCORED ZERO IN MATH



(C) SCORED ONE OR LESS IN READING



(D) SCORED ONE OR LESS IN MATH

Notes: Proportion of students who scored zero or one in the reading and math test by grade. Students enrolled in a government schools from grade 3 to grade 8 are included.

Tables

TABLE A1: DROPOUT AND PRIVATE SCHOOL

VARIABLES	(1) Dropout	(2) Private School
Treatment	-0.0598***	0.00484
	(0.00675)	(0.00337)
Observations	2,467,475	2,183,061
R-squared	0.759	0.151
Dependent Variable Mean	0.0410	0.244

The dependent variable in column 1 takes the value one if the child is in school and zero otherwise, and in column 2 the dependent variables takes the value one if the child is in a private school. Treatment takes the value 1 if the stage-grade-year had a no detention policy in place.The sample includes only children older than age 5. Grade 3 to grade 8 are included. The regressions control for state-grade fixed effects, state-year fixed effects and state-grade specific linear time trends. Controls for family size, child gender and age are also included. Standard errors are clustered at state-grade level.

TABLE A2: INCLUDING BUDGET SHARE ON EDUCATION

VARIABLES	(1) Reading	(2) Math	(3) Reading	(4) Math
Treatment	0.0765***	0.122***	0.0851***	0.104***
	(0.0219)	(0.0247)	(0.0232)	(0.0222)
Observations	1,635,748	1,635,748	1,387,563	1,387,563
R-squared	0.279	0.310	0.274	0.305
Dependent Variable Mean	3.029	2.752	3.029	2.752

The dependent variable in column 1 and 3 is reading test scores, and in column 2 and 4 are math test scores. Treatment takes the value 1 if the stage-grade-year had a no detention policy in place. The sample includes only children older than age 5, currently enrolled in government schools between the grades of 3 and 8. The regressions control for state-grade fixed effects, state-year fixed effects and state-grade specific linear time trends. Controls for family size, child gender and age are also included. The regressions in column 1 and 2 includes yearly budget share on education for each state government interacted with grades. The regressions in column 3 and 4 in addition also includes state year level student teacher ratio, proportion of schools with toilets in each state year, proportion of schools with tap water in each state year, average number of classrooms in a state year interacted with grades. The regression excludes, union territories Dadra and Nagar Haveli, Daman and Diu and Pondicherry. Column 3 and column 4 excludes the year 2008 as ASER school data was not available. Standard errors are clustered at state-grade level.

TABLE A3: ROBUSTNESS

	Quadratic Trend		Rainfall Controlled		Grade 1-Grade 8	
	Reading	Math	Reading	Math	Reading	Math
Treatment	0.0969***	0.104***	0.0860***	0.135***	0.0517***	0.103***
	(0.0328)	(0.0306)	(0.0276)	(0.0290)	(0.0190)	(0.0184)
State Grade FE	Y	Y	Y	Y	Y	Y
State Year FE	Y	Y	N	N	Y	Y
District Year FE	N	N	Y	Y	N	N
State-Grade Linear Trend	Y	Y	Y	Y	Y	Y
Observations	1,648,308	1,648,308	1,607,075	1,607,075	2,176,077	2,176,077
R-squared	0.279	0.310	0.326	0.365	0.482	0.479
Dependent Variable Mean	3.030	2.752	3.023	2.742	2.606	2.383

The dependent variable in columns 1, 3 and 5 are reading scores, and in column 2, 4 and 6 are math scores. Treatment takes the value 1 if the stage-grade-year had a no detention policy in place. The sample includes only children in government school, age greater than five, and in grades greater than 3 and less than or equal to 8 (except for column 5 and 6). Controls for family size and child age and gender are also included. Standard errors are clustered at state-grade level.

TABLE A4: HETEROGENEITY BY MOTHER'S EDUCATION

	Reading		Math	
	High Education	Low Education	High Education	Low Education
Treatment	0.103***	0.0793***	0.117***	0.135***
	(0.0184)	(0.0293)	(0.0210)	(0.0299)
Observations	417,714	1,148,740	417,714	1,148,740
R-squared	0.236	0.296	0.280	0.327
Dependent Variable Mean	3.299	2.932	2.997	2.658

The dependent variables in columns 1 and 2 are reading scores, and in column 3 and 4 are math scores. Treatment takes the value 1 if the stage-grade-year had a no detention policy in place. The sample includes only children older than age 5, currently enrolled in government schools between the grades of 3 and 8. Children of mothers with less than a primary school education form the 'low education" sample, and children whose mothers had at least a primary school education form the "high education" sample. The regressions control for state-grade fixed effects, state-year fixed effects, and state-grade specific linear time trends. Controls for family size and child age and gender are also included. Standard errors are clustered at state-grade level.

TABLE A5: HETEROGENEITY BY BASELINE TEST SCORES

	Reading		Math	
	High Scores	Low Scores	High Scores	Low Scores
Treatment	0.145***	0.0390**	0.185***	0.112***
	(0.0404)	(0.0168)	(0.0423)	(0.0157)
Observations	828,971	814,776	835,845	807,904
R-squared	0.312	0.304	0.368	0.310
Dependent Variable Mean	3.243	2.815	2.970	2.531

The dependent variable in columns 1 and 2 are reading scores, and in column 3 and 4 are math scores. Treatment takes the value 1 if the stage-grade-year had a no detention policy in place. The sample includes only children older than age 5, currently enrolled in government schools between the grades of 3 and 8. District with below median average test scores between 2007-2009 form the "low scores sample, whereas "high scores" sample includes districts with above median average test scores between 2007-2009. The regressions control for state-grade fixed effects, district-year fixed effects and state-grade specific linear time trends. Controls for family size and child age and gender are also included. Standard errors are clustered at state-grade level.