Does teaching children how to play cognitively demanding games improve their educational attainment? Evidence from a Randomised Controlled Trial of chess instruction in England.

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A number of studies suggest that teaching children how to play chess may have an impact upon their educational attainment. Yet the strength of this evidence is undermined by limitations with research design. This paper attempts to overcome these limitations by presenting evidence from a randomised controlled trial (RCT) involving more than 4,000 children in England. In contrast to much of the existing literature, we find no evidence of an effect of chess instruction upon children's mathematics, reading or science test scores. Our results provide a timely reminder of the need for social scientists to employ robust research designs.

Key Words: Chess, RCT, educational attainment, England.

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1. Introduction

Within the United Kingdom and the United States, there is growing interest in whether playing "cognitively demanding" games has a positive impact upon young people's cognitive development and educational attainment. For instance, recent academic work has suggested that cognitively demanding digital games and board games can improve young people's cognitive ability, visual perception, attention, working memory, executive control, reasoning and spatial skills, along with overall brain health (Fissler, Kolassa and Schrader 2015). This has been accompanied by research suggesting that video games such as Portal 2 or Super Mario 64, and board games such as chess, lead to improved performance in problem solving and spatial ability tasks, and can even change the function and structure of grey matter within certain parts of the brain (Kühn et al 2013; Kühn et al 2014; Fissler, Kolassa and Schrader 2015). It is thought that this will translate into improved academic outcomes at school, with such possibilities particularly attracting the attention of the media. For instance, a recent article from the Huffington Post led with a headline "*7 ways video games will help your kids in school*"[1].

Despite being more than 1,500 years old, chess is a prototypic example of a cognitively demanding game. It requires concentration, strategy and logical thinking, and for a long time has been associated with individuals who have higher levels of intelligence and academic achievement (Frydman and Lynn 1992). But is it that learning how to play chess (and other cognitively demanding games) has boosted these individuals' cognitive skills? Or is it rather that individuals who learn to play cognitively demanding games such as chess have other favourable characteristics that mean that they also have higher levels of educational attainment? If it is the former, then encouraging young people to play cognitively demanding games like chess may represent a simple yet effective way for educators to boost young people's cognitive

[1] See http://www.huffingtonpost.com/kara-loo/7-ways-video-games-help_b_6084990.html

achievement. Yet despite a number of studies hinting at a causal link between learning how to play cognitively demanding games and educational attainment, few have provided a robust investigation of this issue. The aim of this paper is to provide some high-quality evidence on this matter. Using chess as an example, we show how the large effect sizes of chess tuition on attainment reported in the existing literature are not replicated when a robust research design is used to measure whether there is a *lasting* effect of learning how to play this game, when the intervention has been delivered at scale and being played by children in the real-world.

Our decision to focus upon chess is driven by the fact that a number of previous small-scale studies have claimed to show a positive association between teaching children how to play this game and their later achievement on academic tests. This evidence is reviewed in Table 1, which provides an overview of 24 studies recently included in a meta-analysis investigating the relationship between chess instruction and children's academic outcomes (Sala and Gobet 2016). Several studies have reported a strong association between teaching children how to play chess and their mathematics test scores. The effect sizes reported are mostly positive, with the final results reporting an average effect size of +0.34 standard deviations. Various authors of these studies have argued how their findings demonstrate chess to be a '*valuable educational tool*' (Aciego et al 2012: 558), that '*chess training can be a valuable learning aid that supports acquisition of mathematical abilities*' (Trinchero 2013:2) and that chess is '*an effective tool for developing higher order thinking skills*' (Kazemi et al 2012: 372).

These findings may help to explain why an increasing number of educators have shown an interest in introducing chess instruction into elementary schools. A number of schools in the United States offer chess lessons both within and outside regular school hours. Data from the 2012 Programme for International Student Assessment (PISA) suggests that almost half a million American 15-year-olds play chess regularly (authors' calculations). In Armenia, chess

is part of the curriculum for 2nd, 3rd and 4th grade pupils, while Hungary recently followed suit.[2] Venezuela introduced chess lessons into schools as far back as 1989, based upon a study that suggested chess could increase students' IQ scores (Ferguson 1995). Ferguson (1995) cites work by Linder (1990), who notes that chess is now part taught in thousands of schools in nearly 30 countries around the world. In this paper, we present results from a £700,000 study funded by the Department for Education in England (via the Education Endowment Foundation) to evaluate the impact of chess instruction upon educational attainment – particularly amongst disadvantaged pupils – within English elementary schools.

Despite the impressive effect sizes reported in Table 1, most existing studies linking chess instruction to educational attainment have significant limitations in terms of research design. First, most research on chess instruction and educational attainment provides evidence of an association only, and not whether there is a causal effect. Second, most of the studies previously conducted have either been very small scale (the median sample size of the studies reported in Table 1 is just 54 children). Third, even within the RCTs that have been conducted, there is some evidence that randomisation may have been compromised (e.g. Boruch and Romano 2011 reported a non-trivial, statistically significant difference between treatment and control groups in terms of baseline test-scores). Fourth, even when a randomised design has been used at a reasonable scale, there have been other significant threats to validity, particularly surrounding how pupils' outcomes have been measured at the end of the intervention. For instance, it was actually the chess tutors who administered the tests in the study by Boruch and Romano (2011), who note how this is a clear threat to validity. Likewise, the outcome test in Trinchero and Sala (2016) was based upon just seven questions from the PISA test, and were clearly not age appropriate (PISA is a test for 15-year-old children, yet their sample consisted of elementary

[2] http://www.illawarramercury.com.au/story/3052639/chess-linked-to-academic-achievement/?cs=25

school children under the age of 10). Fifth, all of the existing literature focuses upon the impact of chess on education attainment directly after the intervention has finished. Yet what is of greater relevance to policy and practice is whether teaching children how to play chess has a *lasting* impact upon their achievement (i.e. it could be that any immediate impact that is observed simply fades out)[3]. These threats to validity are not specific to chess; our reading of the literature is that most studies into other types of cognitively-demanding games have similar limitations as well.

In this paper, we report the results of a large-scale RCT conducted in England that attempts to overcome the problems detailed above. As the study uses a randomised design, we are able to produce a credible estimate of the impact of chess instruction upon children's educational attainment. With over 4,000 participants from 100 schools, the trial is both adequately powered and captures the impact of the programme when implemented across a number of locations within England. In other words, unlike some psychological experiments, the intervention has not taken place in a controlled environment, but captures what happens when chess is taught to children in regular classrooms in the 'real world'. We use high-stakes, age appropriate and externally marked academic tests for schools to measure the effectiveness of the intervention, meaning our results are unlikely to be influenced by limitations surrounding the outcome test. Our study focuses upon the impact of chess instruction upon educational attainment one academic year after the intervention has finished. That is, we concentrate upon whether there is a lasting effect. This overcomes problems with measuring the impact of cognitively demanding games generally (and chess interventions specifically) straight after the programme has finished.

[3] Moreover, by testing immediately after the intervention has finished, this could increase the likelihood of results being driven by Hawthorne effects.

Additional benefits from our design are that all our data have been collected centrally via children's administrative records, with almost no attrition. Also, unlike most existing studies, we also comment upon the likely external validity of our results, and the extent to which they can be generalised to other settings. In doing so, we believe we provide the most compelling evidence to date as to whether providing chess instruction to primary school children really does lead to a significant improvement in their educational achievement.

The paper now proceeds as follows. Section 2 provides further details about the intervention. A description of the data follows in section 3, with the RCT design described in section 4. Results are presented in section 5, with a discussion of potential reasons for differences with the existing literature in section 6. Conclusions follow in section 7.

2. The Chess in Schools and Communities intervention

The intervention was delivered independently of this impact evaluation by the charity Chess in Schools and Communities (CSC) (www.chessinschools.co.uk/). Although CSC usually teaches primary school children of all ages how to play chess, this particular study focused upon pupils in Year 5 (age 9/10).

The CSC programme introduces chess lessons into primary schools as part of the standard school day, with all children within each class receiving the intervention. This is delivered by fully trained tutors, and follows a standardised 30-hour curriculum[4], consistent with the "dose" given in the studies reporting a positive effect of chess reviewed in Table 1. Schools were required to teach chess in place of one regularly scheduled lesson per week, with this normally intended to be art, humanities or physical education[5]. In addition, each participating school was

[4] Further details can be found at http://www.chessinschools.co.uk/sample_curriculum.htm.

[5] Class survey data received from teachers in 30 schools (68% of the treatment group) showed that the chess lesson most commonly replaced a humanities lesson; others replaced included music or PE. However seven schools replaced a maths lesson - six wholly, and one partially - and one school said they replaced an English lesson for the whole of the intervention year.

asked to designate a teacher (or teaching assistant) that would assist the CSC tutor to run the programme in class. This person was asked to attend a training seminar run by CSC and had full access to the programme curriculum. Each school was also sent chess sets for classroom use, workbooks and curriculum books while each child in the intervention school-year received a chess set and chess book to take home.

Whole class teaching was used to deliver the CSC programme. During lessons, material was presented using either a chess demonstration board or via the white board. In order to use the white board, each tutor was given specialist chess software, with the curriculum converted into a proprietary file format. Tutors had learning plans and objectives for each lesson, as well as worksheets for pupils. In each lesson, children shared a chess set on the desk to practice moves, or later, to play complete games. Tutors were encouraged to talk for no more than 15 minutes before allowing children to practice what they had been taught. In each school a chess club was also set up at lunchtime or after school.

The game was taught piece by piece, with visualisation of moves required from lesson 2. By lesson 10, more abstract concepts such as check and checkmate were introduced. By the end of the first term, children were expected to be able to begin to play a game of chess. Then, by the end of the second term, most children were expected to be able to play a game to a reasonable standard. At the end of the school year, CSC organised competitions locally for groups of schools or within individual schools.

A 'business as usual' approach was used in control schools. These schools were not allowed to access the intervention until after the trial had finished and the outcome tests had taken place.

There are a number of reasons to think that teaching children to play chess will have a positive impact upon their educational attainment. First, chess might lead to increased logical thinking and problem solving ability, translating into improvements in mathematics attainment

(Ferguson 1995, Thompson 2003). In addition, being taught how to play chess may help children to understand and explain complex ideas, promoting their academic achievement in a range of areas (Ferguson 1995, Dauvergne 2000, Margulies 1991). Chess may also have a positive effect on children's non-cognitive skills by improving their levels of concentration, motivation, perseverance and self-control (Margulies 1991, Dauvergne 2000, Gobet and Campitelli, 2006).

Several important implications stem from this. Despite much of the existing literature focusing upon children's achievement in mathematics, it is clear that there could be wider impacts across several academic domains. Therefore, while mathematics achievement is the primary outcome for this evaluation, we also consider the impact of chess instruction upon children's reading and science scores. Moreover, quantile regression is also used to investigate the impact of chess instruction across the distribution of attainment. We also explore whether the intervention may be particularly effective for certain sub-groups, such as by gender and for children from low income backgrounds, who tend to have lower-levels of self-confidence and more behavioural problems than other groups (Blanden et al., 2007). These sub-groups were specified in advance in our pre-trial analysis plan.

3. Data

Overview

Our data are from a clustered randomised controlled trial (RCT) of the CSC programme in England. The trial was pre-registered at the independent ISRCTN website with a fully pre-specified analysis plan.[6] It was conducted during the 2013/14 academic year, and involved a total of 4,009 pupils from 100 primary schools (50 treatment and 50 control). This sample size

[6] See http://controlled-trials.com/ISRCTN33648117 and http://educationendowmentfoundation.org.uk/library/chess-in-schools-protocol/ respectively

was chosen in order for us to be able to detect an effect of least 0.20 standard deviations, consistent with the impact other studies of chess have found (see Table 1). Full details of the power calculations are provided online (see Appendix A). In England, pupils attend primary school from age 5 to 11, spending the first three years working towards Key Stage 1 assessments, taken at the end of Year 2 (age 6/7). Then, for the next four years (from age 7 to 11) children work towards Key Stage 2 assessments taken at the end of Year 6 (at age 10/11). Key Stage 2 assessments are external high-stakes tests, used by schools and regulators to track individual-level and school-level performance. This is the main outcome in this work, while Key Stage 1 tests are used as baseline controls to improve statistical power. Data for both tests are available from administrative records, the National Pupil Database (NPD), for all pupils in state schools. We standardise the test scores for this national population to mean 0 and standard deviation 1. Our results are therefore presented as z-scores.

All Year 5 (age 9/10) pupils within treatment schools were taught using the CSC approach. Control schools were asked to proceed with 'business as usual', meaning they would not introduce chess lessons into their school curriculum during the trial period, and would otherwise operate as they had in previous years.

Recruitment

A total of 11 Local Education Authorities (LEA) in England were purposefully selected by CSC where they had capacity to deliver the intervention. These were Hackney, Hammersmith and Fulham, Newham, and Southwark in Inner London together with Bristol, Leeds, Liverpool, Middlesbrough, Sefton (Merseyside), Sheffield and Tameside (Manchester).

To enable us to produce a well-defined population for the charity to sample from, we began by considering all primary schools within these 11 LEAs. We excluded private schools and those state schools already receiving the CSC programme. As the trial funders were particularly

interested in the potential impact of chess instruction upon children from low income backgrounds, we further restricted the population of interest to schools with a high proportion of pupils who were eligible for Free School Meals (FSM). This is a benefit for low-income families and is a measure of socio-economic disadvantage often used in the UK. Specifically, at least 37 percent of children in the school had to have been eligible to receive FSM in the last six years.[7] The population of interest was therefore defined as Year 5 state school pupils within the selected LEAs in England, who attended a school with a high proportion of disadvantaged pupils, and whose school were not currently enrolled in the CSC programme.

After setting these criteria, the population of interest included a total of 442 schools. CSC were then asked to recruit 100 of these schools. CSC sent all 442 schools an information pack. Those that agreed to take part in the trial completed a consent form to participate in the study and to allow access to NPD data prior to randomisation. 100 schools were recruited into the trial (akin to a response rate of 23 percent).

How does the sample of pupils from the 100 participating schools compare to children in the population of 442 eligible schools in terms of observable characteristics? And how does this compare to the state school population of England as a whole? Tables 2a and 2b provides some insight into these issues and thus the likely external validity of the trial.

<< Table 2a >>

The percentage of children reaching each Key Stage 1 performance level is very similar across the 'trial participants' and 'eligible' samples. For instance, in mathematics 12% of pupils achieved at level 1, approximately 20% at level 2C, 30% at level 2b, 24% at level 2A and 12% at level 3. This holds true across both the 'participants' and 'all eligible pupil' groups. Similar

[7] The cut-off of 37 percent was chosen in order for the population of interest to include 450 schools, from whom the CSC charity could recruit from.

findings hold for Key Stage 1 reading, writing and science test scores. Indeed, standardised Key Stage 1 average point scores differ by less than 0.01 standard deviations between the trial participants and all pupils who were eligible for the intervention. In terms of other demographic characteristics, there are slightly fewer children with English as an Additional Language (EAL) amongst trial participants (34%) than in the eligible population (37%). London is over-represented compared to the LEAs from outside the capital – in total 51% of trial participants come from the four London LEAs compared to 39% of all eligible pupils. However, with this exception, differences between all children who were eligible to receive the intervention and participating pupils are small in terms of magnitude. Overall, Table 2a suggests that the sample of schools/children recruited to participate in the trial was broadly representative of the population that the study was designed to represent. The external validity of the trial, judged in this way, seems to be high.

<< Table 2b >>

Given the sample design, there are more low achievers and more children from low income backgrounds enrolled in the trial sample and in the trial's target population than in England as a whole (Table 2b). Hence we cannot say that the schools recruited into the study are typical of all schools/pupils in England. Rather, they are somewhat lower achieving and more socio-economically disadvantaged.

Attrition and crossover / non-compliance

Figure 1 traces schools and their pupils from recruitment into the trial through to the final analysis. A total of 100 schools with 4,009 pupils were initially recruited to the take part. These schools were separated into ten strata defined by historical achievement in national examinations and the proportion of pupils eligible for free school meals. Half of the schools within each stratum were then randomly allocated to receive the CSC programme, while the

other half were randomly allocated to the control group. This resulted in 50 schools (containing 2,055 pupils) receiving the CSC treatment and 50 schools (containing 1,954 pupils) acting as the 'business as usual' controls. Post-randomisation, six out of the 50 treatment schools (containing 201 pupils) dropped out of the CSC programme before the intervention had begun. Moreover, one school was unwilling to accept their random allocation to the control group, and delivered chess lessons to its 54 pupils. Hence there was a small amount of non-compliance, though at a level that is unlikely to significantly affect the key conclusions drawn from the trial. (See the results section for further details).

<< Figure 1 >>

All schools and pupils initially enrolled into the study have been tracked via the NPD. Consequently, missing post-test data due to attrition from the study is extremely low. Specifically, for the 4,009 children initially enrolled, Key Stage 2 (post-test) scores are available for 3,865 (97 percent) pupils. Hence, even the small number of schools/pupils who did not comply with their initial random allocation were included in the final analysis on an Intention-To-Treat (ITT) or Local Average Treatment Effect (LATE) basis (see below for further details).

Implementation and fidelity

In addition to the quantitative impact evaluation, a complementary process evaluation was also conducted. Full details can be found in Jerrim et al (2016).

On the whole, the CSC programme was successfully implemented and well-received within the intervention schools. Teachers were positive about many aspects of the programme, while children reported high levels of enjoyment with respect to the chess lessons. For instance, 92 percent of pupils said they liked the chess lessons 'a little' or 'a lot', with only 8 percent reporting that they did not like them. This is further supported by the fact that many children

were continuing to play chess seven months after the intervention had finished. In particular, around 40 percent of pupils in intervention schools reported playing up to three games of chess per month, and 28 percent playing at least once a week.

In support of theories as to why chess may improve attainment, such as the theory of change (see Jerrim et al, 2016), most teachers thought that the chess lessons had boosted children's self-confidence, levels of concentration and their ability to think critically. A good proportion of teachers also believed that this would translate into a tangible impact upon children's educational achievement. With respect to mathematics, around half of all teachers surveyed thought that the programme would have some positive benefit for children's achievement, while around a quarter of teachers thought the impact would be large.

There were a few departures from the intended delivery of the intervention within some schools. First, due to a slight delay to the start of the intervention, only one-third of schools received the full 30 hours of chess lessons, with the vast majority receiving between 25 and 29 hours instead (which is still around the median number of hours received within trials included in the Sala and Gobet 2016 meta-analysis). Second, although most schools removed an art or humanities lesson to make room for the chess lessons as intended, seven intervention schools substituted chess for one of their weekly mathematics lessons. In the sub-sections that follow, we have tested the robustness of all our estimates to excluding these schools from the analysis, and find that this leads to little change in our substantive results. Finally, although all regular class teachers were expected to attend a one-day training session about the CSC programme, only around one-in-three took up this opportunity. Consequently, some class teachers may have been less prepared at the start of the intervention than they could have been[8].

[8] In our analysis, we investigated whether the intervention was more effective in schools where the class teacher attended the training session. There was no evidence that this was the case.

In summary, the overall implementation of the CSC programme was generally quite good, though with some discrepancies in terms of total contact time, the lesson substituted, and the training class teachers received. Schools, teachers and pupils were nevertheless typically engaged and enthusiastic about the programme, with many reporting being able to see the positive benefits of it.

Outcome measures

The tests used as outcome measures were selected by us after discussion with the trial funders (the Education Endowment Foundation) and CSC. It was decided that the primary outcome of the trial would be the scores children achieve on their Key Stage 2 mathematics exam. This has a number of advantages over measures that have previously been used to evaluate the impact of chess upon children's achievement. First, it is a 'high-stakes' examination for schools which is externally marked and moderated by individuals who have no vested interest in the results of the trial. This is in stark contrast to existing trials that almost exclusively rely on low-stakes tests, in some cases delivered by the Chess tutors (e.g. Romano, 2011). Second, we are able to draw this information directly from the NPD, meaning our study is almost completely free from missing data. Finally, this test took place one year after the CSC intervention had finished. We see this as an important strength of this measure, as it means our focus is upon lasting effects of chess instruction upon educational attainment. This is in contrast to the existing literature, which has almost exclusively concentrated on measuring effects directly after a chess-related intervention has finished.

We also consider children's outcomes in their Key Stage 2 (age 11) English tests and science level. While children's English and mathematics outcomes are based upon performance in an externally marked national examination, science scores are based upon their teacher's judgement.

Balance at baseline

Table 3a compares the prior attainment of children in treatment and control schools before the CSC intervention took place while Table 3b compares their characteristics. The distribution of pre-test (Key Stage 1) reading, writing and mathematics scores is very similar across the treatment and control groups, with differences at any given level typically just one or two percentage points and not statistically significant at even the 10% level.

<< Table 3a >>

Table 3b indicates that there are also broadly similar proportions of boys and girls in the two arms of the trial. There are slightly more children eligible for FSM in the control group (36 percent) than in the treatment group (33 percent) but this difference is not statistically significant (p = 0.25). Overall, this suggests that the sample is well-balanced in terms of both prior academic achievement and children's demographic characteristics.

<< Table 3b >>

4. Methods

Overall effectiveness

Our primary analysis is conducted on an 'intention-to-treat' (ITT) basis. Specifically, the impact of the programme is estimated via the following regression model:

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

where:

Y^{post} = child i in school j's post-trial (Key stage 2) score

Y^{pre} = child i in school j's baseline (Key stage 1) test score

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

C = baseline (pre-treatment) controls for other pupil characteristics (gender and FSM).

ε = error term

i = child i

j = school j

To allow for the fact that the programme was a school level intervention and that there is clustering of pupils within schools, all reported standard errors are estimated using the Huber-White adjustment, clustered at the school-level. The coefficient of interest from equation (1) is β. This measures the impact of the CSC programme on children's Key Stage 2 (post-test) scores. In the results section that follows, we also provide results using the simple difference in mean scores between treatment and control groups.

Alternative estimates adjusting for non-compliance

As noted above, this RCT was subject to a small amount of non-compliance. Specifically, six schools and 201 pupils (out of a total of 50 school and 1,965 pupils) moved from the treatment to control condition post-randomisation. Moreover, one control school containing 54 pupils managed to partially gain access to the treatment. To test the robustness of our ITT results, we also present 'Local Average Treatment Effect' (LATE) estimates, following the methodology of Sussman and Hayward (2010). This is essentially an instrumental variable (IV) approach, where initial treatment/control allocation is used as an IV for actual receipt of the intervention. It thereby 'corrects' the ITT estimate of the treatment effect for the non-compliance of some schools.

We implement the LATE analysis via Two-Stage Least Squares (TSLS). A first stage model is estimated, where treatment receipt is regressed upon initial random allocation:

$$Treatment\ Receipt_j = \alpha_1 + \beta_1.Treatment\ Allocation_j + \varepsilon_1 \quad (2)$$

where:

Treatment Receipt = a binary indicator of whether the school actually received the CSC programme.

Treatment Allocation = a binary indicator of whether the school was initially randomly assigned to receive the programme.

Predicted values of school's treatment receipt are then generated from Equation 2 ($\hat{T}$). These are then entered into the second stage of the model:

$$Y_{ij}^{Post} = \alpha_2 + \beta_2.\hat{T}_j + \varepsilon_2 \quad (3)$$

where:

$\hat{T}_j$= Predicted values of school's treatment status based upon the first stage regression model.

The parameter $\widehat{\beta_2}$ then gives the estimated impact of the CSC programme, accounting for the small amount of cross-over ('non-compliance') between treatment and control groups.

Heterogeneous effects

The model presented in equation (1) has specified a common programme effect; that the impact of the CSC intervention will be the same across different groups of children and across different types of school. Yet, in reality, the impact of the programme may vary between children with different characteristics (e.g. boys and girls), and between how the intervention was implemented within schools. We therefore present evidence on possible heterogeneous effects

in two ways. First we investigate whether impacts varied between genders and by FSM eligibility. Second, we examine possible heterogeneity across the achievement distribution.

While we are unable to directly observe non-cognitive outcome measures, evidence suggests that children from low income families and those at the bottom of the achievement distribution tend to have lower concentration and self-esteem (Blanden et al. 2007); skills that previous literature has suggested may be improved by playing chess. By looking across sub-groups and across the distribution of achievement, this allows us to test whether the intervention has a larger impact upon pupils that are likely to have more disruptive behaviour, lower concentration, lower self-esteem and less persistence. Conversely, it may be the case that chess enables high achieving pupils to build on their logic and critical thinking skills, improving their performance even further. Such effects would be missed by an investigation of mean outcomes alone. Therefore, to capture potentially important and interesting effects away from the mean, we re-estimate equation (1) using quantile regression.

5. Results

Impact of the CSC programme on mathematics attainment

Table 4 presents the ITT estimates of the impact of the CSC programme. Three model specifications have been estimated: (a) No control variables included (i.e. the simple difference in mean scores); (b) a single pre-test score controlled; (c) a full-set of controls, including pre-test scores in mathematics, reading, writing and science, gender and FSM eligibility. The left-hand panel refers to impact on overall Key Stage 2 mathematics test scores. Results in the right-hand panel focus upon the impact of the programme upon children's performance within the 'mental arithmetic' sub-domain. In both cases, the point estimate of the treatment is 0.001 with a 95 percent confidence interval ranging from approximately -0.149 to +0.151. In other words, based on an adequately-powered sample, and despite the reasonably successful implementation

of the CSC programme, we find no evidence that this had any impact upon children's mathematics skills one year after the intervention.

<< Table 4 >>

The robustness of this result has been tested in a number of ways. First, we have examined whether the small amount of cross-over between treatment and control groups is likely to have attenuated our estimate of the CSC treatment effect. There is little evidence that this is the case. Specifically, the LATE point estimate is also 0.001 standard deviations, with 95 percent confidence interval running from -0.166 to +0.168. Second, we have also re-estimated the treatment effect having excluded seven schools that decided to remove one of their weekly maths lessons in order to make room for the CSC curriculum. However, this actually led to a slight decline in the estimated impact of the intervention, with the point estimate turning negative (-0.03 with 95 percent confidence interval from -0.18 to +0.13). There is hence little evidence to suggest that either the small amount of non-compliance, or the replacement of mathematics lessons in a minority of schools, is driving this null result.

Impact upon reading and science attainment

We further consider whether the CSC programme had any effects observed in two other academic disciplines – reading and science. The estimated impact upon children's post-test (Key Stage 2) scores was -0.06 standard deviations in reading (95 percent confidence interval from -0.21 to +0.09) and -0.03 in science (95 percent confidence interval from -0.13 to +0.08). Hence there is no evidence that the CSC intervention had any impact upon children's achievement in reading or science.

Heterogeneous effects

Did the CSC programme have a positive effect upon the mathematics attainment of any of our pre-specified sub-groups? We find no evidence that estimates differ between boys, girls and children who are eligible for Free School Meals (FSM). The point estimate for boys is actually negative (-0.03), with the 95 percent confidence interval from -0.18 to +0.12. Although the point estimate for girls was positive (+0.03), the effect size was extremely small and statistically insignificant at conventional thresholds ($p = 0.76$). Moreover, a formal test of the gender-by-treatment interaction failed to reject the null hypothesis of an equal treatment effect for boys and girls. For children from low-income (FSM) backgrounds, the point estimate is essentially zero (+0.01), with the 95 percent confidence interval running from -0.18 to +0.19. There is thus no evidence that the CSC programme was particularly beneficial for the mathematics skills of children from socio-economically disadvantaged backgrounds.

It could be that our finding of zero impact upon mean mathematics scores is driven by a large positive impact upon one group (e.g. low mathematics achievers) and a large negative impact upon another (e.g. high mathematics achievers). Consequently, we have also produced quantile regression estimates of the treatment effect at each decile of the post-test (Key Stage 2) distribution. At each decile, the effect size is below 0.05 standard deviations in magnitude and is never significantly different from zero at even the 10 percent level. Again, this further strengthens the evidence that teaching primary school children how to play chess has little lasting impact upon their achievement.

6. Discussion: why might our results differ from the existing literature?

The previous section highlighted a clear difference between our results and the existing literature in Table 1. We now consider six reasons as to why this may be the case: (1) sample size issues; (2) challenges with taking the intervention to scale and implementation; (3) the

characteristics of the study population; (4) the length of time between the intervention and testing; (5) the nature of the testing; (6) the specific nature of the intervention in question.

Sample size issues

As Table 1 illustrates, the sample sizes in most previous studies are extremely small (median sample size of 54), while our study has been conducted at scale. The existing literature may therefore contain a number of false positive results.

To explore the likelihood of finding false positive findings, we have set up a simulation study, using data we have collected as part of this RCT. This simulation exercise involved the following three steps:

- Step 1. Randomly sample n observation from the 3,865 pupils included in our final analysis. Using this sample, the treatment effect is re-estimated[9].
- Step 2. Repeat step 1 for 1,000 runs of the simulation.
- Step 3. Calculate the proportion of the 1,000 runs where the estimated effect size is greater than 0.2 standard deviations. (We have chosen 0.2 as this is approximately the figure initially chosen in the power calculations for the study sample size).

The results of this analysis are presented in Figure 2. The horizontal axis plots the selected sample size (i.e. the 'n' used in step 1) while the vertical axis plots the percentage of the 1,000 runs where the effect size was above 0.2 (as calculated in step 3). Vertical lines are also plotted on this graph to illustrate the median and mean sample sizes for studies in the existing literature (see Table 1 for further details).

[9] For simplicity, in this simulation exercise we calculate the treatment effect as the unadjusted difference in mean mathematics scores between treatment and control groups.

The first key point is that, if we had drawn a sample size around the median of studies in the existing literature, we would have had around a 25 per cent chance of estimating an effect above 0.2 standard deviations. Second, the simulation reveals that the sample size needs to be at least 400 pupils before the false positive rate falls below five percent. This is complemented by Figure 3, which plots the distribution of estimated effect sizes across all 1,000 simulations for three selected sample sizes (n = 60, 200 and 400). As anticipated, the distribution is very wide when the sample size is around 60 (the median in the literature), with effect sizes up to 0.5 not uncommon. In contrast, the distribution begins to become reasonably tight when the sample size is increased to 400 (dashed red line).

This simulation study therefore highlights a key point; where sample sizes have been so small in existing studies, it is perhaps unsurprising that some studies have managed to produce extremely large results.

Challenges with taking chess programs to scale

Related to the point above is the challenge of taking interventions, which may seem to produce results in very small scale controlled settings, and replicating these at scale. Indeed, it is always questionable whether studies conducted in such small numbers and in specific settings produce informative and generalizable evidence that is useful for real-world policy and practise. Inevitably, implementation quality and fidelity for any educational intervention is likely to vary across schools when delivered at scale, which could mean different results are found compared to when a program is tested in a single school.

It is therefore important for us to consider whether there was significant heterogeneity in the estimated effects depending upon how ‘good’ the chess lessons were. These results are reported in the online appendix (see Appendix B), with the ‘quality’ of the chess lessons divided into

three separate groups (low, medium and high). Overall, there is no clear evidence that children taught chess by tutors of higher quality achieved significantly better Key Stage 2 scores.

Consequently, although the challenges with taking such an intervention to scale must be recognised, we find little evidence that well-delivered (highly enjoyed) chess lessons led to higher attainment.

Characteristics of the study population

The study population in this particular trial were pupils in schools with a high proportion of disadvantaged socio-economic pupils with below-average levels of achievement. Although some previous studies have also focused upon specific groups (e.g. children with visual impairments, special educational needs, or low socio-economic backgrounds), teaching new skills to pupils from lower-achieving and lower socio-economic backgrounds nevertheless raises certain challenges. Indeed, the need to maintain classroom behaviour was flagged as a key ingredient to successful implementation within the process evaluation.

Did our focus upon children within lower-achieving schools influence our results? We explore this possibility by considering if there was a differential impact of chess on attainment across two different measures of school quality (See online Appendix C). In both cases, we find no evidence of differential impact of chess on attainment. This suggests that our failure to detect an effect is unlikely to be due to our particular study population or implementation problems in schools with poorly behaved pupils.

Lessons that chess displaced

There is an opportunity cost to teaching children chess in schools; it replaces either learning time in another subject or becomes an after school activity (potentially displacing a different activity). Reporting of exactly what has made way for chess instruction differs across the

existing literature, and is often patchily reported. On many occasions, it is described as "a regular school lesson" only.

We have investigated whether the effect size varies depending upon the subject the schools chose to drop (see online Appendix D). We find no evidence of differential impact of the trial across the subjects that schools chose to drop.

Caution is of course required when interpreting these results due to (a) the small sample size and (b) a lack of a clear counterfactual – we do not know what subjects the control schools would have dropped had they been assigned to the intervention. Nevertheless, Appendix D does raise an important point regarding the interpretation of our results; our null findings may reflect the fact that learning chess has a similar impact upon children's test scores as the lesson that it has displaced (rather than learning chess has no impact upon test scores at all).

The nature of the testing

The outcome tests used in existing studies are typically "low-stakes" for pupils and schools (i.e. they have little riding upon the results). Moreover, in some studies the tests have been administered and invigilated by the chess tutors (e.g. Romano 2011) or have been very short and not age appropriate (e.g. Trinchero and Sala 2016; Sala et al 2015)[10]. In contrast, our primary outcome is based upon children's performance in national examinations, which are high-stakes for schools (they are publicly ranked by the results). These tests are also age appropriate, externally marked, and are relatively long (testing children's skills in a number of different areas).

[10] Both Trinchero and Sala (2016) and Sala et al (2015) use just seven of the released question from the Programme for International Student Assessment (PISA); a test designed for 15-year-olds that they administered to 8 to 11 year olds. Floor effects, and hence the validity of the post-test instrument used, are therefore a serious concern.

While we believe that this enhances the validity of our findings relative to much of the existing literature, it is difficult to know the extent this can explain why we have failed to detect an effect of the CSC programme. However, it is interesting to recall that the Key Stage 2 science scores we have used in our analysis are somewhat different to the reading and mathematics scores; while the latter reflect children's performance in formal examinations, the former are based upon teachers' judgements. The fact that we do not find any effect in any of these three subjects therefore suggests that our null results are unlikely to be entirely due to the nature of the outcome tests.

Length of time between the intervention and testing

Whereas most existing studies have examined pupils' outcomes directly at the end of the intervention, our focus is upon whether there is a sustained effect of chess instruction one year after the intervention has ended. It is hence possible that there was an initial impact of chess instruction directly after the intervention has finished, but which has then faded out.

However, existing literature on the fadeout effect suggests that it actually takes many years for initial impacts to fade away. For instance, after reviewing the evidence for a number of early childhood interventions, Protzko (2015) noted how '*the fadeout effect is real, but the fade is slow and occurs over years*' – and hence cautioning against focusing upon immediate outcomes only. Likewise, Barrett et al (2016) illustrate how the average effect size across 67 early childhood programmes falls from around 0.2 standard deviations immediately after the intervention has finished to around 0.1 standard deviations one year later, but does not completely fade away until up to four years after the intervention has finished.

The existing literature on the fade-out effect therefore suggests it is unlikely that chess had a large initial impact (of the magnitude claimed in previous studies) which has then completely disappeared after just one year. Rather, we believe it more likely that any initial impact of chess

instruction upon academic skills is small at best, and is then quickly washed out by other factors.

Specific nature of the intervention in question

The impact of chess instruction upon children's outcomes may vary depending upon the nature of the intervention: who delivers the lessons, for how many hours, and the pace at which children are taught. These factors vary greatly across the literature; some chess interventions were under 20 hours while others were up to 90 hours, while some were delivered by specialist chess tutors and others were not.

At 30 hours tuition, the CSC intervention was around the average reported elsewhere in the literature (see Table 1), and above the 25 hours Sala and Gobert (2016) report as the threshold above which chess instruction produces substantial effects. In terms of delivery, the CSC intervention was similar to the only other large scale investigation of chess by Romano (2011), with lessons delivered to children around the same age as in our trial, with instructors following a similar standardised curriculum. Therefore, although details of what 'teaching children chess' actually means is only patchily reported within the literature, we do not believe the specific nature of the CSC intervention to be a major factor leading to the difference in our results.

Summary

Throughout this section we have offered various reasons why the results from our RCT differ from previous studies. Pulling these together, we offer the following conclusion. A combination of small sample sizes, problematic testing instruments and procedures, and operating in just a handful of controlled settings is likely to have inflated the effect sizes reported in this literature to unrealistically high levels. Effect sizes of the same magnitude are unlikely to be reproduced when a valid, important and externally assessed outcome is used, and when such interventions are delivered in 'real-world' classrooms at scale. Although some modest effects of chess instruction may still occur directly after the intervention has finished, these are likely to be

washed out in under a year. Consequently, although there may be some other long-run benefits of learning to play cognitively demanding games such as chess (e.g. upon children's social and emotional skills), we urge caution against blindly accepting the conventional wisdom that such games will have a sustained impact upon young people's educational achievement.

7. **Conclusions**

Chess is enjoyed by millions of people worldwide. To be successful, players require high levels of concentration, to demonstrate logical reasoning and have the ability of think strategically. It is therefore a prime example of a 'cognitively demanding game'. Such games are currently receiving a great deal of attention in countries like the United Kingdom and the United States, due to the potential effect on young people's academic achievement. Indeed, many of the skills outlined above, which chess is thought to develop, are also required to succeed in school – particularly in quantitative disciplines such as mathematics.

A significant body of research has therefore suggested that teaching children how to play chess has a positive impact upon their educational attainment, with studies implying that this relationship is causal. Yet the existing evidence base remains limited due to notable weaknesses in terms of research design. In reality, most existing studies provide correlational evidence only, typically invoking a strong 'selection upon observables' assumption. Only rarely has an experimental or quasi-experimental methodology been used. Yet these typically suffer from difficulties including small sample sizes, measurement of immediate outcomes only, question-marks over the validity of the outcome tests and potentially compromised randomisation.

By implementing a large clustered randomised controlled trial (RCT) across primary schools in England, we attempt to overcome many of the problems that exist with the evidence base on cognitively demanding games. Our key finding is in direct contrast to the conventional wisdom prevailing within the existing literature. Specifically, we find no evidence of any lasting impact

of chess instruction upon children's mathematics, reading or science test scores. This holds true across various sub-groups (boys, girls, children from disadvantaged socio-economic background) and across the sample as a whole.

This finding should, of course, be interpreted in light of the limitations of our study. First, it is important to stress that the focus of this trial was children's academic outcomes only. Yet chess instruction (and cognitively demanding games more generally) may have a number of important additional benefits, including potential impacts upon children's self-confidence and non-cognitive skills. It may also provide children with a consumption benefit – the enjoyment of playing. Second, although we have taken steps to investigate the external validity of our results, we cannot generalise our findings to other geographic areas (e.g. to other countries) or to different age groups (e.g. younger or older pupils). Further research focused upon these two areas, using a strong experimental or quasi-experimental design, represent the next important steps in this line of research.

Despite these limitations, we believe there are at least two wider implications of our findings. First, there is currently a lot of hype surrounding the impact cognitively demanding games may have upon young people's educational achievement, based upon a few relatively small-scale or correlational studies finding positive results. Chess is a prime example, one where many perceive there to be a positive benefit, and where (at face value) there seems to be a reasonable evidence base. However, our analysis has shown that once one scratches below the surface, and employs a rigorous research design delivered to many pupils at scale, the foundations behind claims of a large causal impact of such games upon educational attainment do not appear as strong as has perhaps has previously been suggested.

Second, although economists spend much time and effort planning and executing robust identification strategies, this paper has served as a reminder that this only a necessary (but not

sufficient) condition for determining ‘what works’ in policy and practise. Other elements of the research design, including the use of valid outcome measures, representative samples, delivery at scale and the use of longer-term follow-ups are also important. Future research on cognitively demanding games, whether this be chess, puzzles or video games, will ideally take these wide range of design issues into account.

Overall, claims that chess instruction has a significant impact upon children’s attainment have, in our view, stretched the available evidence too far. We believe this is also the case for many other cognitively demanding games, particularly video games, where there is currently much hype. This paper has sought to challenge the prevailing view and, in the process, has highlighted the need for causal statements to be made only when a robust experimental or quasi-experimental methodology has been used.

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Table 1. Previous studies attempting to measure the causal effect of chess upon children's educational attainment

	Method	Effect size	Sample size (# of pupils)	Hours instruction	Test stakes	Test administrator	Time between intervention and test
Aydin (2015)	None	1.66	26	48	Unknown	Researchers	End of intervention
Barrett and Fish (2011)	Pre-post	1.23	31	25	State assessment	Independent	End of intervention
Kazemi et al (2012)	Randomisation	1.19; 0.79; 0.74; 0.65	180	96	Low	Researchers	End of intervention
Sgirtmac (2012)	Pre-post	1.06	100	50	Unknown	Researchers	End of intervention
Krame and Flipp (unpublished)	Unpublished	0.63; 0.26	167	32	Unpublished	Unpublished	Unpublished
Gilga and Flesner (2014)	Randomisation	0.563; 0.09; -0.06	38	10	Low	Researchers	End of intervention
Sala et al (2015)	Cluster randomisation	0.45; 0.33	566	18	Low	Researchers	End of intervention
Christaien and Verhofstadt-Denève (1981)	-	0.41; 0.28	37	42	Low/moderate	Independent	End of intervention
Trinchero and Piscopo (2007)	Unpublished	0.41	-	30	Unpublished	Unpublished	Unpublished
DuCette (2009)	Matching	0.37; 0.26	352	-	Low/moderate	Independent	End of intervention
Garcia (2008)	Unclear	0.36; 0.12	54	90	Low/moderate	Independent	End of intervention
Trinchero and Sala (2016)	Cluster randomisation	0.34	931	19	Low	Researchers	End of intervention
Romano (2011)	Cluster randomisation	0.34	1756	25	Low	Chess tutors	End of intervention
Sala and Trinchero (2016)	Unpublished	0.29; -0.11	-	10	Unpublished	Unpublished	Unpublished
Margulles (1992)	Pre-post	0.28	53	-	Low	Independent	End of intervention
Yap (2006)	Matching	0.27; 0.15	321	50	Low	Teachers	End of intervention
Forrest et al (2005)	Pre-post	0.24; 0.10	54	37	Low	Researchers	End of intervention
Rifner (1992)	None (unpublished)	0.17; 0.15	18	30	Low	Researchers	End of intervention
Hong and Bart (2007)	Randomisation	0.15	38	20	Low	Researchers	End of intervention
Fried and Ginsburg (unpublished)	Randomisation	0.13; 0.10	30	-	Low	Researchers	End of intervention
Scholz et al (2008)	Randomisation	0.12; 0.02	53	24	Low	Researchers	End of intervention
Aclego et al (2012)	Pre-post	0.12	170	96	Low	Researchers	End of intervention
Eberhard (2003)	None	-0.03	137	60	Low	Researchers	End of intervention
Sala, Gobet, Trinchero, & Ventura (2016)	Cluster randomisation	-0.03	52	15	Low	Researchers	End of intervention

Source: Sala and Gobet (2016).

Table 2a. A comparison of demographic characteristics and prior achievement of CSC participants to the England state school population

	Trial participants %	All eligible pupils %	England %
Key Stage 1 maths			
Level 1	12	12	8
Level 2C	19	20	15
Level 2B	31	30	27
Level 2A	24	24	27
Level 3	12	11	20
Missing	2	3	2
Key Stage 1 reading			
Level 1	17	16	12
Level 2C	15	14	12
Level 2B	27	27	23
Level 2A	24	23	25
Level 3	15	15	26
Missing	3	4	3
Key Stage 1 writing			
Level 1	20	20	15
Level 2C	23	23	20
Level 2B	29	29	29
Level 2A	18	16	20
Level 3	6	7	13
Missing	4	5	4
Key Stage 1 science			
Level 1	16	16	10
Level 2	71	72	68
Level 3	11	10	20
Missing	2	2	2
KS1 average points score (standardised)	**-0.280**	**-0.289**	**0.000**
School n	**100**	**442**	
Pupil n	**3,775**	**16,397**	**570,344**

Table 2b. A comparison of demographic characteristics and prior achievement of CSC participants to the England state school population

	Trial participants %	All eligible pupils %	England %
Eligible for FSM			
No	66	65	82
Yes	35	35	18
Gender			
Female	50	50	49
Male	50	51	51
School n	**100**	**442**	
Pupil n	**4,003**	**16,397**	**571,733**

Table 3a. Balance between treatment and control groups

	Intervention group		Control group		Difference	
	n	Percentage	n	Percentage	Percent	P-Value
Key Stage 1 maths						
Level 1	242	12%	236	12%	0%	0.84
Level 2C	366	18%	356	18%	0%	0.79
Level 2B	590	29%	567	29%	0%	0.86
Level 2A	441	21%	450	23%	-2%	0.45
Level 3	246	12%	191	10%	2%	0.18
Missing	170	8%	154	8%	0%	0.74
Key Stage 1 reading						
Level 1	330	16%	309	16%	0%	0.88
Level 2C	278	14%	280	14%	0%	0.58
Level 2B	523	25%	491	25%	0%	0.84
Level 2A	428	21%	457	23%	-2%	0.16
Level 3	304	15%	243	12%	3%	0.18
Missing	192	9%	174	9%	0%	0.72
Key Stage 1 writing						
Level 1	363	18%	373	19%	-1%	0.38
Level 2C	433	21%	453	23%	-2%	0.21
Level 2B	586	29%	509	26%	3%	0.14
Level 2A	340	17%	319	16%	0%	0.90
Level 3	116	6%	112	6%	0%	0.94
Missing	217	11%	188	10%	1%	0.46
Key Stage 1 science						
Level 1	306	16%	297	16%	0%	0.88
Level 2	1,317	68%	1,369	74%	-6%	0.02*
Level 3	266	14%	131	7%	7%	0.002*
Missing	166	2%	157	3%	-1%	0.97
Key Stage 1 average point score						
Mean	1,932	0.024	1,843	-0.025	5%	0.38

Notes: * indicates statistically significant difference at the five per cent level

Table 3b. Balance between treatment and control groups

	Intervention group	Control group	Difference	P-Value
Eligible for Free School Meals				
No	67%	64%	3%	0.23
Yes	33%	36%	-3%	0.23
Gender				
Female	49%	51%	-2%	0.25
Male	51%	49%	2%	0.25
School n	**50**	**50**		
Pupil n	**1,954**	**2,055**		

Table 4. The impact of the Chess in Schools programme on children's age 11 test scores

	Mathematics overall		Mental arithmetic	
	Effect size	**SE**	**Effect size**	**SE**
Model 1. No controls	0.04	0.08	0.03	0.07
Model 2. Pre-test maths control	0.01	0.08	0.00	0.06
Model 3. All controls	0.00	0.08	0.00	0.06

	Reading		Science	
	Effect size	**SE**	**Effect size**	**SE**
Model 1. No controls	-0.03	0.08	0.01	0.05
Model 2. Pre-test control	-0.05	0.07	-0.06	0.05
Model 3. All controls	-0.06	0.07	-0.03	0.05

Notes: Figures refer to the Intention-to-Treat (ITT) estimates. Estimates refer to effect size (Cohen's D). Model 2 controls for only the pre-test score in the specific subject being considered. Model 3 includes controls for baseline mathematics, reading, writing and science scores, gender and free school meal eligibility. The r-squared in model 3 is 0.45 for mathematics and mental arithmetic, 0.41 for reading and 0.42 for science.

Figure 1: Flow of participants in the CSC trial

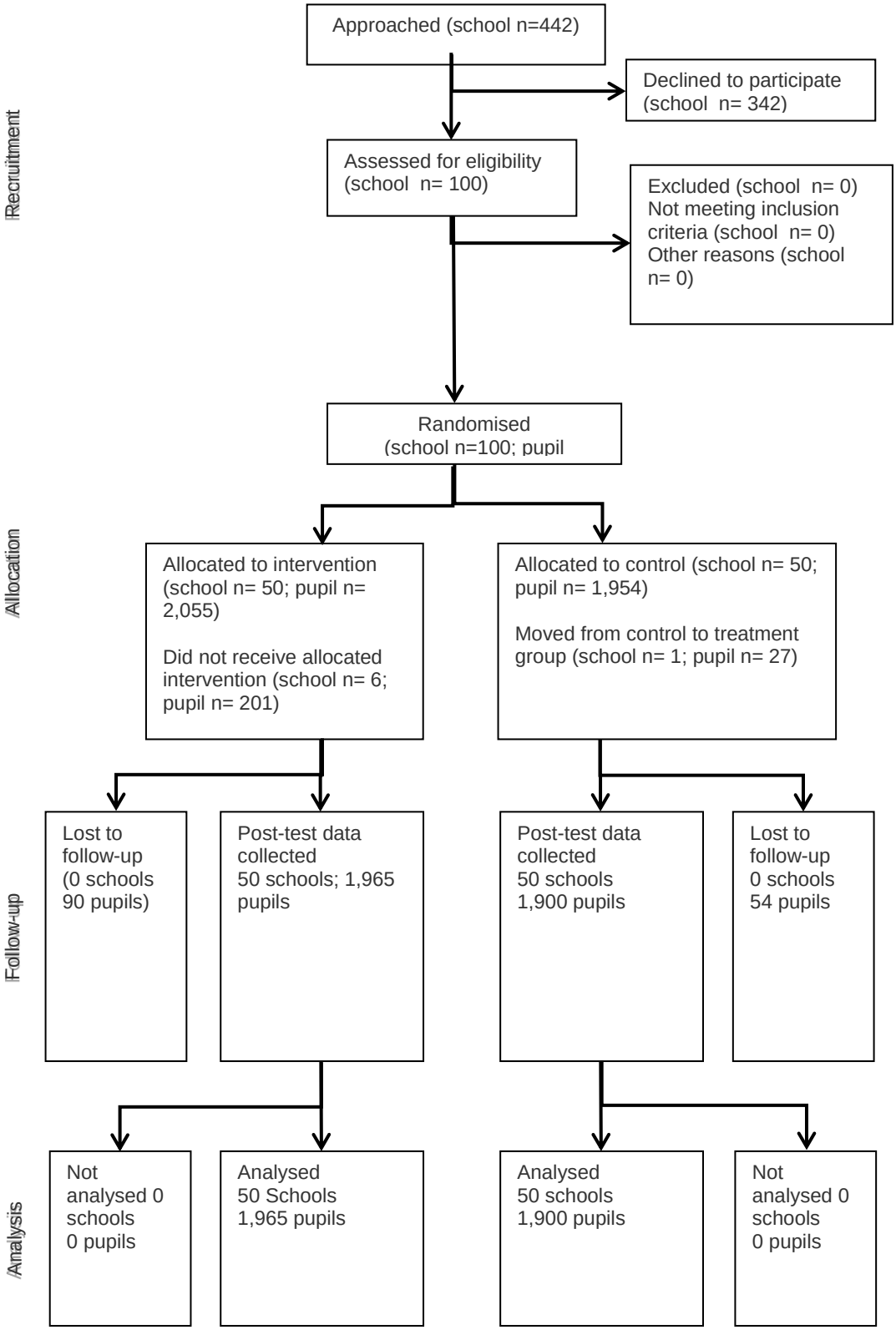


Figure 2. The percentage of false findings (effect size greater than 0.2) across 1,000 simulation runs, using different sample sizes.

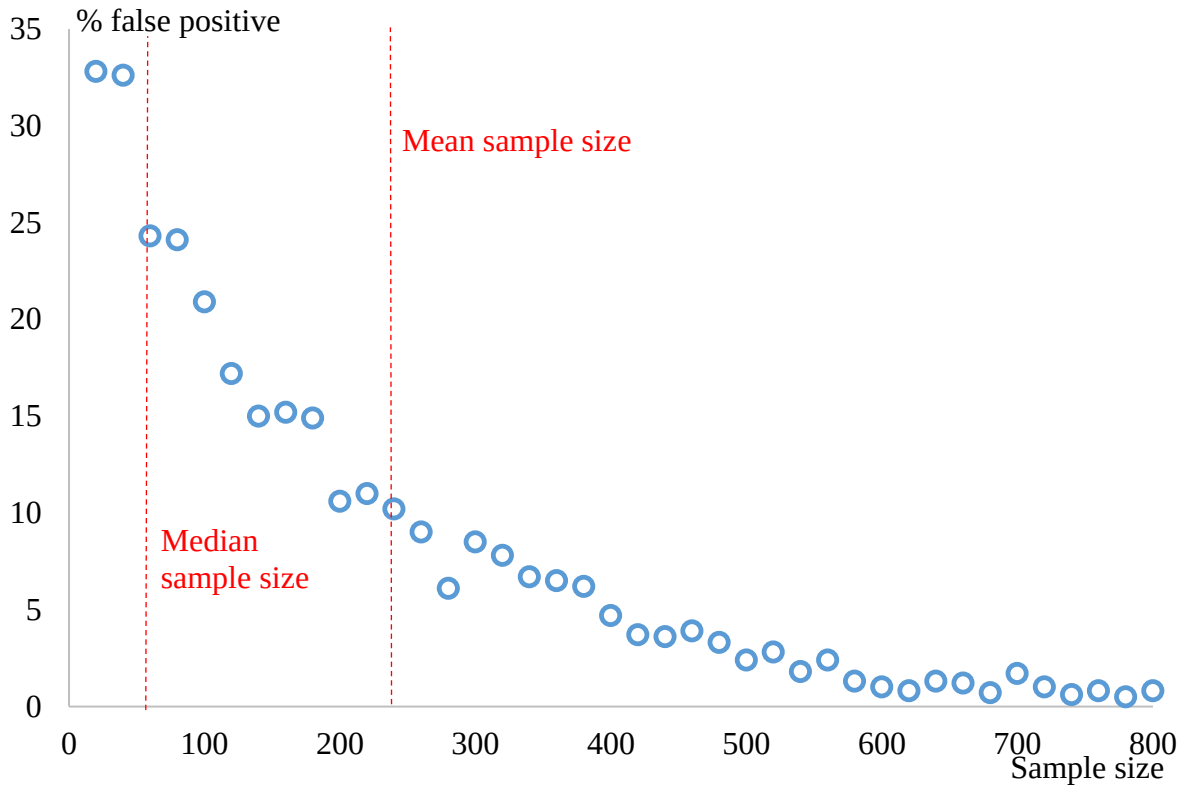
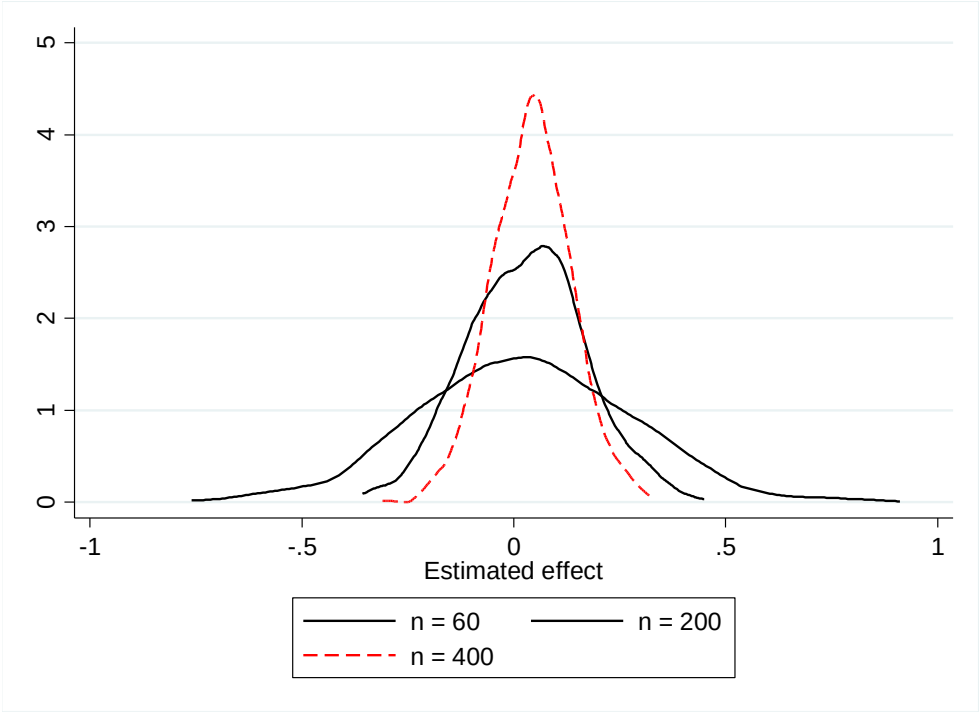


Figure 3. Estimated effect sizes across 1,000 simulations of different sample sizes



Appendix A. Power calculations

We calculated 100 schools as the minimum necessary number to detect an effect of approximately 0.18 of a standard deviation in Key Stage 2 mathematics test scores. This calculation assumed:

(i) An intra-cluster correlation (ICC) of 0.15 at the school level[11]

(ii) Equal cluster sizes of 60 year 5 pupils per school[12]

(iii) 40 percent of the variation in KS2 maths test scores would be explained by baseline covariates[13]

(iv) 80 percent power for a 95 percent confidence interval

Table A1 provides estimates of the ICC for the actual sample of schools/pupils that took part in the study. Estimates are presented for baseline (KS1 average points score) and follow-up (KS2 maths) tests, when using either a fixed or random school level effect. The ICC for KS1 Average Point Scores (APS) was 0.08 when using a fixed effects model. The analogous ICC for KS2 maths was 0.13. In the results section, we illustrate that 45 percent of the variance in KS2 maths test scores can be explained by the baseline covariates. Using these figures in place of (i) and (iii) above, we calculate the minimum detectable effect in this trial was approximately 0.16.

Table A1. Estimated inter-cluster correlation

	Fixed effect	Random effect
Key Stage 1 APS	0.08	0.05
Key Stage 2 Maths	0.13	0.11

Note: Figures refer to the proportion of the variation in pupils' test scores occurring between school

[11] A value of 0.15 for the ICC was chosen after the team conducted an analysis of within and between school variation in key stage 2 test scores within the National Pupil Database.

[12] The figure of 60 pupils was based on the assumption of most recruited schools being two form entry, with each form containing 30 pupils.

[13] A value of 0.4 was chosen after the team conducted an analysis of the association between key stage 1 and key stage 2 test scores within the National Pupil Database.

Appendix B. Estimated effect of the Chess in Schools intervention upon children's Key Stage 2 mathematics scores, by chess lesson 'quality'

Given differences in the size of our trial compared to others, one possible reason for differences in findings is the fact that it is harder to implement high-quality interventions to scale. We therefore investigate whether the impact of our trial varies by quality of the intervention. This information was captured within the qualitative process evaluation, with a basic 'tutor quality' measure created based upon children's reports of their enjoyment and engagement in the lessons. Children taught by 'low quality' tutors achieved Key Stage 2 scores slightly below the control group (-0.05 standard deviations) while children with 'medium quality' tutors scored a little higher than the control group (+0.11 standard deviations). However, there is no clear pattern of a 'dose-response' relationship, as the effect of having a high quality tutor was essentially zero. Moreover, none of the estimates reach statistical significant at conventional levels. There is hence no evidence that the effect of the CSC intervention varied significantly by this particular measure of Chess lesson quality.

Outcome	Effect size (95% CI)	p-value
'low quality'	-0.05 (-0.26 to +0.15)	0.63
'medium quality'	+0.11 (-0.07 to +0.29)	0.25
'high quality'	0.00 (-0.27 to +0.26)	0.99

Notes: Low, medium and high quality lessons based upon the proportion of pupils who enjoyed the chess tutors lessons. Effect sizes reported are relative to the reference group.

Appendix C. Estimated effect of the Chess in Schools intervention upon children's Key Stage 2 mathematics scores, by measures of school quality

It may be the case that we did not find an impact of chess on attainment because our defined sample was particularly disadvantaged. We therefore investigate whether there was any differential impact of chess on attainment across school quality in our sample, using two different measures of school quality.

First, as noted in section 3, schools that participated in the trial were initially divided into ten separate strata based upon historical achievement data and the proportion of children eligible for Free School Meals. We have investigated how the estimated treatment effect varies across these strata, and whether there is any evidence of greater effects observed in higher-achieving, more affluent schools. We find little evidence that this is the case, with no consistent pattern of larger effect sizes within higher-achieving or less-deprived schools.

Estimated effect of the Chess in Schools intervention upon children's Key Stage 2 mathematics scores, by randomisation strata

Strata	Number of pupils (schools)	Effect size	SE
Low achieving, high deprivation	406 (12)	-0.21	0.13
Low achieving, average deprivation	487 (12)	0.15	0.21
Low achieving, low deprivation	294 (7)	-0.32**	0.12
Average achieving, high deprivation	305 (7)	-0.05	0.20
Average achieving, average deprivation	447 (11)	0.15	0.21
Average achieving, low deprivation	561 (13)	0.29*	0.15
High achieving, high deprivation	346 (9)	-0.09	0.20
High achieving, average deprivation	537(10)	-0.17	0.14
High achieving, low deprivation	420 (11)	-0.10	0.10
Late recruitment	178 (7)	0.00	0.14

Notes: * and ** indicate effect size statistically significant at the 10% and 5% levels respectively.

Second, in England, schools are regularly externally inspected and rated on a four-point scale (Outstanding, good, requires improvement and inadequate). These ratings are in part based upon inspectors' judgements of pupils behaviour, with previous research finding the impact of school-based interventions to vary by this factor (Jerrim and Vignoles 2016). It is thought that this likely to be due to the challenges of successfully implementing interventions within challenging schools.

Estimated effect of the Chess in Schools intervention upon children's Key Stage 2 mathematics scores, by school inspection rating

Ofsted rating	Sample size pupils (schools)	Effect size	Standard error
Overall grade			
Outstanding	493 (14)	-0.17	0.14
Good	2579 (65)	0.04	0.09
Requires improvement	646 (17)	0.01	0.13
Missing data	120 (3)	-0.59	0.32
Quality of teaching			
Outstanding	405 (12)	-0.06	0.15
Good	2667 (67)	0.02	0.09
Requires improvement	646 (17)	0.01	0.13
Missing data	120 (3)	-0.59	0.32
Behaviour of pupils			
Outstanding	1098 (30)	0.03	0.11
Good	2395 (60)	0.03	0.11
Requires improvement	225 (6)	0.11	0.11
Missing data	120 (3)	-0.59	0.32

Notes: None of the estimates are statistically significant at the five per cent level.

Even in outstanding schools, with excellent teaching and well-behaved pupils, we still find no evidence that the CSC intervention had a positive impact upon pupil outcomes. Indeed, in contrast to Jerrim and Vignoles (2016), we find no evidence of heterogeneity in the effect by school inspection rating. We therefore believe that our focus upon lower-achieving schools is unlikely to be responsible for our failure to detect a positive treatment effect.

Appendix D. Estimated effect of the Chess in Schools intervention upon children's Key Stage 2 scores, by subject dropped to make way for the chess lessons

In our study, schools were allowed to choose how the hour of chess instruction would fit into their weekly timetable, though with the expectation this would be an art or humanities subject. Given this decision, 15 schools chose to drop an arts or humanities lessons, 13 used a mix of different (though not mathematics) lessons, nine were categorised as 'other' (including science, ICT, and physical education), seven dropped a mathematics lesson, while six did not receive the chess intervention (recall Figure 1).

Due to the small sample size within each category, most estimated treatment effects are statistically insignificant. However, the general direction of the point estimates suggests that schools which chose to drop an arts or humanities lessons tended to do slightly worse than the control group, while schools in the 'other' category tended to do slightly better. Moreover, there is no evidence that schools which replaced a mathematics lesson with a chess lesson did worse than the control groups.

	Sample size pupils (schools)	Mathematics overall		Mental arithmetic	
		Effect size	SE	Effect size	SE
Intervention Group (Ref: Control)	1926 (50)				
Dropped mathematics	333 (7)	0.11	0.134	0.05	0.12
Dropped arts/humanities	683 (15)	-0.13	0.09	-0.15*	0.08
Dropped a mix of subjects	542 (13)	-0.01	0.09	0.08	0.08
Dropped 'other'	311 (9)	0.26*	0.14	0.19**	0.09
Crossed-over	201 (6)	-0.18	0.12	-0.19	0.12

	Sample size pupils (schools)	Reading		Science	
		Effect size	SE	Effect size	SE

Intervention Group (Ref: Control)	1926 (50)				
Dropped mathematics	333 (7)	0.03	0.11	-0.05	0.06
Dropped arts/humanities	683 (15)	-0.16**	0.08	-0.13	0.09
Dropped a mix of subjects	542 (13)	-0.02	0.09	0.05	0.07
Dropped 'other'	311 (9)	0.11	0.15	0.05	0.10
Crossed-over	201 (6)	-0.29**	0.08	-0.07	0.07

Notes: Estimates refer to effect size (Cohen's D). Model 3 includes controls for baseline mathematics, reading, writing and science scores, gender and free school meal eligibility.