

## CONCLUSION

## **Abstract**

So far, this book has reported an enormous amount of evidence, ours and others, on how education systems around the world can make education better for disadvantaged pupils. This has included large-scale reviews, linking and re-analysing secondary data, and in-depth primary fieldwork. This final chapter briefly summarises the key findings, that appear across these various datasets and approaches, and begins to consider their implications for policy and practice.

## **Introduction**

This book is a compilation of research on the social and educational value of the use of monetary policies to improve attendance and attainment at school. There is considerable evidence, from this book and beyond, that financial investments in education can reap benefits, especially for students from more disadvantaged families. This is true of improving attendance in countries where attendance is far from 100%, and/or where the participation of girls is considerably lower. Money in the form of additional grants to schools, like the Pupil Premium in England, can also help reduce socio-economic segregation between schools and the poverty attainment gap, at least at primary school age. Of course, almost any educational intervention, policy or programme involves financial expenditure in some way. This book has looked at some of the ways that money can be best used to improve school outcomes, drawing on worldwide examples, and evidence from prior studies judged to be methodologically strong. These studies also helped to identify what has not worked well, and this is important because it provides guidance on where not to waste limited resources.

In Chapter 2, we outlined some of the main factors that may drive the poverty attainment gap, focussing on those that can be altered most easily in the short to medium term, and those which are the remit of schools and schooling. And we made the point there that this does not imply acceptance of poverty itself, but that the elimination of poverty itself is not a task that education is especially suited to, in the short to medium term. Nor did we suggest that the poverty attainment gap could ever be entirely eliminated. More realistically, in order to reduce the gap through education we proposed considering:

- Improving access, enrolment in, and attendance at school
- Limiting the level of between-school SES, ethnic and other segregation
- Increasing the supply and deployment of well-prepared teachers
- Enhancing the range of activities and interactions in schools and classrooms
- And deciding on the best bets for using money to address all of the above

Having presented a wealth of evidence on these issues from our structured reviews, secondary analyses and primary fieldwork, we are now in a position to summarise what is known, and suggest what the implications might be for policy and practice. The good news is that schools can make a difference, to individuals and to closing the poverty attainment gap. But for this to happen, policy-makers and practitioners need more help in using good evidence on how to do so.

## **Encouraging school enrolment and attendance**

It is clear that money can make a difference. In less developed schooling systems, money helps to create new or extra school places which will tend to benefit the poorest, often rural, children who would otherwise not have the opportunity to attend school. Money can then be used to improve school infrastructure, resources, and student transportation. So, one quick and simple way to address the poverty attainment gap is to ensure that all children have a free place at a nearby school. Money is

needed to build and open new schools, and to provide more school places, where these are not yet universal. This helps to reduce the gap in opportunities and outcomes created by family poverty.

Once there are sufficient school places within reach of all families, schooling should be free at the point of delivery, easy to access, and compulsory. There is further evidence that offering incentives for school attendance has promising results. Monetary incentives to compensate for loss of income through school attendance is also an effective way to increase enrolment and participation in school, presumably as a temporary measure in less developed systems. Provision in kind like transport to school, or meals at school, and the removal of fees, can all encourage attendance. Feeding children at school, free, may have lifelong benefits even in developed systems such as Sweden or Finland (Lundborg and Rooth 2021). These approaches are most likely to encourage the attendance of students from poorer families. These are especially important for girls in developing countries, and for disadvantaged students everywhere, even in more developed countries. This kind of funding is best provided directly to students or families - not to schools, areas, or teachers - as an incentive for attendance, conditional on appropriately full attendance.

Schools can then provide practical, safe environments where learning can happen under the supervision and care of trained teachers and other staff members. Schools can share the burden of responsibility for child development with parents. Our evidence from Pakistan and India shows that in the absence of school learning can be hindered, and that poorer parents do not always prioritise their children's education over more basic economic needs (Chapter 5). In more developed school systems, near-universal school attendance has been achieved, and this has changed and, in some cases, reversed previous patterns of social inequality, with girls now getting higher attainment than boys at school, on average, and the results for pupils from different ethnic groups converging in attainment, for example.

### **Encouraging desegregation**

Once school systems are more developed, and there are free local places for all, attention can turn to the nature of each school intake. An even distribution of disadvantage between schools makes the whole school system fairer, and allows individual schools to focus their energy where it is most needed (see Chapter 2). There is also worldwide evidence that average attainment is higher in the most mixed school systems. This kind of desegregation of socio-economic disadvantage between schools has many societal, individual and educational benefits, and so should be a priority once there are enough places for all in any school system. It will also tend to be linked to ethnic and other forms of desegregation.

This means that all schools should be open access (rather than selective in any way), and as similar as possible in structure and process across the system. School places should not be rigidly allocated in terms of nearby housing, lest the schools then reflect any local residential segregation by SES, ethnicity, or related student characteristics. School desegregation by SES may also involve the temporary use of bussing to school, or the use of lotteries to decide over-subscribed places at any school.

Money can play an important further role in equalising the nature of school intakes. Our large-scale analysis of Pupil Premium funding in England shows that money provided for schools as an incentive/recompense for taking disadvantaged students, and allowing them to prioritise such students, is linked to a reduction in the extent to which these potentially harder-to-teach students are clustered in specific schools, or types of schools.

Since the introduction of Pupil Premium in England in 2011, the clustering of long-term disadvantaged pupils in schools has declined, for all age groups. It is relatively easy to envisage how allocating money like Pupil Premium funding to schools on the basis of their disadvantaged pupil intakes would make such pupils less unattractive to schools in general, and so could reduce the historic clustering of such pupils in specific under-subscribed schools. Also, because schools can now use disadvantage as a positive criterion for allocating contested school places, the funding could have had an almost

immediate (and continuing) impact on school intakes, both directly and by reducing any unconscious bias against potentially harder-to-teach pupils.

The national drop in segregation is most obvious in Years 1 and 7, which is where Pupil Premium would be initially expected to make the most difference. This shift cannot be explained by economic or legal changes, nor by a reduction in the diversity of schooling, substantial changes to admissions arrangements, or the abolition of selection by faith or ability (Gorard 2018). Pupil Premium funding is currently the best explanation for the improvement, which suggests that the policy should be continued for the time being in England, and that similar schemes could be rolled out in more developed systems elsewhere on the basis of this evidence.

As stated, one reason an over-subscribed school might be reluctant to offer places to poorer children (even if unconsciously) is that, on average, they could be harder to teach. The extra funding given to schools as Pupil Premium can be used to implement evidence based catch-up programmes or pay for the cost of extra staff time in supporting poorer children (see below). This is what makes the policy clever – it is both an incentive to desegregate, and a way of funding programmes to reduce the poverty gap. How is the latter addressed?

### **Reducing the attainment gap**

Having free places at equivalent schools for every child, ensuring that every child attends, and discouraging any form of needless segregation between schools, are the basic elements of a good school system. They will encourage overall levels of attainment and minimise any differences in attainment outcomes. Going further than these basic elements involves consideration of how funding is spent by schools, and what happens in classrooms at school, and so depends on the priorities and decisions of local authorities, teaching staff and school leads. These considerations need to be guided by the best available evidence-syntheses (that take the quality of each study into account). We cannot provide a full account here, but readers can refer to the many reviews we have conducted that are cited and described in this book, and in our previous work. We focus more here on our newer evidence, especially from the recent work in England.

#### *Implications from Pupil Premium for policy*

There has been a marked drop in the attainment gap at KS1 in England, since Pupil Premium funding was introduced. There was the beginning of a reduction in the attainment gap at KS2, and the gap is still lower than historically. The picture at KS4 is more mixed. The attainment gap dropped and then began to grow again from 2014 onwards, linked to changes in difficulty, scoring, and value-added scores. It seems that the Pupil Premium policy may be being contradicted by other governmental interventions. Overall, we can say that the Pupil Premium seems to have worked for long-term disadvantaged pupils at primary age. They are less clustered in schools, have better KS1 scores, and somewhat better KS2 scores than before 2011.

There is a clear relationship between the attainment gap and the poverty segregation gap. It has not been proven whether these two are causally related, or in which direction, or whether they are mutually reinforcing. However, the most likely link is that a less segregated school system will tend to have smaller poverty attainment gaps. Therefore, any society should keep operating to reduce between school segregation by poverty, by any means, including using something like Pupil Premium funding when allocating resources (note this does not necessarily mean more funding, rather more calibrated use of whatever funding is available). This may then assist in reducing the attainment gap in the longer term. But meanwhile, more attention needs to be paid by policy-makers to ensure that other policies for assessment do not interfere with the worthwhile drive towards a lower attainment gap.

Areas of England like the North East, the West Midlands and London, with the greatest proportion of long-term disadvantaged pupils, tend to have the lowest attainment gaps and show the greatest

improvement over time. Poverty segregation between schools is lower and declining in the areas of most long-term disadvantage, matched by lower attainment gaps in these areas as well. Policy-makers need to understand that areas like the West Midlands and North East of England, which have been criticised by the inspection regime Ofsted and others for apparently failing their students, are actually getting better results than many other areas, for equivalent long-term disadvantaged students. Policy-makers and too many other commentators are being misled, by ignoring the prevalence of long-term disadvantage and the consequent challenges for schools, into believing that the local schools in poorer areas are somehow failing their pupils in a way that is not happening in the South East or South West. This wrong diagnosis will inevitably lead to the wrong policy solutions. If there are lessons to be learned about how to deploy Pupil Premium funding, and so improve life chances for poorer children, these poorer regions are the areas that policy-makers and other commentators should include in their search for advice (rather than inappropriately denigrating them).

Similarly, the apparently low poverty attainment gap in some London boroughs is, as we have shown, almost entirely an artefact of the number of residents attending private schools, and so being left out of the official attainment gap calculation. There are boroughs where a third or more of local children do not attend state schools. Any area of England would appear to have a low attainment gap if a third of the local children, selected from the highest attaining and richest families, were omitted from the attainment gap calculation there. Again, policy-makers and others are being misled. It is certain that some good work is going on in London to help reduce the poverty attainment, but it is not clear that this is any better than, or should be a role model for, similar work going on in Birmingham or Middlesbrough for example.

Some commentators have been saying that the Pupil Premium is not always being used properly by schools, or is otherwise not being effective. They have called for the money to be used for general school funds (Allen 2018, Morris and Dobson 2020), or to recruit, develop and retain teachers (Staufenberg 2019). In the absence of evidence of the effectiveness of these alternative uses for Pupil Premium funds, it would make sense to assume for the present that Pupil Premium should be retained in something like its current form, perhaps with a greater focus on KS1 (see below), and calibrated to increase funding for the long-term disadvantaged. This is not the same as just giving extra money to schools, which prior studies have suggested is less effective, because the funding must be used for the purposes intended. The government created a parallel programme, including the Education Endowment Foundation (EEF), to provide guidance to schools on how best to spend the funds. The Pupil Premium policy also differs from one based on giving money to poorer families on condition that their children attend school (Baird et al. 2011, de Janvry et al. 2006, Morley and Coady 2003), or to regions, as with the Opportunity Areas (DfE 2018) or Excellence in Cities policies in England, and it differs from a policy based on extra funding for schools themselves that is not tied to their intake, such as the original Specialist Schools and then the Academies programme (Gorard 2005).

There have been suggestions that the UK government, as the contribution of the Lib Dems in the coalition diminished, became less interested in the attainment gap, than they were when Pupil Premium started, and more interested in structural reforms like academisation (Whittaker 2021). If so, the government is being supported in this by other commentators suggesting that Pupil Premium has not worked, or that the funding could be better used otherwise. The policy is worth pursuing for a few more cohorts at least, and then evaluating robustly again. The age cohort that arrived at school in 2011 is still in school at time of writing. In policy terms, the Pupil Premium is quite young.

#### *Use of Pupil Premium funding or similar*

In terms of how funding is actually used in schools, over and above its incentive value, more work is needed to ensure that any interventions, programmes and practices used in schools are evidence-led. It is not likely that the mere existence of Pupil Premium funding would produce a reduction in the poverty attainment gap. Of course, the funding might be partly effective simply by drawing attention to the gap. But it will more likely work by providing money for resources to address the gap. Schools are increasingly encouraged to ensure that the programmes they invest this extra funding in are “backed by

evidence” (Schools Week 2021). However, there are still disputes over what “backed by evidence” means (presumably robust, replicated, transferrable evidence), and it still seems that many schools are instead looking for “evidence” to deploy in support what they want to do anyway (Gorard et al. 2020). There is limited but growing evidence of what has worked to improve the average attainment of poorer children. We need more of this robust evidence, and much more and better research on how to get that evidence into use most appropriately.

Why has the same clear improvement as seen in the segregation gap not also occurred with the attainment gap for older students, and as it did at KS1 in primary schools? It may be that the improvements in primary schools need longer to feed through the system before manifesting themselves in improved KS4 outcomes. However, the changes to the nature of KS4 assessment from 2014 onwards have not helped. This is not to say that the changes were wrong. But it is not clear that their clash with the Pupil Premium objectives was ever considered and accepted as a necessary cost by policy-makers.

However, another possible explanation is the relative lack of evidence on how to use Pupil Premium funding at KS4 level. At the time of writing, the EEF had reported complete evaluations of 120 distinct interventions, of which 17 were listed as “promising”. Promising here means that the evaluation succeeded, the results are deemed trustworthy, and that the intervention was reported as having benefits for pupil attainment. This 10% to 15% of positive outcomes for otherwise plausible (i.e. with equipoise) approaches is to be expected. Of the 120 approaches trialled, 80 (67%) were for early years or primary phases. Of the 17 promising approaches, 12 were for the primary phase (71%). This means that schools and networks seeking evidence on how best they might use their Pupil Premium funding have a much greater number of promising interventions if they teach younger children. This may be part of the reason why the attainment gap has reduced more, in the Pupil Premium era, at KS1 and even at KS2 than KS4.

In any case, as discussed in Chapter 8, not all schools and teachers in England appreciate the advantages of using robust evidence when planning programmes and conducting interventions in schools. Many might see the need to explain their use of Pupil Premium funding as a burden rather than as a condition of funding. And they might only go through the motions of using it for purportedly evidence-led improvement to satisfy Ofsted or the DfE. Of course, it is not clear that Ofsted or the DfE understand properly what it means for actions to be evidence-led (as suggested by some of their own research programmes and claims). And, evidence on how best to promote attainment should only be one of the factors used in such decisions, along with context and intentions (Gemnick et al. 2021). But evidence should be used. Unfortunately, as we pointed out in Gorard et al. (2020), schools might be putting links to research evidence on their websites to support actions that they had already decided on, rather than as a genuine part of the decision itself. This kind of cherry-picking and box ticking seems to be common, and is perhaps even worse in policy-making (Gorard 2020).

### **How can we encourage the use of evidence-led approaches?**

Over the last 30 years, governments and funders worldwide have sought to improve the quality of evidence produced by publicly-funded research. Understanding of effective interventions to inform education policy/practice has improved since the creation of the US Institute of Education Sciences, the Education Endowment Foundation (EEF) and other initiatives. There has also been considerable progress in methods of summarising and synthesising research results, with the work of the Campbell Collaboration, What Works Centres and others. Evidence of what worked, or not, is increasingly available for the first time.

As this body of more robust evidence on how to improve the attainment gap grows, a linked problem is becoming clearer. There is, as yet, no equivalently robust research on how to get high quality substantive evidence to be taken up and used appropriately by the majority of schools and teachers. The small amount of pioneering work that has been done in education so far has shown no impact (Gorard 2020, Gorard et al. 2020). It is crucial that attempts to narrow the poverty attainment gap, and deploy

the funds available worldwide, are actually based on the best available evidence. How are users like school leads and local politicians supposed to judge?

There are several published protocols for judging the robustness of a research finding. Some are intended for the users of research, like That's a Claim (<https://thatsaclaim.org/>). Some, like the Weight of Evidence framework from the EPPI Centre, are more for researchers who want to judge a body of work consisting of many individual studies (Gough 2007). Some, like the security ratings used by the EEF, are intended for use when judging the quality of an individual study based on a randomised control trial or similar (<https://educationendowmentfoundation.org.uk/help/projects/the-eeef-security-rating/>). Others, like the Maryland Scientific Methods Scale, are like that of the EEF in being concerned with what works, and like the EPPI Centre version in being used to judge systematic reviews of evidence (Madaleno and Waights n.d.). In Chapter 3 of this book we presented a simple sieve for use when judging the trustworthiness of single studies, as explained in more detail in Gorard (2021).

All of these processes have a similar purpose, and there is considerable overlap between them, as would be expected. The idea is to provide tools, checklists or guidelines to help others to judge whether a study or a body of evidence is trustworthy, in the sense that its findings could be used ethically, and with a reasonable expectation that this evidence-led use would be effective (a best “bet”).

Applying such a process for judging the quality of research, it becomes apparent that much publicly available research is very weak (for examples see Chapter 3 onwards). It is often inadequately described, using poor designs (or no design at all), mis-represented, overly technical, and/or flawed, including the use of inappropriate techniques like significance testing with non-randomised cases. Even worse, when bodies of research are synthesised to try and present an overall result, this is usually done with no consideration of the quality of the research being synthesised. This is clear in the meta-analyses of meta-analyses in the EEF Teaching and Learning Toolkit, in Hattie's (2008) “Visible Learning”, and many similar attempts. The problems with these are described in more detail in Gorard (2018). Averaging “effect” sizes from very different studies - perhaps involving different outcomes, different age students, or different sampling processes, and ignoring issues such as the amount of missing data – is very dangerous and can yield very misleading summaries.

As an example, Hattie and Timperley (2007) reported a hyper-analysis of enhanced feedback based on synthesising the results of 74 meta-analyses that totalled 4,157 individual studies (reporting 5,755 different effect sizes). They did so without re-examining the original studies themselves or appraising their quality or appropriateness to be combined in this way. The results of Hattie and Timperley (2007) were heavily influenced by the prior meta-analysis by Kluger and DeNisi (1996) because they said it was ‘the most systematic’ and ‘included studies that had at least a control group, measured performance, and included at least 10 participants’. This sounds good until the implications are realised. Other meta-analyses among the 74 therefore included studies with no control group, fewer than 10 cases, and/or did not measure performance!

When we conducted a review of enhanced feedback, we used the sieve approach described in Chapter 3. We ignored all of the single studies rated 0, and focused only on studies with primary age mainstream students. We found 19 studies of at least 1 🟡, and the majority of these (14) were only 1 🟡. Of these 14 tiny weak studies, 11 (or 79%) reported positive findings (Gorard et al. 2017). If there are included in a synthesis without a quality rating (as is usual) this poor research would skew the overall result towards positive. Of the other five studies, three were considered 2 🟡, of which only one was positive and two were negative about the use of enhanced feedback. The final two were considered 3 or 4 🟢 (the strongest), of which one was positive and one negative. When quality is taken into account the picture for enhanced feedback is far less rosy than had been painted by prior reviews. Our point here is not about feedback itself, which is only used as an example, but about how untrustworthy most summaries of existing research can be. This has to change.

## **What should money not be spend on?**

We have reviewed a considerable body of evidence on how to improve school outcomes (e.g. See et al. 2020). There is increasing evidence on school, family and classroom interventions/resources that can help reduce the attainment gap (Sharples et al. 2011, Gorard et al. 2017, See and Gorard 2020). There is an even larger body of evidence on plausible-sounding ideas that do not seem to work. And there remains a large number of plausible approaches whose impact is unclear because they have not been properly tested. We will only mention a few of these ideas here, concerning things that did not work or have not been tested. Given scarce and limited educational resources, these are approaches that money should not be spent on at present, while there are more evidence-led approaches available.

Parental engagement, for example, has been shown to be closely associated with children's learning and children's attainment (e.g. EEF 2021). Evidence from developing countries suggests that financial incentives given to parents can improve children's school attendance (Akresh et al. 2016, Cardoso and Souza 2004). Some studies even suggest that cash incentives given directly to disadvantaged children, instead of parents, can improve their school attendance and attainment (Alam et al. 2016, Sharma 2010). But intervening to enhance parental engagement in children's learning and school life has not yet been demonstrated to be a clear causal factor in equalising the academic outcomes of disadvantaged children. Therefore, funding programmes to support parental involvement for disadvantaged children are not a promising use of money if the main objective is improvement in student attainment (See et al. 2015a, 2015b, 2021a). This may seem counterintuitive, but most research on parental engagement fails to find evidence of effects, mainly because those parents who are most in need of support are the least likely to take part, or most likely to drop out.

The situation is similar for many ideas, such as the concept of family "cultural capital", which is often used by academics and government as an explanation for social inequalities in school and later-life outcomes. There is little evidence that engagement in highbrow cultural activities by itself, or enhancing so-called cultural capital, makes a positive difference in attainment outcomes (Stopforth and Gayle 2022). This is because a large majority of relevant studies are only correlational in design (or worse), so are unable to control for unobserved or confounding factors.

There are many examples like these, where an association is too often mistaken for causation. Positive attitudes to education, high aspirations, good "behaviour", and a positive self-concept are strongly associated with educational attainment at school. Correlational studies, however, do not and cannot indicate the direction of any causation. For example, is it high aspirations and a positive attitude that lead to better performance at school, or is it that students who perform well develop high aspirations and a positive attitude? If it is the former, then raising aspirations and attitude should improve attainment. Structured reviews of evidence suggest that this either does not work or has not been tested properly (e.g. Gorard 2012).

Similarly, engagement in uniformed activities like Scouting, and in youth social action like litter-clearing, shows some promise in assisting wider school outcomes such as self-esteem and team-work. But they are less promising for attainment outcomes (Siddiqui et al. 2019). The same could be said for extra-curricular activities in general (Kravchenko and Nygård 2022).

As a final example, just because successful schools invest heavily in IT or Edtech does not necessarily mean that technology is the cause of the success (See et al. 2021a, 2021b). The overall evidence on the use of technology in schools is not promising, despite what advocates might claim (Gorard et al. 2016, Evans and Acosta 2020). There are still many unknowns and many caveats with regards to the use of educational technology.

In fact, many popular approaches reported by others as most promising in improving the attainment of disadvantaged pupils (including current favourites like metacognition) are not nearly as clear once the quality of the evidence is taken into account. The evidence and the findings presented in this book do not always support popular beliefs. This is because the evidence we have collected has been carefully

calibrated and filtered in terms of its trustworthiness, a process which is largely missing in most previous reviews.

## Final word

The good news is that school attendance has increased, and the attainment gap has decreased in the areas of India and Pakistan in our study. National segregation and the long-term poverty attainment gap in England have declined over time and across regions, mostly in a way that can be associated specifically with the post-2011 Pupil Premium era. This has happened in all regions, and for students recognised as having a special educational need or disability, and for major ethnic groups. If the improvements noted can be (at least partly) attributed to the Pupil Premium funding policy then there are also implications for school funding in other countries faced with similar issues. Given a choice between providing incentives for teachers, families or students, general funding for schools, and the kind of targeted resource represented by Pupil Premium funding, education systems should prefer the latter on the basis of the overall evidence. The funding must be tied to school intakes (following students if they move) and not to schools or areas, and its use must be exclusively for the most promising evidence-led resources and interventions. This use would have to be audited better than it is now, in order to make the payments conditional on coherent evidence use.

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