Politics, Markets and Schools: Quasi-Experimental Evidence on the Impact of Autonomy and Competition from a Truly Revolutionary UK Reform

Damon Clark
Nuffield College, Oxford
Post-Doctoral Research Fellow: Center for Labor Economics,
University of California, Berkeley
clarkd@econ.berkeley.edu

July 19, 2005

Abstract

Supporters of market-based education reforms argue that school autonomy and between-school competition can raise student achievement. Yet U.S. reforms based in part on these ideas - charter schools, school-based management, vouchers and school choice - are limited in scope, complicating evaluations of their impact. In contrast, a series of remarkable reforms enacted by the Thatcher Government in Britain in the 1980s provide an ideal testing ground for examining the effects of school autonomy and between-school competition. In this paper I study one reform - described by Chubb and Moe (1992) as 'truly revolutionary' - that allowed public high schools to 'opt out' of the local school authority and become quasi-independent, funded directly by central Government. In order to opt out schools had to first win a majority vote of current parents, and I assess the impact of school autonomy via a regression discontinuity design, comparing student achievement levels at schools where the vote barely won to those where it barely lost. To assess the effects of competition I use this same idea to compare student achievement levels at neighbouring schools of barely winners to neighbouring schools of barely losers. My results suggest two conclusions. First, there were large gains to schools that won the vote and opted out, on the order of a one-quarter standard deviation improvement on standardised national examinations. Since results improved for those students already enrolled in the school at the time of the vote, this outcome is not likely to be driven by changes in student-body composition (cream-skimming). Second, the gains enjoyed by the opted-out schools appear not to have spilled over to their neighbours - I can never reject the hypothesis of no spillovers and can always reject effects bigger than one half of the 'own-school' impact. I interpret my results as supportive of education reforms that seek to hand power to schools, with the caveat that I do not know precisely what opted-out schools did to improve. With regards to competition, although I cannot rule out small but economically important competition effects, my results suggest caution as to the likely benefits.

Acknowledgments

I am indebted to David Card for encouragement and advice throughout the project. I also thank Ken Chay and David Lee for helpful discussions. Emilia Del Bono, Graham Hobbs, Jochen Kluve, Ethan Kaplan, Rob McMillan, Tom Nicholas, Imran Rasul, Eric Verhoogen, Anna Vignoles, Till Von Wachter, Andrea Weber, seminar participants at UC Berkeley and the Insitute of Education, London, and especially Jane Leber Herr gave extremely useful comments and suggestions. Many people helped in the construction of the dataset used in the paper: John Elliott coordinated access to various datasets held by the Department for Education and Skills (DfES), Kate Bradford and the National Archive of Digital Data were extremely efficient in preparing the Grant Maintained Schools database and Graham Hobbs kindly collected data when I could not collect it myself. I am grateful to all of these people. I am also grateful to Ros Levacic for sharing her school competition data and for helpful discussions on school competition and the GM reforms, and to Joan Payne for sharing her YCS data. Finally, I would like to thank the many LEA officials, Head Teachers and representatives of the Grant Maintained Schools Trust who took time to talk to me. I particularly thank Joe Clark for answering my countless questions on UK secondary education with at least a degree of patience. None of the above has responsibility for the shortcomings of the paper.

The British..., not the Americans are on the cutting edge of reform... In terms of the sheer magnitude of the changes it stands to bring about, their 1988 Education Act may well be the most significant educational development in either country during the postwar era...What is happening there is happening in the United States: the problems, the reforms, the conflicts and the alliances are all roughly the same. The only real difference is that Britain, owing to its parliamentary form of Government, has been able to move much farther and faster toward a radical overhaul of its educational system - and is far more likely to succeed. We can only hope that it does, and that America can someday follow in Britain's footsteps.

John Chubb & Terry Moe (1992), p.50.

1 Introduction

The structure of the public education system has, in recent years, been one of the most contentious elements of U.S. public policy, both at the federal and state level. Market-oriented critics of the current structure envision a system in which schools have complete autonomy over staffing, discipline, tracking and other policies, and individual schools compete aggressively for students on the basis of their ability to deliver education services (see, for example Chubb and Moe (1990)). Although recent reform initiatives such as charter schools, 'school-based management', vouchers and school choice are motivated in part by these ideas, the extent to which any of these policies increase autonomy or competition is the subject of intense debate.[1] Coupled with conflicting findings about the actual impacts of the limited reforms that have been adopted so far, there is little consensus about the efficacy of recent reform efforts.[2]

In contrast to the limited U.S. reform initiatives, a series of remarkable reforms enacted by the Thatcher Government in Britain over the 1980s provide an ideal testing ground for evaluating

[1] Charter laws were passed to promote innovation as well as autonomy, and charter school growth has slowed in recent years (Ladd (2002)). Chubb and Moe (1990) do not regard school-based management as granting real autonomy and the furthest reaching reform (in Chicago) was eventually aborted (Wong (2003)). Since school choice policies are often designed to ensure racial balance (Cullen, Jacob, and Levitt (2000)) this may blunt their competition-enhancing effects. Only Milwaukee has a large voucher program and school vouchers are typically 'partial', requiring that families 'top up' the costs of private education (Peterson, Howell, Wolff, and Campbell (2003))

[2] Bettinger (2004) finds that Michigan charter schools have no impact on achievement; Finn, Manno, and Vanourek (1993) report gains from a multi-state study. On the Chicago school-based management reform the evidence is mixed (Bryk, Thum, Easton, and Luppescu (1998)). On school choice, Cullen, Jacob, and Levitt (2000) report gains among students 'opting out' of the local school, but these disappear once selection is accounted for. On vouchers, Rouse (1998) finds some positive gains for students attending Milwaukee voucher schools. Peterson, Howell, Wolff, and Campbell (2003) find gains for a New York voucher experiment, but Krueger and Zhu (2002) dispute these. Hoxby (2003) finds positive competition or spillover effects associated with Milwaukee voucher schools and Michigan charter schools but Bettinger (2004) studies spillover effects from Michigan charter schools and finds none.

the effects of school autonomy and between-school competition. The primary reform, the 1988 Education Act, allowed public high schools to opt out of the local school authority and become independent 'Grant Maintained' (GM) schools, funded directly by the central government. GM schools were granted a large measure of autonomy, including ownership of all school facilities and complete control over staffing. All that was required to achieve GM status was a majority vote among the parents of current students. Between 1988 and 1997, 1082 such elections were held at 950 high schools (approximately one quarter of all English secondary schools); roughly two-thirds were successful. Other reforms of this period, including open enrollment, per-pupil funding and the publication of 'League Tables' of school performance, ensured that both GM and non-GM schools were operating in a competitive education market. Commenting on the GM school reforms, Chubb and Moe called them 'truly revolutionary' (Chubb and Moe (1992)).

In this paper, I use school-level data on standardised exam outcomes to measure the impacts of GM status on student achievement at the schools that became GM, at neighbouring schools, and at the district level. Following recent work on US Congressional elections (Lee (2003)) and union recognition elections (DiNardo and Lee (2004)), I use a regression discontinuity design to compare student achievement levels at schools where the GM vote barely won to those where the vote barely lost. Whereas a direct comparison of those schools that opted out versus those that did not might be biased by unobservable characteristics underlying the opt out decision, if we believe that the distribution of these characteristics is continuous at the 50% win threshold, this comparison of barely winners to barely losers will provide a valid estimate of the causal impact of GM status, at least for schools with an evenly divided electorate.[3][4] I use this same idea to compare student achievement at neighbouring schools of barely winners to neighbouring schools of barely losers to estimate the competition or spillover effects associated with GM status.[5]

Though informative, these comparisons may be biased if the better students within a neighbourhood migrate to the local GM school. I address this concern in two ways. First I look for trends in impacts in the period following the election, since between-school mobility is likely to

[3] In fact we follow Lee (2003) and DiNardo and Lee (2004) and make use of schools with votes away from the 50% win threshold (typically in the (15,85) interval). These are regression-adjusted for the vote share achieved, and so can still be given a quasi-experimental interpretation to the extent that the regression adjustment is specified correctly. There is in general a trade-off between the lack of precision involved in using only close elections and the possibility of mis-specification associated with using votes further away from the win threshold.

[4] This is, to my knowledge, the first nationally representative evaluation of GM schools although Levacic and Hardman (1999) provide a careful and in-depth study of GM schools in six Local Education Authorities. Based on cross-sectional data they find that GM is associated with higher exam results, but that this effect disappears once student socio-economic status is controlled for.

[5] In other UK-focused research Bradley, Crouchley, Millington, and Taylor (2000) show that change in school test scores is positively correlated with change in neighbour-school test scores and interpret this correlation as a competition effect. Levacic (2004) tests for the impact of self-reported competition measures, finding positive effects of having five or more perceived competitors in cross-section and first differenced models of performance.

occur slowly. Second, I aggregate data for all schools in a district, and examine differences in district-level outcomes as the vote shares in favour of GM status pass the 50% threshold at one school, two schools, three schools and so on.

My results point to two main conclusions. First, there were large gains to schools that become grant-maintained. By my estimation the schools that won a vote and became GM enjoyed increased exam pass rates on the order of one quarter of a standard deviation. These gains are unlikely to be driven by changing student-body composition (cream-skimming), since results improved for students already enrolled in school at the time of the vote. In contrast, medium-run effects four or more years after the vote could have been influenced by student mobility - I find post-vote enrollment increases of between 5% and 10%. From the perspective of the schools losing students these are important: Hoxby (2003) argues that schools must be in danger of losing 5% of enrollment before competitive responses can be expected. However, based on my analysis, I argue that changes in student enrollment across neighbourhood schools can only explain a part of even the medium-run gains to GM status.

My second main conclusion is that the grant-maintained system did not create strong competition or spillover effects on student achievement in neighbouring schools. I experiment with several definitions of the neighbourhood surrounding a voting school, but can nearly always rule out an effect greater than one half of the student achievement effect within the GM schools themselves, and can never reject the hypothesis that there was no effect at all. When I aggregate data to the district level, my estimates are consistent with an 'own-school' effect only.

I interpret these results as providing cautious support for policies that increase school autonomy. Support for autonomy is hedged by the fact that GM schools were given additional funding. More generally, I do not know precisely what these schools did to improve outcomes. With regard to market competition, the results suggest caution as to the likely benefits. Whilst new school types such as charter and voucher schools may benefit the students who attend them, my results suggest spillover effects may be much smaller. I find it significant that I cannot find large effects even in a system providing schools with clear incentives to maximise achievement (via the performance 'League Tables'). Whereas studies of school competition in other countries interpret small effects as evidence that schools compete along dimensions that do not affect achievement, my results are consistent with schools pursuing objectives other than rent-maximisation.[6] Against

[6] Hsieh and Urquiola (2003) argue that schools emerging after a large-scale voucher reform in Chile marketed themselves with smart uniforms and English names but had no impact on school performance. Fiske and Ladd (2000) make a similar point in relation to an open enrollment reform in New Zealand, and Ladd (2002) draws the more general conclusion that competition will work best when schools compete on a 'level playing field'. Her emphasis on peer groups is shared with Rothstein (2004b), who tests and rejects an implication of parents choosing school districts on the basis of effectiveness ('Tiebout choice'). He interprets the results as suggestive of parents

this, it should however be stressed that I cannot reject the hypothesis of small but economically important effects, nor the suggestion that competitive responses were stifled under the old regime in which non-GM schools operated.

2 Education Reform in the UK

In this section I describe in greater detail the UK's 1988 Education Act, upon which this analysis is based. In addition, as an introduction to the institutional factors that this reform was developed to address, I begin with a brief history of secondary education in England.

2.1 English Secondary Education, 1902-1988

The first English schools were established by the Church and voluntary associations, but were gradually taken over, expanded and unified by local government.[7] The most important milestones for the English education system over the first half of the 20th century were the 1902 Education Act, which established Local Education Authorities (branches of local government that assumed responsibility for compulsory education), and the 1944 Education Act, which expanded LEA responsibilities to church schools.[8]

Post-war, the main debate in British education policy was on 'selection' or 'tracking' issues. Traditionally, children were segregated into different types of secondary school based on their results on the 'eleven plus', an I.Q. test taken at the end of primary school (age 11). Those that passed were sent to a 'grammar' school; the remainder went to the local 'modern' school. In 1965, however, the then Labour Government ruled that LEAs must move to a system of 'comprehensive' schools, in which children of all abilities would be educated together.[9] Although the transition was slow, by 1988 only a handful of 'selective' LEAs remained (out of more than 100), with a few grammar schools surviving in non-selective areas. LEAs that remain selective continue to segregate students on the basis of an eleven-plus test; grammar schools that survive in non-selective LEAs select students using their own version of the test.

Several types of comprehensive school were adopted. The most common was the 'all-through' school which took children from ages 11 to 16 (the compulsory school leaving age since 1972), and

choosing districts for peer groups, and challenges the Hoxby (2000) finding that Tiebout choice can increase student achievement (Rothstein (2004a))

[7] See Timmins (2001) for a more detailed account.

[8] The 1944 Act formally established three types of secondary school: County schools (controlled by LEAs), Voluntary Controlled schools (typically Church of England, with land ownership retained by the church) and Voluntary Aided schools (typically Roman Catholic, with land ownership and control over admissions and teaching staff retained by the Diocese).

[9] This did not preclude students being tracked within a school.

allowed them to continue to age 18 if they wished ('A' levels). Other types included schools that took children from age 11 to 16 but required them to transfer to separate 'sixth form colleges' if they wished to continue their education, and combinations of middle and upper schools, in which children attended the former from ages 9 to 13 and the latter from ages 13 to 18 (if they chose to stay beyond 16).

An important feature of the English education structure that has remained largely unchanged over the second half of the postwar period is the school examinations system ('O' Levels and 'A' Levels).[10] Under this system, all students in their final year of compulsory education (grade 11, age 15) sit standardised national examinations in a range of subjects (typically from five to ten) studied in their final two years of compulsory education. Since these are standardised national examinations (and are externally graded on a scale from 'A' to 'G') they provide a well-understood and accepted measure of UK educational outcomes. Traditionally, students were required to pass five or more exams to continue into post-compulsory education and the school performance 'League Tables' - published annually since 1992 - aggregate this information to calculate a school-level 'pass rate', the proportion of grade 11 students achieving this level of attainment. In the empirical analysis that follows I use this school pass rate as my main outcome variable.

2.2 1988 Education Act: Choice and Competition

The 1988 Education Act contained four important 'choice' reforms.[11] The first, 'more open enrolment', was designed to increase parental choice by forcing LEAs to accept all applicants to a given school up to a school-specific capacity level (the 'standard number') defined as enrollment in 1979.[12] The second reform, per-pupil funding or 'money follows pupils', aimed to link school popularity to school revenue by requiring LEAs to distribute at least 75% of their schools budget according to (weighted) pupil numbers.[13]

The third reform was an increase in the autonomy enjoyed by LEA maintained schools, both financial and managerial.[14] Financial autonomy involved a greater say in how the budget was

---

[10] The system was restructured in 1988, when the 'O Levels' were replaced with 'GCSEs'. These emphasised coursework at the expense of final examinations, and the proportion of students attaining five or more passes at grade C or above rose sharply in the years that followed.

[11] The Act contained other elements, including the introduction of the National Curriculum and a new system of national tests for children at ages 7, 11 and 14. See Leonard (1988) for more detail.

[12] Parental choice or 'open enrolment' had been established by the 1980 Education Act, but the Government felt LEAs were frustrating choice by imposing artificial ceilings on the capacity of some schools to ensure the survival of others.

[13] The Government accused LEAs of subsidising unpopular schools in the name of helping schools with low SES compositions.

[14] The 1988 Act also established 'Grant-Maintained' schools, schools outside of the LEA system, hence the distinction of LEA-maintained schools. See section 2.3 for greater detail.

spent, with school-level administrators given more freedom to move money across budget categories. Managerial autonomy meant some control over the deployment of school staff, although the LEA remained the teachers' formal employer, and so the extent of this freedom depended on LEA cooperation.[15] This gave schools more power than they had enjoyed before 1988, but their Governing Bodies were still dominated by LEA representatives and the LEA still exercised a great deal of control over most aspects of school conduct. Indeed, it was frustration with their inability to reform the existing system of governance that inspired the Thatcher government's fourth choice reform, which allowed schools to opt out of this system altogether.[16]

2.3 1988 Education Act: Grant Maintained Schools

The fourth reform - 'opting out' - established a new type of school - 'Grant Maintained' - that would be funded centrally ('maintained by a grant from the Secretary of State') and owned and managed by the school's Governing Body. Hence Grant Maintained (GM) schools would no longer be under the jurisdiction of the LEA.

Consistent with the Government's goal of increasing efficiency in the public education system, the 1988 reform gave GM schools control over staff contracts, allowing them to dismiss as necessary, and pay teaching and non-teaching staff as they wished.[17] The Governors appointed the Head Teacher but lower-level appointments were typically made by the Head.[18] The reform also gave schools ownership of school buildings and grounds, and School Governors were free to make alterations to school premises and write contracts with outside organisations for the use of premises.

GM schools became their own admissions authority. This mean that parents wishing to enrol their children at a GM school would apply directly to the school, and not to the LEA. Schools applying for GM status had to include in their proposal a statement of their admissions policy, and were required to publish this on an annual basis.[19]

---

[15] Voluntary Aided schools were not affected by these reforms, since they had similar powers under the 1944 Act.

[16] Chubb and Moe (1992) and Bush, Coleman, and Glover (1993) document some of the ways in which school policy was constrained by the LEA. The former emphasize the hiring and firing of teachers, quoting one Head as saying the LEA insisted on placing all teaching advertisements in 'ethnic journals', and another claiming the LEA refused to dismiss any teaching staff. Associated with the GM reform described below was an explicit grant designed to help with 'Redundancy and Restructuring'.

[17] The prospect of these changing conditions were a driving element in teaching unions' fierce opposition to GM, as was the fact that they had no special role in the opting-out process (described below) and the possibility that unions would not be recognised in the GM schools. Case study evidence suggests practice in this respect was mixed, with some schools formally recognising unions, others granting unofficial recognition and others establishing staff forums. With regards to pay, a survey conducted by one teaching union showed the basic conditions unchanged but with GM schools more inclined to make bonus payments and use part-time and fixed-term contracts (Thompson (1992)).

[18] There is some anecdotal evidence of a high degree of staff movement in some schools at the time of opting out, perhaps associated with GM-opposed teachers leaving voluntarily (or involuntarily).

[19] This was expected to include a 'standard number' and details of the over-subscription criteria. Schools were

Funding for GM schools, the 'Annual Maintenance Grant', consisted of two elements: revenue funding to replace what would otherwise have been received under the LEA formula, and an element to cover the costs of LEA-provided 'central services' such as school meals and special needs support. This second factor was based on the fraction of the LEA education budget not distributed to ('held back' from) schools, and averaged around 16% of the first. By most calculations, not all of this extra money was needed to replace LEA central services, so that GM status was associated with up to 10% extra funding.[20] GM schools would also receive capital formula funding based partly on pupil numbers, and could apply to the Secretary of State to have major capital works funded by the Government. This gave was a clear advantage over LEA schools, which relied on the LEA for capital funding.

The Government wished to avoid GM schools being seen as new private schools. First, GM schools could not charge fees. Along these same lines, they had to follow a complicated procedure, culminating in the Secretary of State's approval, if they wished to become selective, change their age range, expand their premises, or close. Also unlike private schools, GM schools had to comply with the National Curriculum and the associated assessment and testing procedure. Table 1a contrasts the main characteristics of GM schools with those of LEA-maintained schools.

The 1988 reform established three steps required to achieve Grant Maintained status. First, eligible schools had to pass two resolutions (later changed to one) of the school Governing Body proposing that an election on GM status be held.[21] Second, the schools had to win a simple majority, amongst the parents of its existing students, in favour of opting out.[22] Lastly, the school Governing Body had to submit and have accepted a proposal to the Secretary of State GM status, and successfully submit GM proposals to the Secretary of State. The criteria for acceptance were the school's viability and the capacity of the Governing Body to manage it. The small minority of schools (approximately 5%) whose proposals were rejected are thought to have been marked for closure.[23]

also required to publish an annual statement of the policy.

[20] GM schools also received 'special purposes grants' to compensate for the extra costs associated with leaving the LEA. These included grants for teacher training, building insurance, redundancy and restructuring and VAT. 'Redundancy and restructuring' paid the cost of redunancy (or early retirement) in the first year of a GM school, where this was 'shown to assist in the operation of the school'. They also received transitional grants that were in theory designed to smooth the changeover process. Bush, Coleman, and Glover (1993) found these were typically used to cover the costs of employing a school bursar or investing in new technology.

[21] Schools eligible to apply for GM status included all LEA secondary schools and primary schools with more than 300 pupils, although this lower limit was removed in 1990. A school was not eligible if LEA proposals to close the school had been approved by the Secretary of State.

[22] Parents voted in a secret postal election. A simple majority in favour was required, although a second ballot was needed if first-election turnout was less than 50%. The second election result was binding, but schools that failed to achieve a majority at the first attempt could have a second try after after waiting for one year.

[23] We do not have detailed information on why school proposals were rejcted, although conversations with the Director of the GM Trust confirm that closure is the only possible reason.

After the proposal was accepted, a GM school became fully operational when the Instrument and Articles of Government came into force. The Instrument was the constitution of the school Governing Body, which set out its composition and how it would operate.[24] The Articles detailed the restrictions on school policy described above. Unless otherwise stated in the Act, the Instrument or the Articles, the Governing Body had the power to do 'anything which appears to them to be necessary or expedient for the purpose of, or in connection with, the conduct of the school'.

2.4 Developments Post-1988

Although the main opposition party (the Labour Party) was strongly opposed to the development of GM schools in 1988, by the time they regained power in 1997 their position had shifted. While they blocked any more schools from opting out of the LEA system and promised to end the GM-LEA funding disparity, they allowed existing GM schools to keep their extra powers, redefined as 'Foundation' schools.[25]

With regard to school choice, the Labour Party policy has again been one of softening the edges, whilst leaving the fundamental pieces of the 1988 Act intact.[26] In other areas, policy has continued in the same direction, often at a faster pace. In particular, school 'League Tables' have been expanded to include more information, the school inspection system (redesigned in 1993 to give inspectors more powers) has been further strengthened, and LEAs have been forced to delegate a larger proportion of their schools budget directly to schools. Table 1b summarises the important events post-1988.

3 Empirical Framework

In this section I describe the strategy used to identify the effects of GM status on student achievement at the schools that become grant-maintained, at neighbouring schools and at the district level. To fix ideas, I begin with a discussion of the theoretical framework behind school autonomy and between-school competition.

[24] A typical Governing Body was composed of the Head Teacher, one teacher governor, five parent governors and eight governors appointed by the governing body at the time GM proposals were submitted. Regrading its operation, statutory committees were required to deal with appeals against admissions and with staff disciplinary matters, but other committees formed would typically include finance, premises, education/curriculum, staff and admissions.

[25] Had the new funding regime (introduced in 1999 as part of the 1998 School Stanards and Framework Act) been rigorously applied, many GM schools would have experienced real funding declines. Instead, transitional protection arrangements were designed to cushion a blow that was further softened by a substantially increased national education budget.

[26] By, for example, requiring LEAs to coordinate the application deadlines for LEA and Foundation schools.

A complete theoretical model of autonomy and competition would require three basic ingredients: parents choosing schools, schools choosing 'behaviour' and the effects of school 'behaviour' on student achievement potentially being influenced by the degree of school autonomy. The last ingredient is necessary if, as advocates of autonomy claim, local authority control lowers school effectiveness. The first and second - parental choice and endogenous school 'behaviour' - are important since it is the threat of parental exit and the effect this has on school behaviour that underpins the case for competition (although 'voice' effects may also operate (Hirschman (1970)). [27]

I know of no models that treat all of these issues together. As Hoxby (2003) notes, the literature has traditionally viewed schools from a public finance perspective. As such, the typical assumption is that schools are equally effective (given equal peer group quality) or that school effectiveness varies but is determined outside of the model.[28] The few papers that model school behaviour directly have taken a numerical approach to the calculation of equilibria (Manski (1992)) or make assumptions that simplify the school choice problem (McMillan (2004) assumes no peer effects).[29]

In what follows, I outline the demand side model closest in spirit to English practice (Epple and Romano (2000)). Ignoring issues of existence and uniqueness of equilibrium, I consider how school behaviour could, in principle, be endogenised in a way that allowed its effects on student achievement to be influenced by school autonomy. I then consider the impact of a GM school on student achievement in this framework, making clear which variables are endogenous and which are exogenous. This serves as a useful backdrop to my discussion of the identification strategy. I follow McMillan (2004) in equating school behaviour with a school effort choice, but this need not be interpreted literally: the important assumption is that schools can take actions to increase levels of student achievement, but that these actions are costly to the school.[30]

[27] The assumption that exit threats drive administrators to increase effort in response to competition is the standard industrial organisation approach to competition. Besley and Ghatak (2003) argue that when agents (for example Head Teachers, teachers, parents) have preferences over the 'mission' of an organisation, competition can also improve efficiency by improving the match of providers (schools) to employees (teachers) and customers (parents) to providers (schools). Preferences of this kind are clearly relevant in education, although the English secondary school context - via the National Curriculum and the Performance Tables - may constrain the degree to which they can be realised.

[28] In the first category are Epple and Romano (2000), Epple and Romano (1998) and Nechyba (2000); in the latter is Rothstein (2004b).

[29] Since models of school choice with peer effects are often characterised by multiple equilbria or the potential for no equilibria (e.g. Epple and Romano (2000), Rothstein (2004b)), it is perhaps not surprising that schools have been viewed as passive. The intuition is that a parameter that ought to make a neighbourhood (or school) less attractive can induce the exit of low-income households, driving up average peer quality and making the neighbourhood more attractive. The mechanism is similar to the 'unravelling' that can occur in adverse selection models.

[30] McMillan (2004) uses effort to describe school actions; Manski (1992) uses expenditure. As examples of school policy that influence achievement, Chubb and Moe (1990) give school discipline, homework and tracking, although it should be stresssed that there is little evidence as to the efficacy of these types of policy. See for example Betts and Shkolnik (2000)).

3.1 Theoretical Framework: Choice, Competition and Autonomy

Consider a school market in which parents choose schools based in part on effectiveness and schools choose effort to improve effectiveness and attract students.

Demand (School Choice): Epple and Romano (2000)

Epple and Romano (2000) take school effectiveness as given, and model housing and school choices under three sets of school enrollment rules: neighbourhood enrollment (children must attend the local school), frictionless school choice (children can attend any school) and school choice with frictions (transport costs), the assumption closest to practice in England. In each case, single-child households are assumed to trade student achievement off against household income (net of housing costs and home-to-school transport costs) and the model allows student achievement to depend on the quality of the school peer group.[31] Equilibrium exists when the housing market clears and when no household wishes to deviate from the housing/schooling choice. Assuming only two schools, Epple and Romano (2000) show that equilibrium is characterised by a threshold income level such that households with income above this level attend one school with the rest attending the other.[32]

Adopting my own notation and a linear functional form for test scores, the demand side can be summarised as:

$$U_i = U(T_{ij}, x_i) \tag{1}$$

and

$$T_{ij} = \beta_0 + \beta_1 x_i + \beta_2 \overline{x_j} + \varepsilon_{ij} \tag{2}$$

where i refers to students, $j = \{1, 2\}$ to the two schools and T is student achievement. Household socio-economic status (SES) is denoted by x_i, and I equate SES with net income and student ability so that households differ in only one dimension.[33] The school peer group $\overline{x_i}$ is simply the school-average SES level.

Supply (School Effort)

[31] The model is more general since households also vote on local property tax levels.

[32] In this school choice (with frictions) equilibrium, households do not care about residence choice except insofar as it affects home-to-school transport costs. Hence house prices adjust to offset transport costs. Equilibrium may not exist however, for the reasons explained above.

[33] The setup in Epple and Romano (2000) is more general, since household income and student ability are assumed to be jointly distributed, and different assumptions are made about the properties of this joint distribution.

To allow for the endogenous determination of school effectiveness, the model could be extended to two stages. Schools would choose effectiveness levels at the first stage and parents would observe effectiveness and choose houses and schools in the second. Since this would require an assumption about school preferences, a natural assumption in the English context would be that school profit or utility weighs the benefits of increased effort (increased enrollment via increased achievement) against the direct costs of effort:[34]

$$\Pi_j = E_j(e_j) - C(e_j, E_j(e_j)) \tag{3}$$

For schools to have incentives to exert effort, the achievement production function must be augmented to include a term in school effectiveness. To make clear the possible link between effort, autonomy and achievement, I allow for a possible complementarity between effort and autonomy. Since resources are an issue with GM schools, I also allow effectiveness to depend on resources, with autonomy mediating the link to achievement. Hence:

$$T_{ij} = \beta_0 + \beta_1 x_i + \beta_2 \overline{x_j} + \beta_3(a)_j(e_j + r_j) + \varepsilon_{ij} \tag{2'}$$

where e_j represents effort, r_j represents resources and $\beta_3(a)_j$ may be increasing in autonomy (a). The combined effect of effort and resources mediated by $\beta_3(a)$ is what is thought of as school effectiveness.

Pre-GM Equilibrium

The complete two-stage model would be described by equations (1), (2)' and (3), and the equilibrium concept would be extended such that schools did not wish to deviate from chosen levels of effectiveness. The exogenous variables in the model are those embedded in the achievement production function (such as autonomy and resources), the school profit function (effort costs) and the household utility function (income and transport costs). The endogenous variables of interest are school effort, school enrollment levels, SES composition and school test scores. Assuming existence and uniqueness of equilibrium, a pre-GM world will be characterised

[34] In Manski (1992), the variable chosen by schools is expenditure and the (monopoly) public sector competes with a competitive private sector. As a function of per-pupil expenditure e, utility is:

$$U = (v - e)E(e)$$

where E(.) is enrollment and v is per-pupil expenditure received from the school district (say). But this could be rewritten as:

$$U(e) = vE(e) - eE(e)$$

or $U(e) = E(e) - C(e, E(e))$ where e is interpreted as effort.

by $(e_1^*, e_2^*, \overline{x_1}^*, \overline{x_2}^*, E_1^*, E_2^*, T_1^*, T_2^*)$ where the subscripts denote schools 1 and 2.

Grant Maintained Schools

To imagine how the equilibrium might change were one of these schools to become GM, consider how the presence of a GM school would affect equations (1), (2)', and (3). Although parents may value GM for its own sake, the more plausible assumption is that it is only valued to the extent it raises achievement. Hence (1) remains unchanged.

On the school side, GM schools may have a production function that differs from the regular production function in two ways. First, since GM schools are autonomous, they may have a higher level of β_3. Second, GM schools received more resources. I model these dependencies by supposing that:

$$\begin{aligned} \beta_3(a)_j &= \beta_3 + a g_j \\ r_j &= r g_j \end{aligned}$$

where g_j is 1 for GM schools and zero otherwise. Substituting into (2') gives:

$$T_{ij} = \beta_0 + \beta_1 x_i + \beta_2 \overline{x_j} + \beta_3 (e_j + r g_j) + a(e_j + r) g_j + \varepsilon_{ij} \qquad (2")$$

A final effect may operate via the cost of school effort, if the increased accountability requirements of GM increase the absolute cost of effort, but decrease the marginal cost.

After the GM policy is introduced and some schools acquire the new production function, equilibrium effort will change, and SES, enrollment and test scores with it. From a social welfare perspective, the most important question is what happens to effort levels.[35] In other words, assuming two schools and that school 2 becomes GM, what are the signs of $\frac{de_2}{dg_2}$ and $\frac{de_1}{dg_2}$? Proponents of autonomy would argue that $a > 0$ and that GM schools respond with increased effort ($\frac{de_2}{dg_2} > 0$). Supporters of competition rest their case on the prediction that GM-induced pressure will force neighbouring schools to improve ($\frac{de_1}{dg_2} > 0$).

The second effect seems straightforward, although McMillan (2004) presents a model in which competition can reduce effort.[36] The first is likely to depend on the channels though which GM status operates. For example, if GM increases resources, GM schools may maintain pass rates with

[35] This assumes that a social planner has no concern for equity, no concern for the costs of school effort and no concern for transport costs. As shown by Hsieh & Urquilo (2003) it also relies on the assumption of linear peer effects, so that the allocation of children to schools does not affect aggregate scores.
[36] The key assumption is that the school market is imperfectly competitive, so that competition can induce schools to reduce effort, with these schools enjoying larger rents on fewer students.

lower levels of effort. On the other hand, when GM and autonomy increase the 'return' to effort, a positive effort response seems more likely. Since the direction and magnitude of both autonomy and competition effects are an empirical question, I turn next to my strategy for estimating the impact of GM status.

3.2 Estimation Strategy

Taking this theoretical discussion as a starting point, I now outline my strategy for identify the effects of GM status. Starting with equation (2") and aggregating up to the school level gives us a school level test score of:

$$\overline{T}_j = \beta_0 + (\beta_1 + \beta_2)\overline{x_j} + \beta_3(e_j + rg_j) + a(e_j + r)g_j + v_j$$

Dropping school subscripts, comparing a post-GM world with some GM schools to a pre-GM world with no GM schools and taking differences within schools gives:

$$\Delta\overline{T} = (\beta_1 + \beta_2)\Delta\overline{x} + \beta_3\Delta e + [(\beta_3 + a)r + a\Delta e]g + \Delta v \tag{4}$$

where I assume that first-stage effort is zero ($e_0 = 0$) without loss of generality. To allow for effort responses to GM on the part of the own school and the neighbour school an auxiliary regression can be specified:

$$\Delta e = \rho_0 + \rho_1 g + \rho_2 g_{-i} + \rho_3 Z + \xi \tag{5}$$

where Z represents market-level variables such as transport costs and g_{-i} is a dummy variable that indicates whether the other school became grant-maintained.

What are the parameters of interest here? There are three parameters of potential interest: one associated with the impact of autonomy in the education production function (a) and two associated with the impact of GM on school effort (ρ_1 and ρ_2). Exactly what can be identified in this context depends on how GM is assigned and what can be observed. For example, if GM were randomly assigned, and we had data on effort and resources, all schools could be pooled and (4) and (5) estimated by least squares. Since GM is not randomly assigned, and since neither effort nor resources are observed, I follow a three-step estimation strategy that looks first at the own-school impact of GM, second at the impact of GM on neighbour schools and third at the impact of GM on market-level scores.

3.2.1 Impact of GM Status on the Schools that become Grant-Maintained

Suppose that GM was randomly assigned, then (5) can be rewritten as:

$$\Delta e = \rho_0 + \rho_1 g + w \tag{5'}$$

where g_{-i} and Z are subsumed in the error term w, which is mean-independent of g. Substituting (5') into (4) gives:

$$\begin{aligned}
\Delta\overline{T} &= (\beta_1+\beta_2)\Delta\overline{x} + \beta_3(\rho_0+\rho_1 g + w) + [(\beta_3+a)r + a(\rho_0+\rho_1+w)]g_j + \Delta v \\
&= (\beta_1+\beta_2)\Delta\overline{x} + \beta_3(\rho_0+w) + [(\beta_3+a)r + a(\rho_0+w) + (\beta_3+a)\rho_1]g_j + \Delta v \\
&= \gamma_0 + \gamma_1\Delta\overline{x} + \gamma_2 g + \xi \qquad (6)
\end{aligned}$$

The coefficient γ_2 identifies a combination of three GM effects: the effect of increased autonomy given no change in resources and no effort response ($a(\rho_0 + w)$), the effect of increased resources given no effort response ($(\beta_3 + a)r$), and the effect of any effort response ($(\beta_3 + a)\rho_1$). Even under random assignment, these effects cannot be separately identified. Notice also that whilst equation (5) supposes a constant effect of GM status on effort, the theoretical framework described above could allow the effort response to depend on market-level factors Z such as the distribution of transport costs. This implies potentially heterogenous effects of GM status on student achievment.

When GM is assigned via a voluntary vote, as it was, GM status may no longer be mean-independent of the error term in (6) for standard reasons of sample selection. Suppose however that $E(\xi|V)$ is continuous in V at $V = 50$, where V is the vote share.[37] In other words, assume that changes in the omitted variables in equation (6) would have been the same regardless of whether the election was barely won or barely lost, or equivalently, that GM is randomly assigned conditional on the election being close (Lee (2003)). Then consistent estimates of γ_2 can be obtained from least squares estimation of the following model:

$$\Delta\overline{T} = \gamma_0 + \gamma_1\Delta\overline{x} + \gamma_2 g + g(V) + \xi \tag{6'}$$

where $g(V)$ is a function of the vote share.

Even under this assumption, identification is complicated by two further factors. First, the regression discontinuity design is a 'fuzzy' one, which is to say that obtaining more than 50% of the vote does not guarantee that a school is assigned the treatment. Rather, the coefficient identified

[37] The assumption of continuity in E(ξ|V) at V=50 - assumption (A1) in Hahn, Todd, and der Klaauw (2001).

by equation (3) is the reduced-form coefficient associated with winning the vote on achievement. Second, selection into the vote and possibly non-constant treatment effects imply that neither an average treatment effect (ATE) nor the effect of the treatment on the treated (GM) schools (TT) will be identified.

Without a model of the decision to hold a vote, the exact relationship between the ATE, the TT and the parameter identified by the above estimation strategy - the local average treatment effect (LATE) at the 50% threshold cannot be determined.[38] But supposing perceived school utility gains to GM status are increasing in test score improvements, and assuming the costs of GM can be represented by a single index of 'resistance', it seems likely that both the TT and the LATE will be larger than the ATE.[39] The correlation between gains and resistance will determine the relationship between LATE and TT: if resistance and gains are uncorrelated, schools at the 50% win threshold are those for whom the prospect of losing is outweighed by the large potential gains from GM, hence LATE exceeds TT. If resistance is (negatively) correlated with gains, schools with most to gain score vote shares well above the 50% threshold, hence TT exceeds LATE. The important point to take from all of this is that whilst we will be seeking to identify the causal impact of GM status on those schools at the vote win threshold, this need not be the same as the impact that would have been seen by schools that did not vote, or by schools away from the threshold.

3.2.2 Impact of GM Status on the Neighbours of GM Schools

To identify the impact of a school becoming GM on the pass rate of one or more neighbour schools, the above steps can be retraced, with own outcome, own SES and own unobserved error terms in equation (6') replaced by their neighbour-school equivalents:

$$\Delta T_{-i} = \mu_0 + \mu_1 \Delta x_{-i} + \mu_2 g_i + h(V_i) + \varsigma_{-i} \tag{7}$$

The identifying assumption, that $E(\varsigma_{-i}|V_i)$ is continuous at $V = 50$ - neighbour-school characteristics are randomly assigned conditional on a close election - is the analogue of the own-school version, and no new conceptual issues are introduced. Empirical issues relating to the definition of neighbourhoods are discussed when I present the results.

There are two channels though which own-GM can affect neighbour-school performance. Own-GM may encourage other schools to seek GM status, and/or it may encourage other schools to

[38] See Angrist (2004b) for a general discussion of heterogenous treatment effects.

[39] In other words, the schools that vote are those with the most to gain. This must be true unless gains and resistance are for some reason positively correlated.

exert more effort without a change of status. Since it turns out that there is no evidence for the first effect, it is interesting to note that when only the second effect operates, μ_2 will identify $\beta_3(0)\rho_2$.[40] This is the combined effect of an effort response and the mediation coefficient in the case in which schools operate under the old LEA regime. I return to this observation when interpreting the results.

3.2.3 Impact of GM Status at the District Level

Assuming unbiased estimates of the own-school and neighbour effects can be obtained, a market effect can be derived by taking a weighted sum of the two. There may however be a concern that I do not have sufficiently good proxies for SES composition to estimate either or both of these effects consistently. I can check this possibility by estimating the market-level impact of GM status. Assuming market-level SES remains unchanged over time, and assuming linear peer effects, changes in SES composition between schools do not affect market-level test scores, which will instead be a weighted sum of the 'true' own and neighbour school effects.

Since markets in my data will be characterised by the number of schools that become GM, I regress change in market-level pass rates on the number of vote wins. To control for non-random voting across markets, I can again use functions of the vote shares as controls. To see how this works at the market level, suppose there are two votes in every market. Provided the vote shares are known, it is also known whether there were zero, one or two wins. Further, I can condition on a function of both vote shares, and make an identifying assumption that conditional on two 'close' votes, none, one or two neighbour wins can be thought of as randomly assigned:

$$\Delta T_d = \phi_0 + \phi_1 1(\ Win(1)) + \phi_2 1(\ Win(2)) + f(V_1, V_2) + \varrho \tag{8}$$

where 1(.) is the indicator function and V_1 and V_2 are the two vote shares.[41]

Under these assumptions, the effects of one and two wins will identify a weighted average of the impact of GM status on exam results in the school(s) that became GM (γ_2 in equation (6')) and, in the case of only one win the impact on the other school (μ_2 in equation (7)). Under the

---

[40] Since the true model is still (4) and (5), for the non-GM schools we have:

$$\Delta T_{g=0} = (\beta_0 + \beta_1)\Delta\overline{x} + \beta_2(0)\Delta e + \varepsilon$$

and:

$$\Delta e_{g=0} = \rho_0 + \rho_2 g_{-i} + w$$

and the result comes from substituting the second equation into the first.

[41] Picture the function $f : R^2 \rightarrow R^1$ mapping the two vote shares to the number of wins. In the neighbourhood of {50,50}, the function will be discontinuous. For example, wins will equal zero at {49.9, 49.9} one at {49.9,50.1} and {50.1, 49.9} and two at {50.1,50.1}.

assumption that district-level SES does not change between the two periods, $\Delta \overline{x}_d = 0$ does not enter equation (8). When different numbers of votes are held across markets, dummy variables for the number of votes can be included, and interacted with the vote shares.[42] The idea can be extended to any number of votes, assuming that the effect of one GM win is the same for different numbers of GM votes.

4 Data

To assess the effects of autonomy and competition on the education market I construct a panel dataset of English secondary schools. My principal data source is the Annual School Census (ASC), which I have for secondary and 'middle-deemed-secondary' schools for the period 1975-2003.[43] Linking schools across Census years leaves me with an unbalanced panel; Appendix Table 1 describes some features of the panel over the main window of interest (1988 onwards). In addition to counts of students, the census provides my main proxy for SES composition, the proportion of students taking a Free School Meal (FSM).[44]

My primary measure for assessing the effects of the GM reform on school outcomes is the proportion of students, per school, who pass at least five GCSEs with grade C or higher. As mentioned above, from 1992 forward, every school with students taking these exams had its school 'pass rate' published in the annual 'League Tables' of school performance.[45] For my analysis I match the League Table results to the school-level census data.

I also link these data to the 'Index of Educational Establishments' (later 'Edubase'), the address file containing school postcode information. I use the postcodes to match schools to area-level information via the All-Fields Postcode Directory (August 2004 version).[46] Area-level data allows us to match schools to 1981, 1991 and 2001 Census data and the Local Elections Database.[47] Postcode information also allows me to define neighbourhoods around schools and place schools in local education districts or markets. Rather than use LEAs as districts, I define districts as Parliamentary Constituencies, the areas from which Members of Parliament are selected. For County LEAs, these typically correspond to sub-LEA units, although they have no such counterpart in

[42] Hence I interact whether there were two elections with the maximum and second-maximum vote; whether there were three elections with the maximum, second- and third-maximum vote and so on.

[43] Every year schools were required to submit census returns containing information on the numbers of pupils and teachers on a particular day in January. Over the 1980s and 1990s the Census developed to include more information.

[44] Students are eligible for FSM if their parents are entitled to certain types of state benefits.

[45] Most students take five to ten GCSEs at the end of their 11th year in school.

[46] I have a snapshot of this register in 2000, and current Edubase data on all open schools and the majority of recently-closed schools.

[47] The database includes all local election results from the mid-1970s to 1992 - see the UK Data Archive for details.

Metropolitan LEAs. As seen from Maps 1 and 2, LEAs are considerably larger than US school districts for example. As well as corresponding to well-defined educational administrative units (at least in County LEAs), Parliamentary Constituencies appear to correspond more closely to the idea of a school market (discussed below). For schools that remained open after 1993 I have complete postcode information; for those that closed before 1988 I have none. The information for the remaining schools in displayed in Panel C of Appendix Table 1.[48]

Data on the results of the Grant Maintained elections come from the 'GM schools database' held at the National Digital Archive of Datasets (NDAD). I have data on 1082 elections associated with 1030 GM attempts by 950 secondary schools. After dropping ballots with missing data (2) and schools that cannot be linked to the schools census (2) I am left with data for 946 schools. Although schools could make a second attempt if the first attempt failed (requiring a one year gap between attempts), I only use information from the first attempt. This minimises the risk of my results being distorted by behavioural responses to the loss.[49] For a given attempt, a second election (a few days after the first) was required if turnout in the first was less than 50%. Again to minimise behavioural responses, I use the results of the first election.

I use the GM schools database to link GM election data to the schools database. I define as my base GM year - the year I choose to represent the pre-treatment conditions - the last census year before the school became grant-maintained. Since the vote typically takes place two terms before GM status is achieved, if the 'treatment' begins before a school formally acquires GM powers (e.g. there is staff turnover), my base year data may not be a valid pre-treatment observation. To balance this, parents typically apply to schools one year before the date of the Census, so the student composition is already set,. Similarly, since my definition of baseline is driven by the census date, not the date, those schools acquiring GM status during this four-month period will have a 'baseline' outcome measure (the pass rate) that was in fact exposed to two or three months of GM status (because the Census is taken in mid-January, and exams are sat in mid-May). Assuming a positive GM impact however, this will only bias down my estimates of the impact of GM status on student achievement.

Starting in 2002, the Annual Schools Census expanded to include individual-level student data. This included basic demographic information which can be matched to results in GCSE exams and in national exams taken at earlier ages (for instance the Key Stage 2 tests taken at age 10 (grade six), typically the last year of primary school. Since this data is only available from 2002,

[48] Of the 39 schools unidentified in 1992, 25 are associated with a middle/upper school reorganisation in one LEA, Humberside.

[49] Suppose all schools with first attempt vote shares between 40% and 50% decide to have a second attempt, and exert just enough campaigning effort to put them over the 50% threshold. If we were to pool these schools with those requiring only one attempt, we would not expect comparability.

I cannot use it to for example assess the effects of GM status on particular subgroups of students. It is however useful in assessing the extent to which student-body composition changed as a result of GM status.

5 Descriptive Statistics

Before addressing the formal regression discontinuity tests of the effect of the 1988 GM reforms, I present some simple descriptive statistics that provide at least suggestive evidence supporting the claim that GM schools emerged into a market characterised by choice and competition and present some facts describing the evolution of GM schools.

5.1 School Choice and Competition

Over the period in which GM schools emerged (and still today), school choice meant only that LEAs had to allow parents to 'express a preference' for schools, so it is important to consider what this meant in practice. In practice some LEAs named the school that children were 'expected' to attend and allowed parents to opt out, whilst others allowed parents to choose schools from a list. I do not have figures for the early 1990s (when GM schools emerged), only for the mid-1980s and late 1990s. Stillman and Maychell (1986) finds that in the early 1980s, the ratio of LEAs offering 'catchment' to 'free choice' systems was roughly 50-50; Williams, Coldron, Stephenson, Logie, and Smith (2001) find closer to 80-20 by the late 1990s. Based on case study evidence it may be that most of the change occurred before the early 1990s, although it is also said that GM schools helped accelerate the move to choice.[50] The important point from my perspective is that parents did in the early 1990s play an important role in deciding which school their children attended. To the extent that GM schools were popular with parents, this would have important enrollment consequences for non-GM schools.

Another key factor driving the potential for school choice was cohort size; if schools were close to capacity there would be little flexibility to allow choice. Figure 1 presents data on cohort size, the number of 11 year olds enrolled in the English school system from 1975 to 2002. Although cohort size was increasing over the period in which GM schools began to emerge, cohort size was always lower than 1979 levels. Since 1989, school capacity has been defined in terms of 1979 enrollment, which implies a degree of spare capacity in the system throughout the period of

[50] Since GM enrolment was taken out of LEA hands, it could not be drawn from particular areas, making it harder to define catchment areas for neighbouring schools. It was also driven by the 'Greenwich judgment' of 1988, which clarified that LEAs could not use the fact that parents lived in a different LEA - and by extension a different catchment area - in their over-subscription criteria.

interest. Since number of students alone will not define average capacity if schools were closing over this period Figure 1 presents data on schools and school closures.[51] Even allowing for the possibility that exit is over-estimated, only a small fraction of schools closed each year, and I believe it is safe to assume that school closure is not a first-order consideration over the period I focus on. To complete the times series picture, Figure 1 presents data on school pass rates. From 1988 forward these increased sharply, due in part to a restructuring of the exam system in 1988.

Next consider evidence on how parents chose across schools. To see this at the school district level, Figure 2 plots enrolment (as reflected in spare capacity) against exam results within districts. The x-axis shows school-level 'pass rates' relative to its district mean; the y-axis shows enrollment relative to capacity. Each line is the quadratic fit of enrollment to capacity for a different time period: 1984 (before cohort size dropped sharply), 1989 (when cohort size was at its lowest) and 1998 (after cohort size had recovered to mid-1980s levels). In 1984 (the solid line) the relationship is relatively flat, suggesting that the high-achieving schools were no closer to capacity than low-achievers. Since the line is also close to the zero spare capacity line it suggests that schools were relatively full in 1984. By 1989 (dashed line) the relationship had shifted: the upward slope implies that high-achieving schools remained close to capacity whilst low-achieving schools were under-subscribed. This suggests that parents were shunning the low-achievers, who bore the brunt of the cohort size drop. The 1998 line shows the same relationship over most of the support, and the slight upward slope over the range of high achievement may be due to capacity constraints being more likely to bind in the highest-achieving schools.[52]

Direct measures of school competition are more difficult to find. Perhaps the best evidence comes from a survey of 227 schools in six LEAs conducted by Levacic (2004). This survey asked head teachers a number of questions relating to competition and cooperation between schools in their local education market. Almost 90% of respondents described the local market as being highly or fairly competitive.[53] Also striking is the proportion reporting an increase in parental awareness following the publication of the school 'League Tables' in 1992: 97% reported an increased awareness against only 3% reporting no change. Regarding the consequences of compe-

[51] It is important to realise that 'closure' refers to leaving the Census, which does not necessarily imply school closure. Schools may change their identifying number (and be observed to leave the census) as a result of any major reorganisation, for example from selective to non-selective admission, or from a middle (9-13) to a secondary-school (11-16) age structure.

[52] There are two caveats to this conclusion. First, the pass rate used in the quadratic fit is taken from 1992, the first year that performance tables are available. Second, I cannot rule out neighbourhod enrolment and population shifts within markets. Against this however, the same result holds when I use pre-1984 measures of the proportion of students staying in school beyond the compuslory school-leaving age (another potential measure of school quality), and I see no relationship between pass rates and ward-level population shifts in the 1981 and 1991 Censuses.

[53] When asked how 'how would you describe the degree of competition between local schools?', 41% report 'very competitive', 45% 'fairly competitive', 12% 'little competition' and only 1% 'no competition'.

tition for enrolment and school budgets, only 18% (enrollment) and 34% (budget) reported no change. Not surprising given these results, a majority (63%) reported an increased emphasis on exam results following the introduction of the League Tables with less than 10% reporting no change.

5.2 Grant Maintained Schools

Having discussed the environment into which GM schools emerged, I turn now to the GM schools themselves. From the perspective of my analysis of the effects of GM status, the main questions now are which kinds of school voted on GM status, and which kinds of schools became grant-maintained. Figure 3 charts GM elections by calendar year and school term. It is clear from the graph that GM take-up was slow in 1989 and 1990. Political opposition made opting out potentially costly. If Labour or Liberal Democrat parties gained political power in the next General Election, due before 1992, both promised to return GM schools to LEAs (and made thinly veiled threats of punishment).[54] Instead the Conservative party won this election in April 1992, and GM votes spiked in the following six months. The large number of votes in 1993 may be the tail end of this spike, or may be related to the 1993 Education Act which further reaffirmed the Government's commitment to GM schools.

The slowdown in voting after 1995 may also be explained by political uncertainty, although as noted above Labour Party policy toward the GM system had become less hostile by this stage. Perhaps more significantly, by 1995 more than one quarter of all secondary schools had voted on GM status, thus demand may have dropped by this time.

This statistic masks the uneven spread of grant-maintained schools across the country. Maps 1 and 2 show the distribution of GM schools, with LEAs mapped according to a Conservative party control index. The obvious correlation between GM status and Conservative control reflects the pervasive impact of local politics on the grant-maintained system. Yet local politics is not a complete explanation; even across LEAs with comparable levels of Conservative support, GM experiences differed.[55] Case study and anecdotal evidence suggests that other important factors at the LEA level may have been the attitudes of local Conservative politicians (who were typically less enthusiastic about GM than their national counterparts), and school perceptions of their relationship with the LEA.

[54] Bush et al (1993) find more than 80% of the first 100 GM schools citing 'vulnerability to political change' as the main disadvantage of being GM.

[55] For instance, Norfolk and Suffolk are neighbouring Counties, both rural, and both with similar levels of Conservative support, yet far more schools opted out in Norfolk.

GM Attempts

To shed light on which kinds of schools held GM votes, I construct a sample of schools at risk of becoming GM and estimate a series of linear probability models that control for LEA fixed effects.[56] Column 1 of Table 2 presents the results when only school-type dummies are included. As mentioned above, despite the move away from a 'selective' system based on the 'eleven plus' I.Q. test, a small number of selective LEAs survived, as did a small number of grammar schools in non-selective LEAs. These schools (the base category in Table 2) were by far the most likely to attempt to become GM, reflecting the difficult relationship between these schools and their LEAs (which were often opposed to the 'selective' principle and campaigned for their abolition).[57] Middle schools were less likely to vote. Since they are on average smaller, there may have been economies of scale concerns, given the fixed costs involved with becoming GM.[58] Religious (especially Voluntary Aided) schools were also more likely to attempt GM status, consistent with the sense that these schools were already somewhat detached from the LEA.

Adding neighbourhood characteristics in column 2, the results suggest interesting effects of neighbourhood composition and education market structure - schools in closer proximity to other schools (and perhaps more competitive markets) are more likely to vote - but the effects are quantitatively small.[59] Only when I add measures of 'school success' in columns 3 and 4 do coefficients increase. All measures of success - SES (percent FSM), spare capacity and recent enrollment changes - take the right sign (column 4), as do variables intended to capture success at the neighbourhood level (hence school relative success). Overall, school pass rates (which I have for the subset of these schools enrol grade 11 (age 15) students) are the best predictor of whether a school votes, but a one standard deviation increase in pass rates increases the probability of voting by only six percentage points.[60]

---

[56] The sample at risk are those schools in the schools census data in 1988 and 1989 (4049 and 3875 - see Appendix Table 1 and Appendix Table 2a). I lose observations when matching neighbourhood politics data (as a result of boundary changes), school capacity data (this is only available from 1992) and school pass rate data (available from 1992 for schools with students in grade 11). I make the requirement that the school be in the data in 1989 since a school not in existence in January 1989 was unlikely to have had the opportunity to become GM. I drop schools for which we do not have postcode information (103 - see Appendix Table 1), the single school that we cannot match to the 1991 census, and schools in the Isle of Wight and the Scilly Isles (22) since we do not have a full set of data for these. This leaves me with a base sample of 3747 schools.

[57] This is consistent with Modern schools (supported by the LEA) being less likely to vote than non-selective schools.

[58] This was the rationale behind initially limiting GM to primary schools with more than 300 pupils.

[59] The positive coefficient on Conservative support suggests that politics may be important even within an LEA. Distance to the nearest school in a different LEA is negative and significant, suggesting that schools close to an LEA border are more likely to try for GM. One explanation is that these schools feel less guilt about leaving an LEA when they already have other LEAs to deal with. The alternative explanation - that these areas are more competitive - is consistent with the findings regarding distance to the nearest LEA school and the number of neighbours.

[60] The pass rate measure is taken from 1992. This is not strictly pre-period, but between-school exam success is fairly stable over time, so this this should be a reasonable proxy for the 1988 pass rate that we would like to

All of this suggests that whilst more successful schools, schools in richer neighbourhoods, schools in Conservative areas and schools in more densely concentrated education markets were more likely to vote for GM status, all of these factors had only second-order influences on voting.[61] This is consistent with the survey evidence, which points to 'increased independence' and 'increased revenue' as the most commonly cited reasons for seeking GM status among eventual vote-winners (Bush, Coleman, and Glover (1993)). Similarly, the only evidence from schools that did not vote reports that non-voters considered GM status as running against the 'spirit of cooperation', and that they believed their LEAs were doing the best they could given the constraints they were operated under.[62] This and other case study and anecdotal evidence suggests that decisions on whether to run for GM status were determined in large part by the views and personality of the school Head Teacher.

GM Elections

For the schools that chose to vote on GM status, raw vote shares are plotted in Figure 4. Since I would expect schools to hold elections when success was more likely, it is not surprising that the majority of vote shares are greater than 50%. That there are any vote losers at all is consistent with a great deal of uncertainty surrounding the support for and opposition to GM status. The Bush, Coleman, and Glover (1993) survey find that even among schools that won the vote, the LEA had opposed the move in 76% of cases and more significantly perhaps, a majority of teachers supported the move in only 48% of cases.[63]

To check that vote winners became GM, Figure 5 plots the proportion of schools achieving GM status at the first GM attempt and the proportion that ever become GM (recall that schools that lose an election could hold another after a gap of more than one year). Schools are grouped into 5-vote intervals and I plot local averages amongst them. For example, the two points on the graph corresponding to a vote share of 45 are simple averages among schools receiving vote shares between 45% and 50%. I plot a cubic fit through the points on either side of the 50% (win) threshold to give a visual description of GM status as a function of vote share.

The graph shows that vote losers almost never became GM at the first attempt,[64] although a proportion of vote losers (increasing in vote share) did attain GM status after making a subsequent

include but do not have.

[61] Indeed, they cannot explain much more of the variation in GM voting than a model including only LEA dummies.

[62] This comes from a study reported by Fitz, Halpin, and Power (1993) that interviwed 32 Head Teachers that did not consider opting out.

[63] Bush, Coleman, and Glover (1993), Table 8.5.

[64] The two exceptions are where the election resulted in less than 50% turnout and a second election (held a few days later was won).

attempt. The overwhelming majority of vote winners become GM at the first attempt, and those who did not were schools that had GM proposals refused by the Government. The claim that these were nearly always due to close is consistent the large votes in favour and the fact that none become GM after a second attempt.

To inspect the correlation between vote share and measures of school success, Figure 6 plots baseline pass rates and Free School Meal (FSM) take-up, my best measure of SES composition. I plot separate graphs for all schools and all schools excluding grammar schools. It makes sense to separate grammar schools since their students are selected on the basis of an I.Q. test and they consequently have baseline pass rates in excess of 90% (compared with baseline pass rates of less than 40% for non-grammar schools). Grammar schools are dropped from most of the subsequent analysis.

In Figure 7 I look at enrolment. In the top part of the graph I plot (entry-level) capacity and enrolment in the base year, with the difference (spare capacity) graphed underneath. The existence of capacity constraints complicates any discussion of enrolment, but it is apparent from the graph that vote winners are on average smaller (in terms of capacity) and have lower levels of spare capacity.[65]

6 Empirical Results

I have described the system into which GM schools were introduced and presented descriptive statistics relating to the spread of GM schools. I now present my estimates of the impact of GM status. In keeping with the three-step estimation strategy discussed above - I assess the impact of GM status on schools that became grant-maintained, on their neighbours and at the district level - I present my results in three subsections.

6.1 Impact of GM Status on Schools that became Grant-Maintained

Starting with the impact of GM status on the schools that become grant-maintained, I first establish there are no differences in survival rates amongst vote winners and vote losers, since differential survival rates could cause sample selection problems. I argued above that school entry

[65] In Appendix Figure 2 I plot the distribution of baseline spare capacity. There are a significant fraction of schools with negative spare capacity. Two explanations are that capacity could in principle vary from year to year (although this does not seem common), whilst our capacity measure is a taken from a pair of years (1992 and 1993). More importantly, once a school reaches capacity, parents not accepted may appeal against the decision, and where the appeal is won must be admitted to the school regardless of capacity. In 1995/96, the first year for which national-level appeals data are available, 7,921 appeals were decided in favour of parents' out of a total of 34,860 appeals and 582,184 admissions. In 2000, the first year we have school-level appeals data in the Census (for GM and VA schools, both of which are their own admissions authority), successful appeals average around 20.

and exit is not a first-order consideration in the English context, and consistent with this fact there are not marked differences in survival between vote winners and vote losers (Appendix Figure 3). There are exceptions in the tails of the voting distribution, but again as noted above, schools with high vote shares may have been under threat of closure, whilst few schools received very low vote shares.

The main outcome of interest is school performance, as measured by the school 'pass rate'. Recall this is defined as the proportion of grade 11 (age 15) students passing five higher-grade GCSE exams, the traditional requirement for continuing in post-compulsory academic education. Figure 6 showed no obvious discontinuity in baseline pass rates, and so the question is whether vote winners enjoyed bigger pass rate improvements than vote losers. I consider pass-rate improvements in both the short-run (one to three years after the base year) and over the medium- to long-run (four or more years after the base year). The key difference between the two periods is that the short-run effects will reflect the exam performance of students *that were already enrolled in the voting school at the time of the vote*. It is highly unlikely (and I provide evidence on this point below) that any improvements in these years were driven by changes in student composition such as 'cream skimming'. Over the medium- to long-run improvements will reflect the exam performance of students *that enrolled in the voting school after the vote*. Over these years, changes in SES composition are more of a concern, and so I discuss pass rate improvements alongside evidence on enrollment and SES changes.

Short-Run Impact of GM Status on Schools that became Grant-Maintained

Figure 8 plots the difference between baseline pass rates and pass rates two years after the baseline year. The two-years later cohort was the first to take all of their GCSE courses in a GM environment.[66] I consider only vote shares in the (15,85) interval since we saw that schools outside of this interval have different baseline characteristics and are less likely to survive. I exclude grammar schools since their baseline pass rate is already above 90%, far higher than the non-grammar average (less than 40%). Since pass rate data are only available from 1992 and not every school enrols grade 11 students, sample sizes are smaller than the total number of elections.[67]

Figure 8 groups schools into 5-vote intervals and plots local averages amongst them. I connect points on either side of the 50% (win) threshold to give a visual description of pass rate trends as functions of vote share. In the upper part of the graph we plot baseline pass rates and pass rates

[66] GCSE courses (Key Stage 4) begin in grade 10 and finish in grade 11.

[67] A simple comparison of sample size and the number of voting schools exaggerates the difference, since the early elections resulted in dispoportionately high vote shares and involved a disproportionately high number of grammar schools.

two years later. Pass rates two years later are uniformly higher, reflecting the general increase in pass rates seen over the 1990s. Although there are no obvious trends by vote share, the gap between the two lines appears to widen at the 50% threshold, consistent with bigger improvements among vote-winners.

Larger improvements for vote winners are much clearer in the lower part of the graph, which plots the difference between the top two lines. At the 50% threshold there is a raw difference in pass rate improvement of around five percentage points, although this comparison is based on the small number of (around 50) schools on either side of the threshold. To make use of schools further away from the threshold, pass rate improvement can be regression-adjusted for vote share, and the difference between winners and losers compared. In Figure 8 I do this using a linear function of vote share interacted with a 'vote win' dummy. Although the raw data are somewhat 'bumpy', they do not suggest any particular non-linear form, and the fitted lines are relatively flat. The estimated difference is slightly less than four percentage points, but this needs to be scaled up by one divided by the probability of attaining GM status conditional on winning the vote (approximately 1.25 - see Figure 5) to derive the estimated impact of GM status. This gives an estimate of around five percentage points.

To check the robustness of these estimates and to place standard errors around them, Table 3a presents regression versions of Figure 8. Column 1 describes the mean improvement difference between winners and losers, column 2 adds a control for vote share, and column 3 interacts vote share with win (the specification used to generate the fitted lines in Figure 8). In column 4 I weight according to the size of the school exam-taking cohort, since I wish to give more weight to schools with more exam-takers (to the extent the model is correctly specified). In column 5 I use a quadratic vote share control function and the estimated impact of winning falls. This is not surprising given the degree of concavity in the (50,85) interval (Figure 8), although a cubic fit would be expected to pick up this shape (and the dip to the left of the 50% threshold). In the interests of parsimony, since the baseline levels are relatively flat and I have no priors regarding the appropriate functional form, I revert to the linear specification.

My preferred estimates are in column 6. In addition to vote share controls, these regression-adjust for change in base level pass rates and school SES composition (based on Free School Meal take-up). Although the point estimates are essentially unchanged, my estimates of the SES effects are correctly signed and statistically significant.[68] That schools with lower base levels are found to have greater improvements, suggests mean reversion or ceiling effects. For completeness, I present

[68] They suggest that a one percentage point change in the proportion of students taking up Free School Meals is associated with a 0.3 percentage point reduction in pass rates.

results from a model that puts grammar schools and schools outside of the (15,85) vote share interval back into the sample (column 7). Although the point estimate falls, I argued above that grammar schools had particular motives for seeking GM status and exceptionally high baseline pass rates, whilst many schools in the tails of the voting distribution were threatened with closure. Finally, column 8 estimates using least absolute deviations (median regression) to check results are not driven by outliers. Although the point estimate falls slightly, it still suggests a strong impact in the middle of the improvement distribution.

Relative to the standard deviation in baseline pass levels (approximately 15 - see Appendix Table 2a) these effects are substantial. Moreover, as argued above, they are unlikely to reflect changes in SES composition since grade 11 students taking exams two years after the base year were very likely to have enrolled in school in grade 7, well before the base year. To check this, Figure 9 plots the number of grade 11 (age 15) students two years after the base year (the exact number used in the pass rate denominator) against the number of grade 9 (age 13) students in the base year. As expected, the lines lie almost on top of one another.[69][70]

It is still possible that SES composition among exam takers changed whilst student numbers remained constant. But this would require that schools replaced low ability with high ability students in grades 10 and 11, and I know of no anecdotal evidence suggesting this occurred, despite a lot of anecdotal evidence suggestive of *post-vote* changes in *entry-grade* SES composition. Consistent with this, Figure 10 plots the change in school SES between the base year and two years later, and it is hard to detect any differences at the 50% threshold.[71] I conclude that the impact of winning the vote (and becoming GM) on school improvement two years after the base year is due mainly to changes in school effectiveness.

Figure 11a plots pass rate changes by vote share from one to three years after baseline. To guard against the possibility that connecting five-vote averages obscures changes very close to the 50% win threshold (amongst the group with vote shares of 48-50 for example), I use the same raw data but plot smoothed mean changes (bandwidth 0.2) on either side of the threshold.[72]

[69] We use grade 9 rather than grade 7 as a comparison, since for a fraction of (middle) schools grade 9 is the entry grade. We obtain similar results using grades 8 and 7 on the smaller sample of schools.

[70] We can use the pupil-level census data (for 2002) to provide a second check on the claim that post-entry mobility is low, since the data contain variables that allow us to determine the month and year that the student joined the school. Among all grade-11 students in 2002 enrolled in a secondary school with grade 7 entry (496,138 students), almost 84% joined in grade 7, with 3% (3%, 2%, 1%) starting in grade 8 (9 10 11). Figures for the voting schools that survive into the 2002 census are similar.

[71] This is particularly impressive, since the comparison may be distorted by changing SES composition among new entry cohorts (our SES measure refers to the whole school rather than individual cohorts).

[72] As a further test of the specification, Appendix Figure 4 estimates models based on a series of hypothetical discontinuities in vote shares from 15 to 85, and plots the t-ratio associated with the coefficient on the dummy for being on one side of the conjectured discontinuity or the other. Consistent with a true discontinuity at the 50% win threshold, the t-ratio is maximised at exactly 50%, and is roughly symmetric around it.

Regression-based estimates based on the same models are presented in Table 3b.

Looking at Figure 11a and Table 3b, the effect of winning the vote (hence the effect of becoming GM) appears to increase over the first three years after the vote win.[73] An obvious explanation is that as we get further from the base year, the grade 11 students taking exams have been exposed to more years of GM status. That said, improvement increases seem far from linear, perhaps because exams test material covered in the final two grades of school, so that returns to three or more GM years are diminishing.

Medium-Run Impact of GM status on Schools that become Grant-Maintained

Suppose, as argued above, that attaining GM status did not change the existing composition of students but may have changed *post-vote* entry enrollment and *post-vote* entry SES. In that case, whilst pass rates in the first three years after the base year will reflect the achievements of students in school at the time of the vote, pass rates four of more years after the base year will reflect the achievements of post-vote entrants. As such the estimated impact of GM status might be expected to increase over these years.

Figure 11b and Table 3c provide some evidence for an increased impact four or more years after the base year, but the increase is not a large one. Indeed, only seven years after the base year does the point estimate in my preferred column (6) exceed that estimated for the third year. To the extent that enrollment and SES changes were expected to further increase pass rates over the later years, this presents something of a puzzle.

To resolve this puzzle, I investigate enrollment changes in the first few years after the base year. To that end, Figure 12 plots entry-age enrollment change against vote share. The graphs are extremely 'bumpy', suggesting high variability in year-to-year enrollment. This may reflect true enrollment changes, but a part will reflect coding errors or the fact that entry enrollment can be zero for schools in the process of closing or restructuring. To avoid the second problem I take a sample of schools that survive for at least five years after the vote. To address the first I present regression versions of these graphs that estimate models by least absolute deviations (median regression) as well as OLS.[74]

Regression-based estimates are presented in Table 4. My preferred estimates adjust for vote year and school-type (columns (5) and (10)). OLS estimates suggest increased enrollment of around 10 students in the first few years after the vote, with the median regression estimates

[73] The estimates are obviously too imprecise to pass a formal test of this hypothesis.

[74] I also estimated models using a trimmed sample. Based on my reading of the raw data, I conjectured that trimming 2.5% of observations in either tail (roughly corresponding to year to year changes of more than 50 students) would eliminate coding errors. OLS estimates based on the trimmed sample are similar to those based on median regression.

pointing to slightly smaller effects. On a base of around 180 students, this is a change of roughly 5%. From the perspective of pass rate improvements, effects of this size are unlikely to be associated with an SES shift large enough to account for all of the medium-run GM impact. Even under the most extreme assumptions, they are unlikely to have contributed much more than one half of the change in pass rates estimated above.[75] Moreover, I find no evidence for SES changes of this size in the data: the measured change in SES between base year and six years after base year is small or non-existent,[76] and in case my proxy for change in SES (Free-School Meal take-up) is not a good one, for the schools that survive until the 2002 Schools Census I can also use the Pupil-Level Census to paint a more precise picture of student SES. I construct a single index of predicted exam performance based on a model that includes basic demographic information, test scores in grade six (pre-secondary school) and neighbourhood variables generated by matching pupil postcode (at the seven-digit level) to characteristics of the associated Census enumeration district,[77] but do not see any discontinuities between winners and losers at the 75th percentile, the mean or the 25th percentile (Figure 13).

To summarise, I found medium-run impacts in line with short-run impacts. Consistent with the absence of an increased impact over the later years, I also showed small enrollment changes in the earlier years and no obvious change in SES. An outstanding question is why enrollment changes were not more marked given such large achievement gains and the evidence presented earlier that parents were choosing schools over this period.

The first point to note is that we cannot give a straightforward demand interpretation to enrollment changes, since many schools were already operating at capacity before the vote. With reliable capacity data censored regression models of enrollment could be estimated, but my capacity measures are taken at a point in time, and the underlying model linking parental demand to enrollment is not straightforward.[78] I nevertheless estimate a series of censored regression

[75] Suppose an extra 10 students with pass rate 1 are added to a base cohort of 180 with pass rate 0.4. Then the pure compositional change in pass rate is 4.6 percentage points ($\frac{63+10}{180+10} - \frac{63}{180} = 0.034$).

[76] I do not present the graph, which looks similar to Figure 10.

[77] I do this in four steps. First I link PLASC and GCSE exam result data for the cohort of pupils taking exams in 2002. Second, I link these data to the grade-six scores of these pupils, giving us a sample of just under 500,000. Third, I use this sample to estimate models of GCSE attainment (measured as GCSE points scored) using as explanatory variables a full set of school dummy variables, demographic information in PLASC (Free School Meal eligibility, Special Educational Needs, Mother Tongue, twelve categories of ethnicity, gender and birth month) 1991 census-level variables (race, education, socio-economic group) and Key Stage 2 test scores (handwriting, writing, reading, spelling, three maths tests and three science tests). The schol dummies alone explain 21.2% of the individual-level variance, and the complete model 64.7% of the individual variance. School-level predicted points score explains 79.4% of the school level variances in mean points score. Finally, I use the model to predict a points score for students in schools (including those that have yet to take GCSEs).

[78] First, LEAs frequently operate systems in which the order of choice is listed as an over-subscription criterion (see Williams, Coldron, Stephenson, Logie, and Smith (2001)). Parents may therefore be unwilling to apply to their true first-choice school if there is a risk that the school is full, since they may then be denied access to their true second-choice school. This will not be the case when parents apply to a GM school since those are outside

models with the capacity data that I have, and obtain results slightly larger than those based on uncensored models.[79] The second point is that pass rates increased two to three years after the base year, so delayed enrollment effects might be expected if parents choose schools to maximise expected exam results. Other reasons for expecting larger responses in later years include parental uncertainty regarding the quality of new GM schools (understandable after a heated GM campaign and strong arguments aired against GM status), the time taken by new schools to manage their reputation and adjustment costs associated with enrolling children in schools other than those where older siblings are enrolled. Consistent with these considerations, the enrollment impact doubles to between 15 and 20 students four or more years after the base year (Table 4). This is slightly more than 10% of base enrollment and (scaled up by one divided by the probability of becoming GM conditional on winning a vote (1/0.8)) close to one 'form' of entry (around 30 students).[80]

6.2 Impact of GM Status on the Neighbours of GM Schools

I have shown that GM schools enjoyed large increases in pass rates. To see whether GM status was associated with spillover or competition effects - whether the reform was a tide that lifted all boats - I turn now to the impact of GM status on the neighbours of GM schools. As discussed above, the identification strategy is a variation on that used to identify effects on schools that become grant-maintained. Now however, rather than compare pass rate changes among schools that win and lose, I compare pass rate changes among *schools in the neighbourhood around schools that win and schools that lose.*

Clearly, the definition of the neighbourhood around a voting school is central to this part of the analysis. At a conceptual level, the relevant neighbourhood consists of all of those schools that *could* be affected by the presence of a nearby GM school. In a model of exit-driven competition in which parents choose place of residence as well as school, school B could be affected by GM school A if the structure of local employment is such that households working in the area can choose to live near either. This would be in the spirit of a 'Tiebout choice' model in which place of residence is chosen conditional on place of work. For example, households make a residence choice between different school districts in the same metropolitan area. In my case this definition would allow for

of the regular LEA system. Second, the appeals system adds a further degree of separation between demand and enrollment.

[79] The censored regression versions of column (5) generate estimates of 9.927 (5.927), 7.503 (6.707) and 20.144 (8.377) over the first three years after the base year. Approximately one third of the observations in each regression (around 600) are censored.

[80] English secondary schools are often described (informally and sometimes in the schools Census) in terms of the number of entry forms. The modal secondary school is 'six-form entry' (i.e. 30*6 = 180 pupils in the entry grade).

very large neighbourhoods - there are only 195 'Travel to Work' areas in England - and would for example allow school A to be affected by a voting school B more than 50 miles away.

By contrast, in a model of exit-driven competition that took household residence as fixed, travel time between home and school would determine the degree of influence. If schools A and B were a sufficient distance apart that no household could afford to enrol their children in both (without relocating), the assumption might be that school B could not be affected by voting school A. Clearly, this would not be true were they located half a mile apart.

A second, narrower, definition based on this fixed-residence assumption seems more relevant to the GM case. Whilst I would not deny that schools are an important determinant of residence choice, households are far less geographically mobile than in the US for example.[81] Moreover, since I consider performance changes over short horizons - of around five years - I require only that households are relatively immobile in the short-run. This would be the case if, for example, households made a residence decision when their children enrolled in primary school and took residence as fixed when making secondary school choices. Most persuasively in my view, I know of no anecdotal or case study evidence suggesting that Head Teachers are concerned with attracting parents into the neighbourhood. All the evidence I have suggests Head Teachers are concerned with students already residing in the area, and with the competition from other schools to which those students could travel.

In a voice-driven competition framework, parents can exert pressure on schools even when enrollment is fixed (due for example to binding capacity constraints). In the GM case, annually published school performance League Tables are the obvious means by which this might happen, with GM schools potentially providing others with a type of 'yardstick' competition. In that case the fact that Head Teachers consider themselves in relation to only local schools is further evidence in support of the second, narrower definition.

With all this mind, my starting definition of neighbouring schools includes all schools in a circle of 12km around the voting school, and I give each school equal weight in the calculation of neighbour pass rates. I think 12km is probably too large an area - in the pupil-level data just under 3% of secondary school children travel more than 12km to school[82] - but it is a reasonable point from which to experiment with tighter definitions. I do this using distance from the voting school to neighbouring schools in two ways. First I retain a 12km neighbourhood but rather than weight outcomes equally, I weight outcomes according to distance from the voting school.[83]

[81] Gregg, Machin, and Manning (2004) calculate a moving rate for the UK of 8.2% in 1996/7. In the same year, the equivalent US figure was 16.5% **?**.

[82] 92,833 students (all ages/grades) out of a total of 3,205,284 (all ages/grades).

[83] I use triangular weights such that a school 1 metre from the voting school takes a weight of one and a school

Second I restrict the definition of the neighbourhood from 12km to 8km and 4km. Approximately 8% (22%) of English secondary students travel further than 8km (4km) to school. I also weight schools based on the difference between their base year pass rate and that of the voting school. If, as Ladd (2002) conjectures, schools compete most effectively on a level playing field, we might expect the biggest improvement from with similar levels of baseline performance.

The main results of the competition analysis can be viewed in Figure 14. For each outcome definition considered, I calculate the (unweighted) mean change in this outcome and plot running-smoothed means of these points against the vote share achieved by the voting school. I use calendar years rather than years before and after the base year because I wish to compare neighbour-level and market-level results (discussed below).[84] Notice also that in moving from the autonomy to the competition sample I lose the few voting schools for which I do not have postcode information. I again exclude voting grammar schools and vote schools in the tails of the vote share distribution.

To facilitate comparisons with Figures 11a and 11b, I retain the same range of pass rate changes (0,20). As was the case in those graphs, Figure 14 shows no obvious relationship between vote share and change in (neighbourhood) pass rates. Unlike those graphs however, Figure 14 shows no sign of a discontinuity in outcomes at the 50% win threshold. The absence of any obvious discontinuity has two important implications. First, it suggests that GM status was not associated with a large competition response. Second, it is evidence against the possibility that GM schools improved by drawing in higher ability students from neighbouring schools. Notice that this conclusion does not change when we tighten the neighbourhood definition by weighting schools according to their distance from the voting school (panel B), when we restrict the size of the potential neighbourhood to radii of 8km and 4km around the voting school (panels C and D) or when we weight according to the similarity of baseline pass rates (panel E).

To move beyond the broad-brush visual evidence, Table 5 presents regression-based estimates of the impact of GM status. Consistent with the graphs in Figure 14, estimates of the raw difference in pass rate changes between neighbourhoods around vote winners and neighbourhoods around vote losers are small and (in four out of five cases) negative. Adding controls - including the vote share interacted with a vote win dummy variable - tends to increase point estimates above zero. This implies that they are at least correctly signed from a competition point of view. The estimates become less precise as we restrict the neighbourhood to 8km and 4km (and reduce sample sizes accordingly), but the point estimates remain close to those obtained for the other

12 km from the voting school a weight of zero.

[84] Most of the voting schools in the competition (and autonomy) sample have base years of 1992 and 1993, as seen in Figure 3. I also present results in which the sample of voting schools excludes those with base year before 1992.

outcome definitions (in the region of 0.5 percentage points). Even for these less precise estimates, I can rule out changed pass rates of more than one half of the effect on the schools that became grant-maintained (four of five percentage points depending on specification and estimation method - see Tables 3a, 3b and 3c). For those based on larger neighbourhoods I can rule out effects outside of a range of one percentage point either side of zero.

In panel B of Table 5 I provide similar robustness checks to those used in Tables 3a, 3b and 3c. Beginning with the vote share control function, estimates appear sensitive to a change from linear to quadratic interaction. This should however be viewed in the light of Figure 14. For example, the raw data in panel D of Figure 14 suggest a slight U-shape to pass rate changes as a function of vote shares. The quadratic vote share term will pick this up and estimate a negative effect of a vote win. Although I again have no reason to suppose a quadratic fit and continue to work with a linear interaction, it is worth bearing in mind that estimated neighbourhood effects are more sensitive to this choice than estimated school effects. As in Tables 3a, 3b and 3c, including grammar schools and schools in the tails of the vote share distribution reduces the GM impact estimate slightly, whilst least absolute deviations estimation produces the same basic pattern of estimates. Estimates increase somewhat when I restrict the sample to neighbourhoods around schools that voted after 1992. This might suggest that the 1992-1997 comparison incorrectly measures changes for schools in the neighbourhood of early voters, but few votes occurred before 1991, and this might imply implausibly quick competition responses given the gradual impact of GM status on the schools that became grant-maintained.

Estimates do not change substantially when I extend the observation window to 1998, 1999 and 2000. For neighbourhoods of 8km or larger I can always rule out negative changes in excess of one percentage point and positive changes greater than one half of the estimated 'own-school' effect. For 4km neighbourhoods I obtain a wider range of point estimates, consistent with the higher standard errors associated with these estimates. Given that other outcomes are already distance-weighted and the concern that almost one-quarter of students travel further than 4km to school, I prefer estimates based on larger neighbourhoods.

6.3 Impact of GM Status at the District Level

To recap, I have shown that GM schools saw large improvements in pass rates, whilst any improvements enjoyed by their neighbours were substantially smaller. In this the third and final part of my empirical analysis I present evidence at the district level.

The advantages of using pre-defined districts rather than neighbourhoods around voting schools are twofold. First, the standard definition of a school district or market as one in which the SES composition of students is fixed allows me to check once again that the estimated effects on GM schools are not driven by peer effects or cream-skimming. Second, results at the market level provide another dimension along which competition effects can be assessed, one that does not require me to define the 'neighbourhood' myself.

I wish to estimate versions of equation (8) for changes in district-level pass rates over the years 1992 to 1997-2000. I continue to define districts as Parliamentary Constituencies, and I lose a small number of districts as a result of a result of school reorganisation and closure, which I assume was not influenced by the presence of GM schools. For the remaining districts, Appendix Table 3a describes relationships between the number of schools, the number of votes and the number of wins, where I again focus on arguably more 'genuine' vote wins by excluding grammar schools and schools with vote shares outside of the (15,85) interval. Since relatively few districts had at least three wins but not at least four wins, I focus on three GM vote win variables: at least one vote win, at least two vote wins and at least three vote wins. The base category is no GM vote wins.

In panel A of Table 6 I first check that GM vote wins were associated with increased fractions of students in GM schools. These estimates will reflect both the 'mechanical' effects of vote winners becoming GM (which we saw they did with high probability), and the enrollment response of parents to the availability of GM schools. Since we found enrollment responses on the order of 5% to 10%, we would expect mechanical effects to dominate. It is not therefore surprising that results are robust to specification, with one GM vote win (at least one win but not at least two wins) increasing the fraction of students in GM schools in 1997 by around 0.15.

In panel B of Table 6 I investigate how vote wins translated into pass rate changes. In column (1) I present raw differences in pass rate changes among the four types of district (no vote wins, at least one vote win, at least two vote wins, at least three vote wins), in column (2) I add controls for the number of schools and number of votes and in column (3) I interact the number of votes with functions of the vote shares. For example, for markets with at least one vote I interact 'at least one vote' with the maximum vote share (from which we can determine whether or not there was at least one GM school); for schools with at least two votes I interact 'at least two votes' with the maximum and second-maximum vote shares in the same way.

The estimated effect of at least one GM vote win is around one percentage point when the vote shares are included (column (3)), which when divided by the impact on the fraction of students in GM schools (0.19) gives an estimate close to the impact of GM status on the schools that became grant-maintained (four or five percentage points). The estimated impact of one vote win is reasonably robust to the inclusion of additional controls, although the estimated effects of at least two and at least three wins are imprecise and in the case of three wins negative. In part this reflects the small number of districts with at least two (three) but not one (two) wins, a consequence of excluding grammar schools and those with vote shares outside of the (15,85) range. When I include these schools in the definition of a vote win (panel C) I find positive effects of at least three wins in some years, which should warn against concluding that there were for example decreasing returns to vote wins. Estimates for the impact of at least one win are also slightly more precise when all schools are included, although differences in the estimates between years (1997 and 1998 for example) caution against reading too much into the point estimates. A reasonable conclusion to draw from the Table might be that district-level results are not inconsistent with those estimated for schools that became grant-maintained and neighbouring schools: large 'own-school' effects and smaller effects on neighbouring schools.

7 Discussion and Interpretation of Results

I considered the impact of GM status on the schools that become grant-maintained, their neighbours and the district. I found large gains for the grant-maintained schools, small effects on their neighbours and district-level effects consistent with a combination of the two. How should these results be interpreted?

Since I argued the effects on GM schools were not the result of cream-skimming, the outstanding question that I do not address in this paper is what the GM schools did to improve. In the framework set out above, absent cream-skimming, schools can improve as a result of increased resources or increased 'effort', which I interpreted as proxying school behaviours at given levels of resources. I do not have information on school budgets, and so I cannot say how much of the pass rate increase could plausibly be due to increased school resources.[85] I can however speak to this issue using other estimates of school resource effects.

There is as yet no consensus on the impact of school resources on student outcomes, with estimates in the literature ranging from zero to small and positive (see the discussion in Burtless (1996)). Zero effects have clear implications for the interpretation of GM effects, but to interpret GM effects under more optimistic assumptions about resource effects, a useful but controversial estimate towards the top of the range of published estimates suggest a 10% increase in school resources is associated with a 1-2% increase in lifetime average earnings (Card and Krueger (1996), but see Heckman, Layne-Farrah, and Todd (1996)). Suppose I combine this estimate with the

[85] I will shortly receive pupil-teacher ratio data so I can at least assess one channel through which resource effects may operate.

estimated effect of a 'pass' on earnings of 28% (McIntosh (2002) provides the most detailed assessment of returns to UK qualifications and reports this as his central estimate). Then, given a GM-induced budget increase of 10%, a pass rate change of between 3 and 8 percentage points would be required to increase lifetime school average earnings by 1-2% (on a pass rate base of 35%).[86] This suggests that increased resources can explain the change in pass rates under optimistic assumptions about the impact of resources on earnings. Of course with less optimistic resource assumptions, the increased pass rates must be driven by changing school behaviours resulting from increased school autonomy.

Which aspects of the school behaviours might increased autonomy have changed? Chubb and Moe (1990) stress the impact of autonomy on the school principal's ability to assemble a 'team' of teachers. I have no data on teacher turnover, but there is some anecdotal evidence suggesting that turnover increased after a vote win (Bush, Coleman, and Glover (1993)). Indeed, one of the special purpose grants paid to GM schools - the 'Redundancy an Restructuring' grant - was at least implicitly aimed at helping Head Teachers turn staff over. There is also some evidence suggesting GM schools were more inclined to make bonus payments, which may be consistent with team-building efforts. What is undoubtedly true is that Head Teachers in schools that become GM wielded substantially more power than they previously had, checked only by the oversight of Governing Bodies whose composition was to a large extent determined by the Head Teacher. In of itself, this will presumably have changed relationships within the school, although it is difficult to say whether a more coherent team would have emerged as a result.

Chubb and Moe (1990) also argue that autonomy allows schools to determine school policies free of political control, examples of which include discipline and tracking. There is as mentioned some evidence that GM schools followed enthusiastic exclusion policies. Regarding tracking, I have data consistent with the idea that GM schools were more like to track students, but this is based on a small sample of (post-vote) schools and the evidence on tracking is itself mixed.[87][88] Other evidence on discipline comes from case studies in which students report tightening of school uniform standards although this is also consistent with GM schools marketing themselves more aggressively. The same consideration applies to school websites. Regression discontinuity estimates suggest vote winners were around 20% more likely to operate a school website (website data from 2001). This might be a proxy for the efficiency with which the school is managed or the way in which the school is sold to parents.

[86] $\frac{0.28(5.7)}{100+0.28(35)} \sim 0.015$.

[87] See Betts and Shkolnik (2000) for example.

[88] Data from Levacic (2004) show that among schools responding to the competition survey, 44/70 GM schools tracked pupils in grade 7 versus 64/114 non-GM schools.

Since we know relatively little about the education production function, my inability to pin down the mechanisms by which GM schools improved should not be surprising. In the spirit of the school effectiveness literature there are numerous case studies describing GM schools that were 'turned around' (Chubb and Moe (1992) and Bush, Coleman, and Glover (1993)), but case studies do not allow us to draw conclusions about particular school policies.

Whatever the mechanism by which vote winners improved, we would expect them to have put pressure on other schools in the neighbourhood. Mechanisms by which this might happen are parental exit (we showed medium-run enrollment growth in GM schools of roughly 10%) and parental voice, both of which the performance 'League Tables' were designed to encourage. Consistent with these possibilities, the Levacic (2004) data show a much higher proportion of GM schools reporting the local education market as being competitive and a much lower proportion reporting a 'high degree of cooperation'.

In the framework sketched above, competitive responses are, again absent a change in SES composition, the product of a change in school behaviour or effort and the impact of this change on achievement (see the discussion following equation (7)). [89] Small competition effects could therefore be consistent with no change in behaviours or changes in behaviours that do not appear to raise performance. The first of these would be consistent with school administrators already exerting maximum 'effort', either because they are motivated to do so or because other institutions (such as the rigours of the League Tables and/or the Inspection Regime) demand it of them. The second would be consistent with the types of marketing behaviour discussed by Hsieh and Urquiola (2003) and hinted at in Fitz, Halpin, and Power (1993).

Since I have even less information on the neighbours of grant-maintained schools than I do on the schools themselves (I know of no studies surveying neighbours), it is hard to say whether neighbour-school behaviours changed. One obvious behavioural response would be for the neighbours to hold a vote themselves. 'Copycat' phenomena are referred to in case study and survey evidence and the typical claim is that one school holding a vote encouraged others to do so. One test of interdependent voting behaviour within districts assesses the extent of across-market over-dispersion in voting, roughly, the extent to which the across-market mean number of votes is exceeded by the across-market variance. Consistent with the vote patterns seen in the Maps, my data show strong evidence of voting over-dispersion. Even allowing for interdependent voting, we might still expect to see copycat win effects on top of copycat vote effects. That I do not find any

[89] As mentioned above, Besley and Ghatak (2003) argue that competition can improve efficiency via an improved matching of agents, for example Head Teachers, teachers and parents. That we do not find strong competition responses could be consistent with institutions (the National Curriculum and 'League Tables') supressing this kind of matching or with matching improving welfare but not necessarily exam results.

(based on regression discontinuity estimates of the impact of a win on the number of future votes in the district) takes us back to an earlier theme in the paper, that the decision to hold a vote was driven as much by political considerations - personalities within the school and the Head-LEA relationship - as by school performance or market pressures. If GM status is interpreted as a (costly) school policy that can raise student achievement, non-response to GM in this dimension may indicate non-response in other dimensions.

A second possibility suggested by this framework is that non-GM school-level behaviour responded, but that the non-GM environment in which these schools operated mitigated against these responses translating into changed pass rates. Thinking back to the last mechanism, it may be that Head Teachers pushed for GM status in response to a neighbour becoming GM, but that a combination of LEA and perhaps school-level resistance prevented this 'policy' from being implemented. For those who would interpret the impact on the grant-maintained schools as a pure autonomy effect, this may be an appealing interpretation. Under a less rigid interpretation of those effects, one might incline to the view that GM status did not change neighbour school behaviour.

8 Conclusion

To investigate whether autonomy and market competition can improve student achievement, this paper studied a UK reform that allowed public schools to 'opt out' of local political control and become quasi-independent. Schools wishing to opt out were required to win a majority vote among the parents of current students, and I used a regression discontinuity design to exploit this requirement. This type of research design is unusual in the education field, where quasi-experimental evidence is not commonly found (see Angrist (2004a)).

Two key results emerge from the analysis. First, schools that opt out enjoy large improvements in performance on national examinations, in the region of one quarter of a standard deviation. I am confident in claiming these effects are not driven by cream-skimming, mainly because results improved for students already enrolled in school at the time of the vote. In addition, I find no evidence of SES changes and limited changes in immediate post-vote enrollment, although these may understate parental demand given capacity constraints and I find larger medium-run enrollment increases.

Also pointing against cream-skimming are the positive effects found for schools in the neighbourhood of vote-winning schools. These estimates are not however large, and this is the second key result to emerge. Across a range of definitions, I can rule out neighbourhood improvement of more than one half of the effect on the voting schools themselves, and when I aggregate to the district level estimates are consistent with a combination of strong own-school effects and weaker effects among neighbours.

What do these results say about school choice reforms that seek to increase school autonomy and encourage between-school competition? On the one hand, the improved performance seen by GM schools should encourage those wishing to see more power handed to schools. But this encouragement comes with a caveat, namely that I do not know precisely what these schools did to improve. The caveat reflects uncertainty as to the importance of school resources in school improvement, and the precise channel through which the granting of autonomy may have operated. This is important, since there are costs associated with reforms on the scale of the one studied here, at least to the extent that local democratic control of public schools is valued. The removal of public schools from local democratic control may, as some assert, be a pre-condition for improved performance, but until we can be sure that GM schools did not implement policies that could be introduced within the existing system of control, a more cautious approach may be sensible.[90]

With regards to market competition, the results themselves suggest caution as to the likely benefits. New school types such as voucher and charter schools may benefit the students who attend them, but my results suggest much smaller spillover effects. Against this it should be stressed that even second-order effects can be important and that features of the institutional setting that I consider may be biased against finding competitive effects. In particular, since non-GM schools operated under the 'old' regime, advocates of school autonomy would argue that the odds were already stacked against them (Chubb and Moe (1992) expressed scepticism about other aspects of the 1988 Education Act for this reason). Further research based on other institutional settings will help to determine whether competition can increase school performance.

[90] In future work I plan to survey a sample of winning and losing schools to investigate this issue.

References

ANGRIST, J. (2004a): "American Education Research Changes Tack," *Oxford Review of Economic Policy*, 20(2), 198–212.

——— (2004b): "Treatment Heterogeneity in Theory and Practice," *Economic Journal*, 114, C52–C83.

BESLEY, T., AND M. GHATAK (2003): "Competition and Incentives with Motivated Agents," STICERD Discussion Paper No. TE/03/465, LSE, November 2003.

BETTINGER, E. (2004): "The effect of charter schools on charter students and public schools," *Economics of Education Review (in press)*.

BETTS, J., AND J. L. SHKOLNIK (2000): "Key difficulties in identifying the effects of ability grouping on student achievement," *Economics of Education Review*, 19, 21–26.

BRADLEY, S., R. CROUCHLEY, J. MILLINGTON, AND J. TAYLOR (2000): "Testing for Quasi-Market Forces in Secondary Education," *Oxford Bulletin of Economics and Statistics*, 62(3), 357–390.

BRYK, A., Y. THUM, S. EASTON, AND S. LUPPESCU (1998): *Academic Productivity of Chicago Public Schools: A Second Technical Report*. Consortium on Chicago School Research, Chicago, Il.

BURTLESS, G. (1996): *Does Money Matter? The Link between Schools, Student Achievement and Adult Success*. Brookings Institution, Washington, D.C.

BUSH, T., M. COLEMAN, AND D. GLOVER (1993): *Managing Autonomous Schools: The Grant Maintained Experience*. Paul Chapman, London, UK.

CARD, D., AND A. KRUEGER (1996): "The Labor Market Effects of School Quality: Theory and Evidence," in *Does Money Matter? The Link between Schools, Student Achievement and Adult Success*, ed. by G. Burtless. Brookings Institution, Washington, D.C.

CHUBB, J. E., AND T. M. MOE (1990): *Politics, Markets and America's Schools*. Brookings Institution, Washington, DC.

——— (1992): *A Lesson in School Reform from Great Britain*. Brookings Institution, Washington, DC.

CULLEN, J., B. JACOB, AND S. LEVITT (2000): "The Impact of School Choice on Student Outcomes: An Analysis of the Chicago Public Schools," NBER Working Paper 7888, September 2000.

DINARDO, J., AND D. LEE (2004): "Economic Impacts of Unionization on Private Sector Employees: 1984-2001," NBER Working Paper 10598, July 2004.

EPPLE, D., AND R. ROMANO (1998): "Competition between Public and Private Schools, Vouchers and Peer Effects," *American Economic Review*, 88(1), 33–62.

——— (2000): "Neighbourhood Schools, Choice and the Distribution of Educational Benefits," NBER Working Paper 7850, August 2000.

FINN, C., B. MANNO, AND G. VANOUREK (1993): *Charter Schools in Action: Renewing Public Education*. Princeton University Press, Princeton, NJ.

FISKE, F., AND H. LADD (2000): *When Schools Compete: A Cautionary Tale*. Brookings Institution, Washington, DC.

FITZ, J., D. HALPIN, AND S. POWER (1993): *Grant Maintained Schools: Education in the Market Place*. Kogan Page, London, UK.

GREGG, P., S. MACHIN, AND A. MANNING (2004): "Mobility and Joblessness," in *Seeking a Premier League Economy*, ed. by D. Card, R. Blundell, and R. Freeman. University of Chicago Press for the NBER, Chicago, IL.

HAHN, J., P. TODD, AND W. V. DER KLAAUW (2001): "Identification and Estimation of Treatments with a Regression Discontinuity Design," *Econometrica*, 69(1), 201–209.

HECKMAN, J., A. LAYNE-FARRAH, AND P. TODD (1996): "Does Measured School Quality Really Matter?," in *Does Money Matter? The Link between Schools, Student Achievement and Adult Success*, ed. by G. Burtless. Brookings Institution, Washington, D.C.

HIRSCHMAN, A. (1970): *Exit, Voice and Loyalty: Responses to the Declines in Firms, Organisations and States*. Harvard University Press, Cambridge, MA.

HOXBY, C. (2000): "Does Competition Among Public Schools Benefit Students and Taxpayers?," *American Economic Review*, 90(5), 1209–1238.

——— (2003): "School Choice and School Productivity: Could School Choice be a Tide that Lifts All Boats?," in *The Economics of School Choice*, ed. by C. Hoxby. University of Chicago Press for the NBER, Chicago, IL.

HSIEH, C., AND M. URQUIOLA (2003): "When Schools Compete, How Do They Compete? An Assessment of Chile's Nationwide School Voucher Program," NBER Working Paper 10008, September 2003.

KRUEGER, A., AND P. ZHU (2002): "Another Look at the New York City Voucher Experiment," NBER Working Paper 9418, September 2002.

LADD, H. (2002): *Market-Based Reforms in Urban Education*. Economic Policy Institute, Washington, DC.

LEE, D. (2003): "Randomized Experiments from Non-Random Selection in U.S. House Elections," UC Berkeley, mimeo.

LEONARD, M. (1988): *The 1988 Education Act: A Tactical Guide for Schools*. Basil Blackwell, Oxford, UK.

LEVACIC, R. (2004): "Competition and the Performance of English Secondary Schools: Further Evidence," *Education Economics*, 12(2), 177–193.

LEVACIC, R., AND J. HARDMAN (1999): "The Performance of Grant Maintained Schools in England: An Experiment in Autonomy," *Journal of Education Policy*, 14(2), 185–212.

MANSKI, C. (1992): "Educational Choice (Vouchers) and Social Mobility," *Economics of Education Review*, 11(4), 351–369.

MCINTOSH, S. (2002): "Further Analysis of the Returns to Academic and Vocational Qualifications in the UK," Department for Education and Skills (DfES) Research Report 370, September 2002.

McMillan, R. (2004): “Competition, Incentives and Public School Performance,” *Journal of Public Economics*, 88, 1871–1892.

Nechyba, T. (2000): “Mobility, Targeting and Educational Private-School Vouchers,” *American Economic Review*, 90(1), 130–146.

Peterson, P., W. Howell, P. Wolff, and D. Campbell (2003): “School Choice and School Productivity: Could School Choice be a Tide that Lifts All Boats?,” in *The Economics of School Choice*, ed. by C. Hoxby. University of Chicago Press for the NBER, Chicago, IL.

Rothstein, J. (2004a): “Does Competition Among Public Schools Benefit Students and Taxpayers? Comment,” Princeton University, mimeo.

——— (2004b): “Good Principals or Good Peers? Parental Valuation of School Characteristics, Tiebout Equilibrium, and the Incentive Effects of Competition among Jurisdictions,” NBER Working Paper 10666, August 2004.

Rouse, C. (1998): “Private School Vouchers and Student Achievement: An Evaluation of the Milwaukee Parental Choice Program,” *Quarterly Journal of Economics*, 113(2), 553–602.

Stillman, A., and K. Maychell (1986): *Choosing Schools*. NFER-NELSON, London, UK.

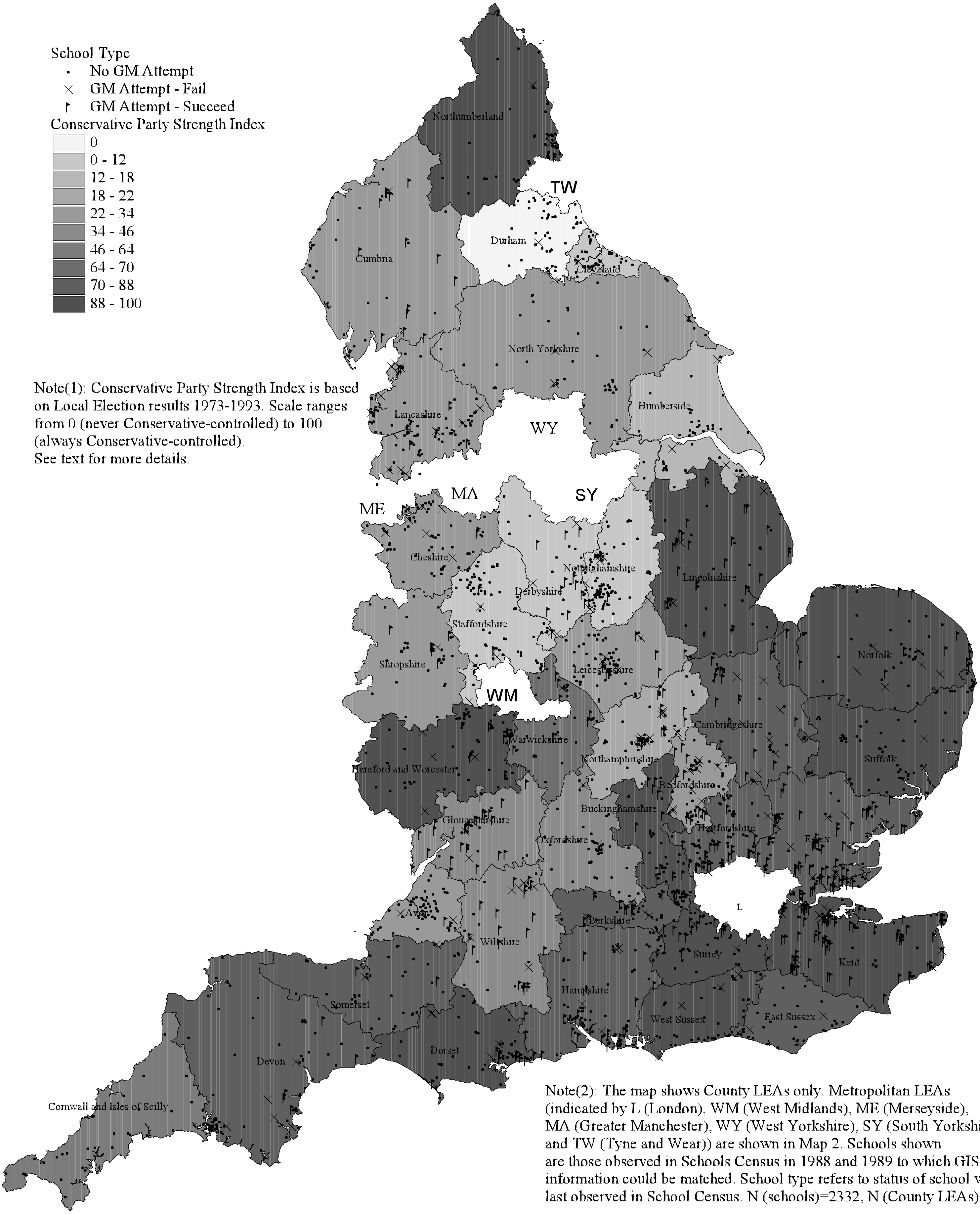
Thompson, M. (1992): “The experience of going grant maintained: the perceptions of AMMA teacher representatives,” *Journal of Teacher Development*, 1(3), 133–140.

Timmins, N. (2001): *The Five Giants: A Biography of the Welfare State*. HarperCollins, London, UK.

Williams, J., J. Coldron, K. Stephenson, A. Logie, and N. Smith (2001): “An Analysis of Policies and Practice of LEA Admissions Authorities in England,” Research Report 278, Department for Education and Statistics (DfES), London, UK.

Wong, K. (2003): “The Big Stick,” Education Next, Winter 2003.

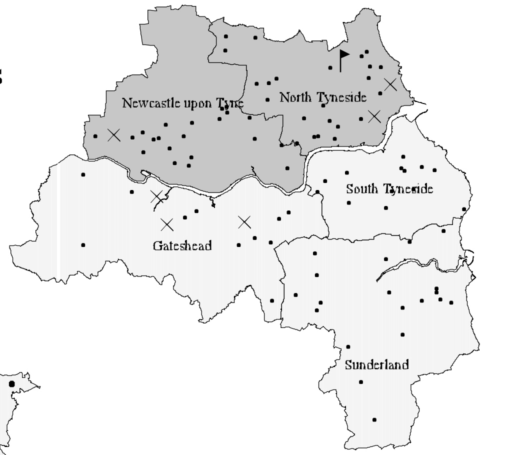
Map 1: County LEAs: Local Politics and Grant Maintained Schools



Note(1): Conservative Party Strength Index is based on Local Election results 1973-1993. Scale ranges from 0 (never Conservative-controlled) to 100 (always Conservative-controlled). See text for more details.

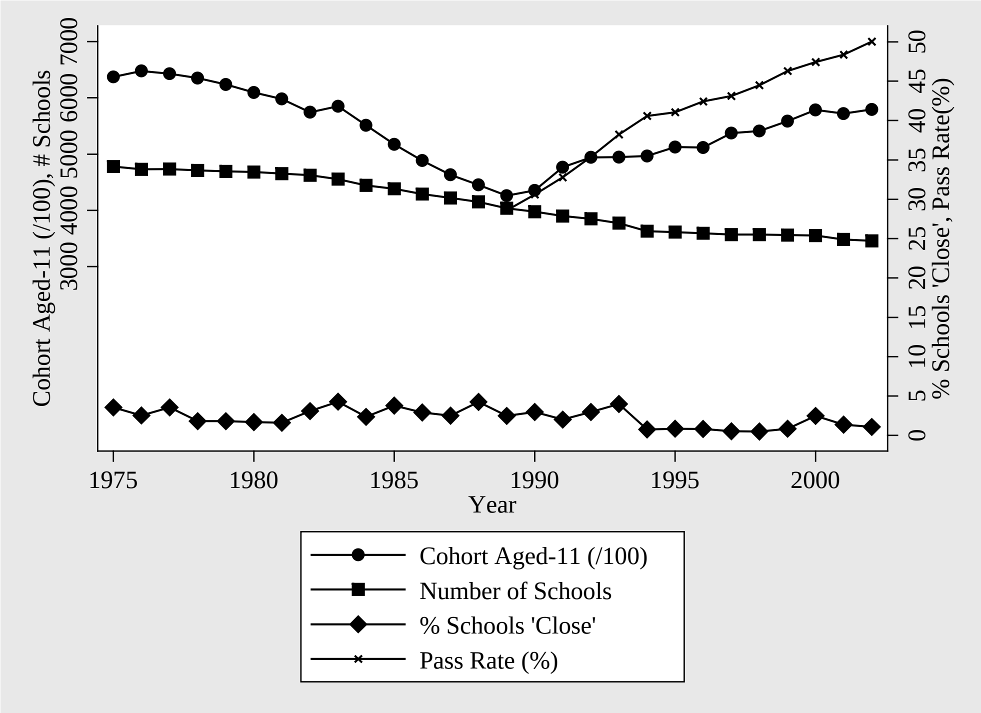
Note(2): The map shows County LEAs only. Metropolitan LEAs (indicated by L (London), WM (West Midlands), ME (Merseyside), MA (Greater Manchester), WY (West Yorkshire), SY (South Yorkshire) and TW (Tyne and Wear)) are shown in Map 2. Schools shown are those observed in Schools Census in 1988 and 1989 to which GIS information could be matched. School type refers to status of school when last observed in School Census. N (schools)=2332, N (County LEAs)=38.

Map 2: Metropolitan LEAs: Local Politics and Grant Maintained Schools



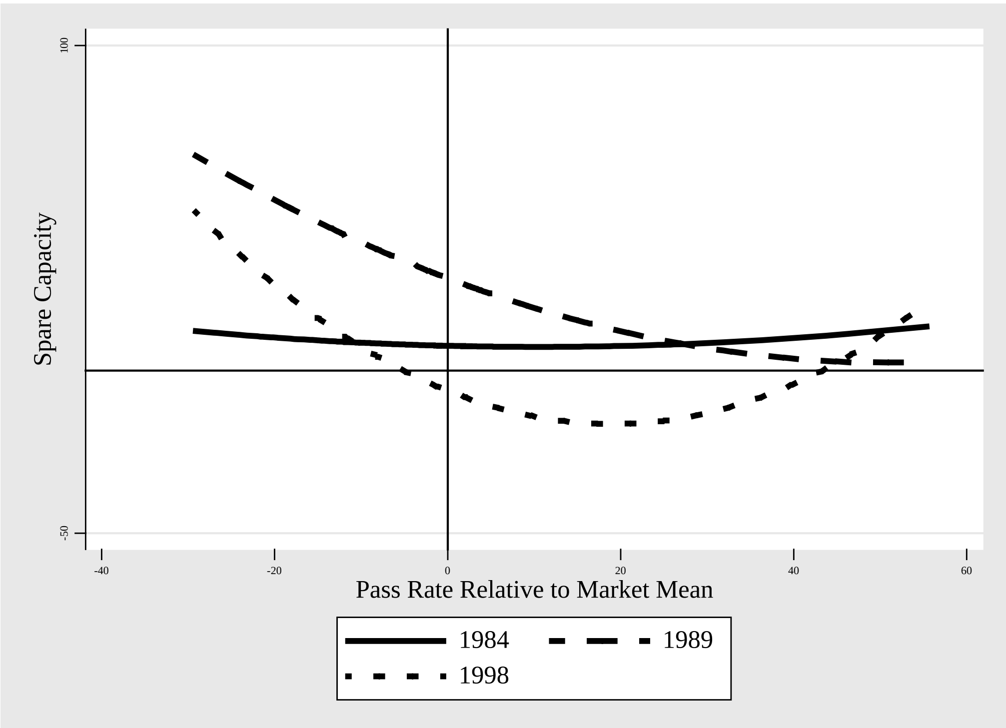
Notes: Map shows Metropolitan LEAs excluded from Map 1. Clockwise from top right these are Greater Manchester, Tyne and Wear, South Yorkshire, Greater London, West Midlands, West Yorkshire, Merseyside. School Type key and shading the same as Map 1.

Figure 1: Cohort Size and School Closure



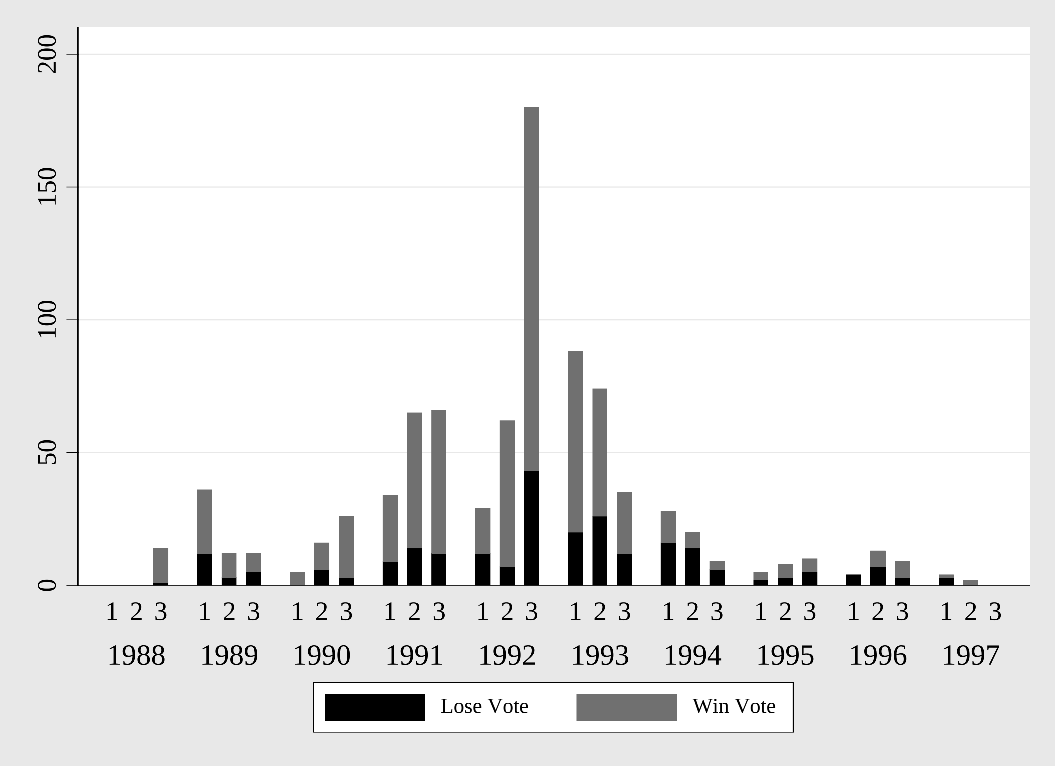
Notes: All Secondary and Middle-deemed-Secondary schools in England. Pass rate for 1988/89-1990/91 come from Statistical Bulletins (1/1991, 11/1991, 15/1992) that take numbers from the School Examinations Survey. From 1992 figures come from school performance League Tables.

Figure 2: Enrollment-Pass Rate Correlation: 1984-1998



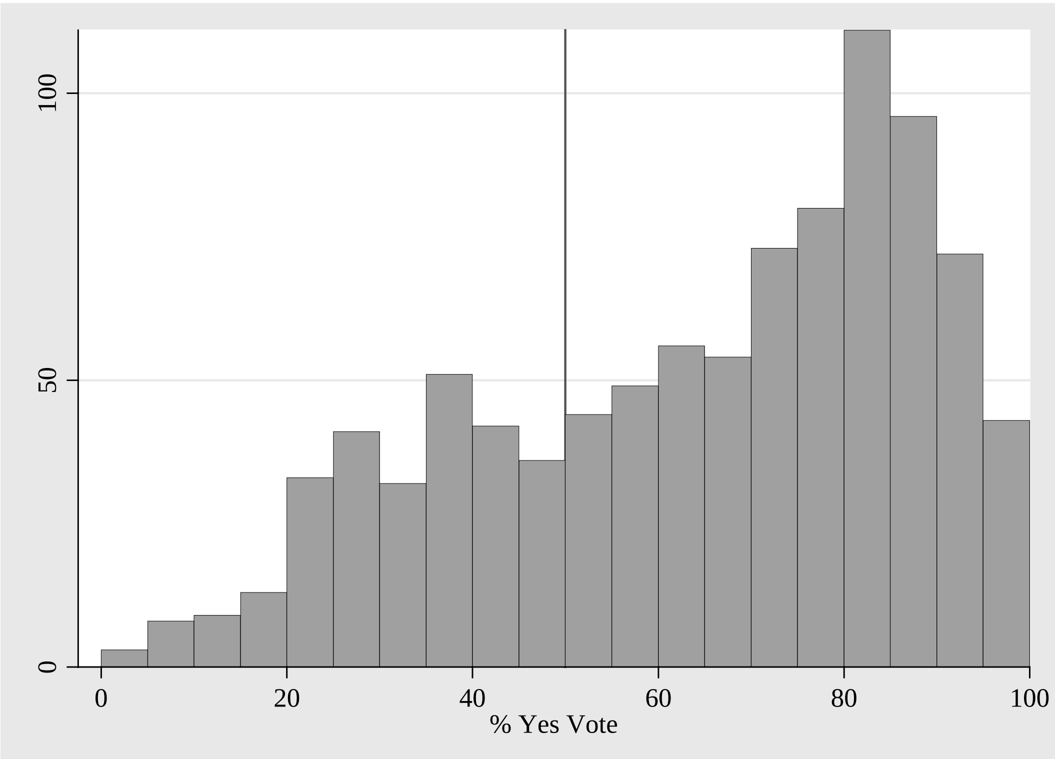
Notes: Lines are quadratic fits of spare capacity (capacity less entry age enrolment) against pass rate relative to district (market) mean in 1992. Excludes middle schools, selective districts, reorganised LEAs, and schools that close between 1991 and 1993. N(districts)=466, N(schools)=2793. Districts are Parliamentary Constituencies.

Figure 3: GM Ballots by Year/Term



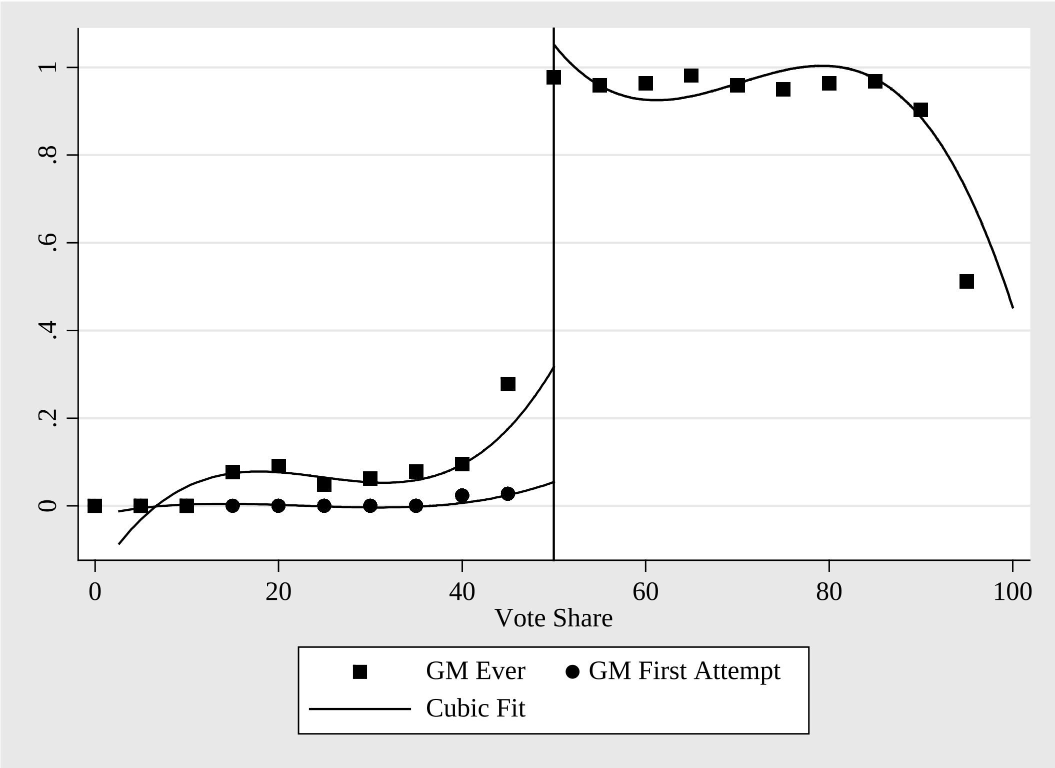
Notes: Figure includes all 946 ballots. The 1988 Education Act took effect from September 1988, the 1992 General Election (which the Conservatives won) was held on 9 April. The 1997 General Election (which Labour won) was held on 1 May. GM applications refused from that point onwards. '1', '2' and '3' are Easter, Summer and Autumn school terms.

Figure 4: Vote Counts by Vote Share



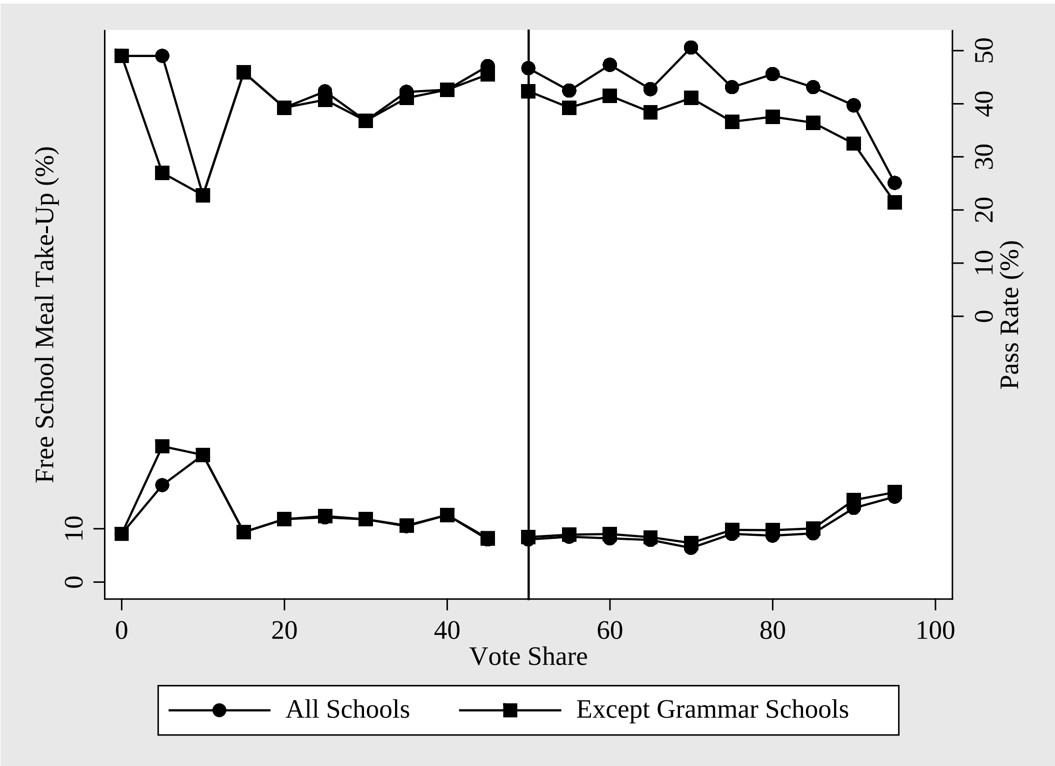
Notes: Figure includes all 946 ballots

Figure 5: Vote Share and GM Status



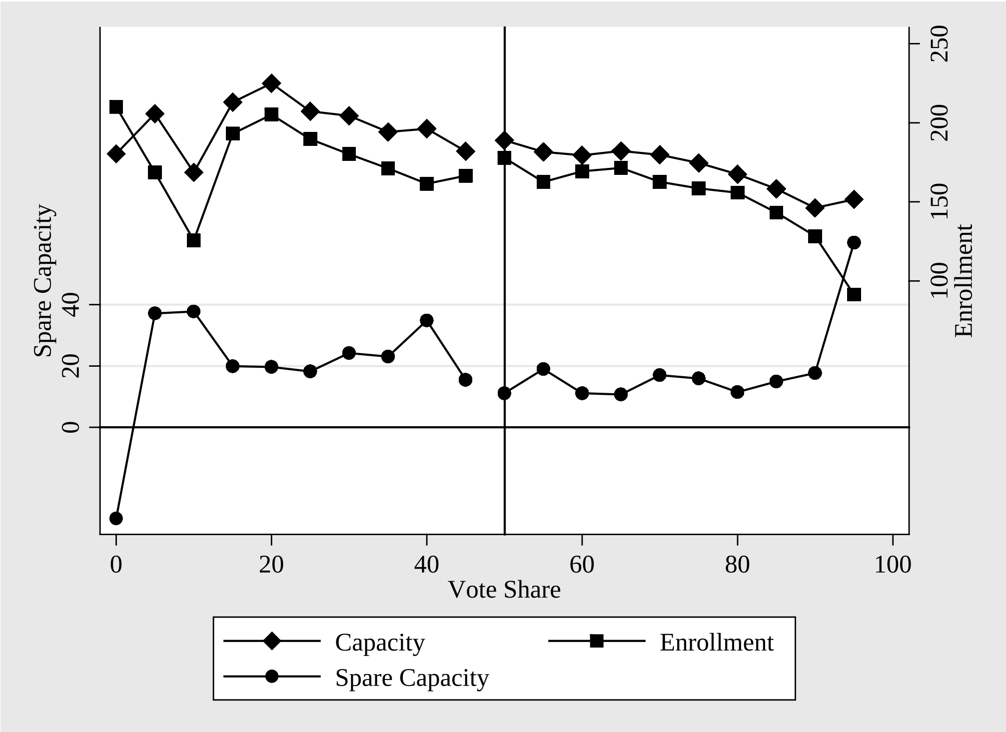
Notes: Figure includes all 946 ballots

Figure 6: Base Year Pass Rate and SES



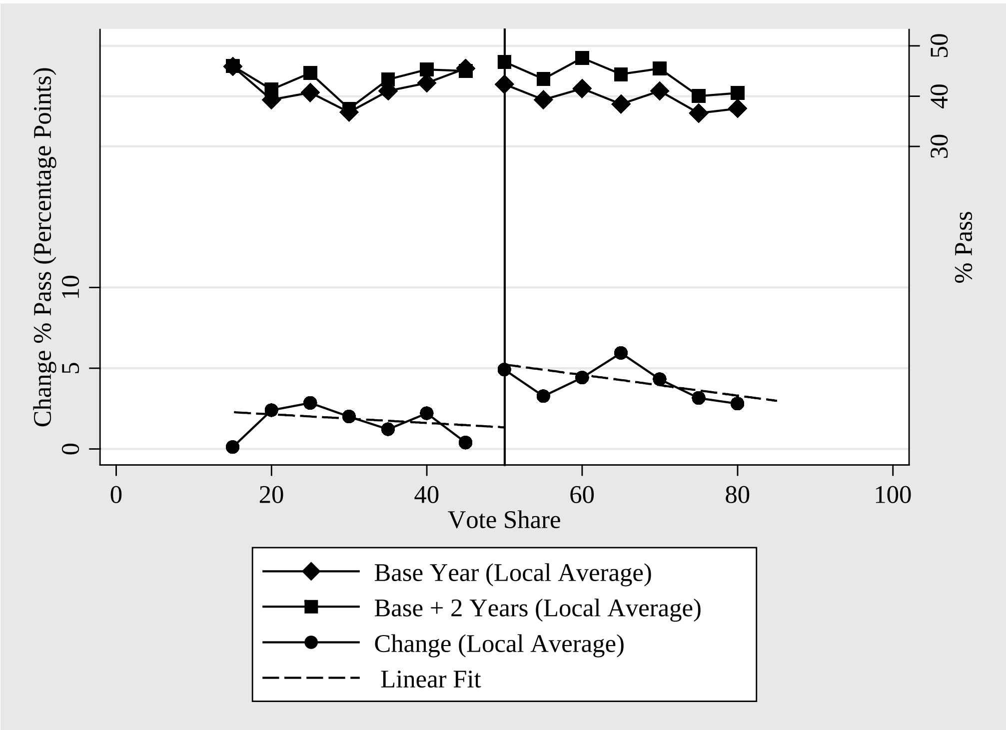
Notes: Sample includes the 745 (678) schools with baseline pass rates. SES measured as Free School Meal take-up. Excluding grammar schools has little impact to the left of the 50% win threshold since very grammar schools lost a vote on GM status.

Figure 7: Base Year Enrolment and Spare Capacity



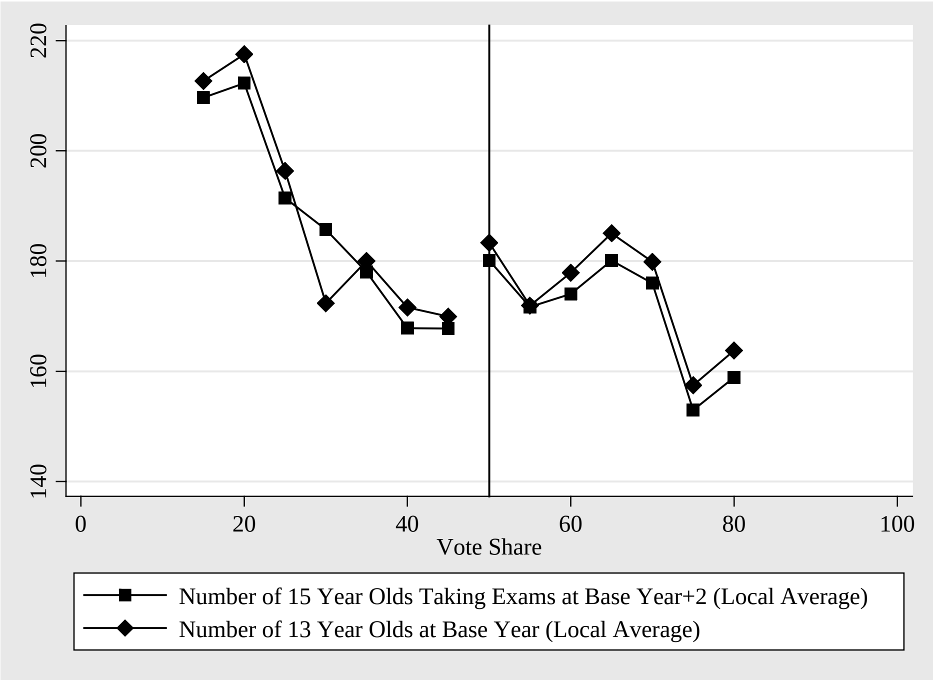
Notes: Sample includes all GM ballot schools (946) with valid capacity data (898)

Figure 8: Impact of GM Status on Schools that Become Grant-Maintained: Two Years after Base Year



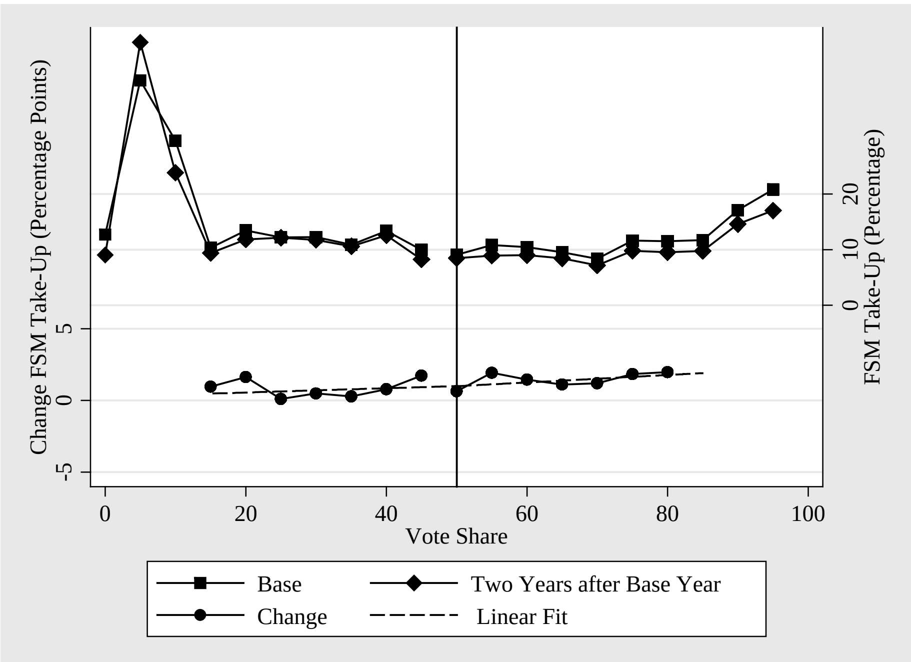
Notes: Sample includes all non-grammar schools with exam information and vote shares in the (15,85) interval (N=524). Unweighted. See Table 3a for regression versions.

Figure 9: Cohort Size: Base Year and Two Years After Base Year



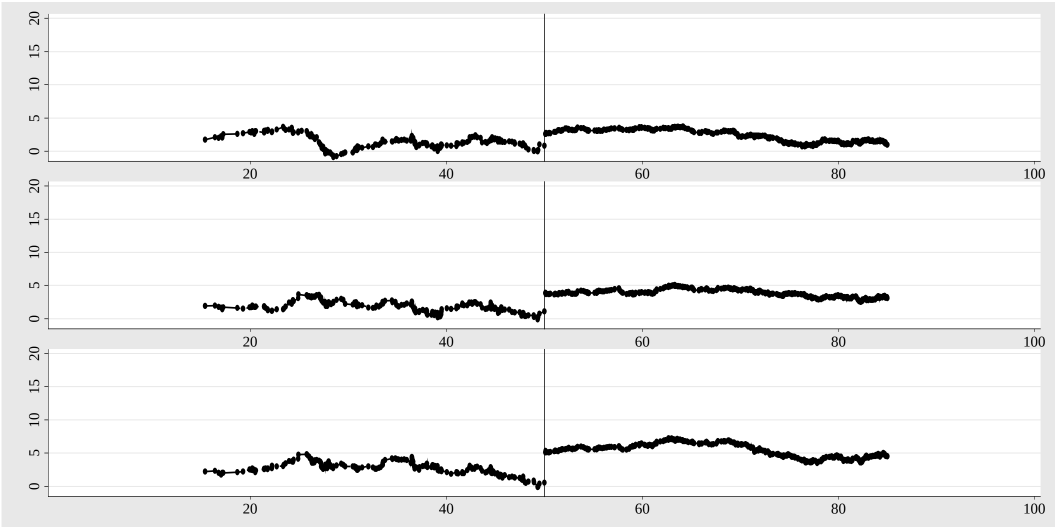
Notes: Figure shows number of 15 year olds (pass rate denominator) versus number of 13 year olds two years previous. Sample identical to that in Figure 8. Local average at vote share=30 driven by one school with 1 pupil aged 13 at baseline and 460 aged 15 at baseline+2.

Figure 10: SES Change: Base Year versus Two Years after Base Year



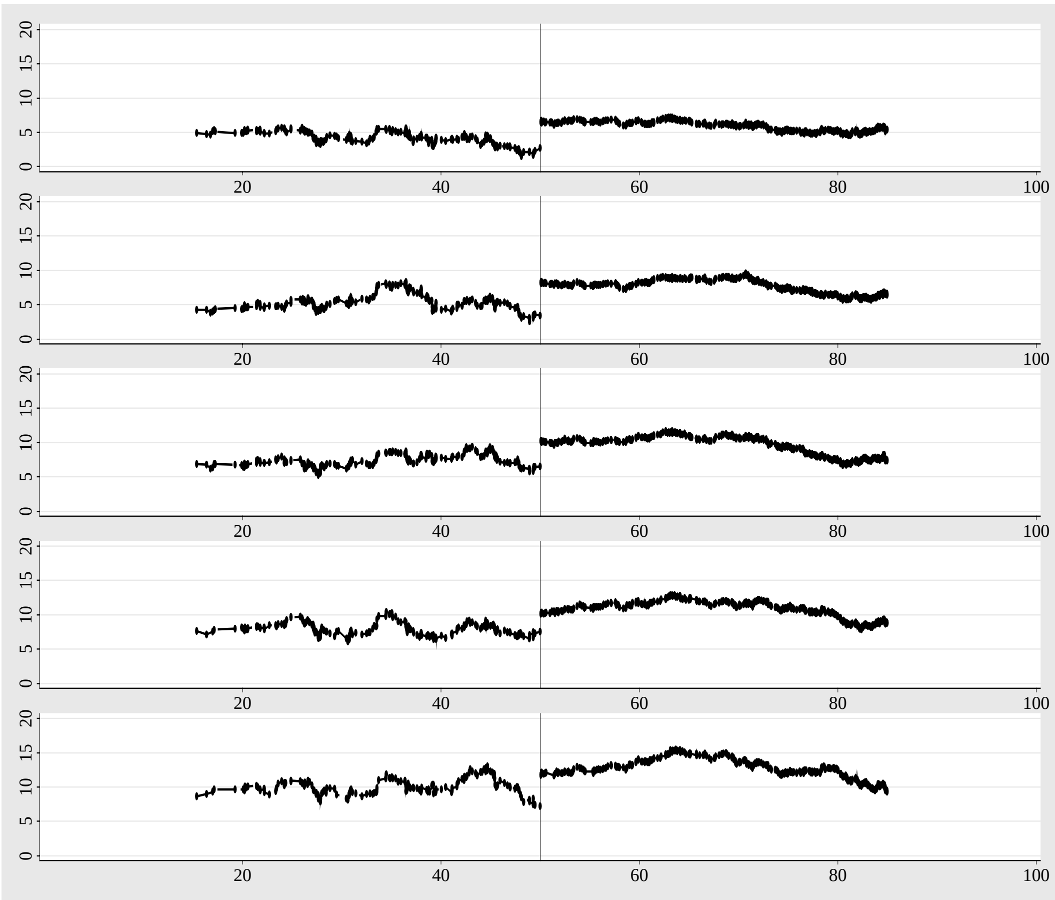
Notes: Sample identical to that in Figure 8 (N=524).

Figure 11a: Impact of GM Status on Schools that become Grant-Maintained: One-Three Years after Base Year



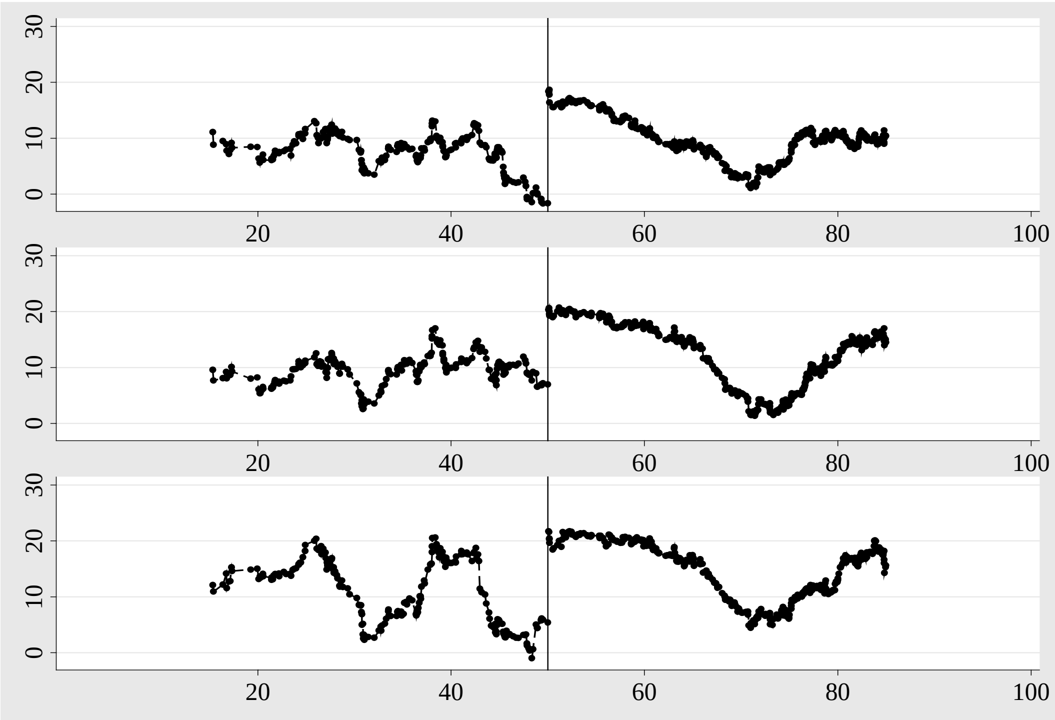
Notes: The lines are running-means of pass rates changes (percentage points, bandwidth 0.2) plotted on either side of the 50% win threshold. Sample includes all non-grammar schools with pass rate information and vote shares in the interval (15,85). Sample sizes are 525, 524 and 524. See Table 3b for robustness checks.

Figure 11b: Impact of GM status on Schools that become Grant-Maintained: Four-Eight Years after Base Year



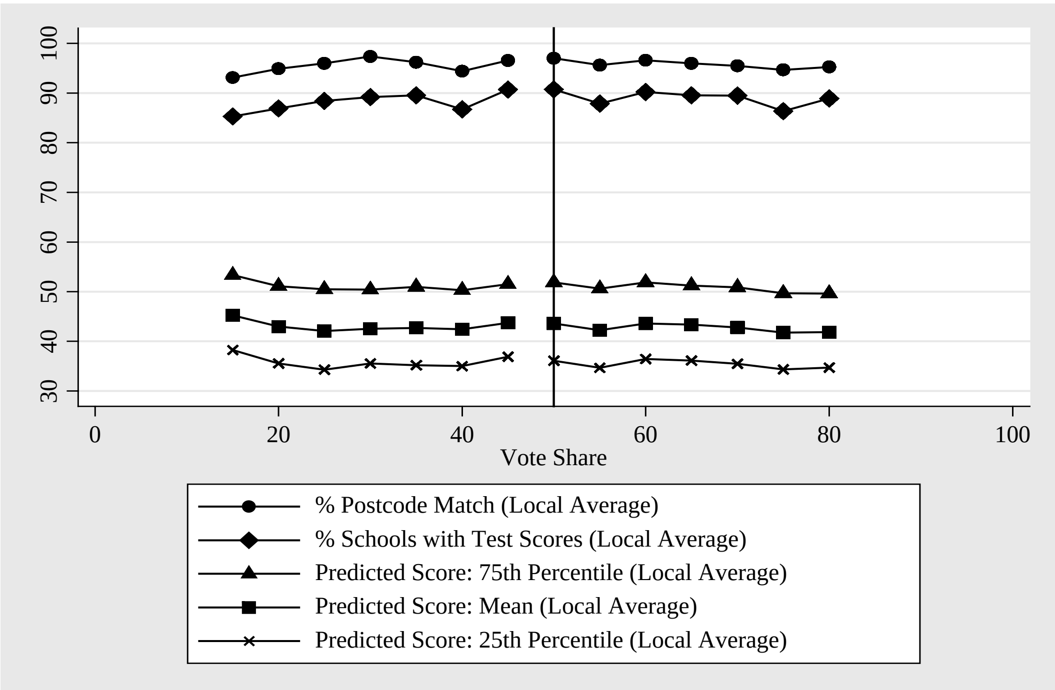
Notes: see Figure 11a. Sample sizes 522, 520, 519, 502, 487.

Figure 12: Impact of GM status on Entry-Age Enrollment in Schools that became Grant Maintained: One-Three Years after Base Year



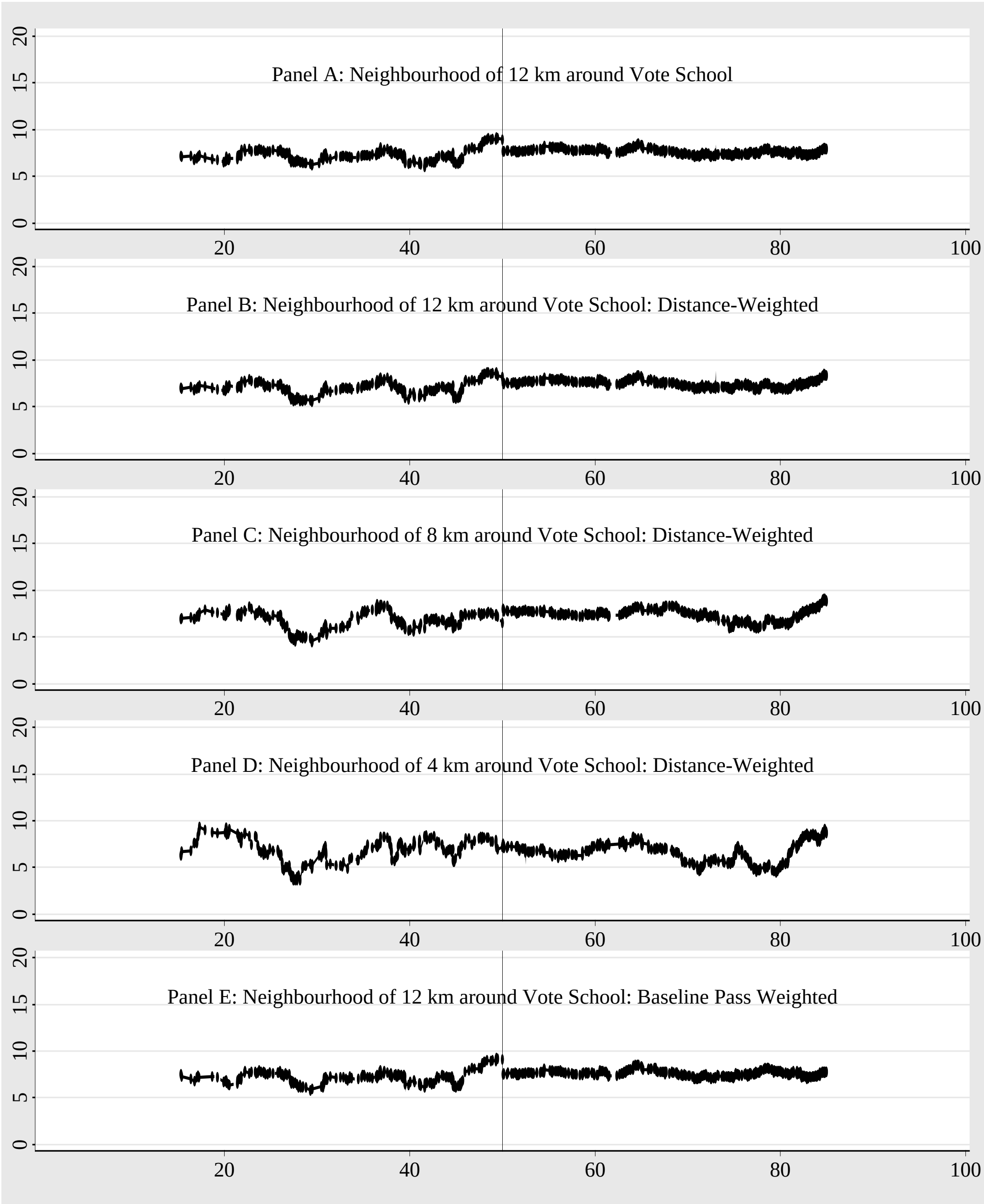
Notes: Sample includes all non-grammar schools observed in Census data between two years before and five years after base year. Base year enrolment is simple average of enrolment in the two years before the base year. N=611.

Figure 13: SES Comparisons Based on Pupil-Level Census Data: 6 Years after Base Year



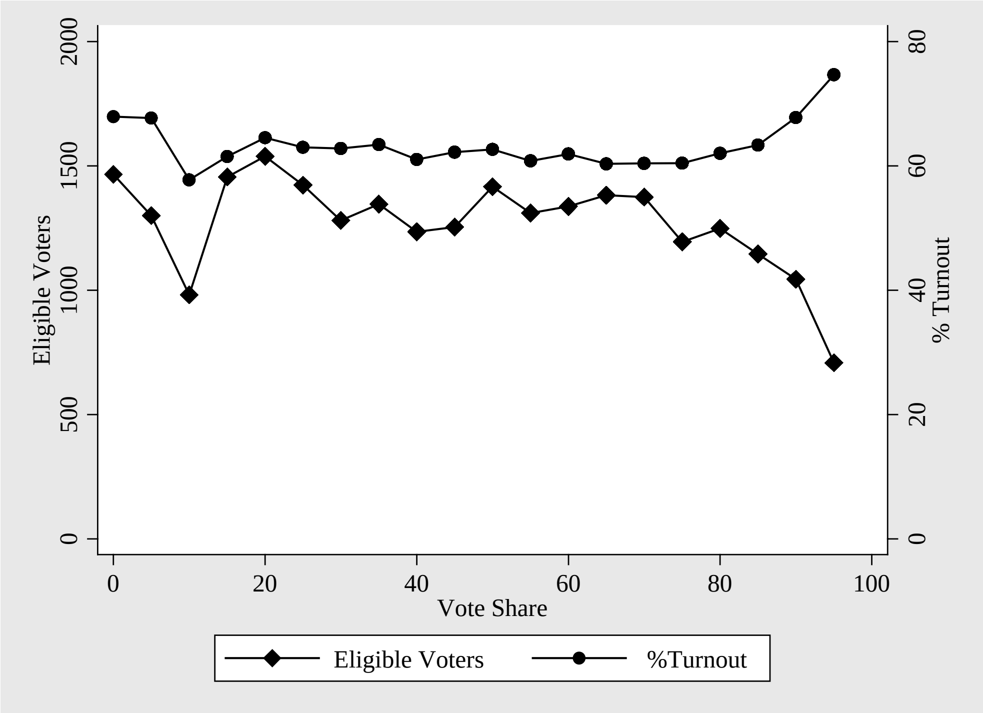
Notes: The sample includes schools with base year pass rates less those schools that do not survive until 2002 hence have no Pupil-Level data (34). Predicted score based on a single index of predicted score – see text.

Figure 14: Impact of GM Status on Schools in the Neighbourhood of GM Schools: 1992-1997



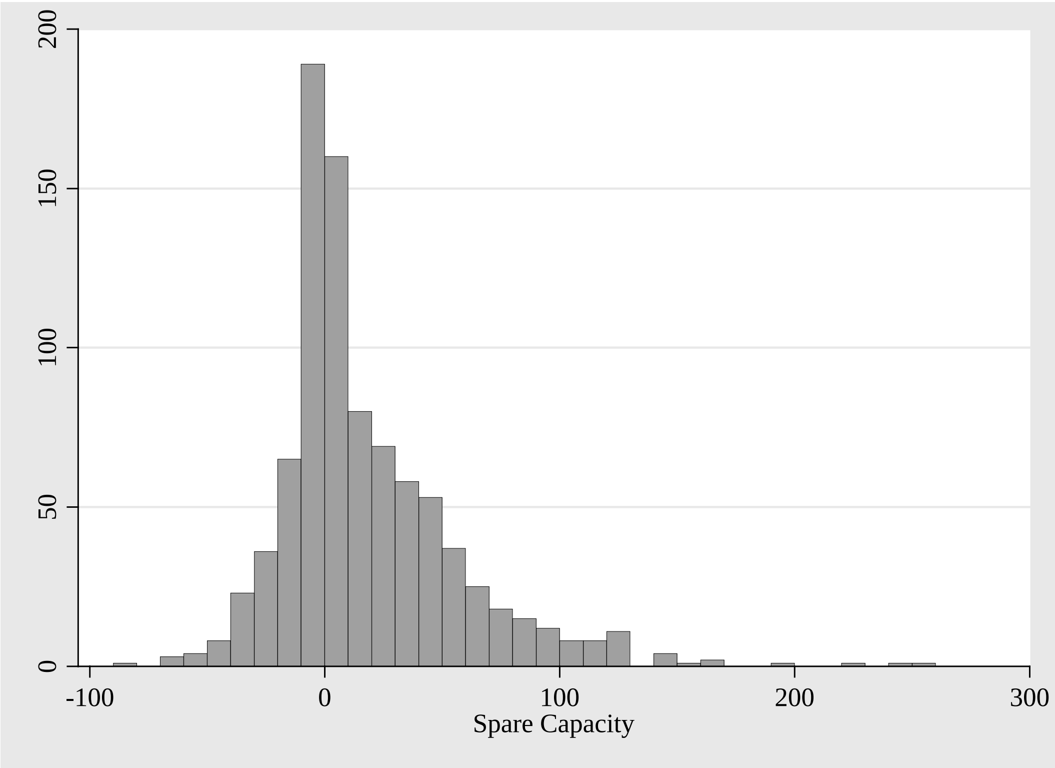
Notes: In each panel, running-smoothed means (bandwidth 0.1) of the neighbourhood outcome variable are plotted against the voting school vote share. Voting schools included in the panels are non-grammar schools with vote shares in the (15,85) interval. Number of voting schools is, from top to bottom, 604, 604, 565, 500, 565 and 562. We lose schools as the neighbourhood gets smaller and when we use the baseline pass weights (available only for voting schools with baseline bass rates).

Appendix Figure 1: Eligible Voters, % Turnout



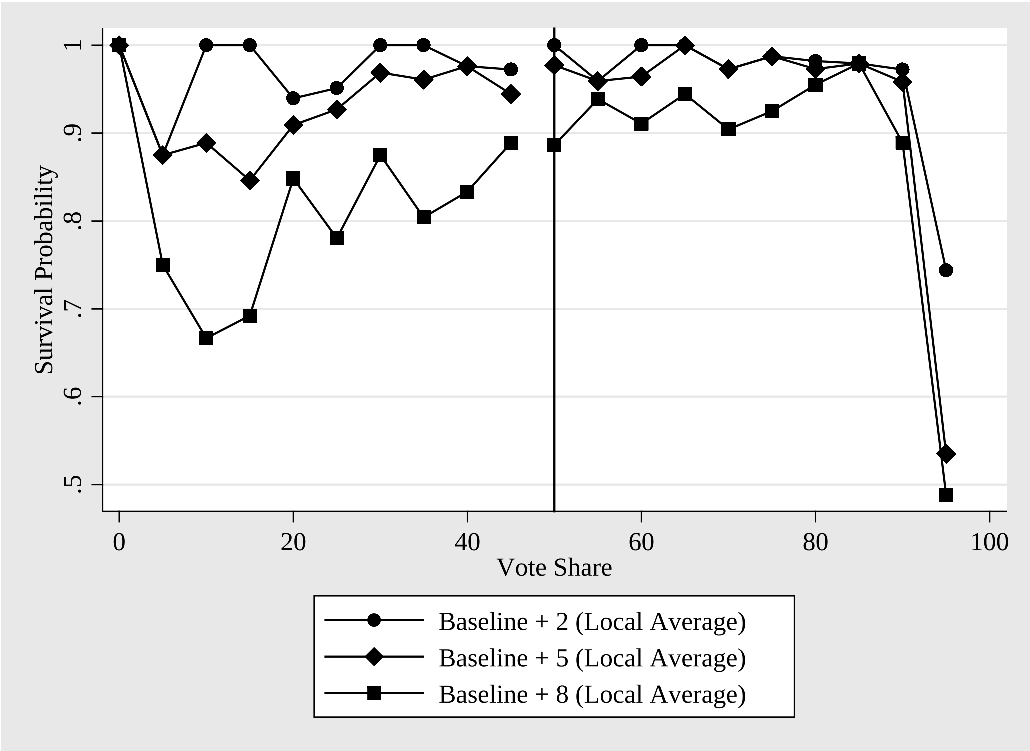
Notes: Sample includes all 946 ballots.

Appendix Figure 2: Base Year Spare Capacity Distribution



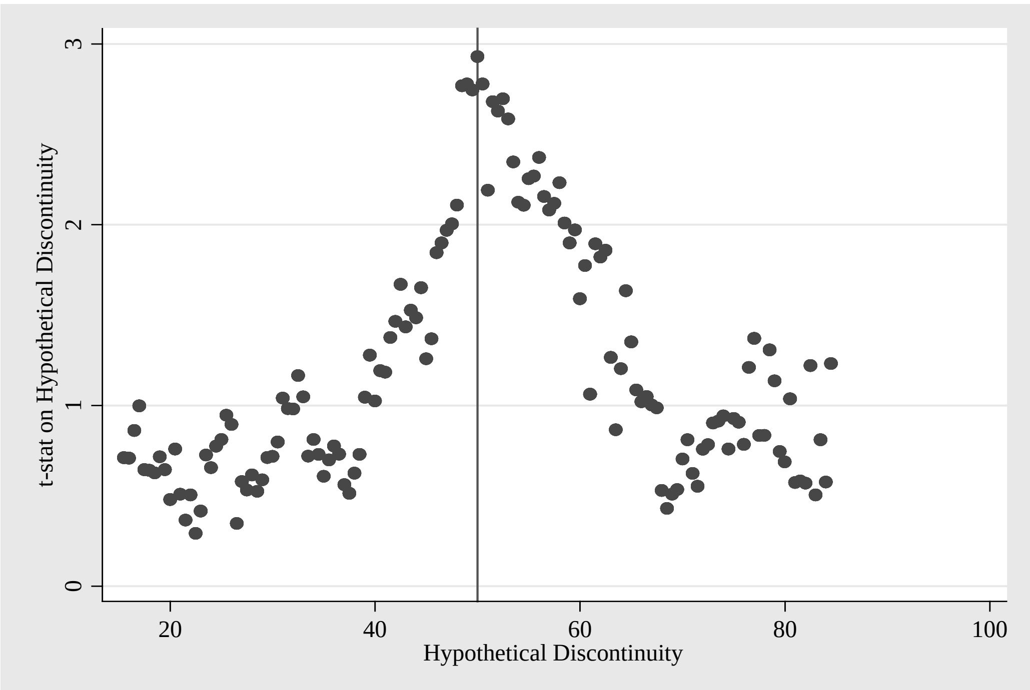
Notes: Sample includes all GM ballot schools (946) with valid capacity data (898). Capacity data taken from 1992 Census (where available) or 1993 Census.

Appendix Figure 3: Survival Probability



Notes: Sample includes all 946 ballots.

Appendix Figure 4: Pass Rate Discontinuity Checks



Notes: Every point in the graph is the mean of 8 absolute t-ratios. These are the t-ratios associated with the estimated discontinuity when the discontinuity is assumed to occur at this vote share level, over the years 1, 2, …8 after the base year. Hence we estimate 8*140 models, for hypothetical discontinuities 15, 15.5, 16, …85. In each case we regress the outcome on the discontinuity and the vote share interacted with win/lose.

Table 1a: LEA-Maintained versus Grant-Maintained Schools in 1988

	LEA Maintained Schools	Grant Maintained Schools
Governing Body (GB)	Must have some parental representation from 1986 but still LEA-dominated	GB incorporated. Typical composition: 8 appointed governors; 5 parent governors; 1 teacher governor; 1 head teacher
Buildings/ Grounds	Landlord-tenant relationship between LEA & school.	GB becomes owner of buildings/grounds.
Capital Projects	Capital projects decided & funded by LEA	Capital Formula Funding & Application to Secretary of State for major capital funding
Funding	LEA decides how much of education budget to spend on ‘central services’ (e.g. school transport, special needs) and how much to allocate to schools. Specifies formula for distributing to individual schools. Formula must be approved by Secretary of State and must distribute 75% of funds based on pupil numbers.	GB receives Annual Maintenance Grant equal to its revenue under the LEA formula and its share of LEA money ‘held back’ for central services. Also receives transitional and ‘special purpose’ grants.
Staff	LEA is the employer of teachers. GB can determine number of teachers but appointments and dismissals in consultation with LEA. Teachers paid according to national pay scale.	GB becomes employer of staff. Need not use the national pay scale.
Admissions	LEA is ‘admissions authority’ and determines admissions policy (application procedure, over-subscription criteria, appeals procedure). Parents’ must be allowed to express preference; preference must be respected until numbers reach school capacity level (determined by Secretary of State).	GB becomes admissions authority. Must publish admissions policy in proposals to become GM.
Curriculum	‘National Curriculum’ introduced in 1988 Act: splits compulsory school years into four ‘key stages’ and subjects into ‘core’ and ‘foundation’. Specifies in detail what is studied at each stage.	Same as LEA-maintained schools.
Testing	Common examinations taken in every subject at the end of compulsory education (aged 16). In addition, after the 1988 Act, assessment and testing at the end of every ‘key stage’.	Same as LEA-maintained schools.
Information	Parents have right to see school examination results.	GB must publish annual report, annual admissions policy, annual School Development Plan. School accounts must be audited. Must organise annual parents meeting.

Table 1b: Significant Events post-1988

April 1992	Conservative Party win General Election
October 1992	School Performance Tables introduced
September 1993	New school inspection regime
October 1995	Labour Party changes GM policy: support autonomy, oppose ‘unfair’ funding
May 1997	Labour Party win General Election. No new GM applications considered.
July 1998	Under 1998 School Standards and Framework Act, existing GM schools become Foundation schools, LEA-GM funding equalised. LEA formula changed such that 85% delegated to schools.

Table 2: Probability of Ever Holding a GM Vote (Linear Probability Models)

	(1)	(2)	(3)	(4)	(5)	(6)
Modern	-0.369	-0.365	-0.342	-0.341	-0.106	-0.116
	(0.042)	(0.043)	(0.044)	(0.044)	(0.065)	(0.066)
Comprehensive 11-18	-0.188	-0.185	-0.160	-0.160	0.017	0.013
	(0.038)	(0.039)	(0.040)	(0.040)	(0.054)	(0.055)
Comprehensive 11-16	-0.206	-0.201	-0.186	-0.184	0.005	-0.000
	(0.039)	(0.040)	(0.041)	(0.041)	(0.056)	(0.057)
Middle School	-0.338	-0.337	-0.342	-0.342	0.000	0.000
	(0.044)	(0.044)	(0.046)	(0.046)	(0.000)	(0.000)
Upper School	-0.239	-0.237	-0.198	-0.196	-0.078	-0.066
	(0.045)	(0.046)	(0.047)	(0.047)	(0.061)	(0.064)
Church of England	0.040	0.040	0.015	0.016	-0.005	0.000
	(0.028)	(0.028)	(0.029)	(0.029)	(0.036)	(0.037)
Catholic	0.081	0.082	0.067	0.068	0.059	0.063
	(0.021)	(0.022)	(0.024)	(0.024)	(0.027)	(0.027)
Other Religious	0.132	0.134	0.131	0.131	0.106	0.100
	(0.034)	(0.034)	(0.035)	(0.035)	(0.037)	(0.039)
Urban		-0.018	-0.022	-0.019	-0.028	-0.037
		(0.026)	(0.026)	(0.026)	(0.031)	(0.033)
Conservative		0.034	0.029	0.028	0.026	0.026
		(0.017)	(0.018)	(0.018)	(0.020)	(0.021)
Distance to:						
- Nearest LEA School		-0.362	-0.263	-0.267	-0.150	-0.276
		(0.293)	(0.333)	(0.334)	(0.506)	(0.511)
		[0.0196]	[0.0142]	[0.0144]	[0.008]	[0.0149]
- Nearest Non-LEA School		-0.210	-0.233	-0.229	-0.288	-0.319
		(0.087)	(0.089)	(0.089)	(0.102)	(0.105)
		[0.0243]	[0.0256]	[0.0252]	[0.0334]	[0.037]
Spare Capacity (1989)			-0.053	-0.051	-0.029	-0.032
			(0.020)	(0.020)	(0.023)	(0.023)
			[0.022]	[0.021]	[0.0122]	[0.0131]
Enrollment Change: 86-89			0.076	0.103	0.041	0.069
			(0.030)	(0.032)	(0.033)	(0.036)
			[0.0213]	[0.0282]	[0.0131]	[0.0164]
% FSM (1989)			-0.112	-0.114	-0.015	-0.028
			(0.076)	(0.076)	(0.089)	(0.090)
			[0.0112]	[0.0114]	[0.0015]	[0.0028]
% Pass Rate (1992)					0.035	0.034
					(0.007)	(0.007)
					[0.0699]	[0.0681]
Neighbourhood Mean:				-0.072		-0.069
--Spare Capacity (1989)				(0.041)		(0.049)
				[0.0175]		[0.0149]
--Enrollment Change 86-89				-0.174		-0.173
				(0.081)		(0.095)
				[0.0204]		[0.0203]
--% FSM (1989)				0.105		0.195
				(0.223)		(0.289)
				[0.0014]		[0.00273]
--% Pass Rate (1992)						-0.001
						(0.009)
						[0.00166]
Observations	3747	3738	3535	3531	2936	2865
R-squared	0.24	0.25	0.27	0.27	0.28	0.28

Notes: The model in column (1) is estimated on the sample of schools at risk of GM (the ‘competition sample, see Appendix Table 1 and 2b). The base category is non-religious grammar schools. Smaller sample sizes in later columns due to missing observations. Each model also includes LEA dummies and a linear spline in the number of neighbours in the neighbourhood. The neighbourhood is defined in terms of a 4km circle around the school. Robust standard errors in parentheses. Calculated effect of a 1 standard deviation change in the regressor in square brackets.

Table 3a: Impact of GM Status on Pass Rates of Schools that become Grant-Maintained: Two Years after Base Year

	Least Squares Regression							Least Abs Dev
	Non-Grammar Schools with Vote Shares in [15,85] interval						All Schools	Non-Grammar Votes [15,85]
Win	2.169 (0.636)	4.052 (1.367)	3.894 (1.392)	3.297 (1.352)	2.721 (2.190)	3.454 (1.339)	3.188 (1.206)	2.698 (1.591)
Vote		-5.3 (3.2)						
Vote*Lose			-2.693 (5.568)	-2.966 (5.324)	-16.418 (22.617)	-3.208 (5.241)	-4.251 (4.802)	-6.053 (6.735)
Vote*Win			-6.424 (3.937)	-3.652 (3.915)	17.945 (16.758)	-5.012 (3.872)	-2.768 (2.659)	0.831 (4.563)
$Vote^2$*Lose					40.750 (63.377)			
$Vote^2$*Win					0.006 (0.004)			
SES change						-0.366 (0.094)	-0.296 (0.086)	-0.320 (0.084)
Weighted	N	N	N	Y	Y	Y	Y	Y
Polynomial	None	Linear	Linear *Win	Linear *Win	Quad *Win	Linear *Win	Linear *Win	Linear *Win
Controls	N	N	N	N	N	Y	Y	Y
N	524	524	524	524	524	524	729	524
R-sq	0.02	0.03	0.03	0.02	0.03	0.07	0.07	0.04

Notes: Robust standard errors in parentheses. Vote share is divided by 100. SES change proxied by Free School Meal take-up. Additional controls are school type dummies and vote year dummies.

Table 3b: Impact of GM Status on Pass Rates of Schools that become Grant-Maintained: One-Three Years after Base Year

Year After Base Year	Least Squares Regression							Least Absolute Deviations
	Non-Grammar schools with Vote Shares in [15,85] interval						All Schools	Non-Grammar Votes [15,85]
1	0.908 (0.605)	3.488 (1.207)	3.353 (1.218)	2.651 (1.218)	0.374 (1.857)	2.791 (1.232)	2.402 (1.110)	-0.050 (1.289)
2	2.169 (0.636)	4.052 (1.367)	3.894 (1.392)	3.297 (1.352)	2.721 (2.190)	3.454 (1.339)	3.188 (1.206)	2.698 (1.591)
3	2.745 (0.666)	5.774 (1.344)	5.693 (1.354)	4.746 (1.364)	4.382 (2.069)	5.001 (1.361)	3.879 (1.218)	4.107 (1.779)

Notes: See Table 3a. Sample sizes are 525, 524 and 524.

Table 3c: Impact of GM Status on Pass Rates of Schools that become Grant-Maintained: Four-Eight Years after Base Year

Year After Base Year	Least Squares Regression							Least Absolute Deviations
	Non-Grammar Schools with Vote Shares in [15,85] interval						All Schools	Non-Grammar Votes [15,85]
4	1.945 (0.735)	4.584 (1.497)	4.636 (1.523)	4.658 (1.469)	3.784 (2.208)	4.788 (1.518)	3.601 (1.381)	5.192 (1.097)
5	2.777 (0.748)	4.639 (1.521)	4.288 (1.582)	4.040 (1.564)	3.655 (2.416)	4.349 (1.571)	3.491 (1.436)	5.600 (1.742)
6	2.751 (0.788)	5.421 (1.622)	4.796 (1.676)	3.938 (1.677)	2.326 (2.484)	4.184 (1.644)	3.184 (1.485)	5.174 (2.024)
7	3.282 (0.858)	5.974 (1.778)	5.549 (1.802)	5.143 (1.781)	1.655 (2.745)	5.165 (1.756)	3.836 (1.565)	3.953 (1.904)
8	3.471 (0.929)	5.096 (1.974)	4.525 (2.018)	3.829 (2.008)	0.750 (3.069)	3.627 (1.946)	3.018 (1.815)	3.460 (2.152)

Notes: See Table 3a. Sample sizes are 522, 520, 519, 502, 487.

Table 4: Impact of GM status on enrollment of schools that become GM: One-Eight Years after Base Year

Year After Baseline	Least Squares Regression					Least Absolute Deviations				
Year 1	1.966	8.438	8.776	20.259	8.901	2.616	3.193	2.696	10.348	1.604
	(2.094)	(4.317)	(4.456)	(7.075)	(4.365)	(2.440)	(5.511)	(5.441)	(7.566)	(3.866)
Year 2	2.224	8.721	7.717	18.174	6.644	3.175	10.295	10.497	16.625	3.197
	(2.188)	(4.389)	(4.373)	(6.466)	(4.313)	(2.518)	(4.620)	(5.443)	(7.861)	(4.662)
Year 3	2.839	12.823	13.516	24.146	14.450	6.048	10.920	11.517	20.880	7.315
	(2.445)	(4.950)	(5.084)	(7.432)	(4.978)	(2.470)	(5.790)	(5.871)	(8.212)	(4.971)
Year 4	3.088	9.081	9.723	15.647	8.725	3.459	8.609	9.923	4.362	7.052
	(2.713)	(5.676)	(5.722)	(8.701)	(5.730)	(2.619)	(5.430)	(5.362)	(8.058)	(5.837)
Year 5	6.104	14.091	16.209	22.370	16.704	5.310	12.056	15.794	21.557	13.369
	(2.757)	(5.839)	(5.941)	(8.684)	(5.970)	(2.352)	(5.073)	(5.494)	(9.017)	(5.699)
Year 6	4.475	14.435	15.226	24.384	15.497	7.153	15.069	15.686	26.041	16.469
	(2.963)	(6.472)	(6.627)	(10.205)	(6.578)	(3.577)	(6.525)	(6.869)	(10.753)	(7.752)
Year 7	8.994	13.496	16.207	11.911	15.625	9.821	19.363	19.709	20.860	15.705
	(3.342)	(6.660)	(7.028)	(9.954)	(6.607)	(3.935)	(6.286)	(6.960)	(11.829)	(7.674)
Year 8	6.007	11.422	13.686	14.531	12.551	9.898	18.960	22.552	19.217	17.777
	(3.249)	(6.562)	(6.598)	(9.670)	(6.426)	(3.735)	(6.896)	(6.504)	(9.309)	(6.149)
Polynomial	None	Linear	Linear *Win	Quad *Win	Linear *Win	None	Linear	Linear *Win	Quad *Win	Linear *Win
Controls	N	N	N	N	Y	N	N	N	N	Y

Notes: Sample comprises all non-grammar schools with vote shares in the (15,85) interval observed in the Schools Census at least two years before and at least five years after the vote. Sample sizes range from 611 (Year 1) to 572 (Year 8). The dependent variable is the difference in entry age enrolment 1,2,..8 years after the base year and the mean enrolment across the two years before the base year. Controls are school type dummies, year of vote dummies and baseline enrolment levels. Robust standard errors in parentheses.

Table 5: Impact of GM Status on Pass Rates of Schools in the Neighbourhood of GM Schools: 1992-1997

	Neighbourhood 12km		Neighbourhood 12km		Neighbourhood 8km		Neighbourhood 4km		Neighbourhood 12km	
	Unweighted		Distance-weighted		Distance-weighted		Distance-weighted		Baseline weighted	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: 1992-1997: Basic Specification										
Vote Win	-0.090	0.293	-0.040	0.462	-0.033	0.617	0.084	0.557	-0.149	0.290
	(0.180)	(0.378)	(0.223)	(0.452)	(0.297)	(0.587)	(0.527)	(1.038)	(0.199)	(0.393)
Baseline		-0.073		-0.072		-0.064		-0.075		-0.079
		(0.012)		(0.014)		(0.016)		(0.017)		(0.012)
Change SES		0.115		0.048		-0.084		-0.310		0.131
		(0.041)		(0.047)		(0.057)		(0.063)		(0.042)
N	604	604	604	604	565	565	500	500	562	562
R-squared	0.00	0.11	0.00	0.09	0.00	0.08	0.00	0.12	0.00	0.14
Panel B: 1992-1997: Robustness Checks										
Quad*Win	-0.090	0.170	-0.040	0.158	-0.033	0.016	0.084	-0.895	-0.149	0.352
	(0.180)	(0.586)	(0.223)	(0.662)	(0.297)	(0.827)	(0.527)	(1.514)	(0.199)	(0.596)
All Vote Schools	-0.072	-0.005	0.035	0.129	0.164	0.215	0.391	0.507	-0.167	0.068
	(0.159)	(0.319)	(0.195)	(0.377)	(0.257)	(0.477)	(0.448)	(0.819)	(0.173)	(0.341)
Least Absolute Dev	0.020	0.152	-0.055	0.826	0.048	0.552	-0.202	0.334	-0.015	0.517
	(0.210)	(0.256)	(0.324)	(0.285)	(0.331)	(0.364)	(0.688)	(1.062)	(0.336)	(0.326)
Base Year>=1992	-0.005	0.380	0.059	0.585	0.148	0.895	0.349	0.926	-0.101	0.373
	(0.187)	(0.380)	(0.230)	(0.439)	(0.311)	(0.581)	(0.560)	(1.104)	(0.203)	(0.408)
Panel C: 1992-1998, 1992-1999, 1992-2000: Basic Specification										
1992-1998	0.400	0.241	0.438	0.445	0.554	0.747	0.994	1.213	0.349	0.258
	(0.229)	(0.480)	(0.270)	(0.531)	(0.346)	(0.661)	(0.553)	(1.093)	(0.248)	(0.497)
1992-1999	0.409	0.331	0.592	0.494	0.706	0.638	0.810	0.478	0.329	0.280
	(0.473)	(0.454)	(0.552)	(0.513)	(0.721)	(0.683)	(1.268)	(1.222)	(0.504)	(0.469)
1992-2000	0.508	0.369	0.573	0.519	0.727	0.768	1.234	0.850	0.491	0.386
	(0.509)	(0.467)	(0.610)	(0.541)	(0.795)	(0.720)	(1.362)	(1.307)	(0.555)	(0.487)

Notes: Neighbourhood defined in terms of a circle of various radii around the voting school. Unless otherwise specified, sample excludes grammar schools and includes only schools with vote shares in the (15,85) interval. For every outcome, the first specification includes no controls, the second includes a linear interaction of vote share and win, vote year dummies, vote school type dummies, change in neighbourhood SES as measured by change in FSM take-up and the number of schools in the relevant neighbourhood. All estimates are weighted by the correctly weighted number of neighbourhood exam takers across the two years. Robust standard errors in parentheses.

Table 6: Impact of GM Status on District-Level Pass Rates

	1992-1997						1992-1998	1992-1999	1992-2000
	(1)	(2)	(3)	(4)	(5)	(6)	(6)	(6)	(6)
Panel A: Dependent Variable: Fraction of 1997 District Exam-Takers in GM Schools (Non-Grammar Vote (15,85))									
>=1 Win	0.192	0.179	0.151	0.149	0.150	0.156	0.165	0.161	0.161
	(0.010)	(0.010)	(0.020)	(0.021)	(0.022)	(0.026)	(0.027)	(0.027)	(0.028)
>=2 Wins	0.162	0.145	0.102	0.086	0.085	0.126	0.119	0.108	0.109
	(0.019)	(0.019)	(0.046)	(0.042)	(0.043)	(0.053)	(0.054)	(0.055)	(0.054)
>=3 Wins	0.202	0.201	0.238	0.227	0.221	0.204	0.184	0.207	0.204
	(0.033)	(0.032)	(0.060)	(0.058)	(0.059)	(0.067)	(0.067)	(0.067)	(0.065)
N	505	505	505	505	505	505	503	501	499
R-squared	0.81	0.83	0.85	0.87	0.88	0.93	0.93	0.93	0.93
Panel B: Dependent Variable: Change in District Pass-Rate (percentage points)									
>=1 Win	1.115	1.338	1.025	1.044	1.349	0.827	2.768	2.068	0.781
	(0.511)	(0.637)	(1.231)	(1.183)	(1.158)	(1.398)	(1.443)	(1.615)	(1.562)
>=2 Wins	0.154	0.477	2.293	2.338	3.136	2.274	-0.492	2.244	-1.113
	(0.716)	(0.811)	(1.711)	(1.590)	(1.569)	(1.884)	(1.994)	(2.144)	(1.898)
>=3 Wins	-1.384	-1.504	-0.320	-0.633	-0.641	-0.068	-1.584	-1.255	-1.933
	(0.788)	(0.953)	(1.755)	(1.784)	(1.637)	(2.024)	(2.177)	(2.110)	(2.251)
Baseline				-0.093	-0.174	-0.195	-0.184	-0.160	-0.193
				(0.021)	(0.029)	(0.037)	(0.041)	(0.042)	(0.043)
FSM previous				-0.149	-0.286	-0.733	-0.670	-0.548	-0.748
				(0.0705)	(0.0769)	(0.114)	(0.159)	(0.192)	(0.173)
FSM concurrent				-0.082	-0.406	-0.602	-0.689	-0.713	-0.955
				(0.086)	(0.098)	(0.144)	(0.122)	(0.123)	(0.142)
N	505	505	505	505	505	505	503	501	499
R-squared	0.01	0.08	0.12	0.20	0.32	0.58	0.61	0.61	0.64
Panel C: Robustness Check: All Vote Wins (including Grammar and Vote Shares (0,100))									
>=1 Win	0.725	0.786	0.412	0.235	0.720	1.100	2.351	2.048	1.216
	(0.493)	(0.620)	(1.129)	(1.093)	(0.996)	(1.270)	(1.443)	(1.538)	(1.456)
>=2 Wins	0.964	0.760	1.612	1.526	2.263	2.305	-0.433	1.790	-0.811
	(0.792)	(0.888)	(1.571)	(1.594)	(1.533)	(1.619)	(1.845)	(1.911)	(1.793)
>=3 Wins	-0.881	-0.964	0.039	-0.172	0.040	-0.219	1.206	2.027	-0.108
	(0.780)	(1.020)	(2.096)	(1.991)	(1.915)	(1.785)	(1.727)	(2.136)	(2.021)

Notes: School districts defined as Parliamentary Constituencies. Some districts are excluded if school reorganisation meant that more than 25% of schools ‘turned over’ during the period (e.g. a majority of schools change age structure, type or close). All models are weighted by an average of the number of district exam takers over the two periods. Robust standard errors in parentheses. Specification (1) does not include controls. Controls included in other columns are number of schools and elections (specification (2)), 13 linear vote share*election interactions (specification (3)), controls for baseline pass rates, change in FSM and school type fractions (specification (4)), controls for 1991 census variables (proportion individuals black, proportion individuals Indian, Pakistani or Bangladeshi, proportion households owner-occupied, proportion households council house, proportion households with children under 16 single-parent), 1991-2001 census trends (changes in these variables), 9 region (standard statistical region) and 3 region type (County, Metropolitan, London) dummies (specification (5)) and a complete set of LEA (or modal LEA where district straddles two LEAs) dummies (specification (6)).

Appendix Table 1: The School Dataset

Panel A: School Census Data							
# Schools with this data pattern	1988	1989	1990	1991	1992	1993-2002	2003
3135	√	√	√	√	√	√	√
338	√	√	√	√	√	√	
113	√	√	√	√	√		
75	√	√	√	√			
115	√	√	√				
97	√	√					
176	√						
43		√	√	√	√	√	√
36			√	√	√	√	√
35				√	√	√	√
27					√	√	√
144						√	√
5		√	√	√	√	√	
1		√	√	√			
1		√	√				
1			√	√	√	√	
1				√			
12						√	
16							√
Total # Schools in window	Total # observations in Census Year:						
4371	4049	3923	3863	3783	3733	-	3436

Panel B: Matching to the Performance Tables (PT) Data							
# Middle Schools in Census	640	569	554	515	514	-	309
Expected PT obs					3219	-	3125
Actual PT obs					3203	-	3092

Panel C: Matching to the Edubase Data							
# Schools closed	176	97	116	77	113	356	3436
- w/ missing GIS	81	26	33	25	3	17	0
# Schools open 1988 closed	176	97	115	75	113	304	3135
- w/ missing GIS	81	26	33	24	3	17	0

FN: Schools ‘closed’ are schools observed in the Census for the last time. Could be closure, merger or other ‘significant change in character’ – see text.

Appendix Table 2a: Autonomy Sample – Descriptive Statistics

	All Schools		Non-Grammar Schools						
	Win & Lose		Win & Lose			Win		Lose	
	Mean	N	Mean	St. Devn	N	Mean	N	Mean	N
			Panel A: GM Database (Election) Data						
# Attempts	1.05	946	1.05		844	1.00	586	1.15	258
1st Attempt: #Ballots	1.08	946	1.09		844	1.10	586	1.08	258
1st Attempt; 1st Ballot:									
- Eligible Voters	1252.1	946	1258.2	466.89	844	1221.0	586	1342.7	258
- % Turnout	62.99	946	61.99	9.84	844	61.81	586	62.40	258
- % Yes	64.38	946	63.41	23.91	844	76.95	586	32.68	258
GM First Attempt	0.70	946	0.68		844	0.97	586	0.01	258
Ever GM	0.72	946	0.70		844	0.97	586	0.10	258
			Panel B: School Census Data						
1989: Entry Enrollment	142.98	923	149.04	59.16	823	142.28	570	164.26	253
FSM take-up	5.99	923	6.50	6.53	823	6.05	570	7.50	253
1992: Entry Enrollment	163.27	929	169.73	62.52	828	163.47	576	184.03	252
FSM take-up	8.24	929	8.97	8.15	828	8.60	576	9.83	252
1995: Entry Enrollment	167.50	907	173.40	61.51	808	167.88	560	185.85	248
FSM take-up	11.03	907	11.99	9.49	808	11.64	560	12.80	248
1998: Entry Enrollment	177.68	897	183.95	62.49	798	180.89	553	190.87	245
FSM take-up	10.11	897	11.03	8.62	798	10.72	553	11.73	245
1998: Entry Enrollment	188.02	882	194.77	63.15	783	191.73	546	201.78	237
FSM take-up	8.70	882	9.55	8.40	783	8.98	546	10.85	237
			Panel C: School Performance (League Table) Data						
1992: #Grade 11/Age 15	148.83	887	155.39	56.87	786	149.62	552	169.00	234
% Pass	42.47	887	36.25	16.32	786	35.88	552	37.13	234
1995: #Grade 11/Age 15	162.34	862	168.74	60.14	763	161.71	533	185.05	230
% Pass	48.48	862	42.44	16.44	763	42.63	533	42.00	230
1998: #Grade 11/Age 15	164.36	856	170.27	58.47	757	164.31	529	184.08	228
% Pass	52.60	856	46.85	16.71	757	47.32	529	45.76	228
2001: #Grade 11/Age 15	176.14	845	182.78	59.19	746	178.27	525	193.49	221
% Pass	57.11	845	51.72	17.16	746	52.48	525	49.92	221
			Panel C: Base Year Information (Census & Performance Data)						
Type: Grammar	0.11	946	0.00		844	0.00	586	0.00	258
Modern	0.07	946	0.08		844	0.11	586	0.02	258
Comp 11-18	0.43	946	0.49		844	0.50	586	0.46	258
Comp 11-16	0.30	946	0.33		844	0.32	586	0.36	258
Middle	0.04	946	0.04		844	0.03	586	0.06	258
Upper	0.05	946	0.06		844	0.04	586	0.10	258
Control: LEA	0.75	946	0.76		844	0.74	586	0.82	258
Vol. Control	0.05	946	0.05		844	0.06	586	0.03	258
Vol. Aided	0.11	946	0.13		844	0.15	586	0.08	258
Other	0.08	946	0.06		844	0.06	586	0.07	258
Entry Enrollment	159.69	942	165.99	62.87	841	159.67	585	180.45	256
FSM Take-Up	9.28	942	10.11	8.71	841	9.72	585	11.00	256
# Grade 11/Age 15	142.25	942	146.93	64.93	841	141.61	585	159.08	256
% Pass	43.30	745	38.39	15.95	678	37.35	473	40.78	205

Notes: Sample means (and standard deviations for non-discrete data) are shown according to ‘win’ (>50% of the vote) on the first ballot of the first attempt. Sample sizes in census data vary because of entry and exit of voting schools into the Census. Sample sizes in performance data vary for the same reason, and because not every school enrols grade 11/age 15 students (the age group that takes the exams).

Appendix Table 2b: Full Sample and Competition Sample

	Full Sample					Competition Sample (in Census in 1989 with postcode information)							
	All		Vote			Non-Vote		Non-Grammar Vote		Non-Grammar Win		Non-Grammar Lose	
	Mean	Count	Mean	St Devn	Count	Mean	Count	Mean	Count	Mean	Count	Mean	Count
1989: Entry Enrollment	141.93	4035	142.98	59.35	923	137.63	3747	149.40	813	142.93	566	164.23	247
FSM take-up	0.09	4035	0.06	0.06	923	0.09	3747	0.06	813	0.06	566	0.07	247
1995: Entry Enrollment	161.73	3614	167.50	61.08	907	161.50	3385	172.73	781	166.98	541	185.70	240
FSM take-up	0.14	3614	0.11	0.09	907	0.14	3385	0.12	781	0.11	541	0.13	240
2001: Entry Enrollment	179.59	3480	188.02	63.11	882	180.24	3188	194.04	756	190.75	527	201.61	229
FSM take-up	0.12	3481	0.09	0.08	882	0.11	3188	0.09	756	0.09	527	0.11	229
1992/3 Capacity	187.52	3172	182.61	71.44	761	185.71	3047	192.17	670	184.16	463	210.07	207
1992: %Pass	34.22	3319	42.47	23.31	887	35.87	3031	36.75	761	36.46	536	37.45	225
#Grade 11/Age 15	146.72	3319	148.83	57.37	887	151.12	3031	155.01	761	148.80	536	169.80	225
1998: %Pass	43.82	3137	52.60	22.40	856	44.43	2923	47.27	732	47.90	512	45.82	220
#Grade 11/Age 15	163.95	3137	164.36	58.07	856	164.38	2923	169.97	732	163.87	512	184.18	220
Type: Grammar	0.04	3773	0.11	0.31	924	0.04	3747	0.00	813	0.00	566	0.00	247
Modern	0.05	3773	0.07	0.26	924	0.06	3747	0.07	813	0.09	566	0.01	247
Comp 11-18	0.35	3773	0.44	0.50	924	0.40	3747	0.53	813	0.54	566	0.51	247
Comp 11-16	0.34	3773	0.30	0.46	924	0.29	3747	0.30	813	0.29	566	0.32	247
Middle	0.12	3773	0.04	0.19	924	0.14	3747	0.04	813	0.03	566	0.06	247
Upper	0.06	3773	0.05	0.22	924	0.07	3747	0.06	813	0.05	566	0.10	247
Control: LEA	0.79	3773	0.72	0.45	924	0.80	3747	0.76	813	0.73	566	0.82	247
Vol. Control	0.06	3773	0.06	0.23	924	0.06	3747	0.05	813	0.06	566	0.04	247
Vol. Aided	0.10	3773	0.11	0.32	924	0.10	3747	0.13	813	0.15	566	0.08	247
Other	0.05	3773	0.11	0.31	924	0.04	3747	0.06	813	0.05	566	0.06	247
# Neigbours 12km						7.12	3747	6.30	813	6.22	566	6.48	247
# Neighbours 8km						21.53	3747	19.56	813	19.26	566	20.24	247
# Neighbours 4km						37.46	3747	34.61	813	33.95	566	36.14	247
County	0.60	3773	0.67	0.47	924	0.62	3747	0.67	813	0.69	566	0.62	247
Metropolitan Non-London	0.28	3773	0.17	0.38	924	0.26	3747	0.16	813	0.12	566	0.26	247
London	0.12	3773	0.16	0.37	924	0.12	3747	0.17	813	0.19	566	0.12	247

Notes: 'Vote', 'Non-Vote' refers to schools that ever (never) attempted to become grant maintained. Neighbours includes middle-deemed-secondary as well as secondary schools. FSM refers to Free School Meals. Entry/FSM data from the schools census, pass rates from Performance Tables. In the full sample, type, control and county refer to schools in the schools census in 1993.

Appendix Table 3a: Schools, Votes and Wins across Districts (Excluding Grammar School Wins, Wins outside of (15,85) Vote Share)

# Schools	Votes = 0	Votes = 1	Votes = 2	Votes >= 3	Total
2	5	0	0	0	5
3	22	10	2	1	35
4	20	17	12	4	53
5	59	29	12	12	112
6	53	37	20	21	131
7	34	25	12	20	91
8	17	11	9	12	49
9	3	9	8	10	30
Total	214	138	75	80	507

# Votes	Wins=0	Wins =1	Wins =2	Wins >=3	Total
0	214	0	0	0	214
1	81	57	0	0	138
2	15	25	35	0	75
3	5	7	23	45	80
Total	315	89	58	45	507

Notes: School districts defined in terms of Parliamentary Constituencies (PCs). All PCs included except the Isle of Wight (for which we lack census data) and the (21) PCs in which more than one quarter of schools were closed or reorganised over between 1989 and 1997.

Appendix Table 3b: Schools, Votes and Wins across Districts (All Wins)

# Schools	Votes = 0	Votes = 1	Votes = 2	Votes >= 3	Total
2	4	1	0	0	5
3	14	13	1	2	30
4	16	15	11	6	48
5	48	31	11	16	106
6	42	34	15	34	125
7	28	17	10	24	79
8	13	13	6	25	57
9	3	9	11	34	57
Total	168	133	65	141	507

# Votes	Wins=0	Wins =1	Wins =2	Wins >=3	Total
0	168	0	0	0	168
1	72	61	0	0	133
2	13	24	28	0	65
3	4	13	22	102	141
Total	257	98	50	102	507

Notes: see notes to Appendix Table 3a.