THE CENTRE FOR MARKET AND PUBLIC ORGANISATION

How should we treat under-performing schools? A regression discontinuity analysis of school inspections in England

Rebecca Allen and Simon Burgess

March 2012

Working Paper No. 12/287

Centre for Market and Public Organisation
Bristol Institute of Public Affairs
University of Bristol
2 Priory Road
Bristol BS8 1TX
http://www.bristol.ac.uk/cmpo/

Tel: (0117) 33 10952
Fax: (0117) 33 10705
E-mail: cmpo-admin@bristol.ac.uk

ISSN 1473-625X

CMPO Working Paper Series No. 12/287

How should we treat under-performing schools? A regression discontinuity analysis of school inspections in England

Rebecca Allen[1] and Simon Burgess[2]

[1] *Institute of Education, University of London*
[2] *CMPO, University of Bristol*

March 2012

Abstract
School inspections are an important part of the accountability framework for education in England. In this paper we use a panel of schools to evaluate the effect of a school failing its inspection. We collect a decade's worth of data on how schools are judged across a very large range of sub-criteria, alongside an overall judgement of effectiveness. We use this data within a fuzzy regression discontinuity design to model the impact of 'just' failing the inspection, relative to the impact of 'just' passing. This analysis is implemented using a time-series of school performance and pupil background data. Our results suggest that schools only just failing do see an improvement in scores over the following two to three years. The effect size is moderate to large at around 10% of a pupil-level standard deviation in test scores. We also show that this improvement occurs in core compulsory subjects, suggesting that this is not all the result of course entry gaming on the part of schools. There is little positive impact on lower ability pupils, with equally large effects for those in the middle and top end of the ability distribution.

Keywords: School inspection; school accountability; school attainment; regression discontinuity

JEL Classification: I20, I28

Electronic version: **www.bristol.ac.uk/cmpo/publications/papers/2012/wp287.pdf**

Acknowledgements
We are very grateful to Ofsted for retrieving policy documents and data from their archives and to the Department for Education for providing access to the National Pupil Database. Interpretation of our findings has been aided through a large number of conversations with policy officials at Ofsted, former school inspectors and headteachers. We are grateful for useful comments to seminar audiences at NIESR, the Institute of Education and University of Cambridge. Particular thanks to Mike Brewer, Jane Cooley Fruehwirth, Hamish Low and Iftikhar Hussain for reading drafts and discussing ideas.

Address for correspondence
CMPO, Bristol Institute of Public Affairs
University of Bristol
2 Priory Road
Bristol BS8 1TX
Simon.burgess@bristol.ac.uk
www.bristol.ac.uk/cmpo/

1. Introduction

What is the best policy for dealing with under-performing schools? Most education systems have a mechanism for identifying such schools, typically an inspection system where an external body is responsible for monitoring and reporting on educational standards. But what should then be done with schools which are highlighted as failing their pupils? There are important trade-offs to be considered: rapid intervention may be an over-reaction to a freak year of poor performance, but a more measured approach may leave many cohorts of students to under-achieve. Similarly, it is hard to impose penalties alongside greater support, but it is unclear which is more likely to turn a school around. Designing a process of intervention is important even in a market-based system where we might hope that new pupils shun failing schools because we must protect the interests of children whose parents do not exercise choice or who remain 'loyal' to a local school.

In this paper, we evaluate the effect of making and publicly announcing an inspection judgement of 'unsatisfactory' on poorly performing secondary schools in England. Such schools are identified by a national school inspection body, Ofsted (the Office for Standards in Education, Children's Services and Skills).[1] On the basis of its on-site inspections, Ofsted's judgement of 'unsatisfactory' triggers a set of policy actions, described below. We evaluate the impact of this intervention on overall school performance in high-stakes national tests at age 16 over different time horizons, on test scores in maths and English specifically, and test for differential impact on students of different ability. We also analyse the effect on demand for places in the school. We implement a regression discontinuity design (RDD) in a panel data context, comparing the time series of performance statistics for schools that are designated as just failing with those just passing. By dealing with the obvious endogeneity of failure, this approach estimates the causal impact of that policy on school performance. The intuition behind an RDD in this context is that schools around the failure threshold are very similar, except for as-good-as-random measurement of quality by inspectors that causes some schools to just pass their inspection while others just fail. The converse of the strong internal validity of the RDD approach is weaker external validity. Schools which fail their inspection by a considerable margin may react very differently to their failure, and we make no claims about the applicability of our results to severely failing schools. We return to this issue in the Conclusion.

In principle, the effects of failing an Ofsted inspection could go either way: it could be a catalyst for improvement or a route to decline. It could lead to improved performance if it induces the school to work harder to pass a subsequent inspection: more focussed leadership and more effective teaching

[1] See http://www.ofsted.gov.uk/about-us , accessed 10/10/11.

could raise test scores. On the other hand, if students and teachers leave the school, the failure may trigger a spiral of decline where falling rolls lead to low morale, financial difficulties and even lower test scores. So it is a meaningful question to ask whether test scores improve or worsen following the treatment.

This paper contributes to a large international literature on the effect of school inspections and sanctions. In the US a large literature has focussed on the accountability mechanisms built into the No Child Left Behind Act.[2] Our RDD approach and our findings bear similarities to Fruehwirth and Traczynski (2011) and to Ahn and Vigdor (2009), who both find that schools in North Carolina facing sanctions under No Child Left Behind (NCLB) improve performance. The former confirms Neal and Schanzenbach's (2010) finding that schools facing these forms of accountability tend to focus on students at the threshold of performance measures at the expense of the lowest performing pupils.

In the UK, there are fewer studies of the dual accountability systems of publicly published performance tables[3] and Ofsted inspections. Rosenthal (2004) studies the impact of Ofsted visits and has a negative conclusion: there is no gain after the visit and there is a fall in performance in the year of the visit. Using a more sophisticated econometric approach, Hussain (2012) identifies the very short-term impact of failing an inspection in primary schools. He compares the performance of schools failed early in the academic year with those failed later in the academic year, specifically after pupil exams. This identification strategy compares like with like, and isolates the effect of having some 8 months to respond to the failure. Our approach differs by focussing on the high-stakes exams at the end of compulsory school, by using a panel data run of 10 years to estimate longer-term effects, and by adopting a different identification strategy that allows us to leverage in more data.

We find that schools failing their Ofsted inspections improve their subsequent performance relative the pre-visit year. The magnitudes are quantitatively very significant: around 10% of a (student-level) standard deviation or one grade in between one and two of their best eight exam subjects. The main impact arises two years after the visit in this data – not unreasonable given that the exam scores we use derive from two-year courses. The typical time pattern in our results is little effect in the first post-visit year, increasing considerably the following year, then remaining flat or slightly increasing in the third post-visit year.

---

[2] This includes Reback (2008), Jacob (2005), Neal and Schanzenbach (2010), Rockoff and Turner (2010), Figlio and Rouse (2006), and Chakrabarti (2010), Dee and Jacob (2011), Krieg (2008); Ladd and Lauen (2010], and Ahn and Vigdor (2009).
[3] For example, Burgess et al (2010) show that the publication of league tables does have an impact on school performance, and Allen and Burgess (2011) show that league tables are useful to parents in choosing schools.

In the next section we set out the policies in England regarding school inspections and describe the nature of the treatment received by schools judged to be failing their pupils. In section 3 we describe our data and our identification strategy. The results are in section 4. In the final section we conclude with some comments on the role of these policies in a suite of responses to poorly performing schools.

2. Policy Background

Most countries in the world operate a inspection system where an external body is responsible for monitoring and reporting on educational standards within schools. The nature and purpose of the inspection systems varies considerably. Coleman (1998) describes a continuum with one extreme as the objectives-based approach which has the features of being summative, external and formal, focusing on simply judging whether externally pre-determined objectives for education are being achieved. These types of high-stakes inspections play an important part in an accountability framework by giving parents and officials information about school quality that they can act on, should they wish to. At the other end of a continuum of inspections is a process-based approach which is much more formative, internal and informal and looks to take value from the process of inspection as it advises, supports and helps to improve the education provider. The English schools inspectorate is firmly at the summative, high stakes end of this spectrum.

2.1 The English school inspection system

The Office for Standards in Education, Children's Services and Skills, known as Ofsted, was created in the 1992 Education Act as part of a new era of parental choice and accountability. It is a national body that provides regular independent inspection of schools with public reporting to both parents and Parliament, and the provision of advice to ministers. The current intended role of Ofsted is "*to provide an independent external evaluation of a school's effectiveness and a diagnosis of what the school should do to improve, based on a range of evidence including that of first-hand observation*" (Ofsted, 2011a, page 4). The Inspectorate has focused on the need to make schools accountable for their performance and thus has placed great emphasis on the use of external examination results along with 'snapshot' external inspections to judge schools (Learmonth, 2000). The criteria on which schools are evaluated are both objective, such as exam results, and subjective, such as the inspectors' view of teaching quality observed during inspections. While the model is formal and accountability focused, giving both judgments on individual schools and the system as a whole, it does have elements of support and improvement through mechanisms such as school action plans.

The legal requirement is for schools to be inspected on a five year cycle. Since 2005, the frequency of inspections is proportionate to perceived need such that schools judged satisfactory will be on a three year cycle and schools judged as unsatisfactory will be visited more frequently and without notice. The period of notice schools receive before an inspection has shrunk over time from over two months' notice to the current period of between zero and two working days, with no notice possible where there are concerns relating to pupils' welfare, safeguarding, where the school's academic performance has shown rapid decline or where concern is raised by parents.

School inspections are currently conducted by Her Majesty's Inspectors (HMI), who are employed by Ofsted, and additional inspectors, who are employed full-time, freelance or otherwise commercially contracted by Ofsted's inspection service providers (CfBT, Tribal and Serco). The intensity of the visits has fallen considerably over time, with full scale week-long visits by a large team of inspectors prior to the 2005 inspection reforms. Since September 2005 visits are shorter in duration (around two days) and reliant on fewer inspectors in the school. They are more sharply focused on the school's own self-evaluation of their strengths and weaknesses and particularly focus on management, including how well the school is managed, what processes are in place to ensure standards of teaching and learning improve, and how well the management team understand their own strengths and weaknesses.

2.2 The inspection process and the policy treatment

Before a visit, inspectors draw on attainment data, school performance indicators and reports from previous inspections to decide how to plan their inspection. During the visit, inspectors usually talk with pupils, governors, management and staff to gather specific views on provision, observe a large number of lessons, 'track' individual pupils to monitor provision for specific groups and scrutinise school records and documentation (Ofsted, 2011a). Parents and staff are always invited to give their views on the school in a short questionnaire. Inspectors considering a judgement of 'unsatisfactory' will typically discuss the case with another inspector on the phone to utilise a broader range of experience and will have the findings of their inspection subjected to moderation by the Chief Inspector.

Inspectors base their overall view of a school on a series of sub-judgements about different aspects of the school. For example, during 2009 judgements were made about pupil outcomes (7 sub-criteria), quality of provision (3 sub-criteria), leadership and management (8 sub-criteria), and early years, sixth form and boarding provision where relevant (Ofsted, 2011a). Under the post-2005 framework a school immediately receives one of four overall judgements: outstanding, good, satisfactory or unsatisfactory/inadequate. The impact of this last judgment is the subject of this paper. The overall judgement first became very prominent in the inspection report in the academic year 2002/3, which by law must be delivered to parents of children at the school.

Those schools judged to be unsatisfactory are deemed to have 'failed' their Ofsted inspection. These schools are currently split into two categories of schools causing concern: schools given 'Notice to Improve' and schools placed in 'Special Measures'. Notice to Improve means that "*the school requires significant improvement because either: it is failing to provide an acceptable standard of education, but is demonstrating the capacity to improve; or it is not failing to provide an acceptable standard of education but is performing significantly less well in all the circumstances reasonably be expected to perform"* (Ofsted, 2011a, page 12). Special Measures is a more serious judgement against a school, meaning that "*the school is failing to give its pupils an acceptable standard of education and the persons responsible for leading, managing or governing the school are not demonstrating the capacity to secure the necessary improvement in the school*" (Ofsted, 2011a, page 12).[4]

While our sample includes all inspection failures, our use of the RD design means that we use for estimation only the marginal fails. These are therefore the schools given Notice to Improve, rather than schools put in Special Measures, and so it is this policy treatment that is the focus of our paper. There are two components: the public announcement of the judgement, and the internal pressure on schools. The 'naming and shaming' of failing schools is a significant part of the process, and is typically widely reported in the local media. This stigma of failure is likely to be the most significant factor for many Headteachers and school governors. Otherwise, the treatment is neither strongly punitive nor strongly supportive. These schools are subject to no operating restrictions and will simply receive a monitoring inspection between six and eight months after the publication of their inspection report.[5] They will also be fully inspected around a year after their Notice to Improve was served (this will be generally carried out by a different inspector). The headteacher, chair of the governing body and a representative from the local authority or proprietor will have been invited to attend a school improvement seminar, but there is no requirement for them to attend. The school

---

[4] The special measures judgement was introduced as a measure under Section 14 of the Schools Inspection (1996) Act and has more serious consequences than a notice to improve, such as restrictions on the employment of newly qualified teachers. In addition to the revision of action plans outlined above, schools under special measures will receive a number monitoring inspections over two years. Unlike with notice to improve, there is continuity of inspector for schools in special measures. It is possible for inspectors to judge that sufficient progress has been made at any of these monitoring inspections, but if special measures have not been removed after two years a second full inspection is held (Ofsted, 2011b). Where a school is still judged to be inadequate a year after the first inspection, the Department for Education requires the local authority to examine carefully the options available to it, including replacement of the governing body by an Interim Executive Board, dismissal of senior managers and teachers and even full closure of the school.

[5] Some schools judged to be satisfactory will also receive a monitoring visit.

does not need to prepare an action plan, but is expected to amend their existing school plans in light of the judgement and submit this to Ofsted within 10 working days (Ofsted, 2011c). In summary, the policy treatment here is the stigma of being named as a failing school, plus the instruction to improve the performance of the school. The fact that the judgement is Notice to Improve rather than Special Measures implies that the inspectors believe that the school has the leadership to be able to affect these changes, which is clearly critical to interpretation of our subsequent findings.

There are a number of possible responses by a school to receiving an unsatisfactory Ofsted rating. Schools may improve their overall exam results because teachers work harder, or because individual teachers focus their effort more directly towards exam preparation. The school itself may decide to develop a more targeted approach of focusing on students and classes who have the greatest capacity to improve or reach some threshold. Or in the longer term it can switch the mix of subjects offered to students towards a set where examination success is more likely. In this paper we are able to examine some possible school-based strategies but, absent teacher performance data, we cannot consider teacher responses.

3. Data and identification

This paper uses data from two sources. The National Pupil Database (NPD) provides information on pupil test scores and other pupil characteristics from 2002 to 2011. The Ofsted database of school inspection judgements gives information on school visits from the academic year 2002/3 onwards. These are combined to produce a ten-year panel of secondary schools, which we use to estimate the impact failing an Ofsted inspection.

3.1 Ofsted Data

We use the Ofsted archives and also data publicly available on their website to construct a database of school inspections for each year from 2002/3 to 2008/9 (more recent years are available but lack sufficient outcomes data post-inspection). The database records judgements on a large range of sub-criteria such as value for money and quality of leadership – 19 in 2002/3 and as many as 65 in 2007/8 – for every school inspection visit over this period. The sub-criteria are almost always ranked on a scale from 1 (outstanding) through 2 (good), 3 (satisfactory), to 4 (unsatisfactory), although there are some binary indicators of whether standards are met. We also have an overall ranking for the school on a scale of 1 to 4 (unsatisfactory). We exclude the second inspection visit where two take place in consecutive years for a school. Table 1 summarises the Ofsted inspection data by year for secondary schools included in our analysis dataset.

3.2 National Pupil Database (NPD)

We match the Ofsted database to school-level information aggregated from the National Pupil Database (NPD). NPD is an administrative database of pupil demographic and test score information, available from 2001/2 onwards. For the main analysis in the paper on exam performance, we use data on each school's year 11 (age 15/16) cohort, which takes the high-stakes GCSE exams (described below). For the analysis of the impact on school admissions, we use data from the intake year, year 7 (age 11/2). Throughout we consider state-funded schools, which educate 93% of all students in England.

School performance is measured using the high-stakes GCSE (General Certificate of Secondary Education) exams taken at the end of compulsory schooling. These matter both for the students and the schools. We use the school average across all pupils in their best 8 subjects at GCSE (students typically take 7-12 subjects). This broad measure – 'capped GCSE' – is standardised across all pupils nationally as a z-score before being averaged to school level, so the metric is a pupil-level standard deviation. We also report outcomes for the proportion of pupils achieving five or more 'good' GCSEs (at grades A*-C) and for average school grades in English and maths measured on a scale of 0 (grade U) to 8 (grade A*). The threshold measure – '%5AC GCSE' – is reported in school league tables throughout the period of analysis.

In the analysis of the year 11 cohort we use key socio-demographic statistics to control for the changing characteristics of the school cohorts. These are: the proportion of pupils eligible for free school meals (FSM), an indicator of level of pupil poverty in the school (see Hobbs and Vignoles, 2010, on this measure); the proportion of white British ethnicity pupils in the cohort; the proportion of pupils who are female; the proportion speaking English as an additional language; the average level of deprivation for the neighbourhoods the pupils live in, measured using the Index of Deprivation Affecting Children Index (IDACI);[6] and the average pupil prior attainment at Key Stage 2 (end of primary school) in maths, English and science.

The size and composition of the year 7 cohort are measured using: the number of pupils in year 7; the proportion of year 7 pupils eligible for FSM; and the average Key Stage two test result (normalised across pupils as a z-score) for the year 7 cohort of the school. This is a measure of prior attainment of cohort since the tests are sat at the end of primary school at age 10.

Summary statistics for all these variables are in Data Appendix Table 1.

---

[6] The Income Deprivation Affecting Children Index (IDACI) comprises the percentage of children under 16 living in families reliant on various means tested benefits (see http://www.communities.gov.uk/documents/communities/pdf/733520.pdf).

3.3 Defining the running variable and the bandwidths

Typically in RD studies there is an obvious continuous running variable available that assigns observations to treatment groups. A classic example is modelling college enrolment as a function of financial aid (Van Der Klaauw, 2002), using a continuous test score as a measure of ability, and a discontinuity in the test score as the generator of the scholarship. Another example is the use of prior vote share to model underlying popularity in elections (Lee, 2008), plus a discontinuity in that vote share to capture the effect of incumbency.

In this paper, we do not have such a variable directly. Ofsted failure is determined by a large number of sub-criteria and we construct a continuous running variable from these. This problem has often occurred in the RDD literature (e.g. Bacolod et al, 2012; Ahn and Vigdor, 2009) and bears some similarities to US accountability data used by Fruehwirth and Traczynski (2011) and others because they exploit the set of 10 sub-criteria in No Child Left Behind, producing a multi-dimensional scale of closeness to the failure threshold.

Not all schools that failed their Ofsted inspection did so equally badly: schools that just fail their inspection will only fail a limited number of sub-criteria, compared to schools that comprehensively fail their inspection. If a school fails on many sub-criteria then its overall judgment clearly will be 'unsatisfactory', whereas other schools may fail on only one criterion and may be judged satisfactory overall.[7] We propose to use a single statistic of the set of underlying sub-criteria as the running variable. We face a problem because there is no set rule for turning between 19 and 65 sub-scale measures into an overall measure of the judgement for the school, the sub-criteria change from one year to the next and inspectors are able to exercise an element of discretion in their decision as to whether a school fails their Ofsted inspection.[8]

There are two main features that we require from the continuous running variable that we create from the sub-criteria. First, it must provide sufficient differentiation among the fails and the passes to allow us to define different sample bandwidths: we want to be able to estimate the models on just the best failers and the worst passers. Second, it has to provide a good instrument for the fail variable. There is obviously an inherent trade-off here: a very good instrument could be close to a

[7] The danger is that the sub-criteria are determined after the overall judgement, and so do not provide valid variation. This is essentially a variation of the McCrary (2008) critique, which we consider below.

[8] The choice of inspector is potentially important, particularly for fine judgements close to the satisfactory or unsatisfactory boundary. Ofsted divides all of England into three regions, and operates a 'taxi rank' system within each, so whichever inspector is currently free is assigned to the next school to be inspected. This suggests that the allocation of inspectors to schools is as good as random.

binary variable (and so no differentiation), whereas a lot of differentiation among the failers and the passers represents irrelevant variation for the instrument.

One obvious route is to run a very disaggregated model: to model the probability of failing as a function of all four scores on each individual sub-criterion, allowing for variation year-by-year. This produces a very good predictor for an overall fail judgment, but at the cost of almost no differentiation among fails or passes bar the absolute extremes.

We chose to focus on both 'fail' and 'satisfactory' grades on the sub-criteria. Because the number of sub-criteria has changed throughout the period of analysis (and differs for those without any post-age 16 provision), we normalise by using the proportion of sub-criteria a school scored 'fail' on and the proportion of sub-criteria that it scored 'satisfactory' on. We then estimate the propensity for being judged a fail overall as a function of these two proportions. We run this regression separately year by year, because the actual sub-criteria that form the overall judgment change slightly each year. A typical result of this (for 2008) analysis weights the proportion of fails at 2.23 and the proportion of satisfactories at 0.07, and achieves an R^2 of 73%. We use the fitted value from this model as our running variable: it provides a good instrument and partitions the sample reasonably well into those that fail and those that do not. We scale the data so that zero is approximately in the centre of the fuzzy band where the proportion of fails is neither zero nor one. We refer to this running variable as the school rating.

One of the challenges to a regression discontinuity design is the potential manipulation of the running variable by the unit receiving the treatment (McCrary, 2008). In our context, it is not possible for schools to do this – they are working to influence the views of the inspectors but have no direct impact on the judgements. The possible channel for manipulation here is that some sub-criteria are pivotal and the judgements on other sub-criteria are simply recorded to bolster that overall view. We test this by checking whether any sub-criteria are pivotal. We show that there are no sub-criteria for which failure on that criterion always implies failure overall, and for which passing on that always implies passing overall. Whilst it is very hard to completely rule out manipulation, this suggests that it is not a first order issue.

Figure 1 shows a kernel estimate of the density of the school rating variable along with the proportion of schools failing for each percentile of this variable. This illustrates that only a small minority of schools receive a fail judgment, with the mass of the density function in the region of comfortable passes. There is also a very long upper tail to the distribution with some schools failing on most sub-criteria. The Figure also illustrates the fuzziness of the discontinuity: 10% of the observations lie in a band where the realised chance of failure is neither zero nor one.

This variable forms the basis for our choices of bandwidth. The complete sample is very unbalanced with only 521 (8.5%) out of 5124 observations given a fail judgment. Therefore in attempting to get close to the discontinuity and compare just-fails and just-passes, we have to discard almost all of the data. The choice as always is between bias and precision: a small sample, tight around the discontinuity, will include the most alike schools, but at the cost of imprecisely defined estimates. We focus on three main subsets: a broad sample with the best 300 failing schools and the worst 300 passing schools; a narrow sample with 200 each side and a very narrow sample with 100 each side. We also display results for the whole sample and other subsets. These samples are marked on Figure 1. An alternative would be to include all schools within a fixed distance from the discontinuity rather than a fixed number of schools. However, given that the scale of the running variable is arbitrary we choose to proceed with the fixed number of schools either side. This does mean that the worst included fail and the best included pass will not in general be equidistant from zero.

3.4 Balancing tests on the rating variable

To (partially) meet the continuity assumptions of the RDD, we require schools just below and just above the threshold for passing to be similar in observables, particularly in the pre-test data. These balancing test results are Table 2. For each control variable we show the mean value for those either side of the discontinuity in the narrow sample, and the t-test for their equality. It is clear that the mean values of all the differences are the same either side of the threshold. The levels of the controls at (t-1) show more of a difference, but only the IDACI score is significantly different at 5%. This suggests that it is important to control for observable differences between schools and to utilise the panel to include fixed effects mopping up unobservable heterogeneity.

It is also important to note that we cannot reject that the prior performance trends are the same of the just-failing and the just-passing schools. The balancing test on the prior trend is particularly important. Evaluation of any school turn-around policy is potentially confounded with mean reversion. While we control for the trend in the schools' results for the three years before the Ofsted visit, the fact that the just-fails and just-passes in the narrow sample have the same prior trends is reassuring.

3.5 Identification and estimation

The major statistical issue we face is that the fail judgment may be correlated with unobservable school characteristics that in turn may be correlated with performance. We implement a number of solutions, all of which identify the effect of failing a school inspection on a school's future outcomes by using schools that passed their inspection as a control group. This is the only available choice of control group since all schools in England are regularly inspected. It is a different question – which we cannot address – to identify the impact of being inspected as opposed to never being inspected.

One of the important statistical issues in evaluating any policy for school recovery is dealing with mean reversion (Ashenfelter, 1978). Schools selected for a recovery intervention of some sort are necessarily performing poorly on some metric, and so there is a good chance that they would turn themselves around anyway even in the absence of an intervention. We address this in two ways. First, the comparison of schools which only just failed with those which only just passed the inspection via the RDD will mean that we are comparing schools which are very alike. Second, we include as a control the school-specific trend in attainment prior to the visit; specifically, performance in the year prior to the visit year minus performance two years before that.

We model school performance in a standard way as follows, for school *s*:

$$Y_s = \alpha + \gamma X_s + \lambda Z_s + \mu_s + \zeta_s \qquad (1)$$

Where *Y* is the school attainment score, *X* denotes mean characteristics of the school's pupils, *Z* are observable school characteristics, μ unobservable school characteristics, and ζ is a noise term.

If we then add the treatment effect to the model, $\beta.fail_s$, the standard problem is that $fail_s$ is very likely to be correlated with μ_s, biasing the estimate. The RD response is to focus on schools very close to the fail/pass discontinuity so that the estimation sample will contain only very alike schools, and to include a flexible function of the running variable to mop up the heterogeneity generated by the unobservable μ_s.

We can exploit the panel data available to us to deal with the heterogeneity in a different way. We look at the change in performance of a school, from before to after the inspection visit, and thereby difference out the role of μ_s. The use of school fixed effects, a rich set of time-varying controls and the school's prior performance trend will account for a great deal of the heterogeneity of performance between schools, obviating the need for the function of the running variable. Note that both the running variable and μ_s are time-invariant. The model we estimate is:

$$\Delta_\tau Y_s = \alpha + \beta fail_s + \gamma \Delta_\tau X_s + \pi . X_{st-1} + \lambda .(Y_{st-1} - Y_{st-3}) + \delta . inspyear_s + \varepsilon_s \qquad (2)$$

Where $\Delta_\tau Y \equiv Y_{t+\tau} - Y_{t-1}$, and we include the pre-visit levels of the student characteristics. We estimate this by OLS and by IV, described below, exploiting the fuzzy regression discontinuity design.

We estimate on a range of bandwidths, getting very tight to the discontinuity. We do not include the polynomial in the rating variable, for reasons explained above. We show below that conditional on the included controls and the school fixed effect (implicitly, through differencing), a polynomial in the running variable adds nothing to the explanation of school performance change, though its inclusion expands the standard errors on all the other variables. Nevertheless, for completeness we report the results of including it in our robustness tests.

a) Before-after OLS regressions

We first run a simple OLS difference-in-difference model, equation (2), comparing the change in performance before and after the inspection between those who just fail and those who just pass their Ofsted inspection. We do this for a variety of difference windows: $\Delta_\tau Y \equiv Y_{t+\tau} - Y_{t-1}$, for $\tau = 1, 2, 3, 4$. Our outcome variable ΔY_s is the change in GCSE results for school *s* over the window.[9] $fail_s$ is a binary indicator for whether the school received an unsatisfactory rating for their inspection, or not. We include dummy indicators for the inspection year, $inspyear_s$, to mop up varying rates of grade inflation in our outcome variables over the time period. We include variables for the pre-treatment level and change over period for a set of school observable composition measures, *x,* that directly affect exam outcome. These are: variables which measure mean prior attainment of the cohort in English, maths and science separately; the percentage of free school meals eligibility; average neighbourhood deprivation level; percentage female; percentage non-English speakers; and the percentage of the school population who are white British.

Clearly this analysis ignores the endogeneity of Ofsted failure. That said, our treatment has characteristics that do make it amenable to this before-after design since the treatment is clearly defined, takes place quickly, and the effect should be felt quickly before other covariates change (Marcantonio and Cook, 1994).

b) Fuzzy regression discontinuity design

We use a fuzzy RDD analysis to deal with the potential endogeneity of the fail variable. The RDD approach assumes subjects near the threshold are likely to be very similar and thus comparable. This 'threshold' randomisation identifies the causal impact of the treatment, provided Hahn et al.'s (2001) minimal continuity assumptions hold. The fuzziness arises from unknown inspector judgement conditional on the sub-criteria judgements, and is illustrated in Figure 1. In this sense our RD bears similarities to that implemented by Angrist and Lavy's (1999) class size study because we

---

[9] To be clear, this is the change over time in a school-average attainment level, not an average pupil progress measure.

believe the discontinuity is 'sharp' in principle, but the unknown rule and vagaries of implementation by different inspectors makes it slightly fuzzy.

Following standard practice, we estimate (2) using an IV approach. The first stage uses as additional instruments the threshold indicator of whether the running variable is positive and a flexible function of the running variable, with linear and quadratic terms on either side of the threshold (Angrist and Pischke, 2009). The assumptions required for this to be a valid method are that the likelihood of receiving the treatment does depend on the assignment variable; that there is a clear (if not necessarily sharp) discontinuity in the chance of the treatment; that the assignment variable is not manipulable by the units to place themselves just one side of the discontinuity; that there is greater continuity in other variables influencing the outcome, and in particular, that these other factors do not change discontinuously at the same threshold.

c) Difference-in-differences panel with regression discontinuity design

Finally we utilise the full panel to get a broader sense of the dynamics of the response to the fail judgement within one regression equation. Inclusion of school fixed effects allows us to absorb all unobservable differences between schools and we estimate the effect of failing an inspection one year out, two years out and so on. We estimate:

$$\Delta Y_{st} = \beta_0 + \beta_{\text{fail, K}}\, fail_s \text{*D}(\text{year} = inspyear_s + \text{K}) + \beta_{\text{post-visit, K}}\, \text{D}(\text{year} = inspyear_s + \text{K}) \qquad (3)$$

$$+ \beta_{\text{visit}}.\text{D}(\text{year} = inspyear_s) + x_{st} + yeardummies + \mu_s + \varepsilon_{st}$$

For K = 1, 2, 3, 4.

Implementing this panel data approach allows us to exploit all years of data for each school and therefore compares the impact of failure to a school's long-run average, rather than performance at time t-1. However, the specification we choose here actually imposes greater constraints on the data than the before-after approach. For example, we model school specific levels rather than trends in performance.

4. Results

We present the main results on school performance, using the three approaches just described, along with a set of robustness checks. We then explore how the results may be achieved by schools, attempting to isolate the extent to which they reflect a real improvement in teaching or 'gaming' by schools. The final set of results relates to the effect of Ofsted failure on demand for places in the school, as measured by the size and composition of the intake cohort in subsequent years.

4.1 Impact of Ofsted failure on school performance

a) Before-after OLS regressions

We start with a simple difference-in-difference analysis, treating fail status as if it were exogenous, before going on to utilise the discontinuity design. As we have a panel of up to ten observations per school, we can look at differences over shorter and longer time windows. This is useful because scope for schools to change their practices varies depending on the timescale.[10] It is difficult for schools to implement responses to failure that yield immediate (one year) gains as students will be half way through their GCSE courses, teachers will be allocated to classes and so on. Over a two-year window, covering the whole GCSE course, more can be changed. Beyond two years, it is a question of the extent to which changes implemented by the school 'stick' or whether the long-run environment of the school – such as its location – re-exerts its influence. Also, beyond five years after the visit the composition of pupils taking their GCSEs will start to reflect any changes in intake resulting from the failure. We take a pragmatic approach and present results for four different window lengths, with lack of data preventing longer windows.

We quantify these effects in the OLS difference-in-difference results in Table 3. The table presents four different difference windows: $\Delta_\tau Y \equiv Y_{t+\tau} - Y_{t-1}$, for $\tau = 1, 2, 3, 4$. Results are given for four specifications, starting with simply the fail variable and a set of dummies for the year of the visit to control for cohort effects. We add differences of the control variables[11] (the difference of the same order τ as the dependent variable) and then we add the levels of the same set of control variables dated at t-1 and the measure of the prior trend in school performance.

There are strong and consistent patterns in the results. In the top three rows, the effect is positive, quantitatively substantial, and consistent in size. In this table, all of the estimated effects are significant at the 1% level. We focus in more depth on the quantitative significance of the effect below, once we have addressed the endogeneity of the fail status, but note here that an impact of around 10% of a pupil SD is a very substantial effect.

The impact increases the further out the window goes, higher for $Y_{t+4} - Y_{t-1}$ than for $Y_{t+1} - Y_{t-1}$. This cannot be interpreted directly as the sample is different in each column – in the third row, 3966 inspection events have data to allow us to compute Y_{t+2} but only 2359 have sufficient 'after' periods

[10] Note that there is very little pupil movement out of failed schools, so any changes in the characteristics of the cohort taking the exams is exogenous to the Ofsted failure.

[11] These are, all averaged over pupils to school level: KS2 English, KS2 Maths, KS2 Science, the fraction of students eligible for Free School Meals, fraction of students female, fraction of students white British, fraction of students with English as an additional language, and neighbourhood poverty score.

to yield data for Y_{t+4}. The final row of the table addresses this by imposing a common sample. The pattern is in fact roughly replicated: a minor increase in t+1, a doubling of that effect at t+2, and then further smaller increases in t+3 and t+4.

In table 4 we begin to deal with the endogeneity by estimating the full model with levels, differences and prior trends, focussing on schools tightly around the fail/pass discontinuity. The top row simply reports the results from the full sample in the previous table (3966 observations in column 2); subsequent rows report for the broad sample (466), the narrow sample (314) and the very narrow sample (156). The patterns of results are preserved in the broad and narrow samples. The coefficients are sizeable and precisely defined. They follow the same pattern: doubling in the second year relative to the first, and thereafter rising more gently. Our preferred estimate is the narrow sample, focussing on the (t+2) outcome. Here the effect size is 0.103 SDs, with a standard error of 0.024. This means that at the least, we can very strongly rule out a negative impact of the treatment. In the very narrow sample, the same pattern across the rows is preserved again, but the coefficients are somewhat lower and the standard errors much higher because of the very small sample.

Looking across the two tables, we can see that adding control variables reduces the size of the estimated coefficient, but that narrowing the bandwidth has little effect on the coefficient estimates apart from the very narrow sample.

b) Fuzzy regression discontinuity design

We now address the endogeneity of the fail status variable. We use the running variable to instrument fail status and again estimate only on small samples either side of the discontinuity to focus on very similar schools.

We introduce the results with Figure 2, showing the change in outcome for different windows of time against the school rating variable using a non-parametric smooth (lowess). All four panels show little overall trend of $Y_{t+k} - Y_{t-1}$ against the rating variable. This is to be expected – the vertical axis is measuring subsequent change, while the rating variable is a point in time measure. The main point of interest is that there is a jump in school performance just to the right of the discontinuity; that is, for schools who just failed their Ofsted inspection relative to those who just passed. This is strongly apparent in panels 1, 2 and 4, less so in panel 3. We do not discuss issues of statistical significance at this point, as these plots are unconditional and the balancing tests suggest we need to control for school factors.

These are included in the main IV results in Table 5. The top row presents a basic specification, equivalent to that in the top row of Table 4, and the second row adds the differences and levels, equivalent to the third row of Table 4. Focussing on the latter, the coefficients are very similar at around 0.104 to 0.112 for $\Delta_2 Y$ and for $\Delta_3 Y$, and about half that for $\Delta_1 Y$. The rest of the table reports the outcome of the IV procedure for the different bandwidths. At the narrow bandwidth, the effect sizes again follow the same temporal pattern, are about the same magnitude and are precisely estimated for all these time windows. None of the coefficients are significant in the very narrow bandwidth, but most of the sizes are comparable to the bigger samples.

We can illustrate the stability of the coefficients across different bandwidths graphically. Figure 3 shows the results of estimating the models of Table 4 (panel A) and Table 5 (panel B) at a range of sample sizes from the full sample down to just 20 schools either side of the discontinuity. There is very little variation in the point estimates throughout the range, although obviously the precision decreases as we cut the sample size more and more. Although obviously not statistically significantly different, it is interesting that the effect size is somewhat greater when we restrict to around 200 failing schools and exclude the most drastic 250 fails. The lack of any very large differences across the sample ranges derives from the fact that we are looking at changes in outcomes rather than levels (so implicitly differencing out a lot of fixed school factors) and because we also have good time-varying control variables.

The school rating variable is a reasonably strong instrument in all of the specifications, bar the very narrow sample. Some diagnostics are presented in Appendix Table 2. We present the full set of coefficient estimates for one specification (narrow sample, *Y(t+2) – Y(t-1)*) in Appendix 3. It is noteworthy that very few control variables are significant, suggesting that the schools in the narrow sample are very similar.

c) Difference-in-differences panel with regression discontinuity design

Finally in this section we utilise the full run of data for each school in a panel regression. This means that we are comparing any given year (say, visit year + 2) to all of the years of data we have for that school, not just to (visit year – 1). We include school fixed effects, all the control variables used above, year dummies, and a dummy for the year of the visit. We introduce consecutively a dummy for the year after the visit (column 1), two years after the visit (column 2) and so on, and each of these interacted with fail status. This latter is the variable of interest and this is what is reported in each cell of the table. The set of regressions is repeated for each of the four bandwidths. The results are in Table 6.

We see similar patterns to those reported above for different approaches. The strongest effects are three and four years after the Ofsted visit, and the parameters on these effects are generally stable across the bandwidths. In the narrow sample, we see no effect one year after the visit, some effect two years after and an increasing effect three and four years post-visit.

The effect sizes are smaller than in previous tables, around 0.03 to 0.06 rather than 0.1 to 0.12. Our main results compare performance after the inspection with the performance the year before the visit. If we take a slightly longer run of years before the visit, we show that the fail judgement causes a smaller but still quantitatively and statistically significant effect. It is lower on average because school performance typically declines for schools leading up for two or three years before the inspection (discussed in Ashenfelter, 1978). Our main results attempt to deal with potential mean reversion issues by including the prior trend in performance, and by the matching inherent in the RD design.

4.2 Robustness and falsification checks on the performance effects

The general threats to an RD design are the existence of other discontinuities, control variables changing discontinuously at the cut-off, and manipulation of the forcing variable. We considered the second and third of these above in sections 3.3 and 3.4. In this section we consider alternative specifications and placebo tests.

a) Robustness to specification changes

We return to the question of modelling the heterogeneity in the context of an RD with panel data. We add a quadratic in the rating variable, allowing the coefficients to be different either side of the discontinuity. The effects are as follows. In the OLS, the treatment coefficients are insignificantly different from our main results and much less well determined, the standard errors being about 50% higher; the pattern in the coefficients is that they are typically about one standard error lower. The control variables are always strongly important in the regressions. The split quadratic in the rating variable however is never significantly different from zero in either the broad or the narrow samples, and in just one case in the very narrow sample.

We see the same pattern in the IV estimation. If we include the polynomial in the rating variable as well as the control variables, the standard error of the treatment coefficient is dramatically increased to about five or six times larger. For example, in the narrow bandwidth and looking at the {(t+2)-(t-1)} model, the standard error is 0.183 compared to 0.037 using just the control variables to deal with heterogeneity. Given this lack of precision, the coefficients are insignificantly different from before, but also insignificantly different from zero. Again, the control variables are always strongly significant predictors of the change in performance while the split quadratic is never significant in the broad, narrow or very narrow samples.

We interpret this as follows. The rating variable is a time-invariant snap-shot measure taken at the inspection. It is likely to be correlated with other time invariant characteristics of the school that we do not observe; this is the reason for including this in RD models, to pick up that unobserved heterogeneity. But we have a panel dataset and the outcome we are evaluating is a change in school performance. If there is little correlation between time-invariant school characteristics (the school fixed effect) and the capacity to change, then we would expect little further contribution from the rating variable to explaining the change in performance. This appears to be the case.

One potential school factor that might drive a correlation between a fixed effect and subsequent change is school leadership, and the data on the sub-criteria allow us to test that explicitly. The key sub-criteria are: "the leadership and management of the headteacher and key staff", and "the effectiveness of the governing body in fulfilling its responsibilities". For both of these in turn, we include a binary variable of whether the school failed that sub-criterion and that interacted with the treatment variable (the overall fail judgement). Neither of these are significant in either level or interaction.

b) Falsification tests

We run two different placebo tests: a change in the location of the discontinuity, and a change in the timing. First, we place the discontinuity in the middle of the rating variable for schools judged overall as 'satisfactory'. We re-run the IV analysis in the narrow sample using this discontinuity. The results, in Table 7, show no effect in any time window, which is as it should be. Second, we change the date of the treatment to t-2 and re-run. Again, the results in Table 7, show no effect of the placebo treatment.

4.3 How do schools achieve their improved performance?

The results suggest that failing an Ofsted visit does have a positive causal impact on subsequent performance. How does this come about? The key stakeholders are headteachers, teachers and parents. Over one year, teacher allocation is fixed and any improvement must come in large part from increases in teacher effort. Over two years teacher allocation to classes can change, and over longer horizons more substantial changes to school management, ethos and teacher hiring can take place.

Change of leadership is clearly important but unfortunately we do not have a panel of data that accurately reports the timings of changes in headteachers over the past decade. Having said this, our inspection of the newly created School Workforce Census from November 2010 suggests that schools that fail their Ofsted inspection are indeed more likely to see a change of headteacher within two years than those who pass their inspection.

a) Performance in individual subjects

An analysis of more detailed outcome measures yields some useful information on what schools are doing to achieve these gains for their students. In particular, we are interested in whether the improvement is simply due to strategic manipulation of the subjects sat for exams: easier courses are chosen or especially easier courses that offer a high number of GCSE equivalent points. Alternatively, it could be that the improvement is more genuine and more broad-based.

Some light can be shed on this by examining whether there is any improvement in scores in compulsory core courses. We focus on maths and English[12], and run the same specification as Table 5, instrumenting the fail status with the rating variable. The results are in Table 8, in the same format and specification as Table 5, all run on the narrow bandwidth. The table shows moderately large effects of failing an inspection on achievement in maths (between one-tenth and one-fifth of grade), and to a lesser degree in English. This suggests that there is some genuine and immediate improvement in teaching in those departments taking place.

An alternative question is whether the improvement is narrowly focussed on simply getting more students over the 5A*-C threshold. The second row of Table 8 shows an improvement in the schools proportion of students gaining 5 good GCSE passes about five percentage points. The metric is not the same as our main capped GCSE measure so the magnitudes of the coefficients cannot be directly compared, but it is clear that the effect is statistically much weaker. Again, this suggests a broader-based improvement in teaching rather than gaming focused narrowly on this threshold measure.

b) Differential impact on marginal students

One issue discussed in the NCLB literature is the extent to which there are differential effects on different groups of students. In particular, marginal students for whom a gain in performance is particularly valuable to the school might be expected to be targeted by the school.

Here we define three groups of students, based on their chances of achieving at least 5 C grades or better. We run a student-level probit on this indicator as a function of our prior ability measures, KS2 scores, and all the individual background variables discussed above. We then split the fitted probability index into thirds which we label 'lower ability', 'marginal' and 'higher ability'. The idea is

[12] Comparisons of Science over time are complicated by the many different ways that it can be taken: single science, double science, three separate subjects and so on.

that the performance of the marginal students might improve to pass the 5A* to C grade criterion with some improvement in the schools' effectiveness, and so are of particular interest.

We re-run the procedures in Table 8 separately for the school means over these groups of students. The results are in Table 9. The top row reports on the mean capped GCSE score, and we see a positive and significant effect for marginal students. The impact on higher ability students is larger still and more precisely determined. But the effect is a little lower and less significant for lower ability students. The results for English and maths suggest that the biggest effects are on marginal students, with lower effects on higher ability students, and much less well estimated effects on lower ability students.

These patterns are suggestive of a strategic response by the failed schools on how to allocate their increased effort. They may be responding to what is measured in the school performance tables and so are focussing their activity on students with good chances of getting at least 5 good passes. However, it is equally possible that some lower ability students are simply less able to respond to a school environment that becomes more focussed on exam preparation.

There are obviously other factors that might contribute to schools' improvement, but which cannot be addressed here. These include changes in the pattern of expenditure by schools, changes in teacher turnover leading to changes in average teacher effectiveness, and changes in the input of parents to the children's work in order to compensate for the failing school. Some of these we hope to address in future work.

4.4 Impact of Ofsted failure on changes in the school intake

Finally, we address a question about the long run viability of a school. One of the potential outcomes of an Ofsted failure is that local families turn against the school, parents choose not to send their children, and falling roles and consequent financial pressures force the school's closure. Within that, it could be that more affluent families look elsewhere for school places, so that while numbers hold up, the characteristics of the peer group change adversely.

To address this we look at the intake cohorts into secondary school, rather than the finishing cohorts taking their GCSEs. We analyse three metrics: the number of students entering the school; the mean prior ability score (KS2), and the fraction eligible for free school meals. The results in Table 10 use the narrow sample and again instrument the fail variable with the running variable. They show that enrolment does decline on average in the first three years by around 5 students (relative to a school-cohort mean of around 180, see Data Appendix 1), but that these estimates are not significant.

Futhermore, this decline appears to be evenly spread and there is no change in the average ability or disadvantage of the intake.

5. Conclusion

Every education system needs a method for identifying and dealing with underperforming schools. In England, the accountability system is composed of published performance tables, an independent inspectorate and the right of parents to 'vote with their feet' by choosing a school for their child. In this paper we evaluate the impact of a public judgement of failure by inspectors using a panel of ten years of school performance data. In order to deal with endogeneity of failure, we restrict our attention those that only just fail their inspection by implementing a fuzzy regression discontinuity design, creating a continuous running variable from data on the outcomes of all the sub-criteria used by the inspectors.

Our results suggest a quantitatively and statistically significant effect. Relative to the year before the visit, school performance improves by around 10% of (student-level) standard deviation of GCSE exam performance and the magnitude of this effect is robust to a number of different empirical approaches. The impact is significantly higher in the second year post visit than the first, and remains level into the third and fourth year after the inspection.

We address the typical threats to the validity of an RD approach in both the identification and results sections. The results are robust to a number of alternative specifications that we consider and the two placebo treatments we consider show no effect. The availability of panel data on school performance and student background characteristics is clearly helpful in mitigating problems of the endogenous fail status, even without the benefit of the regression discontinuity. Our strategy also takes into account the alternative explanation of mean reversion, demonstrating the equality of prior trends among just-passers and just-failers in the main estimation sample and controlling for trends in the analysis.

One-tenth of a standard deviation improvement in our broad GCSE performance translates to a one grade improvement in between one and two of a student's best eight exam subjects. Our findings also suggest at most one-tenth of a grade average improvement in English and in maths and a five percentage point growth in the school's overall threshold measure of the proportion of pupils gaining five or more GCSEs at grades A*-C. Although the magnitude of these effects may not appear to be transformative to student's lives, one-tenth of a standard deviation is generally viewed as substantively significant within the field of educational interventions.

It could be argued that these results are implausibly large given that the 'treatment' is so light touch and schools are given no new resources to improve their performance. The treatment involved in the Notice to Improve judgment is essentially two-fold. The school is instructed to improve its performance, and producing year-on-year improvement in exam outcomes is the most visible demonstration of this that a school can make. This may empower headteachers and governors to take a tougher and more proactive line about school and teacher performance, which may not be a trivial channel for improvement. Behavioural economics has provided a good deal of evidence on the importance of norms (Allcott, 2011; Dolan *et al.*, 2009): the school management learning that what they might have considered satisfactory performance is unacceptable may have a major effect. There are similar examples of very light touch policies that rely on norms working elsewhere. For example, Dolton and O'Neill (1996) show that a simple call to an interview about their job search can be enough to change the behaviour of the unemployed. The second part of the treatment derives from the fact that the judgement is a public statement and so provides a degree of public shame for the school leadership. Ofsted fail judgements are widely reported in local press and this is usually not treated as a trivial or ignorable announcement about the school. It seems plausible that this too will be a major spur to action for the school. If this stigma were the most important aspect of the 'treatment', we might hypothesise that schools who fail in areas where there are no other failing schools would feel the stigma more acutely that those who fail alongside underperforming neighbouring schools. We do not see this pattern in our data, but this relationship is complicated because areas with many failing schools are also more deprived and so may lack the capacity to improve for other reasons.

We can summarise the contribution of our results to the question we open the paper with: how should we treat under-performing schools? For schools judged to be failing, but not catastrophically so and with the leadership capacity to improve, the Ofsted system seems to work well, with a non-trivial improvement in school performance over the next three or four years at least. The estimated improvement in performance is substantively important, although not particularly large. It is hard to calibrate the benefit-cost of this policy: the Notice to Improve treatment itself is extremely cheap, but the whole apparatus of the inspection system is not (£207 million or 0.27% of the total schools budget according to Hood et al., 2010). For underperforming schools where inspectors judge they have the leadership and internal resources capacity to improve on their own, our findings suggest that they should be given a number of years to demonstrate they can do this because all alternative interventions for dealing with these schools are relatively expensive. For example, the original Academy programme that began in 2002 was designed to 'relaunch' underperforming schools with greater autonomy, management teams and significant new funds. The early Academy converters have indeed been found to improve exam results by as much as 0.18 standard deviations (Machin and Vernoit, 2011), but at considerable public expense such that their cost-benefit estimation of Academy conversion is likely to be significantly worse than that of delivering a simple Notice to Improve.

Of course, the critical factor about the Notice to Improve judgement, rather than placement into Special Measures, is that it is a statement by the inspectors that they believe the school *does* have the leadership capacity to turn itself around. This reinforces the point that our results should not be over-generalised. The regression discontinuity design inherently has rather limited external validity, and our results are unlikely to apply to severely failing schools. Equally, we cannot claim that 'failing' greater numbers of schools would be effective, not least because those who currently 'just' fail may become de-motivated and feel they cannot improve sufficiently to pass once the threshold has been substantially raised. However, this doesn't preclude similar, but distinct, interventions applying to other schools who do not currently fail their inspection, as suggested by recent Ofsted policy statements. The new Director of Ofsted[13] has argued that schools just above the fail grade should also be tackled: that 'satisfactory' performance is in fact unsatisfactory. Such interventions in 'coasting' or 'just-ok' schools are very likely to be of the same form as Notice to Improve. Our results suggest that this is potentially a fruitful development with some hope of significant returns.

[13] Sir Michael Wilshaw, becoming Director of Ofsted after leaving the Headship of Mossbourne school, one of the most successful disadvantaged schools in the country. Statement dated 16th January 2012.

References

Ahn, T. and Vigdor, J. (2009*) Does No Child Left Behind have teeth? Examining the impact of federal accountability sanctions in North Carolina*. Working Paper, October 2009.

Allcott, H. (2011) Social Norms and Energy Conservation, *Journal of Public Economics*, 95(9-10) 1982-1095.

Allen, R. and Burgess, S. (2011) Can school league tables help parents choose schools? *Fiscal Studies*, 32(2)245-261.

Angrist, J.D. and Lavy, V. (1999) Using Maimonides' Rule To Estimate The Effect Of Class Size On Scholastic Achievement, *The Quarterly Journal of Economics,* 114(2) 533-575.

Angrist, J.D. and Pischke, J-S. (2009) *Basically harmless econometrics*, Princeton, New Jersey: Princeton University Press.

Ashenfelter, O. (1978) Estimating the Effects of Training Programmes on Earnings. *The Review of Economics and Statistics* 60 (1) 47-57.

Bacolod, M., DiNardo, J. and Jacobson, M. (2012) Beyond incentives: Do schools use accountability rewards productively? *Journal of Business and Economic Statistics*, 30(1) 149-163.

Burgess, S., Wilson, D. and Worth, J. (2010) *A natural experiment in school accountability: the impact of school performance information on pupil progress and sorting*, CMPO Working Paper 10/246.

Chakrabarti, R. (2010) *Vouchers, public school response, and the role of incentives: Evidence from Florida*, Federal Reserve Bank of New York Report 306.

Coleman, J. (1998) The Place of External Inspection, in Middlewood, D. *Strategic Management in Schools and Colleges,* London: Paul Chapman.

Dee, T. S. and Jacob, B. (2011) The impact of No Child Left Behind on student achievement, *Journal of Policy Analysis and Management*, 30(3) 418–446.

Dolan, P., Hallsworth, M., Halpern, D., King, D., Vlaev, I. (2009) *Mindspace: Influencing behaviour through public policy*, London: Institute for Government.

Dolton, P. and O'Neill, D. (1996) Unemployment Duration and the Restart Effect: Some Experimental Evidence, *Economic Journal*, 106(435) 387-400.

Figlio, D.N. and Rouse, C.E. (2006) Do accountability and voucher threats improve low-performing schools? *Journal of Public Economics*, 90(1-2) 239-255.

Fruehwirth, J. and Traczynski, J. (2011) *Spare the Rod? The Effect of No Child Left Behind on Failing Schools*, University of Wisconsin: mimeo.

Hobbs, G. and Vignoles, A. (2010) Is children's free school meal 'eligibility' a good proxy for family income? *British Educational Research Journal*, 36 (4) 673-690.

Hood, C., Dixon, R. and Wilson, D. (2010) Keeping Up the Standards? The Use of Targets and Rankings to Improve Performance, *School Leadership Today*, March 2010.

Hussain, I. (2012) *Subjective performance evaluation in the public sector: Evidence from school inspections*, CEP discussion paper CEEDP0135.

Jacob, B. (2005) Accountability, incentives and behavior: Evidence from school reform in Chicago, *Journal of Public Economics*, 89(5-6) 761-796.

Krieg, J. M. (2008) Are students left behind? The distributional effects of the No Child Left Behind Act, *Education Finance and Policy*, 3(2) 250-281.

Ladd, H.F. and Lauen, D.L. (2010) Status vs. growth: The distributional effects of school accountability policies, *Journal of Policy Analysis and Management*, 29(3) 424-450.

Learmonth, J. (2000) *Inspection: what's in it for schools?* London, Routledge Falmer

Lee, D. S. (2008) Randomized Experiments from Non-random Selection in U.S. House Elections, *Journal of Econometrics,* 142(2)675-697.

Machin, S. and Vernoit, J. (2011) *Changing School Autonomy: Academy Schools and their Introduction to England's Education*, CEP (LSE) discussion paper 0123.

McCrary, J. (2008) Manipulation of the running variable in the regression discontinuity design: a density test, *Journal of Econometrics*, 142(2):698-714.

Neal D. A. and Schanzenbach, D. W. (2010) Left behind by design: Proficiency counts and test-based accountability, *Review of Economics and Statistics*, 92 263-283.

Ofsted (2011a) *The framework for school inspection in England under section 5 of the Education Act 2005, from September 2009*, Report reference 090019 (September, 2011).

Ofsted (2011b) *Monitoring inspections of schools that are subject to Special Measures*, Report reference 090272 (September, 2011).

Ofsted (2011c) *Monitoring inspections of schools with a Notice to Improve,* Report reference 090277 (September, 2011).

Reback R. (2008) Teaching to the rating: School accountability and the distribution of student achievement, *Journal of Public Economics*, 92(5-6) 139-415.

Rockoff J.E. and Turner, L.J. (2010) Short run impacts of accountability on school quality, *American Economic Journal: Economic Policy*, 2(4) 119-47.

Rosenthal, L. (2004) Do school inspections improve school quality? Ofsted inspections and school examination results in the UK, *Economics of Education Review*, 23 (2) 143-151.

Van der Klaauw, W. (2002) Estimating the Effect of Financial Aid Offers on College Enrollment: A Regression-Discontinuity Approach, *International Economic Review*, 43(4) 1249-1287.

Tables

Table 1: Ofsted inspections data

Year	2002/03	2003/04	2004/05	2005/06	2006/07	2007/08	2008/09
Number of school visits	476	560	453	924	1103	970	638
Number of sub-criteria used	19	33	33	55	41	58	65
Rating = Excellent	18	10	13	n/a	n/a	n/a	n/a
Outstanding/v good	117	98	109	97	165	180	151
Good	202	264	190	358	436	417	283
Satisfactory	114	130	107	347	414	299	167
Unsatisfactory	18	44	25	122	88	74	37
Poor	5	14	9	n/a	n/a	n/a	n/a
Very poor	2	0	0	n/a	n/a	n/a	n/a
Proportion failing (%)	5.25	10.36	7.51	13.20	7.98	7.63	5.80

Notes:
(a) Figures based on data used in year 11 (age 16) analysis
(b) All secondary schools are included in our dataset, regardless of whether they have a complete exam history over this time period. We match predecessor and successor schools to create time series data only where the successor (usually an academy) immediately opens on the same or a very close site. We do not match data for more complex school re-openings such as 2-to-1 mergers or movement of school sites beyond close walking distance.

Table 2: Discontinuity tests for observable background variables

Narrow sample, 400 observations (200 fails, 200 passes)

	Mean for:		
	Fail	**Pass**	**p-value on difference=0**
KS2 English at age 11			
X(t-1)	-0.183	-0.235	0.069
X(t+2) - X(t-1)	-0.013	-0.010	0.836
X(t+4) - X(t-1)	-0.019	-0.003	0.478
KS2 Maths at age 11			
X(t-1)	-0.156	-0.204	0.083
X(t+2) - X(t-1)	-0.010	-0.006	0.831
X(t+4) - X(t-1)	-0.019	-0.014	0.817
KS2 Science at age 11			
X(t-1)	-0.138	-0.195	0.063
X(t+2) - X(t-1)	-0.023	-0.015	0.690
X(t+4) - X(t-1)	-0.032	-0.033	0.945
Free schools meals eligibility			
X(t-1)	0.187	0.215	0.054
X(t+2) - X(t-1)	-0.004	-0.011	0.302
X(t+4) - X(t-1)	0.001	-0.004	0.607
Local deprivation (IDACI)			
X(t-1)	0.267	0.299	0.011
X(t+2) - X(t-1)	0.009	0.006	0.327
X(t+4) - X(t-1)	0.013	0.008	0.224
Female			
X(t-1)	0.475	0.482	0.595
X(t+2) - X(t-1)	-0.014	-0.005	0.175
X(t+4) - X(t-1)	-0.004	0.005	0.243
English as an additional language			
X(t-1)	0.098	0.111	0.514
X(t+2) - X(t-1)	0.023	0.010	0.119
X(t+4) - X(t-1)	0.042	0.024	0.123
White British			
X(t-1)	0.786	0.819	0.174
X(t+2) - X(t-1)	-0.014	-0.030	0.266
X(t+4) - X(t-1)	-0.023	-0.046	0.157
Prior trend in GCSE outcome variable			
X(t-3) - X(t-1)	-0.026	-0.015	0.583

Table 3: Before-After regressions for all schools

Dependent variable is the difference in school mean GCSE score (capped z-score)
Unit of observation is a school*visit; Metric is (pupil-level) SDs of GCSE score
Just the coefficient on "Ofsted failed" and its standard error reported

Difference in GCSE:	(t+1) – (t-1)	(t+2) – (t-1)	(t+3) – (t-1)	(t+4) – (t-1)
Basic[(b)]	0.069***	0.124***	0.147***	0.166***
	(0.009)	(0.011)	(0.013)	(0.016)
Adj-Rsqd	0.015	0.029	0.032	0.035
N	5060	5022	4349	3380
Differences[(c)]	0.068***	0.123***	0.148***	0.165***
	(0.008)	(0.010)	(0.012)	(0.015)
Adj-Rsqd	0.155	0.142	0.141	0.138
N	5057	5020	4347	3378
Levels[(d)]	0.059***	0.102***	0.111***	0.124***
	(0.009)	(0.011)	(0.014)	(0.017)
Adj-Rsqd	0.203	0.206	0.231	0.263
N	4004	3966	3313	2359
Common sample[(e)]	0.040***	0.074***	0.102***	0.124***
	(0.010)	(0.013)	(0.015)	(0.017)
Adj-Rsqd	0.243	0.239	0.252	0.263
N	2358	2359	2359	2359

Notes:
(a) Each cell of this table reports the results of a separate regression. All regressions on the full sample.
(b) Other variables included in 'Basic': Dummies for the year of the Ofsted visit.
(c) Other variables included in 'Differences': Dummies for the year of the Ofsted visit; differences of the same order as the column of the school mean: KS2 English, KS2 Maths, KS2 Science, fraction students eligible for Free School Meals, fraction students female, fraction students white British, fraction students with English as an additional language; IDACI score;
(d) Other variables included in 'Levels': Dummies for the year of the Ofsted visit; differences of the same order as the column of the school mean: KS2 English, KS2 Maths, KS2 Science, fraction students eligible for Free School Meals, fraction students female, fraction students white British, fraction students with English as an additional language; IDACI score; plus the level of all these same variables at (t-1); plus the prior trend (measured as GCSE(t-1) – GCSE(t-3))
(e) Common sample just uses the inspections with data available for up to the fourth difference.
(f) Levels of significance indicated as * 0.10, ** 0.05, *** 0.01.

Table 4: Before-After regressions for all schools – Different Samples

Dependent variable is the difference in school mean GCSE score (capped z-score)
Unit of observation is a school*visit; Metric is (pupil-level) SDs of GCSE score
Just the coefficient on "Ofsted failed" and its standard error reported

Difference in GCSE:	(t+1) – (t-1)	(t+2) – (t-1)	(t+3) – (t-1)	(t+4) – (t-1)
Full	0.059***	0.102***	0.111***	0.124***
	(0.009)	(0.011)	(0.014)	(0.017)
Adj-Rsqd	0.203	0.206	0.231	0.263
N	4004	3966	3313	2359
Broad	0.054***	0.082***	0.096***	0.125***
	(0.017)	(0.020)	(0.025)	(0.032)
Adj-Rsqd	0.250	0.218	0.230	0.270
N	467	466	421	325
Narrow	0.056***	0.103***	0.108***	0.119***
	(0.022)	(0.024)	(0.031)	(0.038)
Adj-Rsqd	0.247	0.249	0.223	0.249
N	315	314	283	232
Very Narrow	0.030	0.054	0.077*	0.074
	(0.031)	(0.035)	(0.045)	(0.053)
Adj-Rsqd	0.200	0.215	0.262	0.296
N	156	156	139	119

Notes:
(a) Each cell of this table reports the results of a separate regression.
(b) Other variables included in all regressions: Dummies for the year of the Ofsted visit; differences of the same order as the column of the school mean: KS2 English, KS2 Maths, KS2 Science, fraction students eligible for Free School Meals, fraction students female, fraction students white British, fraction students with English as an additional language; IDACI score; plus the level of all these same variables at (t-1); plus the prior trend (measured as GCSE(t-1) – GCSE(t-3))
(c) Levels of significance indicated as * 0.10, ** 0.05, *** 0.01.

Table 5: Fuzzy RDD IV regression analysis

Dependent variable is the difference in school mean GCSE score (capped z-score)
Unit of observation is a school*visit; Metric is (pupil-level) SDs of GCSE score
Just the coefficient on "Ofsted failed" and its standard error reported
The fail status variable is instrumented by quartic in the rating variable, and the threshold variables.

Difference in GCSE:	(t+1) – (t-1)	(t+2) – (t-1)	(t+3) – (t-1)	(t+4) – (t-1)
Basic[(b)], full sample	0.070***	0.130***	0.156***	0.180***
	(0.011)	(0.013)	(0.016)	(0.020)
Adj-Rsqd	0.015	0.029	0.032	0.035
N	5060	5022	4349	3380
Levels, full sample	0.057***	0.104***	0.112***	0.123***
	(0.012)	(0.015)	(0.016)	(0.022)
Adj-Rsqd	0.203	0.206	0.231	0.263
N	4004	3966	3313	2359
Broad bandwidth	0.043*	0.069**	0.092***	0.135***
	(0.022)	(0.027)	(0.031)	(0.043)
Adj-Rsqd	0.249	0.217	0.230	0.270
N	467	466	421	325
Narrow bandwidth	0.046	0.102***	0.121**	0.140**
	(0.032)	(0.036)	(0.044)	(0.055)
Adj-Rsqd	0.247	0.249	0.222	0.248
N	315	314	283	232
Very narrow bandwidth	0.022	0.021	0.135	0.082
	(0.061)	(0.067)	(0.084)	(0.100)
Adj-Rsqd	0.200	0.210	0.251	0.296
N	156	156	139	119

Notes:
(a) Each cell of this table reports the results of a separate regression.
(b) Other variables included in 'Basic': Dummies for the year of the Ofsted visit.
(c) Other variables included in all other regressions: dummies for the year of the Ofsted visit; differences of the same order as the column of the school mean: KS2 English, KS2 Maths, KS2 Science, fraction students eligible for Free School Meals, fraction students female, fraction students white British, fraction students with English as an additional language; IDACI score; plus the level of all these same variables at (t-1), plus the prior trend (measured as GCSE(t-1) – GCSE(t-3)).
(d) Levels of significance indicated as * 0.10, ** 0.05, *** 0.01.

Table 6: Difference-in-difference panel analysis

Dependent variable is the difference in school mean GCSE score (capped z-score)
Unit of observation is a school*visit; Metric is (pupil-level) SDs of GCSE score
Just the coefficient on "Ofsted failed" times D(t+#) and its standard error reported

Fail*Dummy for year:	Year = visit + 1	Year = visit + 2	Year = visit + 3	Year = visit + 4
Full sample	-0.022***	0.037***	0.066***	0.090***
	(0.008)	(0.008)	(0.008)	(0.009)
Adj-Rsqd	0.037	0.037	0.038	0.039
N*T	50202	50202	50202	50202
Broad bandwidth	-0.017	0.013	0.031**	0.057***
	(0.014)	(0.014)	(0.015)	(0.016)
Adj-Rsqd	0.119	0.119	0.119	0.120
N*T	5873	5873	5873	5873
Narrow bandwidth	-0.011	0.033*	0.049***	0.058***
	(0.017)	(0.017)	(0.018)	(0.020)
Adj-Rsqd	0.118	0.119	0.119	0.120
N*T	3926	3926	3926	3926
Very narrow bandwidth	-0.003	0.030	0.045*	0.039
	(0.025)	(0.025)	(0.027)	(0.029)
Adj-Rsqd	0.095	0.096	0.097	0.096
N*T	1965	1965	1965	1965

Notes:
(a) Each cell of this table reports the results of a separate regression.
(b) Other variables included in all other regressions: year dummies; dummy for the year of the visit; contemporaneous values of the school mean of: KS2 English, KS2 Maths, KS2 Science, fraction students eligible for Free School Meals, fraction students female, fraction students white British, fraction students with English as an additional language; IDACI score.
(c) Levels of significance indicated as * 0.10, ** 0.05, *** 0.01. Robust standard errors.

Table 7: Falsification tests on timing of treatment and placement of discontinuity

Dependent variable is the difference in school mean GCSE score (capped z-score)
Unit of observation is a school*visit; Metric is (pupil-level) SDs of GCSE score
Just the coefficient on "Ofsted failed" and its standard error reported
The fail status variable is instrumented by quartic in the rating variable, and the threshold variables.

Difference in GCSE:	(t+1) – (t-1)	(t+2) – (t-1)	(t+3) – (t-1)	(t+4) – (t-1)
Main IV results, narrow bandwidth	0.046	0.102***	0.121**	0.140**
	(0.032)	(0.036)	(0.044)	(0.055)
Adj-Rsqd	0.247	0.249	0.222	0.248
N	315	314	283	232
Placebo 'fail' discontinuity within satisfactory (sharp RDD), narrow bandwidth	-0.037	-0.029	-0.050	-0.057
	(0.021)	(0.027)	(0.031)	(0.046)
Adj-Rsqd	0.196	0.153	0.102	0.133
N	269	265	247	174
Placebo treatment for schools at time t=t-2, narrow bandwidth	0.041	0.059	0.026	0.060
	(0.036)	(0.047)	(0.055)	(0.059)
Adj-Rsqd	0.580	0.476	0.643	0.433
N	154	154	154	154

Notes:
(a) Each cell of this table reports the results of a separate regression.
(b) Other variables included in 'Basic': Dummies for the year of the Ofsted visit.
(c) Other variables included in all other regressions: dummies for the year of the Ofsted visit; differences of the same order as the column of the school mean: KS2 English, KS2 Maths, KS2 Science, fraction students eligible for Free School Meals, fraction students female, fraction students white British, fraction students with English as an additional language; IDACI score; plus the level of all these same variables at (t-1), plus the prior trend (measured as GCSE(t-1) – GCSE(t-3)).
(d) Levels of significance indicated as * 0.10, ** 0.05, *** 0.01.

Table 8: Fuzzy RDD IV regression analysis, different outcome variables, Narrow sample

Dependent variable is the difference in school mean of various outcome measures
Unit of observation is a school*visit; Metric is (pupil-level) SDs for row 1, fraction for row 2, and mean point score (from 1 to 8) for rows 3 and 4.
Just the coefficient on "Ofsted failed" and its standard error reported.
The fail status variable is instrumented by the rating variable.

Difference in GCSE:	(t+1) – (t-1)	(t+2) – (t-1)	(t+3) – (t-1)	(t+4) – (t-1)
Capped mean GCSE score	0.046	0.102***	0.121**	0.140**
	(0.032)	(0.036)	(0.044)	(0.055)
Adj-Rsqd	0.247	0.249	0.222	0.248
N	315	314	283	232
Fraction of pupils achieving at least 5 A*-C grades	0.024	0.037*	0.050**	0.058**
	(0.019)	(0.021)	(0.025)	(0.029)
Adj-Rsqd	0.237	0.275	0.226	0.272
N	315	314	283	232
Mean Maths GCSE score	0.139**	0.164**	0.141*	0.094
	(0.062)	(0.068)	(0.079)	(0.082)
Adj-Rsqd	0.406	0.253	0.221	0.272
N	315	314	283	232
Mean English GCSE score	0.146**	0.114*	0.106	0.074
	(0.059)	(0.064)	(0.074)	(0.081)
Adj-Rsqd	0.279	0.250	0.239	0.276
N	315	314	283	232

Notes:
(a) Each cell of this table reports the results of a separate regression.
(b) Other variables included in all other regressions: dummies for the year of the Ofsted visit; differences of the same order as the column of the school mean: KS2 English, KS2 Maths, KS2 Science, fraction students eligible for Free School Meals, fraction students female, fraction students white British, fraction students with English as an additional language; IDACI score; plus the level of all these same variables at (t-1); plus the prior trend (measured as GCSE(t-1) – GCSE(t-3)).
(d) Levels of significance indicated as * 0.10, ** 0.05, *** 0.01.

Table 9: Analysis of marginal pupils versus others, narrow sample

Dependent variable is the difference in school mean of various outcome measures
Unit of observation is a school*visit; Metric is (pupil-level) SDs for row 1, fraction for row 2, and mean point score (from 1 to 8) for rows 3 and 4.
Just the coefficient on "Ofsted failed" and its standard error reported.
The fail status variable is instrumented by the rating variable.

Difference in GCSE:	(t+1) – (t-1)	(t+2) – (t-1)	(t+3) – (t-1)	(t+4) – (t-1)
Capped mean GCSE score				
Lower ability students	0.010	0.075*	0.117**	0.118*
	(0.037)	(0.045)	(0.048)	(0.062)
Marginal students	0.082*	0.093*	0.113**	0.157**
	(0.044)	(0.048)	(0.053)	(0.064)
Higher ability students	0.085*	0.106**	0.095*	0.216***
	(0.044)	(0.050)	(0.055)	(0.069)
5+ A*-C GCSE score				
Lower ability students	-0.017	0.014	0.030	0.049
	(0.025)	(0.031)	(0.035)	(0.046)
Marginal students	0.055**	0.043	0.061**	0.071**
	(0.028)	(0.030)	(0.029)	(0.030)
Higher ability students	0.028*	0.032*	0.043***	0.058***
	(0.015)	(0.017)	(0.016)	(0.016)
Maths GCSE				
Lower ability students	0.023	0.058	0.052	-0.028
	(0.066)	(0.079)	(0.087)	(0.105)
Marginal students	0.211**	0.136	0.136	0.112
	(0.082)	(0.086)	(0.085)	(0.100)
Higher ability students	0.151**	0.072	0.075	0.161
	(0.073)	(0.089)	(0.082)	(0.105)
English GCSE				
Lower ability students	0.086	0.068	0.046	-0.044
	(0.071)	(0.082)	(0.089)	(0.101)
Marginal students	0.211***	0.103	0.121	0.086
	(0.066)	(0.068)	(0.076)	(0.088)
Higher ability students	0.098	-0.010	0.034	0.083
	(0.071)	(0.076)	(0.087)	(0.097)
N	312	311	280	229

Notes:
(a) Each cell of this table reports the results of a separate regression.
(b) Other variables included in all other regressions: dummies for the year of the Ofsted visit; differences of the same order as the column of the school mean: KS2 English, KS2 Maths, KS2 Science, fraction students eligible for Free School Meals, fraction students female, fraction students white British, fraction students with English as an additional language; IDACI score; plus the level of all these same variables at (t-1).
(c) Levels of significance indicated as * 0.10, ** 0.05, *** 0.01.

Table 10: Analysis of change in pupil intake

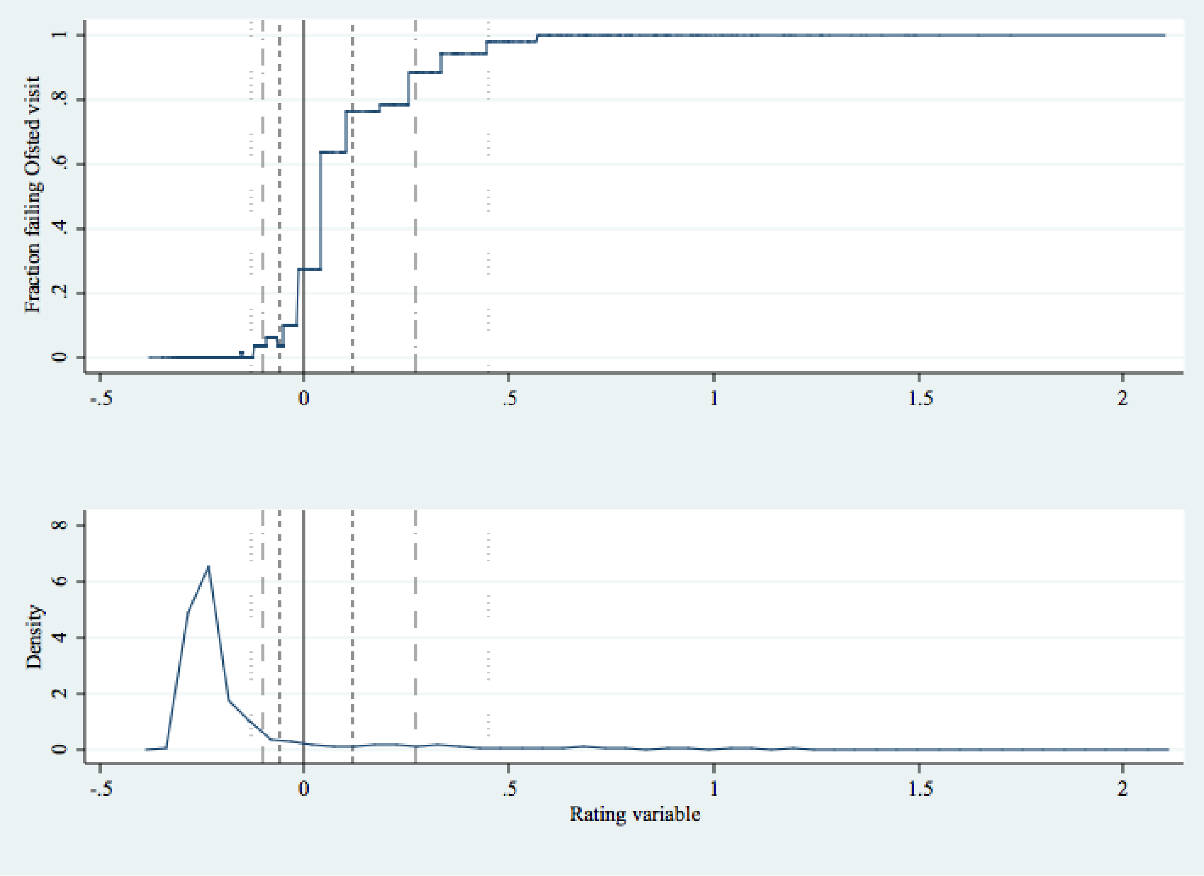
Dependent variable is the difference in school mean of various measures of the school intake
Unit of observation is a school*visit; Metric is number of pupils in row 1, the mean points score in row 2, and the proportion in row 3.
Just the coefficient on "Ofsted failed" and its standard error reported.
The fail status variable is instrumented by the rating variable.

Difference in year 7 metric:	(t+1) – (t-1)	(t+2) – (t-1)	(t+3) – (t-1)	(t+4) – (t-1)
Number of pupils in cohort	-4.493	-4.977	2.265	-0.532
	(5.003)	(6.503)	(6.896)	(8.926)
Adj-Rsqd	0.032	-0.010	-0.006	0.009
N	313	308	280	225
Mean KS2 score	0.027	-0.003	-0.012	-0.011
	(0.024)	(0.027)	(0.029)	(0.037)
Adj-Rsqd	0.142	0.095	0.103	0.100
N	313	308	280	225
FSM proportion	-0.011	-0.009	-0.025	-0.002
	(0.013)	(0.013)	(0.016)	(0.012)
Adj-Rsqd	0.323	0.345	0.369	0.485
N	313	308	280	225

Notes:
(a) Each cell of this table reports the results of a separate regression.
(b) Other variables included in all other regressions: dummies for the year of the Ofsted visit.
(c) Levels of significance indicated as * 0.10, ** 0.05, *** 0.01.

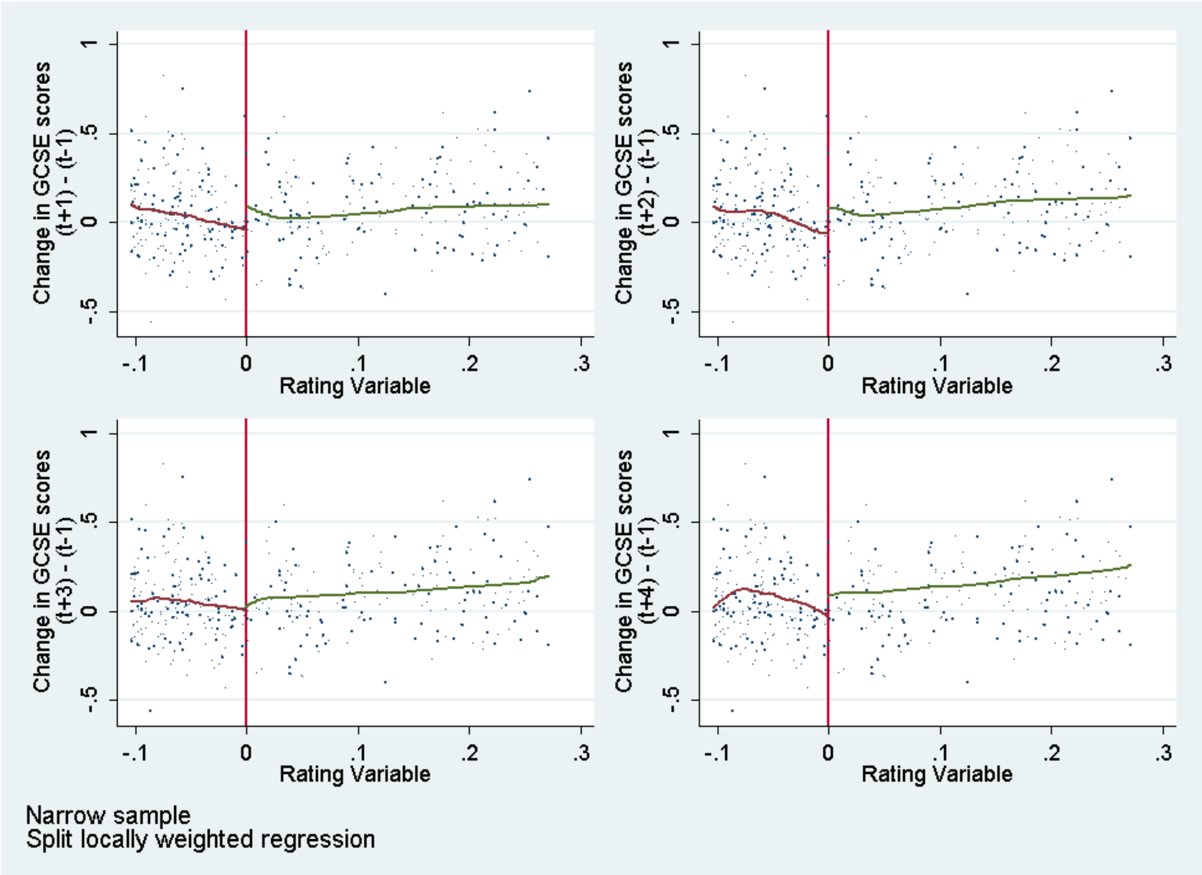
Figures

Figure 1: RDD assignment variable: Fraction failing and density



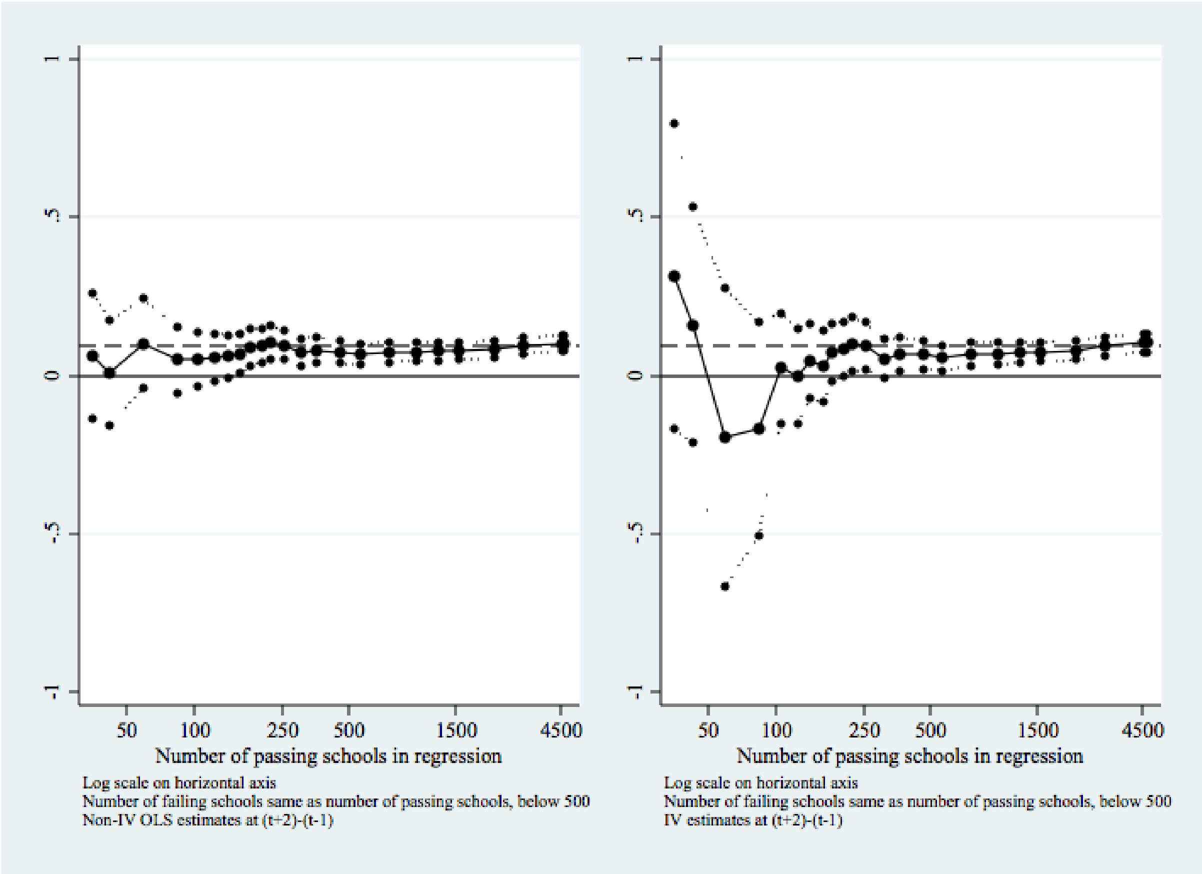
Note: Bandwidths shown are Broad, Narrow and Very Narrow samples.

Figure 2: Change in GCSE scores across the discontinuity, narrow sample



Notes: Change in capped GCSEs metric is (pupil-level) standard deviations
Unit = school*visit.

Figure 3: Results at different sample sizes



Data Appendix

Data Appendix Table 1: Summary statistics for pupil background, prior attainment and outcomes

	2003	2005	2007	2009	2011
Year 11 cohorts:					
GCSE capped (best 8) z-score	0.04	0.03	0.04	0.02	0.05
GCSE 5+ A*-C	52.1%	55.8%	61.0%	70.2%	81.5%
GCSE English score	4.69	4.55	4.73	4.75	5.05
GCSE maths score	4.10	4.30	4.53	4.60	4.88
Mean number of pupils in school	181.68	185.56	191.09	186.65	184.16
KS2 English score (age 11)	0.03	0.03	0.03	0.03	0.02
KS2 maths score (age 11)	0.03	0.03	0.03	0.03	0.02
KS2 science score (age 11)	0.02	0.02	0.03	0.03	0.02
Proportion free school meals	13.9%	13.9%	12.8%	12.9%	14.0%
Mean deprivation IDACI score	0.22	0.22	0.22	0.23	0.22
Proportion female	49.5%	49.7%	49.6%	49.5%	49.5%
Proportion English Additional Language	9.1%	9.2%	9.8%	11.2%	12.1%
Proportion ethnicity white British	79.2%	80.7%	80.8%	78.6%	77.3%
Year 7 cohorts:					
Mean number of pupils in school	188.06	179.56	175.45	174.38	175.06
Mean KS2 score (age 11)	0.03	0.02	0.00	0.01	-0.01
Proportion in top 25% at KS2	25.9%	25.5%	25.0%	25.2%	25.7%
Proportion at bottom 25% at KS2	24.2%	24.6%	25.1%	25.0%	24.3%
Proportion free school meals	17.1%	17.0%	16.9%	17.4%	18.1%
Proportion ethnicity white British	80.7%	80.4%	78.3%	77.3%	75.6%

Notes: For space reasons we exclude alternate years, so 2002, 2004, 2006, 2008 and 2010 are not included in this table
Average across inspection events, not weighted by school size

Data Appendix Table 2: Statistics for first stage regressions for Table 5

Difference in GCSE:	(t+1) – (t-1)	(t+2) – (t-1)	(t+3) – (t-1)	(t+4) – (t-1)
Basic, full sample				
F-stat	2225.8	2163.9	1970.5	1543.4
N	5060	5022	4349	3380
Levels, full sample				
F-stat	842.6	815.3	680.6	451.9
N	4004	3966	3313	2359
Broad bandwidth				
F-stat	30.2	29.9	29.3	19.7
N	467	466	421	325
Narrow bandwidth				
F-stat	12.2	11.7	11.8	9.5
N	315	314	283	232
Very narrow bandwidth				
F-stat	3.3	3.1	2.7	2.5
N	156	156	139	119

See notes to table 5 for details.

Data Appendix Table 3: Example full regression output for fuzzy RDD IV regression

Model shown estimated in narrow sample
Dependent variable is the difference in school mean GCSE score (capped z-score)
Unit of observation is a school*visit; Metric is (pupil-level) SDs of GCSE score
Just the coefficient on "Ofsted failed" and its standard error reported
The fail status variable is instrumented by quartic in the rating variable, and the threshold variables.

Difference in GCSE (t+2 – t-1):	**Beta**	**S.E.**	
Ofsted fail (instrumented)	0.102	0.036	***
KS2 English (t+2 - t-1)	-0.112	0.117	
KS2 maths (t+2 - t-1)	0.258	0.173	
KS2 science (t+2 - t-1)	0.119	0.136	
Free school meals (t+2 - t-1)	-0.429	0.246	
Deprivation (t+2 - t-1)	-0.321	0.429	
Female (t+2 - t-1)	0.559	0.185	***
EAL (t+2 - t-1)	0.130	0.277	
White ethnicity (t+2 - t-1)	-0.006	0.113	
KS2 English (t-1)	-0.281	0.150	*
KS2 maths (t-1)	0.029	0.197	
KS2 science (t-1)	0.098	0.142	
Free school meals (t-1)	-0.121	0.250	
Deprivation (t-1)	0.471	0.227	**
Female (t-1)	0.050	0.099	
EAL (t-1)	0.018	0.173	
White ethnicity (t-1)	0.013	0.120	
Prior trend in GCSE (t-1 - t-3)	-0.155	0.075	**
2005 inspection year dummy	-0.121	0.065	*
2006 inspection year dummy	-0.091	0.050	*
2007 inspection year dummy	-0.055	0.053	
2008 inspection year dummy	-0.086	0.054	
Constant	-0.068	0.129	
N	314		
R-squared	0.302		

Notes: Levels of significance indicated as * 0.10, ** 0.05, *** 0.01.