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The causal effect of East Asian ‘mastery’ teaching methods on English children’s mathematics skills?

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Abstract

A small group of high-performing East Asian economies dominate the top of the Programme for International Student Assessment (PISA) rankings. Although there are many possible explanations for this, East Asian teaching methods and curriculum design are two factors to have particularly caught policymakers’ attention. Yet there is currently little evidence as to whether any particular East Asian teaching method actually represents an improvement over the status quo in England, and whether such methods can be successfully introduced into Western education systems. This paper provides new evidence on this issue by presenting results from two clustered Randomised Controlled Trials (RCT’s), where a Singaporean inspired ‘mastery’ approach to teaching mathematics was introduced into a selection of England’s primary and secondary schools. We find evidence of a modest but positive treatment effect. Moreover, even under conservative assumptions, the programme has the potential to offer substantial economic returns.

JEL codes: I2

Keywords: Maths Mastery; Randomised Controlled Trial; Singapore; PISA

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1. Introduction

The Programme for International Student Assessment (PISA) is a major cross-national study of school children's academic achievement. Since its inception in 2000, its ranking of the world's education systems has drawn the attention of academics, educationalists, journalists and policymakers alike. A small group of high-performing East Asian economies (e.g. Singapore, Japan, Hong Kong, South Korea) consistently dominate the top of these international 'league tables'. This is particularly true of mathematics, where children from such countries are, on average, more than one school year ahead of their Western peers (Jerrim and Choi 2014). Consequently, two of the most frequently asked questions by education policymakers today are '*what drives East Asian educational success'* and '*what can we do to catch up*'?

Although there are many possible explanations for this phenomenon (Jerrim 2014), teaching methods and design of the curriculum have particularly caught policymakers' attention. For instance, to inform upcoming changes to the mathematics curriculum in England, the Department for Education (2012) conducted an extensive review of the mathematics syllabus in a number of East Asian countries. Similarly, a selection of British officials have visited East Asian economies to observe their teaching practises (Department for Education 2014), under the presumption that this is driving their educational success. Indeed, as Liz Truss (former Under Secretary of State for Education in England) noted of one such visit:

> '*this represents a real opportunity for us to see at first hand the **teaching methods** that have enabled their young people to achieve so well in maths.*' [Emphasis our own].

Thus, despite difficulties in even defining the concept of an ‘East Asian teaching method’, policymakers continue to believe this to be a key reason why mathematics achievement is so much greater in the East than the West.

Yet simplistic attempts to ‘borrow policy’ from other countries is problematic (Crossley and Watson 2009). Two particular issues stand out. The first is causality. There are significant cultural, economic and historic differences between countries, meaning it is almost impossible to tell from studies like PISA what is leading to the differences observed. Consequently, there is very little evidence that East Asian teaching methods, however defined, are actually superior to those currently being used in England’s (or other Western countries) schools. Second, even if some East Asian teaching methods are potentially more effective than the status quo, one simply does not know whether they can be successfully implemented within the English (or, indeed, other) educational systems.

This paper attempts to provide some robust evidence to begin to fill this important gap in the literature. Specifically, it provides (to our knowledge) the first evidence as to how introducing a particular East Asian inspired teaching method into a Western schooling system influences children’s mathematics test scores. This is done via estimation of the causal effect of the ‘Maths Mastery’ teaching programme after it has been implemented within a selection of England’s primary and secondary schools for one academic year. This particular programme is based upon approaches to teaching mathematics in Singapore (ranked 2nd out of 65 economies in the PISA 2012 mathematics rankings) and represents a radical change to standard practise in England (see Guskey 2010). In particular, fewer topics are covered in greater depth, with every child expected to reach a certain level (i.e. to ‘master the curriculum’) before the class progresses on to the next part of the syllabus. The notion that Singaporean teachers place more emphasis on whole class mastery of concepts is supported by the Teaching and Learning International Survey (Micklewright et al 2014). This indicates that, whereas three-in-five teachers in England differentiate their lessons for pupils with different abilities, only one in five Singaporean teachers do. Greater emphasis is also placed upon children’s problem solving skills, with this complemented by an integrated professional development programme for teachers, and the sharing of best practise amongst a network of schools.

This paper reports results from two field experiments designed to estimate the causal effect of a one year exposure to this programme. A clustered Randomised Controlled Trial (RCT) methodology is used, involving more than 10,000 pupils enrolled in 90 English primary schools and 50 secondary schools during the 2012/13 and 2013/14 academic years. Both the primary and the secondary school trials suggest a positive impact, though the latter did not reach statistically significance at conventional thresholds. Combining results from across these two trials, and thus increasing statistical power, we find indicative evidence of a modest though positive treatment effect. It is noteworthy that the magnitude of the effect found is similar to that for some other curriculum and pedagogical interventions also attempting to improve basic skills. This includes the 'The Literacy Hour' - a change made to the English curriculum made in the late 1990's – which was found to have a small, positive impact by Machin and McNally (2008).

These experimental results are complemented by estimates of the programme's economic costs and benefits. Specifically, we use a rich British panel survey to estimate how a modest increase in age 10 mathematics test scores (consistent with the effect found for the Maths Mastery programme) influences later lifetime earnings. These labour market benefits are then compared to the costs of the programme, with the Net Present Value (NPV) and Internal Rate of Return (IRR) calculated under three scenarios (conservative, baseline and optimistic). We find evidence of a high IRR (eight percent) even under conservative assumptions. This illustrates how low cost interventions can potentially provide substantial economic returns, even when effect sizes are small.

The paper proceeds as follows. Section 2 overviews the Maths Mastery (MM) intervention, with our empirical methodology detailed in section 3. Section 4 provides results from the two RCT's, while section 5 compares economic costs to estimated labour market benefits. Conclusions and directions for future research follow in section 6.

2. The Maths Mastery intervention

Maths Mastery is delivered in England by the academy chain ARK. Our study considers two particular versions of their programme; one appropriate for Year 1 pupils (age 5/6) and one appropriate for year 7 pupils (age 11/12). The introduction of a ‘mastery curriculum’ is central to the MM approach. This is where the vast majority of pupils’ progress through the curriculum at the same pace, with subject matter and learning content broken into units with clearly defined goals. Academically weaker pupils are expected to reach a basic standard in each unit before the whole class moves on to the next topic together. In the meantime, more able pupils are encouraged to explore the current learning unit in more depth. (This is in contrast to standard practise in England, where more able pupils are accelerated on to learning a new topic). It is thought that this approach reduces the need to repeatedly revisit material, and promotes depth of understanding over memorised procedures.

Other features of MM include a systematic approach to mathematical language (Hoyles 1985), frequent use of objects and pictures to represent mathematical concepts (Heddens 1986; Sowell 1989), and an emphasis on high expectations and a ‘growth’ mind-set (Dweck 2006; Boaler 2010). For younger children, this translates into prominent use of objects and pictures to illustrate numbers. The approach also prioritises problem solving skills, and encourages deep understanding of mathematics over procedural knowledge (Skemp 2006). Every pupil is therefore expected to understand what they are doing, rather than just learning to repeat routines. This in turn means they are better equipped to apply this knowledge when solving numerical problems.

Figure 1 provides an example of the Maths Mastery approach. In this, children are asked:

‘*There are three consecutive numbers that add up to 42. What are these numbers?*’

Young children in England would typically use an iterative ‘trial and improvement’ method to answer this question. In contrast, Maths Mastery emphasizes the representation of numbers and connections between them. A prime example of this is ‘bar-modelling’ as illustrated in Figure 1. (This technique is typically taught to children from around age 6 or 7). Maths Mastery pupils will recognise that, as the total is 42, the total without the ‘ones’ (i.e. the grey portions of the bars) equals 39. Then, if the three sections add up to 39, each must be worth 13 (since 39 ÷ 3 = 13). As the question states the numbers are sequential, children then immediately reach the answer of 13, 14 and 15. Further details and examples can be found at www.mathematicsmastery.org/.

<< Figure 1 >>

In the short-term, it is challenging for schools and teachers to move to such a different approach. Therefore, to assist the transition, ARK offers schools the following support during the first ‘moving to mastery’ year:

- **Training and in-school support**. Before the programme begins, school leaders, maths coordinators and class teachers receive either one or two days of training. This is followed by two in-school development visits, three multi-school cluster workshops, and access to an online toolkit. (This includes detailed information on continuous professional development resources, assessments and leadership frameworks).
- **Curriculum-embedded continuous professional development**. Teachers are supported to put the principles into practice through ‘lesson designs.’ These adapt to the needs of each class via the online toolkit, in the hope that teachers can use lesson planning time to also develop as professionals.
- **Collaboration and peer support**. Teachers from different schools are encouraged to collaborate to develop best practice (Mulford, Silins and Leithwood 2004). This is via both face-to-face and online interaction, with a focus upon sharing ideas and supporting one another in applying the approach.

Within our two RCT’s, schools in receipt of the Maths Mastery treatment had access to all such support. A timeline of when activities were provided to schools can be found in Figure 2.

<< Figure 2 >>

It is important to understand that Maths Mastery is designed to take a long term view of transforming maths achievement. The curriculum is cumulative, thus allowing every child sufficient time to access age-appropriate concepts and skills. Starting in Year 1, the main focus is to ensure all pupils have a firm understanding of number. This then allows them to access and succeed in the other areas of mathematics. Schools roll out the approach to subsequent year groups, with a view to transform achievement by the end of Year 6 (i.e. five years after the programme was first introduced in schools). It has not been possible to evaluate the cumulative effect of Maths Mastery over five years using an experimental design[3]. Our evaluation therefore considers the impact of a relatively small (one year) dose of the MM intervention, immediately at the end of the first year.

3. Data and Methods

Primary school

A clustered Randomised Controlled Trial (RCT) was used to evaluate the Maths Mastery (MM) primary school programme. Two school cohorts were purposefully recruited[4]. Cohort A consisted of 40 schools where the trial was conducted during the 2012/13 academic year, with a further 50 schools participating in 2013/14 (Cohort B). Half of these 90 schools were randomly assigned to receive the Maths Mastery programme, with the other 45 schools assigned as controls. (This randomisation and allocation was conducted by the independent evaluation). All Year 1 pupils within treatment schools were taught using the Maths Mastery approach, while the control schools were asked to proceed with 'business as usual.[5]'

A total of 90 primary schools containing 5,108 pupils (2,647 treatment and 2,461 control) were therefore initially recruited into the trial. Seven of these schools (three treatment and four control) dropped out of the study. Moreover, a small number of children in each school did not complete the post-test, due to either absence on the day of the post-test (e.g. through sickness) or having moved to another school. Appendix A examines the issue of attrition in more detail. It illustrates how attrition from the sample is not random, with lower-achieving children more likely to drop out than other groups. Nevertheless, given the overall high response rates (92 percent at the school level and 82 percent at the pupil level) the impact of this attrition is likely to be small. Our final analysis includes the 2,386 pupils in the treatment group and 2,244 in the control group for whom both pre and post-test scores were available.

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[3] Most ethics committees would judge excluding access to a treatment over a sustained period of time to be unethical.

[4] Private schools and those already using the Maths Mastery programme were ineligible to take part in the trial.

[5] The protocol for this study is published online at: http://educationendowmentfoundation.org.uk/library/maths-mastery-primary

All children were tested at the start and end of the academic year using the 'Number Knowledge' test (Okamoto and Case 1996). This is an individually administered oral test that takes about 10 minutes to complete, and was conducted by trained staff from a specialist data collection organisation. This test has been identified as highly predictive of achievement in primary mathematics (Gersten, Jordan and Flojo 2005). Moreover, Cowan (2011) found this test to have high reliability, with little evidence of either floor or ceiling effects. Further information on the Number Knowledge test can be found at http://clarku.edu/numberworlds/nw_TestInfo.htm). The test was selected for use by the evaluation team, independently of ARK who developed and delivered the Maths Mastery programme.

Figure 3 compares the distribution of Number Knowledge test scores for the treatment and control groups. To facilitate interpretation, we have standardised this measure to have a mean of zero and a standard deviation of one. Mean test scores are slightly higher in the treatment group (0.06 standard deviations) compared to the control group (-0.07 standard deviations), though this difference is not statistically significant at conventional thresholds (t=1.41; p=0.16). Moreover, there is no difference in median test scores between the treatment and control groups, both standing at 0.06. Consequently, balance between the treatment and control groups seems reasonable, at least in terms of baseline achievement, suggesting that the randomisation process was satisfactory.

<< Figure 3 >>

The impact of the Maths Mastery primary school intervention will be determined by the following OLS regression model:

$$Y_{ij}^{Post} = \alpha + \beta . Treat_j + \gamma . Y_{ij}^{Pre} + \varepsilon_{ij} \qquad (1)$$

Where:

$Y^{post} =$ Child's post-test score on the Number Knowledge post-test

$Y^{pre} =$ Child's baseline scores on the Number Knowledge pre-test

Treat = A binary variable indicating whether the child was enrolled in a treatment or control school (0 = control; 1 = treatment).

ε = Error term (with children clustered within school)

i = child i

j = school j

Note that by controlling for prior achievement, we improve statistical power and account for the modest difference in prior achievement between treatment and control groups. To allow for the clustering of pupils within schools, the STATA survey (svy) command is used to make Huber-White adjustments to the estimated standard errors. The coefficient of interest from equation (1) is β – is there a positive effect of the MM treatment?

Secondary school

A similar clustered RCT was used to evaluate the Maths Mastery secondary school programme. A total of 50 schools were recruited to participate in the trial during the 2013/14 academic year. (Independent schools and those already using the Maths Mastery programme were again ineligible for the trial). Half (25) of these schools were randomly allocated by the evaluation team to treatment, with the remaining 25 schools allocated to control. All Year 7 pupils within the treatment schools received the Maths Mastery programme during the 2013/14 academic year. The control schools, on the other hand, were asked to proceed with 'business as usual[6].'

All Year 7 children enrolled in one of the 50 participating schools were considered to be part of the MM trial[7]. 4,004 children were enrolled in the 25 treatment schools and 3,708 in the 25 control schools. Six schools (two treatment and four control) dropped out of the study and did not complete the post-test. A small number of pupils in the 44 participating schools also did not complete the post-test due to either absence on the day of the test or because they had moved to another school. Appendix B considers attrition from the secondary school trial in further detail, and illustrates how lower-achieving, disadvantaged boys were more likely to have dropped out of the study than other groups. Nevertheless, we continue to find

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[6] The protocol for this study is published online at http://educationendowmentfoundation.org.uk/projects/maths-mastery-secondary/ The trial has been registered with the independent ISRCTN website at: http://controlled-trials.com/ISRCTN70922140/ .

[7] This information was drawn from administrative records and was defined using the autumn school census data of October 3rd 2013.

good balance on observable characteristics between treatment and control groups, even after this non-random non-response has been taken into account (discussed in further detail below). Moreover, one implication of the high overall school and pupil response rates (88 percent and 77 percent respectively) is that the impact this has upon our estimates will be limited. The final pupil sample size was 5,938; 3,251 pupils in the treatment group (81 percent of the original allocation) and 2,687 in the control group (72 percent of the original allocation).

Children's Key Stage 2 scores are used as the baseline test. All children in England sit Key Stage 2 exams at the end of primary school, when they are age 10 or 11. These tests were thus completed by children three months before the Maths Mastery secondary school RCT began[8]. The GL Assessment 'Progress in Maths' (PiM) 12 test **(**http://www.gl-assessment.co.uk/products/progress-maths) was used as a post-test to examine children's mathematics skills during one week at the end of the academic year (Monday 30th June 2014 – Friday 4th July 2014). This test was selected by the evaluation team and was administered using paper-and-pencil tests by class teachers and took approximately one hour to complete. All scripts were marked by an independent organisation, who were blind to treatment.

Certain features of the Progress in Mathematics test has important implications for our analysis and how one interprets the results. Importantly, around 40 percent of PiM test questions were on material *not* covered as part of the Year 7 MM curriculum[9]. A clear advantage is therefore that this test is not too closely aligned to the MM intervention, and hence there is low risk of the treatment group having been 'taught to the test'. Yet it also offers the interesting possibility of looking at potential substitution effects. Specifically, two sub-scales have been created within the PiM test. One is formed of test questions closely aligned to the MM curriculum (60 percent of all test questions asked), with the other formed of questions that were not (the remaining 40 percent of questions). (These questions were chosen by ARK

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[8] Pupils took these tests after randomisation. However, as these are high stakes tests, it is unlikely that the allocation of their future secondary school would have influenced their performance. (Indeed, the pupils would be very unlikely to know that they would have been part of the Maths Mastery trial at the point they were taking the Key Stage 2 tests).

[9] Moreover, despite calculator work *not* being covered within the Year 7 MM curriculum, the PiM test included both a calculator and a non-calculator section. (Overall test scores are based one-third on the former and two-thirds on the latter). Appendix C investigates pupils' performance on the calculator versus non-calculator sections of the PiM test.

blind to our analysis). It is expected that children in the treatment group will do no better (and possibly worse) on test questions covering material not part of the MM curriculum. In contrast, a positive treatment effect is expected on questions where the MM programme places more time, effort and emphasis[10]. We investigate whether this holds true within our analyses. (Appendix C additionally considers the effect of the treatment on the calculator versus non-calculator section of the PiM test).

Table 1 investigates balance between treatment and control groups in terms of observable baseline characteristics. Figures before accounting for attrition can be found in the panel on the left; those after accounting for attrition can be found in the panel on the right. (We focus upon the 'including attrition' figures in our discussion below). All information has been drawn directly from administrative data. Standardised Key Stage 2 mathematics test scores (the pre-test conducted approximately three months before the intervention began) equals 0.022 for the control group and 0.036 for the treatment group; a small and insignificant difference of just 0.014 standard deviations (t = 0.21; p = 0.83). Similarly, there is a difference of just 0.02 standard deviations in Key Stage 2 reading test scores and Key Stage 1 Average Point Scores. These data also include some limited information on the family background of children - in particular whether the child is eligible for Free School Meals (an indicator of low income - see Hobbs and Vignoles 2013). There are similar proportions of children eligible for Free School Meals (26 percent versus 28 percent) in both groups. There are also similar proportions of boys and girls (52 versus 49 percent) allocated to treatment and control. None of these differences are statistically significant at conventional thresholds. Indeed, the only statistically significant difference is the greater number of Asian children observed in treatment schools (26 percent) than control schools (13 percent). Nevertheless, the overall message is that good balance between treatment and control groups has been achieved.

The impact of the MM secondary school intervention is determined by the following OLS regression model:

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[10] Of course it is also possible that the MM programme impacts positively on fundamental mathematical understanding and hence children may do better on both parts of the test.

$$Y_{ij}^{Post} = \alpha + \beta.Treat_j + \gamma.Y_{ij}^{Pre} + \delta.C_{ij} + \varepsilon_{ij} \qquad (2)$$

Where:

$Y^{post} =$ Child’s post-test score on the Progress in Maths test

Y^{pre} = Child’s baseline scores on the Key Stage 2 and Key Stage 1 tests

Treat = A binary variable indicating whether the child was enrolled in a treatment or control school (0 = control; 1 = treatment).

C = A series of additional control variables potentially associated with the outcome (e.g. gender, Free School Meals eligibility, ethnicity).

ε = Error term (with children clustered within school)

i = child i

j = school j

Baseline test scores and other covariates are included as controls to increase statistical power. Huber-White adjustments are again made to the estimated standard errors to account for the clustering of children within schools.

Meta-analysis

The primary and secondary school trials have both been designed to detect a reasonably sizeable effect, particularly given the relatively small one year dose of the MM intervention. Specifically, assuming that (i) approximately half the variance in post-test scores can be explained by baseline covariates; (ii) equal cluster sizes of 30 (primary) or 200 (secondary) pupils per school and (iii) an inter-cluster correlation of approximately 0.15, then each trial could independently detect an effect of around 0.20 standard deviations. This is bigger than the apparent effect of other similar changes that have previously been made to the school curriculum in England, such as The Literacy Hour. (This altered the English curriculum in primary schools in the late 1990’s to focus more upon basic reading skills. Machin and McNally 2008 reported an effect size of 0.08 standard deviations for a one year exposure to this particular intervention).

Whilst recognising the limitations with meta-analyses, we nevertheless also present a pooled estimate of the MM treatment. (The primary and secondary school trials are assigned equal weight in this meta-analysis, with overall mathematics test scores being the outcome of interest[11]). This has the advantage of boosting statistical power and reducing the impact of sampling variation upon estimates. However, a well-known limitation of such meta-analyses is that they combine information from RCT's that differ in non-trivial ways. In our application, this includes the target population (primary versus secondary school pupils) and the outcome test used (Number Knowledge versus Progress in Maths). Consequently, as Walker, Hernandez and Kattan (2008) argue, such meta-analyses may therefore be seen as a second-best alternative to the 'gold standard' of a single, highly powered trial. In particular, there is more uncertainty with regards the population of interest (for whom the effect size applies to) and the specificity of the outcome the programme influences. We therefore present both the individual results from the two trials as well as the combined meta-analysis.

4. Results

Primary school

Table 2 presents estimates from the OLS regression model examining the impact of the primary school MM treatment. Results are presented for (a) all schools enrolled in the trial and (b) separately for cohorts A and B. The estimated treatment effect is approximately 0.10 standard deviations, with an almost identical figure for each of the two cohorts. This reaches statistical significance at the ten percent level (t = 1.82; p = 0.07), with the 95 percent confidence interval ranging from -0.01 to +0.21. There is thus some evidence that introducing this particular East Asian teaching method into England's primary schools has had a positive effect upon children's mathematics skills. But the reasonably wide confidence interval suggests there is also a degree of uncertainty around this result.

<< Table 2 >>

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[11] For the secondary school trial, the total test score will include children's performance on both the calculator and non-calculator sections of the PiM test.

Figure 4 presents quantile regression estimates of equation (1), thus examining heterogeneity in the treatment effect across the post-test distribution[12]. The dashed horizontal line provides the OLS estimate, while circular markers gives the quantile regression results. Grey shading indicates statistical significance at the ten percent level.

<< Figure 4 >>

Estimates in the bottom part of the post-test distribution are generally a little smaller than those in the top (around 0.08 standard deviations at p25 compared to 0.13 at p75). Nevertheless, Figure 4 does little to suggest that the effect of the MM programme was concentrated in one particular part of the mathematics achievement distribution, though again we note there is some uncertainty around these estimates (due, in particular, to sampling variation). In additional analysis, we also tested for an interaction between the MM treatment and children's baseline mathematics test scores. The magnitude of this interaction was very small (less than 0.01 standard deviations) and statistically insignificant at conventional thresholds (t=0.31; p=0.76).

Secondary school

Estimates for the MM secondary school trial can be found in Table 3. The left-hand most column refers to results when total test scores are the outcome. The middle and right hand columns then divides this into performance on questions that were and were not covered within the MM curriculum.

<< Table 3 >>

Table 3 suggests that the MM secondary school intervention was associated with a small increase in overall mathematics test scores (effect size = 0.06) though this did not reach statistical significance at conventional thresholds. As perhaps expected, the MM intervention did not have any impact upon children's performance on questions that covered topics outside the MM curriculum, with the estimated treatment effect essentially being zero. Thus, despite substituting away from these areas, there is no evidence that the reduction in children's learning time had any

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[12] Standard errors for the quantile regression estimates have been produced by bootstrapping at the cluster (school) level.

detrimental impact upon their ability in these areas. In contrast, the treatment had a more pronounced effect upon material that was focused upon within the MM curriculum (effect size = 0.10), just reaching statistical significance at the five percent level (t = 2.15; p = 0.04). This effect is of a similar magnitude to that found for overall test scores in the primary school trial (0.099 standard deviations).

In additional analysis, we examined whether there was an interaction between the MM treatment in secondary schools and (i) gender, (ii) Key Stage 2 (baseline) test scores and (iii) eligibility for Free School Meals. All interactions were small and did not approach statistical significance at either the five or ten percent level. This held true for both overall test scores and sub-components of the PiM test. Quantile regressions were also estimated, with estimates in the bottom part of the post-test distribution generally a little bigger than those in the top (around 0.13 standard deviations at p25 compared to 0.07 at p75). Nevertheless, overall evidence of heterogeneity in the treatment effect across the achievement distribution was generally rather weak.

Meta-analysis

The results presented thus far have pointed towards a small, positive effect of the MM intervention. However, neither trial was sufficiently powered for the estimated treatment effect to reach statistical significance at the five percent level. We therefore also perform a meta-analysis of the two trials, combining information from each to boost statistical power. Results from this meta-analysis suggest that a one-year dose of Maths Mastery programme leads, on average, to a 0.077 standard deviation increase in children’s mathematics test scores. This pooled estimate is statistically significant at the five percent level (t =2.16; p = 0.03). This result of course rests upon the assumption that it is reasonable to combine estimates across the two trials, as discussed above.

5. Cost-benefit analysis

To calculate the economic costs and benefits of the MM intervention, we broadly follow the approach of Machin and McNally (2008). Information on the cash costs of schools implementing Maths Mastery have been provided by the charity responsible for delivering the programme (ARK).

For two-form primary schools, there is an upfront cost of £6,000 for participating in the programme. (This is an 'at cost' price charged by ARK to cover basic infrastructure). Seven days of staff time are required for training; one day for the headmaster, two days for the head of mathematics, and two days for two mathematics teachers. To calculate the cost of headmasters' time, we take the median point on the headmaster pay scale in England and Wales (£75,222[13]). This is then divided by 230 (the approximate number of working days in a year) to give a headmaster day rate of £327[14]. We then inflate this figure by a fifth to allow for other costs not directly incorporated into headmasters' salaries (e.g. employer contributions to pensions) giving a total cost of £392[15]. Analogous calculations have been made for the head of mathematics (two days training at a final day rate of £251)[16] and the class teachers (a total of four days training at a final day rate of £141)[17]. Total training costs therefore amount to £1,460. The total annual cost to the primary school is £7,460. We then estimate the average number of pupils per primary school as 57; the number of pupils initially enrolled into the primary trial (5,108) divided the number of primary schools initially enrolled (90). The 'per pupil' cost of delivering the primary school intervention was therefore just £131 for the year. So long as the programme does not negatively influence any other outcome, only minimal economic returns will be needed to offset this low per pupil cost.

A similar exercise has been completed for secondary schools. The upfront cost to a school of participating in the programme is £6,000 per annum. Ten days of staff time is required for training; half a day for the headmaster; two and a half days for the head of maths and one day for each maths teacher (there were on average seven maths teachers per schools). Day rates were calculated as above. Thus total training costs are therefore equal to £1,740 per school per annum. This gives a total cost per secondary school of £7,740. There was, on average, 154 pupils per

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[13] This information has been drawn from http://www.education.gov.uk/get-into-teaching/about-teaching/salary/pay-and-benefits

[14] The headmaster pay scale in England and Wales (outside of London) ranges from £107,210 to £43,232. We have assumed headmasters work 46 five day weeks per year (with the other six weeks as holiday).

[15] We appreciate that this is a rather crude way of accounting for such additional costs. However, using a substantially higher or lower figure here does not radically alter our results.

[16] We have assumed the head of maths to be on the 'leading practitioner' pay scale, which ranges from £38,215 to £58,096 (median £48,155).

[17] It is assumed the teacher's will be on the 'main' pay scale, which ranges from £22,023 to £32,187 (median 27,105).

secondary school (7,712 children across the 50 initially recruited schools). Thus the per pupil cost equals £50 per annum.

These figures are first of all used to calculate the Cost Effectiveness Ratio (CER); how much does it cost to raise children's mathematics test scores by 0.01 standard deviations? Table 4 presents three different estimates, using either 'optimistic', 'baseline' or 'conservative' assumptions (these are explained in more detail below). Using the most conservative numbers (small causal effect of the MM programme and high costs per pupil) it costs £24 per 0.01 standard deviation increase in children's maths test scores. The analogous 'lower bound' figure using 'optimistic' assumptions (high causal effect of the programme and low per pupil cost) is just £5. Although this range is quite wide (reflecting the inherent uncertainty in such analyses), this should not distract from the general message that, under all scenarios, the CER is relatively low.

<< Table 4 >>

Next, we proxy the economic benefit of the MM intervention using predicted labour market earnings. Specifically, we estimate the impact of higher age 10 mathematics test scores on net labour market earnings at age 26, 30, 34 and 38 using the British Cohort Study. (This is a nationally representative longitudinal survey of all individuals born in Great Britain during one particular week in 1970). This is done via estimation of the following median regression model:

$$W_T = \alpha + \beta . M_{10} + \gamma . C + \delta . IQ_{10} + \lambda . ED_{34} + \varepsilon \qquad (3)$$

Where:

W = Net labour market earnings (inflated into 2014 prices)

M_{10}= Mathematics test scores at age 10 (standardised to mean 0 and standard deviation 1)

C = A vector of basic control variables (gender, mother/father education and geographic location)

IQ_{10}= A measure of non-verbal IQ at age 10

ED_{34}= Final level of educational attainment achieved by age 34

T = Age t

Three specifications are estimated. The first includes just age 10 mathematics test scores and basic controls (i.e. δ and λ are constrained to 0). The second also includes non-verbal IQ, while specification 3 adds final level of educational attainment at age 34. Note that as the latter is likely to be partly determined by age 10 mathematics test scores, estimates from specification 3 may be treated as a lower bound. In all three models the parameter of interest is β – which indicates how much median wages increase at age t given a one standard deviation increase in age 10 mathematics test scores (conditional upon the other factors included in the model). Estimates can be found in Table 5.

<< Table 5>>

Under specification 1, a one standard deviation increase in age 10 maths scores is associated with a £2,819 increase in age 26 median wages. The analogous figures at age 30 (£1,782) and age 34 (£2,482) are somewhat lower, before returning to a similar level by age 38 (£2,943). As expected, the size of the estimated returns declines at all ages when non-verbal IQ (specification 2) and the potentially endogenous highest qualification at age 34 (specification 3) variables are included in the model. Nevertheless, the same broad pattern of results is observed, with somewhat higher returns observed in the age 26 and age 38 survey waves. Moreover, across all specifications, the labour market rewards to higher age 10 maths skills remains substantial. For instance, median wages increase by around £1,000 per annum for each standard deviation increase in age 10 maths scores, even once respondents' highest qualification at age 34 has been controlled.

The right hand side of Table 5 multiplies different estimates of the MM treatment effect by the β coefficients. This provides an indication of the nominal (non-discounted) benefit of the MM programme at age t. It is clear that nominal annual returns are sizeable. Using specification 2 and the meta-analysis effect size of 0.077, we estimate the MM intervention to raise earnings by approximately £150 to £200 per year. Indeed, even conservative estimates (specification 3 and a MM effect size of 0.055) puts nominal per pupil annual returns at £50 per annum. These figures are non-trivial, given the low per-pupil costs reported above.

To formally investigate the likely cost-effectiveness of the MM programme, we provide estimates of the NPV and IRR under three sets of assumptions:

Optimistic

- Estimated earnings returns to age 10 mathematics scores based upon specification 1 (high causal impact of mathematics on earnings)
- Effect of the MM programme = 0.099 standard deviations
- Discount rate of two percent per annum
- Cost per pupil = £50 (based upon secondary school)

Baseline

- Estimated earnings returns to age 10 mathematics scores based upon specification 2 (medium causal impact of mathematics on earnings)
- Effect of the MM programme = 0.077 standard deviations
- Discount rate of three percent per annum
- Cost per pupil = £91 (simple average of primary and secondary school)

Conservative

- Estimated earnings returns to age 10 mathematics scores based upon specification 3 (low causal impact of mathematics on earnings)
- Effect of the MM programme = 0.055 standard deviations
- Discount rate of five percent per annum
- Cost per pupil = £131 (based upon primary school)

Under all scenarios, we assume that the Maths Mastery intervention takes place at age 10, all individuals work a 45 year career between ages 20 and 65, and there is continuous labour market participation[18]. Results can be found in Figure 5.

<< Figure 5>>

The results in Figure 5 imply that the investment in Maths Mastery yields a positive economic return. This holds true even under conservative assumptions regarding (a) the causal impact of improved mathematics on earnings, (b) the effect of the programme on children's mathematics achievement and (c) the discount rate. In all cases the Net Present Value is positive (ranging from £525 to £6,734), while the Internal Rate of Return on this investment (ranging from 8 percent to 33 percent) always compares relatively favourably to market rates of interest. Moreover,

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[18] We also assume that the estimated earnings impact holds every year between the upper and lower age bounds. For instance, under specification 3, we assume that a one standard deviation increase in maths test scores increases annual earnings by £910 *each year* between the ages 26 and 30. Then, between ages 30 and 34, this annual return decreases slightly to £724. The value at age 38 (£1,392) is assumed to hold through to retirement at age 65.

depending upon the assumptions made, the programme will have repaid its costs between 11 and 15 years after it has been implemented.

There are, of course, significant limitations to cost benefit analyses such as these. Costs are measured with error, with certain resources unlikely to have been fully accounted. We also assume that individuals work continuously for 45 years; this will not always be the case (particularly for women and for lower achievers). Moreover, the assumptions made also require the labour market returns to improved mathematics skills to remain constant over time. Clearly, in general equilibrium, if sufficient numbers of children improve their mathematics achievement, the price employers will be willing to pay for such skills may decline. Finally, we are also assuming that our meta-analysis estimate is plausible, given the similarity of the two trials, and the fact that a similar effect size is found for both (albeit neither individually reaching statistical significance at the 5% level). Despite these limitations we note that, even under quite conservative assumptions, Maths Mastery appears to provide a reasonable return on investment. Maths Mastery thus shows promise as a cost-effective way to raise mathematics achievement in England's schools.

6. Conclusions

East Asian economies dominate the top of important international educational achievement rankings. Two of the most frequently asked questions by education policymakers have therefore become '*what is behind these countries phenomenal educational success*' and '*what can we in the West do to catch up*'? Although there are likely to be a wide range of explanations for these countries' success (Jerrim and Choi 2013; Jerrim 2014), the impact and implementation of 'East Asian teaching methods', often loosely and ill-defined, have particularly caught Western policymakers' attention. Yet despite this interest, there is currently little evidence as to whether the introduction of any particular East Asian teaching method would represent an improvement over the current status quo in many Western countries. This study provides evidence from two RCT's to start to fill this gap in the literature. It provides an estimate of the causal impact of the 'Maths Mastery' programme – a method of teaching mathematics to school children modelled on the approach used in Singapore. By combining evidence from across two Randomised Controlled Trials, we find consistent evidence of small yet positive treatment effects (reaching the 5 percent significance level within our meta-analysis). The subsequent cost-benefit analysis reveals that, even under conservative assumptions, such an approach is likely to offer non-trivial economic returns (largely due to the low costs per pupil).

These findings have potentially important implications for education policy and practice. On the one hand, the small effect size suggests it is unlikely that widespread introduction of this particular East Asian teaching method would springboard Western countries like England to the top of the PISA educational achievement rankings. In other words, it cannot be seen as a 'silver bullet' that will guarantee a country success in mathematics. Yet this does not mean that implementing this teaching method is not a worthwhile investment to make. Even small effect sizes can be economically efficient, with the combination of several such interventions potentially having a large impact overall. Thus, although we advise policymakers that further evidence is still needed, the Maths Mastery programme nevertheless shows signs of promise, and should now be tested over a longer time horizon and a greater number of schools.

This recommendation should, of course, be interpreted in light of the limitations of this study. Four particular issues stand out. First, we have estimated the effect of a small (one year) 'dose' of the Maths Mastery programme, with our evaluation conducted after the first year it has been implemented in schools. More evidence is needed on its impact after teachers have become more familiar with its novel approach, and after children have been exposed to the programme for a prolonged period of time. Second, impact has only been measured straight after the intervention has finished. Longer-term measurement of the *lasting* impact of this teaching method is needed. Third is the issue of external validity; schools were purposefully recruited into the two trials and were not randomly sampled from a well-defined population. Although this limitation is common to many RCT's, further work should consider the extent to which our findings generalise to the population of England's schools. (Appendix D explores the issue of external validity in more detail, and illustrates how our samples contain a disproportionate number of lower achieving children from disadvantaged backgrounds). Finally, we remind the reader that statistical significance was only reached within our meta-analysis, and that individually the primary and secondary school trials were lacking the necessary statistical power. Future work should look at ways to improve the precision of estimates at different stages of the Maths Mastery programme (e.g. primary versus secondary school), including through the use of quasi-experimental methods.

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Table 1. Balance between treatment and control groups: Maths Mastery secondary school trial

	As randomised			Including attrition		
	Control	**Treat**	T - C	**Control**	**Treat**	**T - C**
Eligible for FSM						
No %	72	70	-2	74	72	-2
Yes %	28	30	2	26	28	2
Gender						
Female %	46	49	3	48	51	3
Male %	54	51	-3	52	49	-3
Ethnic Group						
White %	50	47	-3	51	47	-4
Asian %	13	26	13*	13	26	13*
Black %	21	16	-5	21	17	-4
Mixed %	8	7	-1	7	6	-1
Chinese %	0	0	0	0	0	0
Other / unclassified %	7	4	-3	7	4	-3
Standardised KS1 APS	0.007	-0.006	-0.013	0.041	0.065	0.024
Standardised KS2 mathematics score	0.014	-0.013	-0.027	0.022	0.036	0.014
Standardised KS2 reading score	-0.001	0.001	0.002	0.025	0.045	0.020
School n	**25**	**25**		**21**	**23**	
Pupil n	**3,708**	**4,004**		**2,687**	**3,251**	

Notes: Authors' calculations using the National Pupil Database. KS1 APS and KS2 scores have been standardised to have a mean of 0 and standard deviation of 1 (across pupils within the 50 schools as initially randomised). Figures reported for children where data available. * and ** indicate significant difference between treatment and control groups at the 10 and 5 percent level respectively.

Table 2. The impact of the Maths Mastery primary school programme on children's 'Number Knowledge' maths test scores

	Cohort A		Cohort B		Overall	
	Beta	**SE**	**Beta**	**SE**	**Beta**	**SE**
Intervention Group (Ref: Control)						
Treatment	0.091	0.075	0.105	0.078	0.099*	0.054
Pre-test score	0.695**	0.024	0.711**	0.025	0.704**	0.016
Constant	-0.055	0.052	-0.048	0.050	-0.051	0.036
N	**1,868**		**2,308**		**4,176**	

Notes: Authors' calculations. Dependent variable is total Number Knowledge score (standardised to mean 0 and standard deviation 1). Treatment effect presented in the column labelled 'Beta.' SE stands for standard error. * and ** indicate statistical significance at the 10 percent and 5 percent levels. Standard errors clustered at the school level.

Table 3. The impact of the Maths Mastery secondary school programme on children's 'Progress in Maths' test scores

	Total Progress in Maths score		Not covered in MM		Covered in MM	
	Beta	**SE**	**Beta**	**SE**	**Beta**	**SE**
Intervention Group (Ref: Control)						
Treatment	0.055	0.046	-0.003	0.041	0.100**	0.047
Key Stage 1 mathematics (Ref: Level 1)						
Level 2a	0.21**	0.05	0.208**	0.054	0.222**	0.049
Level 2b	0.14**	0.04	0.161**	0.045	0.130**	0.041
Level 2c	0.06*	0.03	0.077**	0.037	0.046	0.034
Level 3	0.32**	0.06	0.305**	0.066	0.351**	0.064
Key Stage 1 Average Points Score	0.09**	0.02	0.092**	0.023	0.080**	0.021
Key Stage 2 Mathematics test score	0.66**	0.02	0.599**	0.019	0.667**	0.018
Key Stage 2 Mathematics test score squared	0.12**	0.01	0.080**	0.009	0.139**	0.010
Key Stage 2 English score	0.11**	0.01	0.107**	0.015	0.106**	0.015
Free School Meals (Ref: No)						
Yes	-0.10**	0.02	-0.105**	0.021	-0.092**	0.021
Ethnic Group (Ref: Other)						
Asian	-0.08	0.06	-0.094*	0.054	-0.070	0.071
Black	-0.19**	0.05	-0.200**	0.043	-0.170**	0.060
Chinese	0	0.21	0.196	0.142	0.167*	0.091
Mixed	-0.03	0.06	0.021	0.060	-0.058	0.063
Unclassified	0.08	0.09	0.078	0.116	0.059	0.091
White	-0.08	0.05	-0.041	0.048	-0.103*	0.061
Gender (Ref: Female)						
Male	-0.11**	0.02	-0.122**	0.023	-0.090**	0.022
English as Additional Language (Ref: No)						
Yes	0.05**	0.03	0.067**	0.029	0.024	0.029
Constant	-0.15**	0.06	-0.092	0.059	-0.180**	0.074
N	**5,919**		**5,888**		**5,884**	

Notes: Authors' calculations. Treatment effect presented in the column labelled 'Beta.' SE stands for standard error. * and ** indicate statistical significance at the 10 percent and 5 percent levels. Standard errors clustered at the school level.

Table 4. Estimated Cost Effective Ratio (CER) of the Maths Mastery programme

Assumption	Effect of MM programme	Per pupil cost (£)	CER: Cost per 0.01 SD improvement
Optimistic	0.099	£50	£5
Baseline	0.077	£91	£12
Conservative	0.055	£131	£24

Notes: Authors' calculations. See section 4 for discussion of how the 'optimistic', 'baseline' and 'conservative' assumptions have been set. Effect of the MM programme given in terms of standard deviations (effect sizes). The final column provides the CER – the cost of increasing pupils' maths test scores by 0.01 standard deviations.

Table 5. The estimated impact of age 10 mathematics test scores on median net labour market earnings

		Earnings impact per SD increase in math scores		Nominal earnings impact per ….		
	Sample size	Beta	SE	0.099 SD increase	0.077 SD increase	0.055 SD increase
Specification 1						
Age 26	2,099	£2,819	£229	£279	£217	£155
Age 30	2,099	£1,782	£210	£176	£137	£98
Age 34	2,099	£2,482	£283	£246	£191	£137
Age 38	2,099	£2,943	£321	£291	£227	£162
Specification 2						
Age 26	2,099	£2,516	£280	£249	£194	£138
Age 30	2,099	£1,499	£254	£148	£115	£82
Age 34	2,099	£2,097	£355	£208	£161	£115
Age 38	2,099	£2,833	£387	£280	£218	£156
Specification 3						
Age 26	2,099	£910	£290	£90	£70	£50
Age 30	2,099	£724	£252	£72	£56	£40
Age 34	2,099	£723	£335	£72	£56	£40
Age 38	2,099	£1,392	£369	£138	£107	£77

Notes: Authors’ calculations using the British Cohort Study 1970 dataset. Figures on left-hand side illustrate impact upon median net labour market earnings for a one standard deviation increase in age 10 mathematics test scores. Figures on the right refer to the earnings increase for the stated standard deviation increase in mathematics test scores. All earnings data inflated into 2014 prices. Specification 1 includes controls for gender, maternal education, paternal education and geographic location. Specification 2 additionally controls for non-verbal IQ, while specification 3 also adds highest qualification obtained by age 34.

Figure 1. An example maths question and the Maths Mastery route to the solution

<u>Question</u>: There are three consecutive numbers that add up in total to 42. What are these numbers?

<u>‘Standard’ approach (trial and improvement)</u>: Children start with what they believe a reasonable estimate of the answer to be (e.g. 7, 8 and 9). They then find these sum up to 24, and so realise the set of numbers must be higher. Three higher numbers are therefore tried (e.g. 15 + 16 + 17), which in this example sum up to 48. Children will then add together another set of numbers, higher in value than the first set, but lower in value than the second set. This iterative process continues until they reach the answer of 13, 14 and 15.

<u>‘Maths Mastery’ approach</u>: The Maths Mastery approach involves ‘bar-modelling’ (shown below). Children would draw out the bars shown below or make them out of play blocks. They would then recognise that the total ‘without the ones’ is 39 (i.e. that 42 – 3 = 13). From this, they would then deduce that the grey portion of each bar is worth 13 (i.e. 39 ÷ 3 = 13). They would then simply ‘add the ones’ back on to the lower two bars to reach the answer of 13, 14 and 15.

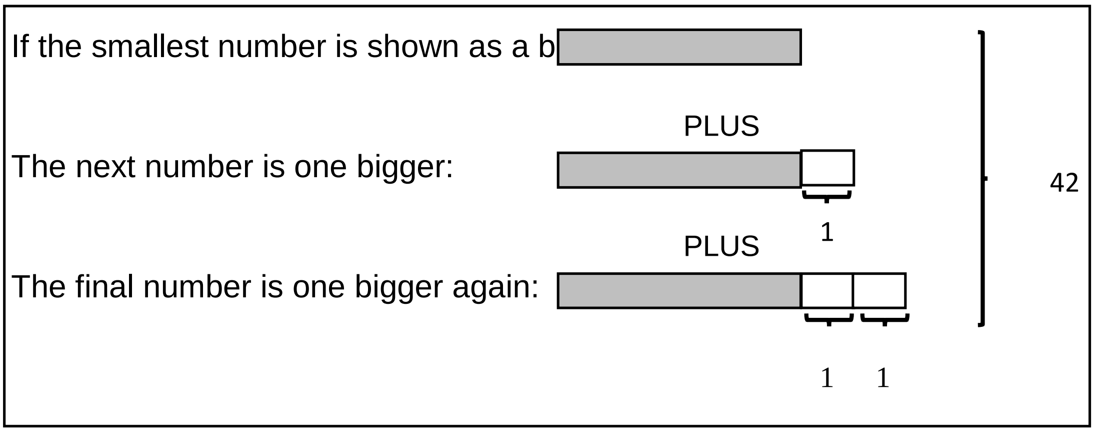


Figure 2. Support given to schools during the ‘moving to mastery’ year

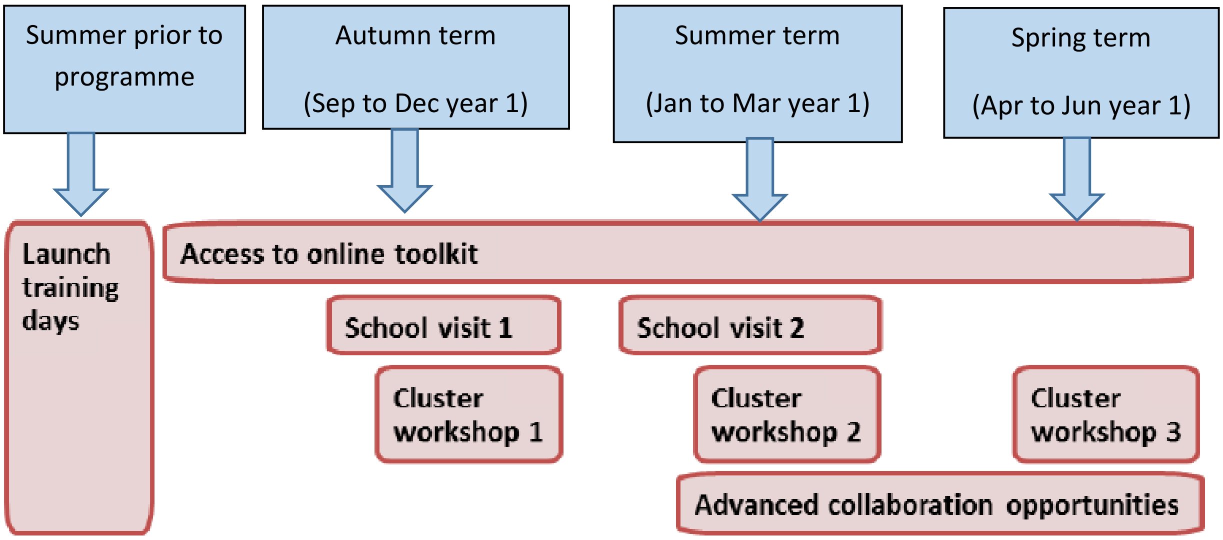
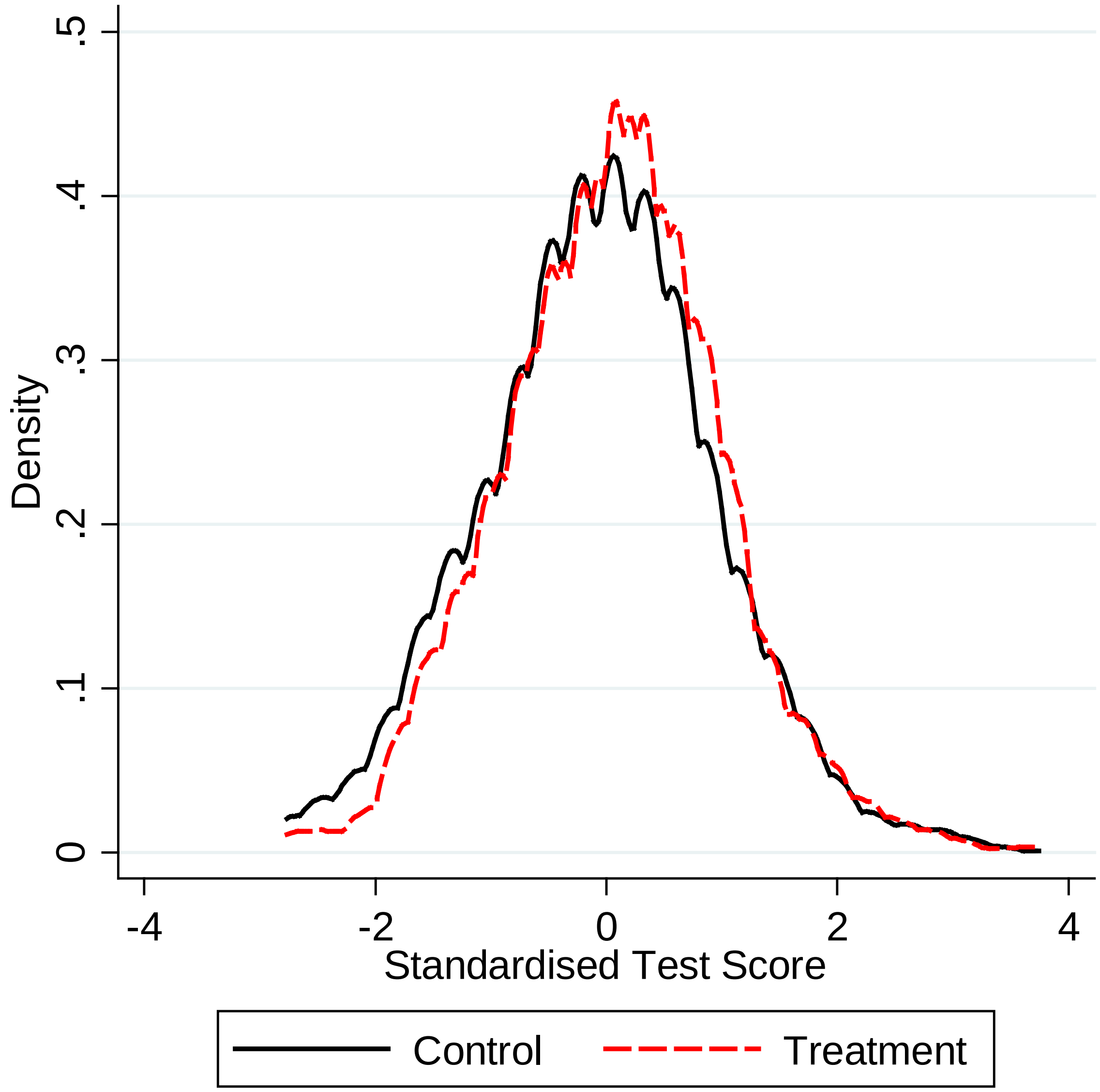
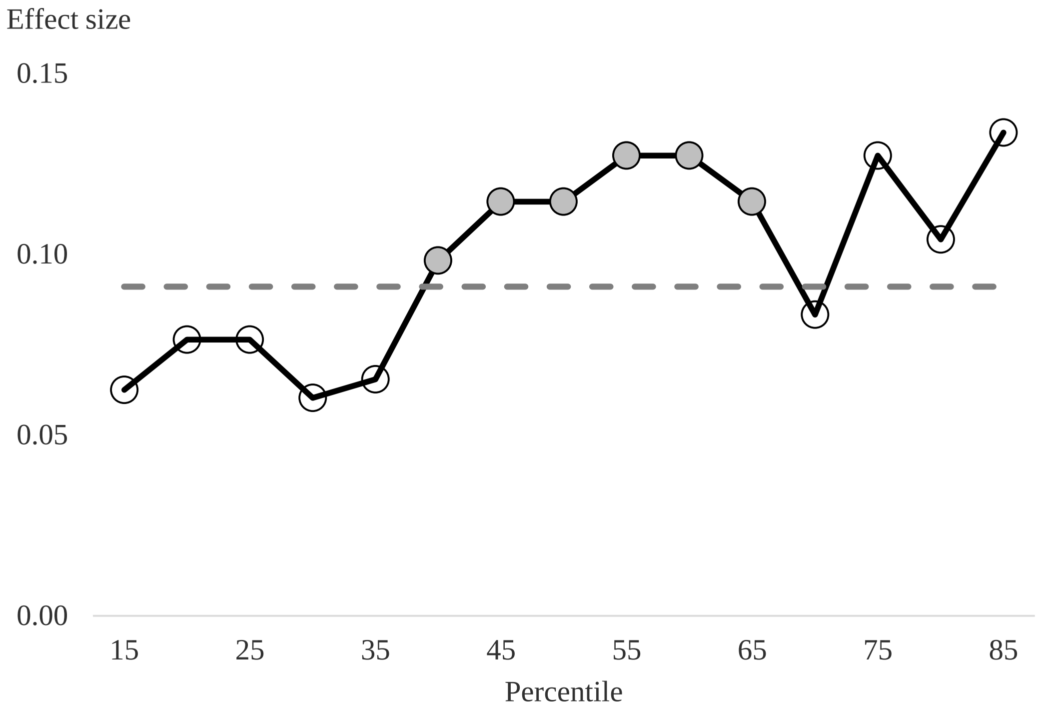


Figure 3. A comparison of (standardised) baseline test scores between the primary school treatment and control groups



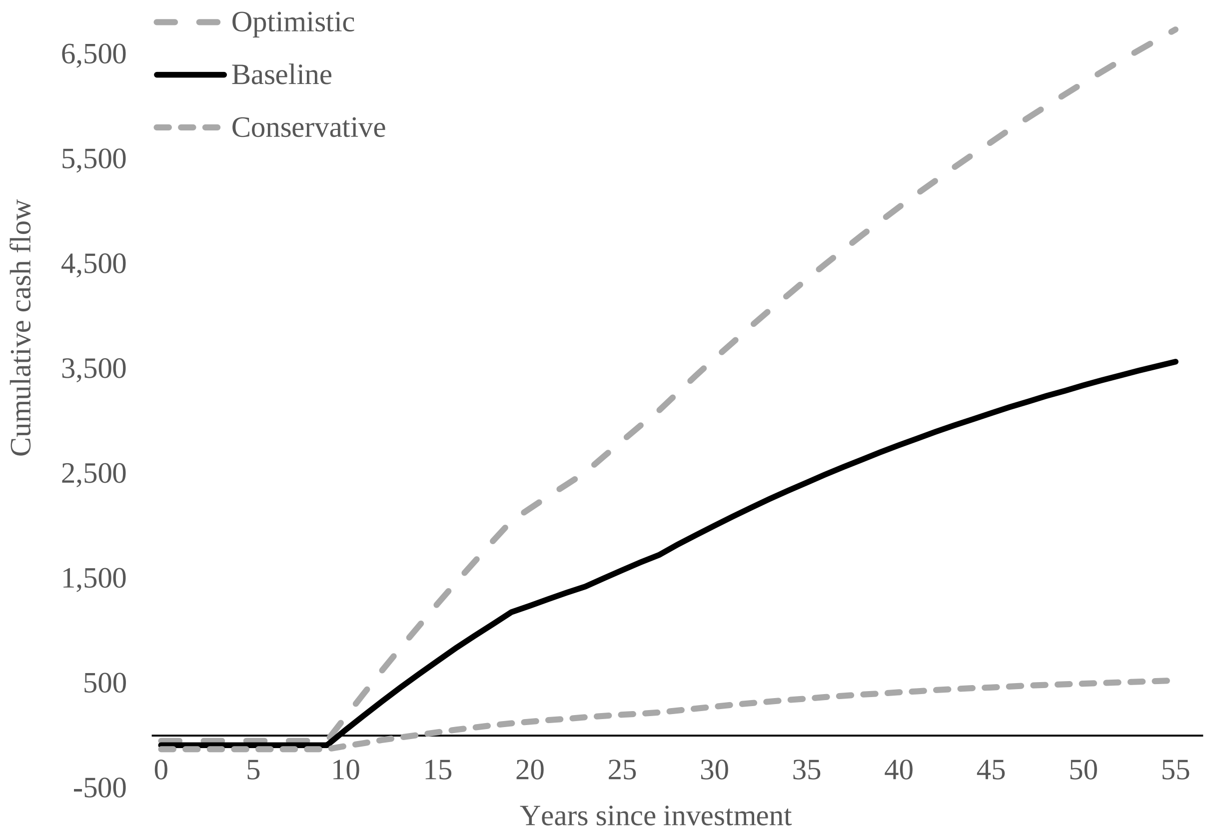
Notes: Authors' calculations. Solid black line presents the baseline test score distribution for the control group. The dashed red line refers to the treatment group. Mean (median) scores equal -0.07 (0.06) for the control group and 0.06 (0.06) for the treatment group.

Figure 4. Quantile regression estimates of the effect of the Maths Mastery primary school intervention across the Number Knowledge test score distribution



Notes: Authors' calculations. Dashed horizontal line illustrates the OLS estimate. Solid grey circular markers indicate whether the treatment effect is significantly greater than 0 as the 10 percent level. Standard errors bootstrapped by cluster using 100 replications. Dependent variable is children's score on the Number Knowledge test.

Figure 5. Estimated cumulative cash-flow from a one-year investment in the Maths Mastery programme



Notes: Authors' calculations. Y-axis refers to the estimated real Net Present Value of the economic costs and benefits of the intervention. X-axis indicates the number of years since the original investment in the Maths Mastery programme. Monetary values inflated to 2014 prices. All figures assume a 45 working career with continuous labour market participation between ages 20 and 65. A summary of the differences between the three sets of estimates can be found below.

	Conservative	**Baseline**	**Optimistic**
Earnings returns specification	Specification 3	Specification 2	Specification 1
Effect of MM programme	0.055	0.077	0.099
Discount rate	5%	3%	2%
Cost per pupil	£131	£91	£50

Appendix A. Attrition from the Maths Mastery primary school trail

Appendix Table A1 presents information on average baseline test scores for children that did not complete the post test (either because their school withdrew from the study or because they were not in that school the day the post-test was conducted). These pupils were of notably lower ability than those children who did complete the post-test. Specifically, children who did not complete the post-test scored around a quarter of a standard deviations below the mean on the baseline test (-0.24 standard deviation for children in the control group and -0.27 for children in the treatment group). In contrast, those children who completed the post-test scored, on average, 0.03 standard deviations above the mean on the pre-test. A similar pattern was found in both Cohort A and Cohort B, though with their being slightly less evidence of selectivity in the former than the latter. Appendix Table A1 therefore suggests that attrition from the sample is not random. Rather, lower-achieving children were more likely to have dropped out of the study than other groups.

Appendix Table A1. A comparison of baseline achievement between children who did and who did not complete the post-test

	Respondent	Non-respondent (Control)	Non-respondent (Treatment)
Cohort A	0.018	-0.159	0.000
Cohort B	0.039	-0.346	-0.403
All pupils	0.029	-0.242	-0.266
Pupil n	**4,176**	**247**	**233**

Notes: This table refers to pupils with valid baseline test data. It does not include children within the five schools that dropped out of the study before baseline testing took place. The 'non-respondent' group refers to children that completed the pre-test, but who did not complete the post-test. The sum of respondents and non-respondents does therefore not equal the total number of children initially enrolled in the trial. All figures reported in terms of effect sizes (standard deviation differences).

Appendix B. Attrition from the Maths Mastery secondary school trail

The National Pupil Database can be used to compare the characteristics of respondents and non-respondents across the treatment and control groups. Results are presented in Appendix Tables B1 and B2. The former illustrates that children who did not complete the post-test tend to have lower levels of prior achievement. This was particularly true for pupils within the treatment group. For instance, non-respondents from the treatment group scored (on average) 0.24 standard deviations below the sample mean on the Key Stage 2 maths test. This compares to 0.04 standard deviations above the mean for respondents in the treatment group. Analogous figures for the control group were -0.01 and 0.02 standard deviations respectively. Similar findings hold for other pre-test scores, including Key Stage 2 reading scores and Key Stage 1 average points scores. Moreover, Appendix Table B2 suggests that boys and children in receipt of FSM were also more likely to have missing post-test data than their female, non-FSM counterparts. Specifically, 37 percent of treatment group non-respondents were eligible for FSM, compared to just 28 percent of respondents. Likewise, 52 percent of control group respondents were male, compared to 58 percent of non-respondents.

Together, Appendix Tables B1 and B2 suggest that attrition from the sample is not random. Rather, lower-achieving, disadvantaged boys were more likely to have dropped out of the study than other groups. It will therefore be important to compare balance of observable characteristics between treatment and control groups both before and after attrition has been taken into account.

Appendix Table B1. A comparison of prior achievement between children who did and who did not complete the post-test

	Treatment		Control	
	Respondent	**Non-respondent**	**Respondent**	**Non-respondent**
Key Stage 1 maths				
Level 1 %	11	18	11	14
Level 2A %	28	20	26	23
Level 2B %	27	27	29	28
Level 2C %	19	26	19	20
Level 3 %	15	9	16	14
Key Stage 1 reading				
Level 1 %	16	26	16	21
Level 2A %	24	17	23	22
Level 2B %	25	25	28	25
Level 2C %	16	18	15	14
Level 3 %	19	14	19	18
Key Stage 1 writing				
Level 1 %	20	29	20	24
Level 2A %	19	14	16	18
Level 2B %	27	22	31	26
Level 2C %	25	30	26	25
Level 3 %	9	5	7	8
KS1 APS (standardised)	0.065	-0.339	0.041	-0.087
KS2 maths score (standardised)	0.036	-0.244	0.022	-0.009
KS2 reading score (standardised)	0.045	-0.207	0.025	-0.072
Pupil n	**3,251**	**753**	**2,687**	**1,021**

Notes: Figures reported for children with complete Key Stage 1 or Key Stage 2 data.

Appendix Table B2. A comparison of demographic characteristics between children who did and who did not complete the post-test

	Treatment		Control	
	Respondent	**Non-respondent**	**Respondent**	**Non-respondent**
Eligible for FSM				
No %	72	63	74	68
Yes %	28	37	26	32
Gender				
Female %	51	39	48	42
Male %	49	61	52	58
Ethnic Group				
White %	47	49	51	47
Asian %	26	23	13	13
Black %	17	16	21	21
Mixed %	6	8	7	11
Chinese %	0.4	0.1	0	1
Other / unclassified %	4	4	7	7
Pupil n	**3,251**	**753**	**2,687**	**1,021**

Notes: Figures reported for children with complete Key Stage 1 or Key Stage 2 data.

Appendix C. The effect of the MM intervention on the calculator versus non-calculator section of the PiM test

In section 3 we described how two sub-scales were developed within the PiM test. The first of these sub-scales contained only questions covering content taught as part of the MM curriculum. The other scale included only questions that covered questions *not* taught as part of the MM curriculum.

This appendix performs a similar analysis, but looking at two alternative sub-scales. Specifically, despite calculator work *not* being part of the Year 7 MM curriculum, the PiM test included a both calculator and a non-calculator section. (Overall test scores are based one-third on the former and two-thirds on the latter). The two elements of the test also offer the interesting possibility of looking for possible substitution effects. Specifically, the MM curriculum substitutes teaching children how to use calculators with learning other, more problem-solving based skills. Children in treatment schools may therefore do no better (and possibly worse) on the calculator part of the test than children in control schools. In contrast, one would expect a positive effect of the treatment on children's scores in the non-calculator section.

Table C1 presents our results. As perhaps expected, the MM intervention did not have any impact upon children's performance on the calculator section of the post-test, with the estimated treatment effect standing at less than 0.01 standard deviations. Thus, despite substituting away from learning calculator skills, there is no evidence that this has had any detrimental impact upon children's ability in this area. In contrast, the MM treatment had a more pronounced effect upon children's non-calculator test scores (effect size = 0.077), though this only approached the boundary of statistical significance at the ten percent level (t = 1.56; p = 0.13). Nevertheless, this effect is of a similar magnitude to that found in the primary school trial (0.099 standard deviations).

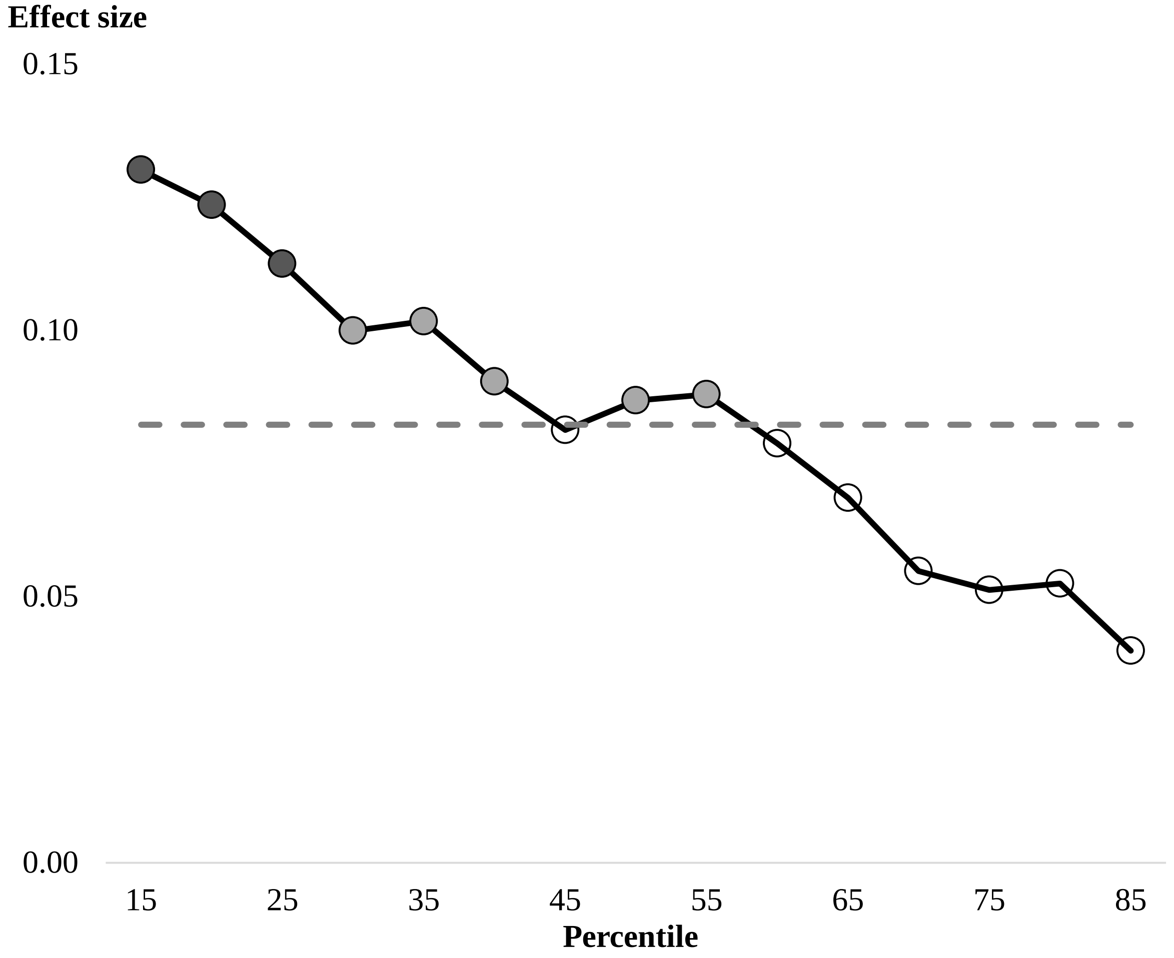
Moreover, quantile regression estimates pointed towards some interesting differences in the treatment effect across the non-calculator test score distribution. Appendix Figure C1 presents results for every 5th percentile between p15 and p85. (A full set of parameter estimates is available upon request). The effect of the MM intervention was approximately 0.10 in the bottom half of the distribution, with almost all estimates significantly greater than zero at either the five percent (p15, p20, p25) or ten percent (p30, p35, p40, p50) level. In contrast, the effect of the intervention is notably smaller in the top half of the distribution (approximately +0.05 standard deviations), with no estimate above p55 reaching statistical significance at even the ten percent level. Together this suggests that, to the extent that the MM secondary school intervention had an impact upon non-calculator test scores, it is doing so by pushing up the lower tail of the achievement distribution. It is again important to recognise that the confidence intervals around these results are reasonably wide, and includes the possibility of a zero effect

Appendix Table C1. The impact of the Maths Mastery secondary school programme on children's 'Progress in Maths' test scores

	Total Progress in Maths score		Calculator only score		Non-calculator only score	
	Beta	**SE**	**Beta**	**SE**	**Beta**	**SE**
Intervention Group (Ref: Control)						
Treatment	0.055	0.046	0.004	0.038	0.077	0.049
Key Stage 1 mathematics (Ref: Level 1)						
Level 2a	0.21**	0.05	0.17**	0.05	0.22**	0.05
Level 2b	0.14**	0.04	0.14**	0.04	0.13**	0.04
Level 2c	0.06*	0.03	0.06	0.03	0.05	0.03
Level 3	0.32**	0.06	0.26**	0.06	0.32**	0.06
Key Stage 1 Average Points Score	0.09**	0.02	0.11**	0.02	0.08**	0.02
Key Stage 2 Mathematics test score	0.66**	0.02	0.65**	0.02	0.64**	0.02
Key Stage 2 Mathematics test score squared	0.12**	0.01	0.12**	0.01	0.11**	0.01
Key Stage 2 English score	0.11**	0.01	0.08**	0.01	0.12**	0.02
Free School Meals (Ref: No)						
Yes	-0.10**	0.02	-0.08**	0.02	-0.10**	0.02
Ethnic Group (Ref: Other)						
Asian	-0.08	0.06	-0.08	0.06	-0.08	0.06
Black	-0.19**	0.05	-0.13**	0.04	-0.21**	0.05
Chinese	0	0.21	0.17	0.15	0.20**	0.09
Mixed	-0.03	0.06	-0.03	0.06	-0.02	0.06
Unclassified	0.08	0.09	0.09	0.12	0.07	0.08
White	-0.08	0.05	-0.06	0.05	-0.09*	0.05
Gender (Ref: Female)						
Male	-0.11**	0.02	-0.05**	0.02	-0.12**	0.02
English as Additional Language (Ref: No)						
Yes	0.05**	0.03	0.09**	0.02	0.01	0.03
Constant	-0.15**	0.06	-0.17**	0.06	-0.11*	0.06
N	**5,919**		**5,887**		**5,871**	

Notes: Authors' calculations. Treatment effect presented in the column labelled 'Beta.' SE stands for standard error. * and ** indicate statistical significance at the 10 percent and 5 percent levels. Standard errors clustered at the school level.

Appendix Figure C1. Quantile regression estimates of the effect of the Maths Mastery secondary school intervention across the non-calculator test score distribution



Notes: Authors' calculations. Dashed horizontal line illustrates the OLS estimate. Solid black (grey) circular markers indicate whether the treatment effect is significantly greater than 0 as the 5 percent (10 percent) level. Standard errors bootstrapped by cluster using 50 replications. Dependent variable is children's score on the non-calculator section of the PiM test. A full set of parameter estimates can be found in Appendix C.

Appendix D. The external validity of the Maths Mastery primary school and secondary school RCT's

Schools were not randomly selected into either the primary or secondary school trial. Rather Ark, who were running the intervention, were allowed to purposefully recruit schools. In this appendix we compare the characteristics of children participating in the trial to the state school population for England, using administrative records. Results are presented in Appendix Table D1.

Panel A provides evidence for the 90 schools initially randomised in the primary school trial[19]. The left hand side refers to cohort A and the right hand side for cohort B. The gender and month of birth distributions for children enrolled in the trial is very close to that for the population as a whole. There are, however, a greater proportion of children eligible for Free School Meals (a marker of low income) enrolled in the trial than found in the national population. The final six rows refer to children's scores on the Foundation Stage Profile – six teacher-based assessments of children's development at approximately age 5. (We have standardised each of these scales to mean 0 and standard deviation 1 across the population). Interestingly, whereas cohort A children scored below the national mean on each of these scales (typically by around 0.10 standard deviations), cohort B children tend to score above the mean (again by around 0.10 standard deviations). This suggests that cohort A included children with below average levels of early cognitive development, while cohort B included children with above average levels at age 5. The fact that a very similar effect size was nevertheless found for both cohorts (recall Table 2) perhaps suggests that the positive effect may generalise across different study populations.

The lower half of Panel B considers whether pupils within the 50 secondary schools initially randomised have similar baseline test scores to pupils in the rest of England. Trial participants were, on average, lower performing in Key Stage 1 (age

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[19] It has not been possible to link administrative records to individual pupils within the primary school trial. The figures presented are therefore based upon administrative records held by the school, based upon the autumn census enrolment data. Figures on pupil enrolment therefore differ slightly to those provided for the primary school trial sample provided in the main text.

7) and Key Stage 2 (age 11) examinations than the state school population as a whole[20]. For instance, their KS1 average points scores (and KS2 maths test scores) were approximately 0.2 standard deviations (0.1 standard deviations) below the population mean. This seems to be driven, at least in part, by the fact the trial particularly under-represented high achievers (relative to the population). For instance, just 12 per cent of children participating in the trial were award Level 3 in their Key Stage 1 maths test, compared to 19 per cent of all state school pupils in England.

The top half of panel B presents a similar comparison for secondary schools in terms of other observable characteristics. 29 per cent of children enrolled in the trial were eligible for Free School Meals, compared to 18 per cent of pupils in the population. This suggests that trial participants were much more likely to come from a low-income background. Similarly, ethnic minorities were over-represented in the trial – particularly Black (19 per cent in the sample versus 5 per cent in the population) and Asian (20 per cent in the sample versus 10 per cent in the population) groups.

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[20] Key Stage 1 and Key Stage 2 exams are national tests all state school children in England sit at age 7 and age 11.

Appendix Table D1. A comparison of demographic characteristics and prior achievement of Maths Mastery participants to the England state school population

(a) Primary School Trial

	Cohort A		Cohort B	
	Trial schools	England population	Trial schools	England population
FSM				
No %	69	80	74	82
Yes %	31	20	26	18
Gender				
Female %	50	49	49	49
Male %	50	51	51	51
Month of Birth				
January %	8	8	8	8
February %	8	8	8	8
March %	8	8	8	8
April %	8	8	7	8
May %	8	9	8	9
June %	8	8	9	8
July %	8	9	9	9
August %	9	9	8	9
September %	9	9	8	9
October %	8	9	9	8
November %	10	8	8	8
December %	8	8	9	8
Foundation stage profile scales				
Personal, social, emotional development	-0.11	0	0.13	0
Communication, language and literacy	-0.04	0	0.07	0
Problem solving, reasoning and numeracy	-0.13	0	0.08	0
Knowledge of world	-0.17	0	0.10	0
Physical development	-0.13	0	0.11	0
Creative development	-0.07	0	0.15	0
Pupil n	**2,162***	**616,014**	**2,880***	**641,871**

Notes: All foundation stage profile scales have been standardised to have a mean of 0 and a standard deviation of 1. * By pupil number indicates that sample size is slightly different to figures reported in the main text. This is due to these figures being based upon administrative records, while trial data was collected directly from schools, and based upon children who were present on the day of the pre-test.

(b) Secondary School Trial

	Trial participants	**England**
Eligible for FSM		
No %	71	82
Yes %	29	18
Gender		
Female %	48	49
Male %	52	51
Ethnic Group		
White %	49	78
Asian %	20	10
Black %	19	5
Mixed %	7	5
Chinese %	0	0
Other / unclassified %	5	2
Mean (SD) KS1 total points score	14.6 (3.5)	15.3 (3.6)
Mean (SD) KS2 mathematics score	68.4 (20.9)	70 (21)
Mean (SD) KS2 reading score	31 (10.2)	33 (10)
Pupil n	**7,712**	**531,145**

Notes: Authors' calculations using the National Pupil Database. Trial participants refers to pupils within the 50 schools initially recruited into the Maths Mastery secondary school trial. 'England' refers to the state school population of England as a whole. All percentages refer to column percentages. Key Stage 1 and Key Stage 2 test scores kept in original metric (i.e. they have not been standardised).