DOES ADDITIONAL SPENDING HELP URBAN SCHOOLS? AN EVALUATION USING BOUNDARY DISCONTINUITIES

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Abstract
This study exploits spatial anomalies in school funding policy in England to provide new evidence on the impact of resources on student achievement in urban areas. Anomalies arise because the funding allocated to Local Education Authorities (LEA) depends, through a funding formula, on the 'additional educational needs' of its population and prices in the district. However, the money each school receives from its LEA is not necessarily related to the school's own specific local conditions and constraints. This implies that neighbouring schools with similar intakes, operating in the same labour market, facing similar prices, but in different LEAs, can receive very different incomes. We find that these funding disparities give rise to sizeable differences in pupil attainment in national tests at the end of primary school, showing that school resources have an important role to play in improving educational attainment, especially for lower socio-economic groups. The design is geographical boundary discontinuity design which compares neighbouring schools, matched on a proxy for additional educational needs of its students (free school meal entitlement – FSM), in adjacent districts. The key identification requirement is one of conditional ignorability of the level of

LEA grant, where conditioning is on geographical location of schools and their proportion of FSM children. (JEL: R0, I21, H52)

1. Introduction

Improvement of the educational attainment of poor children is a top priority in many countries. This is a particular problem in countries, such as the United Kingdom and United States, where there are long tails in the bottom end of the adult distribution of basic literacy and numeracy skills, especially for the younger generation (OECD 1995, 2013). This bottom tail is heavily populated with people who have been disadvantaged since childhood, and many of these children live in inner-city urban areas.[1] In the United Kingdom there is a substantial attainment gap at school entry between low-income pupils who are eligible to receive free school meals (FSM) and the rest, and this gap widens over time (National Equality Panel 2010).[2] Recent academic work on addressing this gap, and raising achievements more generally, has turned attention towards institutional structures and incentives, such as greater school autonomy and competition. However, resources are an important part of educational policy and any lowering of real per pupil expenditure (as is happening currently in the United Kingdom) is extremely controversial. In England, districts and schools with poorer children receive more funding on the implicit assumption that more money helps. In this paper, we revisit this central question of whether simply allocating more money to schools results in higher achievement, where the context is urban areas (which tend to have a higher proportion of disadvantaged students). Our empirical analysis contributes to the literature by using a geographical boundary discontinuity design that focusses on the effects of explicit differences in the grants paid to neighbouring city schools districts in England. The design enables us to identify the effect of expenditure on pupil outcomes, despite the fact that central government funding to Local Education Authorities (LEAs) is explicitly compensatory.

The research design is rooted in education funding policy anomalies in England which means that schools which are close together but in different education districts— LEAs—can get very different levels of core funding from central government. These funding differentials arise between schools that are otherwise very similar in their geographical location, catchment areas, student demographics, and the prices they face

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1. It has long been established that family background and early childhood experiences are the most important determinants of educational outcomes (Coleman 1966). The relationship between family background and educational attainment is stronger in England than in any of the 54 countries included in the Trends in International Mathematics and Science Study (TIMSS) study (Schuetz et al. 2005; Blanden 2009; Freeman and Viarengo 2014).

2. Specifically, the proportion of poor children reaching the "expected level" at school entry (the "Foundation Stage") is 22 percentage points lower than others. This widens over time. For example, on leaving school only 13% of pupils eligible to receive free school meals go on to higher education compared to 32% of all others (NEP report. p. 341).

for their inputs.[3] This discrepancy occurs because core funding is allocated to LEAs by central government according to: (a) the proportion of students from low-income families and those with English as a second language—"additional educational needs" (AEN); (b) an index (the "area cost adjustment", ACA) computed for broader labour market areas that compensates the LEA for high labour costs; and (c) an adjustment for low population density to compensate for fixed costs in rural areas. However, the funding is not redistributed from LEAs to schools according to these rules. Therefore, two neighbouring schools in adjacent LEAs facing students with similar educational needs, staff wages, input prices, and other constraints can get different levels of funding simply because of the difference in the average educational needs of the LEA and the average market wages in the labour market area in which they are located.

We exploit these features of England's educational system in a geographical boundary discontinuity design. This design matches schools according to school-level proxies for key characteristics that determine the grant their LEA receives: the proportion of children entitled to free meals, and geographical location. We then use the discontinuity in funding and student test scores between close-neighbouring primary schools in adjacent LEAs to estimate the causal effect of funding differentials on student outcomes. We use the "sharp" discontinuity in LEA-level average school grant that occurs at the boundary either directly in a reduced-form analysis, or as an instrument for school-level expenditure differences across the boundary. These designs are similar to classic "sharp" and "fuzzy" Regression Discontinuity Designs (Imbens and Lemieux 2008; Lee and Lemieux 2010). However, here we are matching schools both on spatial location and on student free meal entitlement, and exploiting a discontinuity in funding with respect to only one of these—geographical location. The identification condition is thus one of "conditional ignorability": that is, conditional on the location of schools and the proportion of their students entitled to free meals, there are no confounders correlated with these LEA-level grant differences (see Keele et al. 2015 for further discussion in the context of geographical boundary discontinuity designs). In the education literature, similar techniques have often been used to look at the impact of school test scores on house prices as well as being used in other areas of economics and social sciences.[4]

The credibility of claims that any estimates represent causal parameters relies on ruling out alternative causal explanations, so we devote a lot of attention to this question. An important threat to designs of this type is sorting across the boundaries. In our context, the crucial concern is that there is sorting into schools on the basis of

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3. Local Education Authorities are the bodies responsible for distributing money to schools and are described in detail in the next section.

4. With respect to the literature on the effect of test scores on house prices, papers that use regression discontinuity methods include Black (1999), Gibbons and Machin (2003), Kane et al. (2005), Bayer, Ferreira and McMillan (2007), Fack and Grenet (2010), and Gibbons et al. (2013). The method is used in many other areas of economics—for example, Cushing (1984) uses it to look at the effect of taxation on house prices. Duranton et al. (2011) look at the effect of taxation on firms using a combined discontinuity and instrumental variables methodology that is similar to ours.

the grants paid to their LEAs. We argue that in our institutional setting and period of our study, these funding differences were hard to observe during the process of school choice, and expenditure provides a very weak or uninterpretable signal about school quality. These are not factors that parents consider. This is because of the complex compensatory way in which funds are allocated to schools that generally imply that on face value the "worst" schools receive the most money. In addition, we present an extensive range of balancing tests that demonstrate that our matched set of schools along the LEA boundaries are balanced in terms of observable covariates and other salient characteristics such as house price indices and mobility across LEA boundaries that would tend to indicate sorting. Additional tests show that many other factors, such as those related to local public finance, cannot explain our findings either.

This investigation is important because of the policy concern to improve the educational attainment of disadvantaged students and in the light of the age-old debate about whether changes to school expenditure (at the levels typical in developed countries) really have *any* causal impact on outcomes. The causal effect of expenditure on outcomes is hard to identify, because money is often allocated to schools in ways that are correlated with pre-existing pupil advantages and disadvantages. There is also potential sorting of students into schools according to levels of expenditure. Studies that identify the effect in a convincing way are relatively few and economists have varying views on the interpretation of the literature. Hanushek (2008) argues that accumulated research says that there is currently no clear, systematic relationship between resources and student outcomes. Critics of this view suggest that many papers in this literature do not deal adequately with the endogeneity problem and/or have problems with data quality. There are indeed more papers with a strong methodology (using natural or real experiments) that show a positive effect from resource-related factors including Angrist and Lavy's (1999) study on the effect of class size in Israel; studies on the experimental Tennessee STAR class size reduction (Krueger 1999; Krueger and Whitmore 2001; Chetty et al. 2011); studies that have made use of student finance reforms (Guryan 2001; Van der Klaauw 2008a; Bénabou et al. 2009; Roy 2011; Hyman 2016; Jackson et al. 2016); and some of Hanushek's own work (Rivkin, Hanushek and Kain 2005). There have also been a couple of recent papers in England that have found modest effects of increased school resources (Jenkins et al 2006; Holmlund et al. 2010; Machin et al. 2010). However, there are studies with a strong methodology that find little or no effect of class size (e.g., Hoxby 2000; Cho et al. 2012), and thus the efficacy of general increases in school resources as a policy is still highly controversial amongst economists. A review of this literature is provided in Gibbons and McNally (2013). Our study is the first to our knowledge that applies the boundary discontinuity approach in order to provide causal estimates of the effect of school expenditure differentials. Because of the context of this study, our results apply to students who live in urban areas and are more likely to be disadvantaged.

To preview our results, we show that schools that are well matched in terms of pupil characteristics, in different LEAs, but close to the boundaries, do receive different levels of core funding from central government, and that these differences in resources are associated with marked differentials in pupil performance. Our results

imply that an additional £1,000 per student per year (a 30% increase relative to the mean) could raise achievement by around 30%–35% of a standard deviation. Furthermore, the effects are bigger in schools that have higher proportions of disadvantaged students. Our estimates are larger than those typically found in the literature for general resource increases, although comparable to the effects of class size reductions in the benchmark experimental study, the Tennessee STAR experiment (Schanzenback 2006). We suggest that our higher estimates arise because we focus on persistent cross-sectional differences in income and expenditure, whereas many previous studies have identified effects from short run time series variation within schools. Schools are likely to be able to adapt to short run fluctuations in funding and so may appear relatively unresponsive to resources in studies that exploit this type of variation.

The remainder of our paper is structured as follows: we discuss the institutional structure of schools in England and how funding is allocated (Section 2); data (Section 3); empirical strategy (Section 4); regression results (Section 5); and discussion and conclusions (Section 6).

2. Institutional Context Underlying our Research Design

Our research design uses district-level differences in per pupil funding in comparable close-neighbouring schools in a discontinuity-based design, to identify the causal effect of expenditure on student achievement. In this section, we present the relevant background information on the school system in England. The details of the data and research design are set out in Sections 3 and 4.

The education syllabus is based on a National Curriculum and years of compulsory education are organised into four "Key Stages" (ending at the age of 7, 11, 14, and 16). At the end of primary school (end of Key Stage 2, age 11), all students in England undertake national tests in English, Maths, and Science. These are national tests that are externally set and marked and form the basis of School Performance Tables (or "league tables"). Our outcome variable will be these Key Stage 2 scores (ks2).

For the period covered by our study (2004–2009), schooling was organised at the local level through LEAs.[5] These LEAs often have the same geographical coverage as bodies that control other aspects of local government, such as social services, and civic amenities. In some cases, the geographical area of the LEA that is responsible for education encompasses a number of smaller authorities that are responsible for non–school-related services. There are 152 education-related LEAs in England,[6] with an

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5.   After the election in 2010, in the wake of the recession, the UK Coalition Government introduced a range of major changes to school funding and organisation. As our analysis ends in 2009, these changes do not affect our analysis.

6.   Only 150 are represented in our data as we exclude the Isles of Scilly, and there are no primary schools of the type we analyse in the City of London.

average of 23,500 primary school pupils, but with a lot of variation (standard deviation is 18,000). There are about 15,000 primary schools in England.

Although the leaders in local government (councillors) are elected, they delegate all the day to day running of the services to appointed officials, and there is no equivalent to an elected "school board" as in the United States. Although LEAs have a number of roles in the provision of education, schools are largely self-governing. The LEA's main functions in relation to primary schools are in building and maintaining schools, allocating funding, providing support services (e.g., for children with special needs), and acting in an advisory role to the head teacher regarding school performance and implementation of government initiatives. The LEA also appoints one or two representatives on to a school's governing body—a group of parents, teachers, and community representatives that provides governance to the school. Local Education Authorities typically also offer a number of administrative and management functions including training, personnel and financial services.

The majority of pupils (67%) attend "Community schools" and for these schools, LEAs are also the statutory employer of school staff, owner of the buildings and the authority that manages student admissions. Most other state primary schools are faith schools (which have greater autonomy from the LEA). Student admissions are based on principles of parent/student choice rather than rigidly defined catchment areas, and parents can apply to any school. However, popular schools are oversubscribed and places are rationed according to various criteria such as priority for siblings, special educational needs (SEN), and proximity to the school. A Schools Admission Code dictates that student ability or family income cannot be used as a criterion. In our empirical analysis, we restrict attention to Community schools as they are more homogenous in their funding, governance, and admissions structure. Therefore, comparing adjacent Community schools in different LEAs makes it more likely we are comparing "like with like".

Around 85% of funding to schools comes from central government and general taxation. This funding is distributed to the LEAs, who then redistribute it to schools. Over most of the period relevant to this study, this core funding element was allocated to LEAs as a Formula Grant using a national formula. The key features of the primary school grant is that there is a basic allocation per pupil, with an allowance made for the labour market ("area cost adjustment"), LEAs with small schools in isolated areas ("sparsity"), and demographic "additional educational needs". The AEN and ACA are the key components. Additional educational needs are based mainly on the proportion of families on Income Support, children with English as an Additional Language, and families with Working Families Tax Credits. We describe the ACA in detail later in this section.

In addition to the this core Formula Grant, there is a variety of separate central government grants that are ring-fenced for specific purposes, such as to support national educational strategies for raising standards or ethnic minority achievement. These are passed on in full to schools by LEAs according to rules set by the Department for Education (e.g., according to school size or proportion of ethnic minorities).

There have been many changes to the formulae and the various supplementary grants over time (as documented in West 2009). Notably, in 2006/2007 the Formula Grant and supplementary specific grants were combined into a "Dedicated School Grant". However, the allocations across LEAs were based on the shares in previous years under the Formula Grant, so the basic allocations remain, implicitly, based on the same factors that determined allocations under the earlier formula (including the adjustments for area and educational need).

Local Education Authorities use their own rules to allocate the core funding (other than the supplementary grants) to their schools, but the mechanisms have always been much less compensatory than those that allocate money to LEAs (West 2009). In many cases, the majority of funds were distributed equally on a per-pupil basis, with a small proportion targeted to the expected level of "special educational needs" of its pupils. This in itself implies that funding given to a school has a component that is more closely related to the demographics in the LEA as a whole than it is to the demographics of the school itself, giving rise to potential funding gaps between comparable schools in different, less comparable LEAs. We use LEA-level, rather than school-level funding variables to identify the effects of resources, so the exact allocation within LEAs is not crucial to our analysis. When the funding gets to schools, it is for the school to decide how to use it, although the bulk of expenditure is on teacher pay. The broad allocation of spending is as follows: 60% on teachers; 20% on support staff or other staff; 6% on building and maintenance; 5% on learning resources/IT and 8% on a residual category. This has changed little over time (Holmlund et al. 2010).

An important element of our research design is the ACA in the national funding formula that is intended to compensate LEAs for differences in costs. This reflects two kinds of difference between areas in local costs: differences in labour costs (i.e., the main factor) and differences in business tax rates paid on local authority premises. The "labour cost adjustment" is based on differences in mean wages (private and public sector) between areas, regression-adjusted for worker and job characteristics.[7] The labour cost adjustments are intended to compensate for market wage differentials, which in turn are compensating workers for differences in housing costs and amenities (Roback 1982; Moretti 2013). Public sector employment accounted for around 19% of total employment in England at the end of the period we study, so the estimated ACAs are heavily weighted towards private sector pay differentials. The underlying rationale in applying these ACAs to public finance allocations is that local public services have to compete for staff with other employers and therefore authorities in areas with high

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7.   The ACAs were generated by the Department of Communities and Local Government. Using a large national sample of employees, log wages are regressed on a set of individual characteristics (including 80 occupational dummy variables, age, gender, 58 industry dummies, and dummies for public sector and part time workers) and geographical area fixed effects. The Labour Cost Adjustments are then estimated as wage indices from the area fixed effects. For determining the education ACA, the Rates Cost Adjustment and Labour Cost Adjustment are weighted by the estimated contribution of labour (80%) and rates (between around 1% and 2%) to education costs. Thus, the Labour Cost Adjustment is by far the most important factor and the rates adjustment is inconsequential. The methodology is discussed in CLG (2007).

private sector wages can face higher staffing costs.[8] However, teachers and other school staff and LEA employees are public sector workers, and in practice public sector pay is far less variable geographically than private sector pay (Bell et al. 2007; Emmerson and Jin 2012). This is because the public sector has greater union coverage, and unions typically work against the introduction of region-specific pay scales, which they see as unfairly disadvantaging workers in low pay areas. Therefore, this extra funding to LEAs does not necessarily get passed on to staff or teachers in higher basic pay, as they get paid according to national pay scales with very limited regional variation (see below). However, the money can implicitly be used in other ways, to increase staff numbers, provide extra material resources or support services, or could potentially be used to pay recruitment or performance incentives to teaching staff. This discrepancy between the way the ACA is estimated, and the reality of the pay environment faced by schools, is one reason why the ACA can lead to school funding differentials in real terms. A second reason is that the ACAs are defined for 54 subregional geographical units that are aggregates of the 152 LEAs, so neighbouring LEAs can receive different levels of per-pupil funding simply because they have been allocated to different ACA regions.

There are, however, some cases in which teachers in adjacent LEAs will receive different nominal pay automatically. Teacher national pay scales in England have London allowances, which vary according to whether the school in which they work is in one of four pay zones: Inner London, Outer London, London Fringe, or the Rest of England. These pay zones do not coincide with the 54 geographical areas on which the ACA is based, but they are geographical aggregates of contiguous LEAs. Hence, there will be some cases where teachers working on different sides of an LEA boundary get different nominal pay through the national pay scales, and hence higher real wages, assuming that teachers working in neighbouring schools will not genuinely face higher housing or consumption costs. To the extent that these higher real wages induce improvements in teacher quality and hence student performance, it is appropriate to include them within our expenditure measures. In practice, other evidence suggests that these teacher pay differentials do not affect student performance (Greaves and Sibieta 2014)[9], but we will in any case test for the robustness of our results to controls for these London teacher pay allowance zones.

A related concern would be if neighbouring schools genuinely faced different prices for their other labour inputs (e.g., bought in private sector services). Similarly, there would be a concern if any differentials in nominal wages paid to teachers simply compensated them for differential commuting, goods or housing costs and resulted in no improvement in teacher quality through selection or effort. Our underlying assumption then is that market prices and wages do not vary between close-neighbouring schools, and any differential in expenditure and teacher pay induced

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8. See http://webarchive.nationalarchives.gov.uk/20140505104649/http://www.local.communities.gov.uk/finance/1314/methacas.pdf, retrieved October 2017.

9. We thank Luke Sibieta and Ellen Greaves for providing the teacher pay data.

by institutional funding rules represent real expenditure and wage differences. This assumption seems relatively uncontroversial, given that schools we compare in our sample are on average only 1.4 km apart. Average commuting distance in metropolitan areas in Britain is around 11 km (figures taken from the National Travel Survey 2010), so teachers and other workers are unlikely, on average, to view either one of two schools 1.4 km apart as preferable to the other on the basis of distance alone. The official labour market definitions for the United Kingdom are Travel to Work Areas (TTWAs), have an average land area of 1000 km^2 that is obviously large compared to the scales at which we are working.[10] London is a single TTWA with land area of 2700 km^2.

3. Data

Our study is based on the National Pupil Database (NPD, a census of all students in state schools) between academic years 2003/2004 and 2008/2009. The data set contains information on the national ks2 test in English, Maths, and Science, taken in May. There is no grade repetition in the English system so all pupils are in the same year group when they take these tests. We use the average score across these subjects as our main outcome variable. We do not have full information on funding before 2002/2003 and wish to include time lagged funding data so we restrict attention to 2003/2004 onwards. We do not use years beyond 2009 because institutional changes occurred thereafter.

The NPD also has information on the prior test scores (ks1) of each student at age 7 in Reading, Writing, and Maths. Demographic information included in the data set relates to gender, ethnicity; whether English is his/her first language; whether the pupil is known to be eligible for FSM (an important indicator of socioeconomic disadvantage). Geographic information on the pupil's home residence is also available at Census "Output Area" (i.e., small geographic clusters of households).[11] This can be linked Census data from 2001 and an index of income deprivation: the IDACI index (i.e., Income Deprivation Affecting Children: an index based on the proportion of children under the age of 16 who live in low-income households). Additional data at school level come from the Annual School Census (such as pupil numbers; the proportion of pupils eligible for FSM in the School). Information on school expenditure and income is taken from the "Consistent Financial Reporting" (CFR) data set, which contains detailed information on school expenditure and income sources in the financial years. Financial years in England begin in April, so the expenditure in a given financial year relates (approximately) to the annual period leading up to the Key Stage tests in May. All the expenditure and income variables we use exclude capital expenditure for

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10. These are defined using a self-containment rule which implies that around 80% of workers in an area live in an area and around 80% of residents work in an area.

11. The recommended size for "output areas" is 125 households. There are 175,434 OAs in England. They are based on the 2001 Census.

building work, which is funded separately. We have information on national funding formula for LEAs in each year, including how funding is allocated on the basis of AEN, ACA, and sparsity.

To set up these data for our empirical analysis, we carry out a number of data manipulations using a Geographical Information System, computing distances between each school and its nearest neighbours based on the school postcode coordinates, distances to Local Education Authority boundaries. We also derive a subset of LEA boundaries that do not coincide with geographical features (major roads, motorways, railways) using feature data from the Ordnance Survey (these geographical data were obtained from the geographical data service at www.edina.ac.uk).

4. Empirical Strategy

4.1. Introductory Outline

Our aim is to estimate the impact of school resources on student achievement. The first-order problem is that resources are systematically allocated to schools to compensate for student disadvantages. To do so, we compare performance in geographically neighbouring, Community schools close to LEA boundaries, in adjacent LEAs. These LEAs receive different levels of compensatory grant from central government, though the schools we compare face similar demographic and labour market conditions. To ensure this, we match schools not only on geographical location as in standard geographical boundary discontinuity design, but also on an observable school-level proxy for a key element in the formula that determines central government grants to LEAs—the proportion of children eligible for FSM. This matched, geographical regression discontinuity design is broadly similar to that proposed by Keele et al. (2015), who describe the corresponding identifying condition as "conditional geographic treatment ignorability". In the next sections, we set out the underlying structure of relationships between funding, socioeconomic characteristics and school performance that justifies this identification strategy, and set out the design in more detail.

4.2. Eliminating Biases from Compensatory School-funding Mechanisms

We compare test scores and expenditure in pairs of schools s and s' in adjacent LEAs k and k' in a regression specification with the structure:

$$(ks2_{sk} - ks2_{s'k'}) = \beta(expend_{sk} - expend_{s'k'}) + \lambda(v_s - v_{s'}) + (\varepsilon_s - \varepsilon_{s'}). \quad (1)$$

Here, $ks2_s$ is school-mean student Key Stage 2 (age 11) test scores (an average across three subjects: Maths, Science, and English) and $expend_{sk}$ is a measure of per-student, current expenditure in school s in the years leading up to the age 11 test. We estimate

student-level regressions using data from multiple years, but for exposition we first discuss the design using school-level expressions.[12]

Variable v_s is a partially observed index of specific school/demographic characteristics which are, at LEA level, salient in determining the compensatory funding that the LEA receives from central government.[13] Component ε_s represents other school-level unobservables, potentially correlated with v_s. As discussed in Section 2, the central government grant allocated to an LEA in a given year is based on a national per-pupil baseline, plus components compensating for the LEA's low-income population (AEN), labour input prices (ACA), and residential density ("sparsity"). So, richer, high-wage, dense LEAs on average will get less money per pupil than poorer, low-wage, sparse LEAs. We expect v_s to vary smoothly across space, relative to school performance, because it represents local labour market conditions and sorting on amenities and housing that are unrelated to school provision. We can think of v_s as having two spatial components: v_k represents the LEA-mean characteristics that directly determine LEA funding that is, $grant_k = v_k$;[14] school-level components $\tilde{v}_s$ represents the deviations from this mean such that $v_s = v_k + \tilde{v}_s$. Each LEA redistributes the grant to schools according to its own rules each school has its own idiosyncratic sources of income, implying the following "first stage" equation:[15]

$$(expend_{sk} - expend_{s'k'}) = \gamma(grant_k - grant_{k'}) + (\varsigma_s - \varsigma_{s'})$$

$$= \gamma(v_k - v_{k'}) + (\varsigma_s - \varsigma_{s'}). \tag{2}$$

Clearly, school expenditure is explicitly endogenous in equation (1) as is the LEA grant in the reduced form obtained by substituting equation (2) into (1). Both LEA grant and school expenditure depend directly on v_k: the index of LEA-mean low income, labour costs, and sparsity. A second endogeneity concern from the funding mechanism is that school-specific expenditure (ς_s) is determined by other factors (ε_s) that affect achievement in equation (1). This correlation arises because there are school-specific characteristics affecting school-specific funding, conditional on the grant ($grant_k$) provided to the LEA. The reasons are as follows: (a) some LEAs reallocate their grant across schools to compensate for intake demographics or to target low achievement, but not in a way that follows the central government

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12. Our conjecture is that previous estimates of the effects of resources that rely on time series variation within schools have been downward biased, because schools can adapt to short run changes in income. Hence, we aim to estimate effects using persistent cross-sectional differences in expenditure.

13. Given the compensatory nature of the funding, $\lambda < 0$.

14. That is, $grant_k = v_k = \delta_1 + \delta_2 AEN_k + \delta_3 ACA_k + \delta_4\, sparsity_k$, where the δ_1 is the national baseline and the other δ are weights on the various LEA mean indices (that vary by year).

15. A more accurate representation of this underlying process (1c) would have school-specific coefficients γ_s, but we define $\gamma = \bar{\gamma}_s$ subsume this parameter heterogeneity into the error term ς_s for notational simplicity.

funding formula;[16] (b) LEAs provide additional resources to schools for students with disabilities and SEN; (c) schools receive some additional grants direct from central government to target national strategies and ethnic underachievement, which depend on a school's student intake characteristics; (d) a school can raise its own resources through fundraising events, applying for specific grants and charitable donations and its success in doing so will depend on the needs of its students and the motivation and skills of the leadership team.

Our research design tackles these two issues in two steps. The first step is to eliminate the school-level factors (v_s) that are related by construction to the LEA-grant funding formula. Note that for pairs of schools s and s', which are in different LEAs k and k' but which have similar school-level characteristics v_s, it must be the case that $v_k + \tilde{v}_s \approx v_{k'} + \tilde{v}_{s'}$, but need not be the case that $v_k = v_{k'}$.[17] This implies that for a pair of schools, s and s', which are exactly matched on v_s but are in different LEAs:

$$(ks2_{sk} - ks2_{s'k'}) = \beta(expend_{sk} - expend_{s'k'}) + (\varepsilon_s - \varepsilon_{s'}). \qquad (3)$$

Thus, two LEAs containing these schools explicitly receive different levels of funding because they differ in mean characteristics v_k (equation 2), but these differences are eliminated at school level by differencing between schools that are matched in terms of v_s (equation 3). This differencing also eliminates other unobserved school components in ε_s that are correlated with v_s. The credibility of this step depends on making any residual differences in the AEN, labour market, and sparsity of matched schools $(v_s - v_{s'})$ small, random, and uncorrelated with the differences in the LEA grants $(grant_k - grant_{k'})$, satisfying the "conditional geographic ignorability" assumption. We describe this matching of schools across LEA boundaries in Section 4.3.

The second step is to eliminate biases from the correlation between school-specific characteristics and resources $(E[(\varepsilon_s - \varepsilon_{s'})(\varsigma_s - \varsigma_{s'})] \neq 0)$ by exploiting exogenous variation in the difference in the LEA grants $(grant_k - grant_{k'})$. We do this either in a reduced form or instrumental variables (IV) specification implied by first-stage equations (2) and second-stage equation (3). As an alternative to using the LEA grant as an instrument, we also use the ACA element of the index $(v_k - v_{k'})$ that is designed to account for differences in wages (regression adjusted for demographics) between the two LEAs. An advantage of using the ACA as an instrument is that market wage levels vary more smoothly over space than demographics, so it is more likely that neighbouring schools s and s' are matched in terms of their labour market conditions

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16.   The exact details vary from LEA to LEA, school to school, and year to year, but are not crucial in the context of our identification strategy. See West (2009).

17.   In other words, the schools are in the same labour market, their local area has the same sparsity, and they face students with the same additional educational needs, even though their LEAs are different in these respects.

(even though their LEAs as a whole are not).[18] In other words, the assumption of conditional geographic treatment ignorability is more likely to hold.

The remaining sources of endogeneity in equations (2) and (3) are correlation between the cross–LEA-boundary differences in school unobservables and the cross–LEA-boundary difference in LEA grants (i.e., $E[(\varepsilon_s - \varepsilon_{s'})(grant_s - grant_{s'})] \neq 0$). This correlation could arise if there is sorting of students into schools across LEA boundaries based on the LEA grant (or ACA index), or if there are inputs other than school expenditure that are correlated with the LEA grant (or ACA index). We discuss these threats in Section 4.4.

4.3. Matching of Schools by Location and Salient Demographics

If we fully observed the formula used by central government to allocate grants to LEAs, and observed the variables (v_s) in this formula at school level, then we could match schools in different LEAs based exactly on these variables and implement the differencing in equations (2) and (3) on these matched pairs. In practice, this approach is infeasible. We do not observe the proportion with AEN at school level, defined exactly as in the formula. Nor can we observe at school level the relevant labour market characteristics used to construct the ACA index or sparsity index.

An alternative, purely geographical discontinuity design following traditional Regression Discontinuity Design (RDD) practice would compare neighbouring schools on opposite sides of LEA boundaries and control for smooth variation in unobservables across space using a parametric or semiparametric function of distance to boundary. In our case, this is unlikely to eliminate differences in v_s. The units of observation (schools) are geographically fairly sparse, there are multiple discontinuities (boundaries), the assignment variable is multidimensional (two-dimensional geographical space), and the threshold is a complex function of the assignment variable that defines the geographical boundaries. Moreover, the appropriate shape of the assignment variable control function differs from boundary to boundary. As noted by Keele et al. (2015), simple distance-to-boundary controls are unlikely to suffice.

Instead, we use a combination of these approaches. We match schools in different LEAs based on two things: (a) the proportion of students who can claim FSM, which is an observable school-level proxy for the proportion on income support benefits that enters the AEN index at LEA level; (b) geographical location, which serves as a control for labour market conditions in the ACA and other components of the funding formula. Similar matching methods have been proposed by Keele et al. (2015) to address the difficulty of controlling for spatial trends in geographic discontinuity designs. In our

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18. Note that these cross-boundary LEA-grant and ACA differences will not be "relevant" as an instrument (in the sense that it predicts cross-boundary differences in school-level expenditure) if LEAs exactly replicate the formula by which they receive funds from central government when redistributing money to schools. The instruments will also not be relevant for expenditure differences if schools raise or lose resources that in other ways completely offset the LEA-grant differences. These assumptions are easily testable in the first-stage regressions (2).

case, matching on FSM is not ad hoc but informed by the crucial role of the AEN index in determining LEA grants.

Specifically, we match schools to neighbouring schools in the boundary data set that are within +/− 10 percentiles in the distribution of the school proportion of FSM (we explore sensitivity to this matching threshold in the empirics). Appendix A further illustrates the justification and consequences of this FSM-matching procedure. When matching schools based on spatial location and controlling for unobserved smooth spatial trends, we follow the standard practice of restricting the sample to areas close to boundaries and controlling for distance to boundary trends in various ways, including polynomials, linear trends, boundary-specific linear trends, and locally weighted regressions where we apply higher weights to more closely spaced schools.

4.4. The Threat from Sorting and Other LEA Inputs

Even with the above cross-boundary differencing and instrumental variable strategy, we are left with a threat from potential sorting into schools, and unobserved factors at LEA level that might be correlated with the LEA grant or ACA instruments. As is well known (Imbens and Lemieux 2008), discontinuity designs can fail if agents can precisely and strategically manipulate which side of the boundary they are on. Whereas schools are under LEA control and cannot relocate to a different LEA, families can choose on which side of the boundary to live and go to school. This problem of "sorting" across geographical boundaries is pervasive in geographical discontinuity designs applied to house prices, where a standard solution or robustness check is just to control for observables (Bayer, Ferreira and McMillan 2007; Gibbons, Machin and Silva 2013). Given our institutional context, this problem is not necessarily very severe. The threat here is from sorting by ability on the LEA grant or ACA index used as instruments, not on school-specific components of expenditure that are uncorrelated with the LEA grant. Moreover, school choice is generally not made on the basis of expenditure. Burgess et al. (2009) find that "closest or nearness to home" and "general good impression" are the most frequently citied reasons for choosing schools. Publicly available "league tables" provide one information source for parents, but these focus mainly on headline test scores. Information on school expenditure was publicly unavailable and was only introduced into the league table information in 2010 after the period of analysis. Information on the LEA grant was not published and would require considerable research to disentangle.

Moreover, even if school expenditure or the LEA grant is observed, it is impossible for parents to interpret this as a signal of school quality given the compensatory nature of funding. Higher funding and expenditure signal that the school faces significant challenges. In addition, slightly higher levels of mean LEA grant does not necessarily correspond to higher levels of school expenditure or school performance for individual schools close to the boundary, because they can be offset by the idiosyncratic sources of variation discussed in Section 4.2. Although it is true we will find that on average across all boundaries, a higher LEA grant is associated with higher school expenditure and improved outcomes (this is the first stage, and main finding in our regressions); this

is not true across each and every boundary, so a parent trying to choose between two schools in adjacent LEAs would have no way to easily understand the implications of higher school expenditure in particular schools. In other words, the exogenous variation in funding that we use for identification is either not observable by parents (because it is not completely revealed in school expenditures or outcomes) or not informative (because it is uninterpretable).

Note that as well as controlling for a principal determinant of LEA grants at school level, matching on FSM proportions as described in Section 4.3 has the advantage of controlling for other forms of selection into high- and low-funded schools, where this selection is on observables that are correlated with FSM. We present extensive tests to show that our matched schools are balanced in terms of observable school and student characteristics and that our findings are not driven by sorting on these characteristics.

A final concern is that the LEA grant might correlate with other LEA-specific nonschool inputs such as social services that might affect children's education. Although we cannot rule this out *a priori*, we present further discussion and a number of tests in Section 5 which demonstrate that other LEA-specific factors are not credible as alternative explanations for the relationship between school resources and student performance that we uncover in the empirical analysis.

4.5. Data Structure and Estimation

In practice, we estimate equations (2) and (3) by running the regressions on student-level data with the paired s-s' school observations stacked in a panel structure, with school-s-by-year fixed effects in order to obtain estimates based on differences in funding within these school pairs in each year. In other words, we control for school-pair-by-year fixed effects (in robustness checks, we generalise to match each school s to two or more of its closest matched neighbours s', s", s''' and control for school-group-by-year fixed effects). This arrangement with repeated schools generates a complex data and error structure. To make our standard errors robust to this, and to other forms of spatial and serial autocorrelation along and across the LEA boundaries, we "cluster" the standard errors on LEA boundary groups.

Another important point to note is that this research design necessarily creates a selected subsample of schools and students, so there is a potential sacrifice of external validity for the sake of internal validity. The schools in these boundary subsamples are primarily urban (given the greater density of schools and LEA boundaries within urban areas) and the students from more educationally disadvantaged background.

4.6. Interpretation as Sharp and Fuzzy RDD

Note that the reduced form regression is analogous to a sharp Regression Discontinuity Design (Imbens and Lemieux 2008; Lee and Lemieux 2010) in the sense that all schools on one side of a given LEA boundary are assigned the same constant treatment and all schools on the other side are assigned a different constant treatment (the treatments are the LEA-specific grant on each side, whereas in the classic RDD these constant

values would be 0 and 1). The IV approach is analogous to a fuzzy RDD in that the treatment takes many values (the school-specific expenditure) on each side of a given boundary (in the paradigmatic fuzzy RDD these would be a mixture of 0s and 1s). In this geographical regression discontinuity design, there are of course also many different boundaries and therefore many different discontinuities (152 in our main estimates). Therefore, estimation is based not just on the existence of a jump in the LEA grant from one side to the other, but from the magnitude of this jump and its correlation with the jump in test score outcomes across these different boundaries.

5. Results

5.1. Description of the Sample

Table 1 shows descriptive statistics. The left-hand side of the top panel shows figures for the full national student sample for comparison purposes. The right-hand side of the top panel shows figures for the students in pairs of schools that are matched across LEA boundaries as described in Section 4. Figure 1 maps the schools in this boundary subsample. As we have discussed, our research design brings the focus on urban schools because of the greater density of boundaries and schools in urban areas. Schools that are close to boundaries either in or on the periphery of London account for 60% of the sample (as compared to 14% in the population overall).

The first row in Table 1 shows the descriptives for ks2 test scores, which have zero mean and unit standard deviation in the full sample by construction. In the boundary, subsample mean scores are slightly lower. The second row shows school expenditures, averaged over the maximum number of years for which data are available between the ks1 and ks2 tests (rising from 2 years in 2002 to 3 years in 2003 and 4 years from 2004 onwards). This expenditure figure is in 2009 prices and is intended to estimate the average expenditure during each year of the Key Stage 2 period (between ages 7/8 and 10/11). Note that within schools the correlation in spending across years is very high (r around 0.8) and our analysis is cross-sectional, so whether we consider 1 year or multiyear means is largely irrelevant. The boundary subset of schools has higher levels of per-student spending than the national average (£3,763 compared to £3,313 per pupil on average at 2009 prices).

The third row shows the LEA grant per pupil, which will provide the first source of identification in our empirical analysis in what follows. The fourth row shows the ACA index. The schools in our boundary sample have higher levels of LEA grant (£2,923 per pupil per year compared to £2,634 nationally) and a higher ACA index (12% compared to 4%). Note that this core LEA grant is 78% of school income on average in our boundary schools. As discussed in Sections 2 and 4, the remaining income comes from various sources which are school-specific. There is an LEA budget targeted at the expected SEN in the schools, which amounts to around 8.5% of school income in our boundary sample. There are various specific grants from central government for specific national strategies amounting to around 9.5% (such as the "Standards
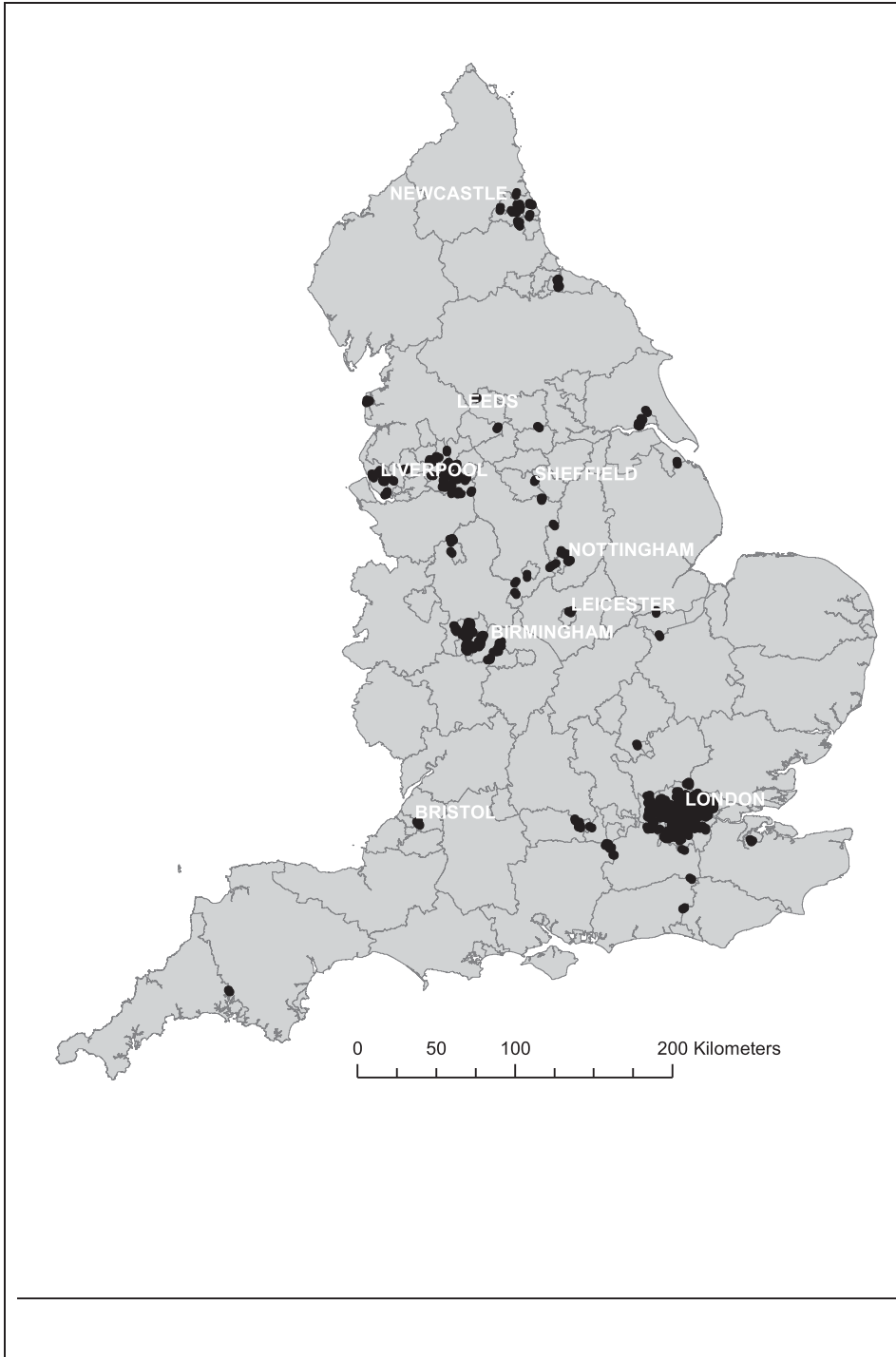
FIGURE 1. Distribution of schools in the school-pair boundary subsample.

TABLE 1. Descriptive statistics. Expenditures are pounds per pupil.

	Full data set		Matched school pair boundary subsample	
	Mean	s.d.	Mean	s.d.
Student data set				
Age-11 total score	0	1.0	–0.030	0.990
School total expenditure (mean annual)	£3,312.5	£636.3	£3,763.0	£844.3
Income from LEA grants (mean annual)	£2,633.7	£264.5	£2,922.7	£433.9
ACA index	1.042	0.064	1.123	0.100
Distance to LEA boundary (metres)			492.6	550.2
Distance between paired schools			1,392.0	430.0
Boys	0.509	–	0.506	–
FSM	0.165	–	0.294	–
Age in months (within year)	5.471	3.484	5.532	3.486
English first language	0.881	–	0.669	–
White British	0.820	–	0.549	–
Student observations	3,311,712		379,194	
Schools	15,308		840	
LEAs	150		103	
School-pair-by-year groups	–		4,540	
LEA-pair-by-year groups	–		593	

	Mean difference (absolute values)	s.d.	95th percentile
Cross-boundary funding differences in school-by-year level data set			
Total school expenditure per pupil per year	£505.9	£442.6	£1,285.4
Cross-boundary funding differences in LEA-by-year level data set			
Mean grant from LEA	£237.7	£326.4	£782.1
ACA index	0.026	0.0581	0.164

Notes: Table reports means and standard deviations. Top panel summarises student-level data. Bottom panel summarises funding differentials in data set collapsed to LEA-k by LEA-k' cell means. Data covers years 2003/2004 to 2008/2009. Expenditure and income in 2009 prices (deflated by Consumer Price Index).

Fund", 6% and ethnic minority achievement, 2%), and these are allocated by LEAs to schools according to national rules based on the school intake characteristics. Other school-varying sources make up about 4%, including catering, other services, income from school trips, voluntary donations and insurance claims. Note that the standard deviation of school expenditure is about twice that for the core LEA grant (£844 vs £434 in our boundary sample), showing that expenditure is much more variable across schools than is the core LEA grant. This higher variability in school total expenditure is due to the fact that these additional income sources are much more dependent on the school's specific socioeconomic characteristics.

Children in the boundary schools are more likely to be on FSM, less likely to speak English as a first language, and less likely to be White British, reflecting their urban locations. The table also summarises the distances between our matched boundary schools. The schools are close to each other, being less than 1.4 km apart on average (about a 15 minute walk) and less than 500 metres from the LEA boundary.

The lower panel of Table 1 shows the differences in school expenditure, LEA grant, and ACA index across the LEA boundaries, based on the boundary subsample of schools and students (aggregated to LEA-by-year cells). We report the mean of the absolute value of the deviation, the standard deviation and the 95th percentile. The mean average absolute cross-boundary difference in school expenditure is just over £500 per pupil per year, the standard deviation is £443 and the 95th percentile is just under £1,300. These differences are substantial, representing respectively 13%, 11%, and 34% of the average per pupil expenditure in the boundary schools. The differences in the LEA grant are similarly large, with a mean of £238, standard deviation of £326, and 95th percentile equal to £782, corresponding to 8%, 11%, and 27% of the average per pupil LEA grant. Note again that around half of the absolute difference in school expenditure across the boundaries is due to the LEA grant differential and around half is due to idiosyncratic school-specific differences in income, arising from the various school-varying income sources discussed previously. If we regress cross-boundary expenditure differences on cross-boundary LEA mean grant differences—the first stage regressions in our subsequent IV estimations—the R-squared is around 15%. This point is important for our subsequent discussion of sorting on LEA grants: schools in LEAs with high central government grants often do not have higher expenditure. There is also marked variation in the ACA index, with an average differential of 2.6%, a standard deviation of 5.8%, and a 95th percentile of 16.4%. These figures include the cases where there is zero differential in the ACA index between the adjacent LEAs in our boundary subsample. Note these descriptive statistics for the boundary subsample related to 216 LEA-pair-year observations with nonzero ACA index differences, corresponding to combinations of 29 different LEAs. The total number of LEAs represented in our boundary sample is 103 (out of 150 in England).

Figure 2 presents this information in another way, and shows the distribution of the between-LEA funding differentials in the school-by-year level data. This top figure shows the variation in the LEA grant between schools in adjacent LEAs, and which we use to estimate the sharp discontinuity regressions in the next section, and which we use as the first instrument for school expenditure differences. The bottom panel shows the variation in the ACA index between schools in adjacent LEAs; this variable provides the second instrument for school expenditure differences. The figures show that there is substantial variation in both of these funding variables.

5.2. Evaluating the Identification Strategy Part 1: Balancing Tests

The validity of our research design relies on the cross-boundary LEA grant and ACA differentials in our data set of matched boundary schools being uncorrelated with
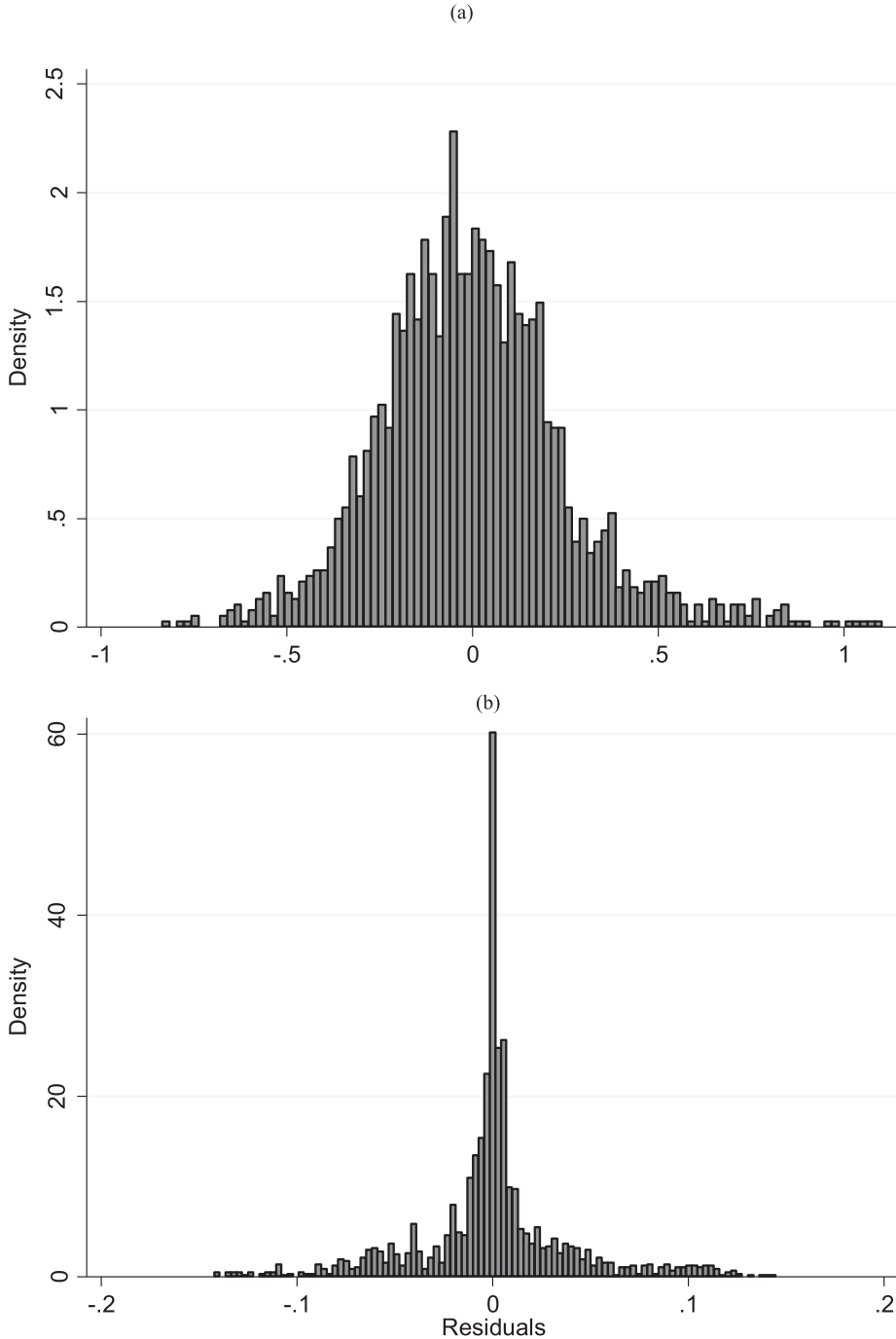
(a)

(b)

FIGURE 2. (a) LEA grant residuals across boundaries (£000s). (b) ACA index residuals across boundaries. Figures show histograms of residuals from regressions of funding variables on LEA-k fixed effects and school distance to boundary polynomial, using student data set collapsed to LEA-k by LEA-k' cell means.

potential confounders. We therefore first present a series of "balancing" tests to show to what extent our process of matching on distance and FSM is successful, and the instruments used in the regression analysis are uncorrelated with differences in student characteristics across LEA boundaries (more detail on how FSM matching works and affects this balancing is given in Appendix A). These tests are highly relevant, assuming that selection on observables provides some guide to selection on unobservables too (Altonji et al. 2005). We do this in Table 2. The table presents coefficients from regressions of various student, school, and residential socioeconomic variables on our identifying instruments. Each row represents a different regression and shows the dependent variable (e.g., expenditure per pupil in row 1). Columns (1), (3), and (5) show coefficients on the LEA grant per pupil. Columns (2), (4), and (6) show coefficients on the ACA index. Columns (1) and (2) present the results using our preferred strategy that matches each school to its nearest school in an adjacent LEA amongst the set with an FSM proportion within +/–10 deciles, and controls for distance-to-boundary polynomials. Columns (3) and (4) show what happens when we remove this FSM matching constraint. Specifications in columns (5) and (6) control for FSM decile dummies in the sample of boundary schools, but with no matching by distance, cross-boundary differencing or controls for spatial trends. In all these regressions, the data are aggregated to school-by-year cells. The instruments are standardised (dividing by their standard deviation in the sample) so that the coefficients on the ACA index and LEA grant per pupil can be compared.

Firstly, in rows (1) and (2), to preview to our main results and provide benchmarks for the balancing tests, we show the relationship between school expenditure and the instruments (analogous to the first stage regressions in the student-level IV analysis that follows in Section 5.3) and the relationship between the instruments and school average test scores at ks2 (analogous to the reduced form in Section 5.3). The first row shows, unsurprisingly, that LEA grant and ACA differentials do affect school expenditure differentials.

Row (2) shows that age 11 test scores increase significantly with the LEA grant per pupil and the ACA index in our preferred specification. The coefficient of 0.133 in column (1) row (2) implies that a £1,000 per pupil per year difference in LEA grant each year over primary school because ks1 leads to a 0.31 standard deviation ($=1000/435 \times 0.133$) increase in ks2 scores. In the other cells in columns (1) and (2), we change the dependent variable to one of a number of variables describing the students, the school, the residential area in which each student lives (Census Output Area), and the mean basic pay for workers in the Local Education Authority.[19] The aim here is to look for differences in these salient characteristics between school locations in LEAs with high and low levels of funding and for sorting of students between high- and low-funded LEAs.

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19. The data on LA staff pay come from a survey of local government pay carried out by the Chartered Institute of Public Finance and Accountancy, and do not cover all LEAs, so the sample size in this column is smaller. The measure of income deprivation affecting children is only available from the Department for Education in our data after 2004 hence is missing 1 year.

TABLE 2. "Balancing" tests on boundary schools.

| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Matched on location and FSM | | Matched on location only | | Controlling for FSM only | |
	Std LEA grant	Std ACA	Std LEA grant	Std ACA	Std LEA grant	Std ACA
Expend. per pupil	0.425***	0.280***	0.477***	0.419***	0.641***	0.449***
	(0.065)	(0.075)	(0.083)	(0.061)	(0.017)	(0.030)
Key stage 2	0.133**	0.064*	0.034	–0.022	0.096***	0.085***
	(0.045)	(0.032)	(0.067)	(0.066)	(0.014)	(0.013)
Predicted key stage 2	–0.002	–0.038	–0.062	–0.091	0.023**	0.006
	(0.026)	(0.025)	(0.047)	(0.052)	(0.008)	(0.009)
Age-7 ks1 tests	–0.002	–0.041	–0.033	–0.038*	0.027***	0.012
	(0.032)	(0.030)	(0.019)	(0.017)	(0.008)	(0.009)
FSM	0.016	0.022	0.058*	0.064***	0.003*	0.005***
	(0.013)	(0.012)	(0.027)	(0.019)	(0.001)	(0.001)
Girls	0.003	0.012	–0.000	0.006	0.003	0.002
	(0.006)	(0.008)	(0.006)	(0.006)	(0.001)	(0.001)
White	–0.019	–0.002	–0.042	–0.039	–0.138***	–0.156***
	(0.025)	(0.030)	(0.023)	(0.022)	(0.015)	(0.018)
Age (months)	–0.012	0.024	0.013	–0.014	–0.001	–0.011
	(0.046)	(0.041)	(0.040)	(0.041)	(0.013)	(0.010)
English first language	0.000	–0.015	–0.020	–0.044	–0.099***	–0.112***
	(0.039)	(0.039)	(0.034)	(0.024)	(0.016)	(0.018)
School cohort students	–2.395	14.424	–4.831	–6.940	13.750*	19.722***
	(17.876)	(20.045)	(15.532)	(11.703)	(6.018)	(5.855)
Ln home-school distance	–0.084	–0.128	–0.086	–0.096	0.001	0.005
	(0.055)	(0.068)	(0.049)	(0.051)	(0.017)	(0.016)
Live in adjacent LEA	–0.036	0.001	–0.040	–0.017	0.019***	0.033***
	(0.024)	(0.019)	(0.020)	(0.016)	(0.004)	(0.004)
School movers in LEA	–0.015*	–0.007	–0.012	0.000	–0.006**	0.006**
	(0.007)	(0.006)	(0.008)	(0.005)	(0.002)	(0.002)
School movers out LEA	0.005	0.006	0.011	0.007	0.022***	0.030***
	(0.009)	(0.009)	(0.010)	(0.010)	(0.002)	(0.002)
Movers into school during ks2	–0.029*	–0.015	–0.004	0.009	–0.003	0.003
	(0.012)	(0.019)	(0.014)	(0.018)	(0.003)	(0.003)
Home house price index	–0.007	–0.013	–0.022	–0.014	0.176***	0.237***
	(0.021)	(0.031)	(0.030)	(0.024)	(0.017)	(0.021)
House prices 1 km radius	–0.008	–0.001	–0.007	–0.007	0.240***	0.356***
	(0.025)	(0.021)	(0.027)	(0.018)	(0.023)	(0.026)
Home area high quals	0.011	0.011	0.007	0.001	0.077***	0.084***
	(0.009)	(0.013)	(0.009)	(0.013)	(0.006)	(0.006)
Home area no quals	0.004	–0.000	0.007	0.005	–0.051***	–0.070***
	(0.008)	(0.011)	(0.010)	(0.009)	(0.005)	(0.005)
Home social tenants	0.035*	0.034	0.050*	0.055**	0.038***	0.026**
	(0.017)	(0.021)	(0.021)	(0.020)	(0.008)	(0.009)
Home area born United Kingdom	0.000	0.006	–0.008	–0.006	–0.078***	–0.094***
	(0.011)	(0.014)	(0.008)	(0.008)	(0.007)	(0.008)
Home area employed	–0.013*	–0.009	–0.017	–0.016*	0.009**	0.024***
	(0.006)	(0.008)	(0.009)	(0.007)	(0.003)	(0.003)
Home area depriv. index	–0.000	0.002	0.023	0.023	0.020***	0.002
	(0.018)	(0.012)	(0.026)	(0.018)	(0.004)	(0.004)
LEA workforce ln pay	0.020	0.060	0.029	0.061	0.090***	0.125***
	(0.042)	(0.054)	(0.045)	(0.054)	(0.011)	(0.013)
Max/min col obs.	9,080/4,245	9,080/4,245	18,408/8,710	18,408/8,710	18,408/8,710	18,408/8,710

Notes: Regressions of row on column variable, aggregated to school-by-year cells, weighted by observations per cell. Standardised coefficients. Specifications (1)–(4) include school-pair-by-year fixed effects. See text for further details. *Significant at 5%; **significant at 1%; ***significant at 0.1%.

Row (3) reports balancing on predicted ks2 scores using a linear index of the basic predetermined pupil characteristics (prior ks1 achievement at age 7, FSM, gender, age, ethnicity, English first language). This index is estimated from a within-school regression of student ks2 test scores on these characteristics. As shown in Appendix A, we effectively eliminate selection on this index by our school-matching procedure. In the remaining columns, we investigate balancing on the individual components of this index and on a range of other covariates. From the remaining rows of columns (1) and (2) in Table 2, it is evident that most coefficients are statistically insignificant at the 5% level and relatively small in magnitude. The only exceptions are that mobility into high-LEA grant schools is lower than in low-LEA grant schools, and pupils in high-funded schools seem to come from residential areas with more social tenants and lower employment, though these coefficients too are insignificant once we look at balancing on the ACA index. Note to convincingly reject the null of no relationship between the funding variables and these 21 characteristics would require a *p*-value of 0.24% (=0.05/21) and a t-statistic of over 3.

Particularly important is the lack of significance and small magnitude of the coefficient on earlier test scores of the students at age 7 (row 5). Evidently, students in schools with higher LEA grants were not performing significantly better much earlier in their education, which reinforces the argument that our findings are not driven by sorting. Equally important is the lack of any evidence that house prices are higher, either in the student's home residential area, or in the 1 km radius around the school (rows 16 and 17). This lack of response of housing prices to school funding in England is consistent with Gibbons, Machin and Silva (2013) who also report no response of housing prices to expenditure despite evidence of a price response to school peer groups and value-added. Whatever components of school performance and inputs households are paying for, expenditure appears not to be one of them. As discussed in Section 4, this is not as surprising as it might first appear given that higher expenditure and/or LEA grant send mixed signals about school demographics and performance due to the compensatory funding mechanisms (as evidenced by the strong negative raw correlation between expenditure and pupil performance). In short, parents would need a lot of information and analysis to deduce whether to choose the higher or lower expenditure school if they are in search of both better outcomes for their child and other desirable school attributes like good peer groups, especially because existing evidence on the benefits of school expenditure is equivocal.

Columns (3)–(6) illustrate why we need to match neighbouring schools in similar parts of the FSM distribution. If we do not match on FSM but match only on distance when forming school pairs, the coefficients in the majority of these balancing tests from row (3) onwards remain small and nonsignificant and the pattern is broadly similar to that in column (1). However, the coefficient on FSM is substantial and highly significant. This is because, as discussed in Section 4.3, FSM is a school-level proxy for the LEA-level additional needs index that enters directly into the LEA grant. Consequently, FSM is also highly correlated with the LEA-level ACA index, because the additional needs and ACA indices are highly correlated at LEA level (inner-urban LEAs with high wages also have high levels of poverty). Now, there is no evidence

of an effect of resources on ks2 test scores (row 2) because the proportion of FSM students is a crucial confounder that is strongly negatively associated with school ks2 performance. Comparing nearest neighbour schools and controlling for spatial trends alone is insufficient to control for FSM differences between schools (in the index v_s in Section 4). Conversely, if we control for FSM but do not compare nearest-neighbour schools in columns (5) and (6), we do find a positive association between LEA grants or the ACA index and school performance. However, balancing on the other characteristics is extremely poor, so we could have no confidence that that this estimate is causal.

In short, Table 2 provides evidence of the "conditional ignorability" identification condition. We must compare nearest-neighbour schools matched by their FSM proportions in order to successfully eliminate the confounders that are correlated with both LEA grants (mainly through the compensatory funding formula) and school ks2 test scores. The LEA grant instrument performs almost as well as the ACA instrument in this respect. Appendix B presents further evidence of balancing across LEA boundaries, showing graphs typical of standard RDD analysis (though here we are averaging over multiple boundaries, so the patterns should not be interpreted in the same way as a standard RDD where there is only one).

5.3. Regression Results

Table 3 presents our main regression results on the relationship between school resources and ks2 scores in our sample of boundary schools. The coefficients are scaled to show the change in standardised student ks2 (age-11) test scores for a £1,000 of additional per-student-per-year expenditure, where expenditure is a moving average over the preceding years (up to 4) before the tests. Standard errors are adjusted for clustering on LEA boundaries to correct for the correlation between observations induced by our sample structure, plus arbitrary spatial correlation along the LEA boundary.

As discussed in Section 4, we implement the cross–LEA-boundary differencing design on student-level data, with each school s paired with a matched nearest-neighbour s' in an adjacent LEA. Columns (1) and (2) present a basic ordinary least squares (OLS) regression of test scores on school expenditure for comparison purposes (without differencing across the boundary). In columns (3)–(10), we then estimate effects using variation in cross-boundary differences in expenditure, controlling for school-pair-by-year fixed effects using the within-groups estimator. To ensure identification comes from differences in expenditure and test scores close to the boundary, we also control for the spatial assignment variable using distance-to-boundary polynomials interacted with a dummy indicating the high-funded side of the boundary (based on the average LEA grant). The first column in each column pair shows results without any control variables other than these distance-to-boundary controls. The second column in each pair includes additional control variables for student characteristics (FSM, ethnic group dummies, gender, month of birth, English first language, prior, age-7 ks1 test scores, school enrolment).

TABLE 3. Main results on the effects of expenditure on student ks2 test scores at age 11.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	School expenditure OLS	School expenditure OLS	OLS school expenditure, school group fixed fx	OLS school expenditure, school group fixed fx	Reduced form sharp discontinuity: LEA grant per pupil	Reduced form sharp discontinuity: LEA grant per pupil	IV fuzzy discontinuity: LEA grant instrument	IV fuzzy discontinuity: LEA grant instrument	IV fuzzy discontinuity: ACA index instrument	IV fuzzy discontinuity: ACA index instrument
Total money per pupil (annual mean, £1000s)	-0.156^{***} (0.018)	0.012 (0.011)	-0.049^{*} (0.023)	0.044 (0.026)	0.287^{***} (0.069)	0.282^{***} (0.064)	0.362^{***} (0.086)	0.360^{***} (0.074)	0.265^{*} (0.111)	0.365^{***} (0.097)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Distance to boundary cubic	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage: F-stat	–	–	–	–	–	–	85.32	107.0	28.21	54.40
First stage: Coefficient	–	–	–	–	–	–	0.792 (0.086)	0.782 (0.076)	2.228 (0.419)	2.432 (0.330)
Observations	379,194	379,194	379,194	379,194	379,194	379,194	379,194	379,194	379,194	379,194
School-pair-by-year fixed fx	–	–	4,540	4,540	4,540	4,540	4,540	4,540	4,540	4,540

Notes: Table reports regression coefficients and standard errors from student-level regressions, 2004–2009. Estimation based on all 840 Community schools in England that can be matched by location and FSM intake (the main determinant of within-LEA funding differences). Schools are matched to others that are within 2 km in adjacent LEA and with FSM proportion within 10 percentiles (of the national distribution of boundary schools). Regressions in columns (3)–(10) include school pair-by-year fixed effects that is, estimation is based on differences between matched pairs. Dependent variable is standardised mean student total score in English, Maths, and Science. "Distance to boundary cubic" means control variables for third-order polynomial series in distance from school to LEA boundary interacted with high-/low-funded side of boundary (based on LEA grant). Control variables are as follows: student key stage 1 (age 7) test scores (15 dummies), gender, FSM, month of birth, English first language, nine ethnic dummies, national funding formula educational needs index, number of pupils in school year group. Standard errors robust to heteroscedasticity and autocorrelation along LEA-pair boundary. "LEA grant instrument" is mean grant per pupil from LEA (mean across primary schools in LEA as a whole); ACA index instrument is LEA-specific ACA in central government funding formula (see text) *Significant at 5%; ***significant at 0.1%.

Looking at the OLS results in column (1), we see a strong negative association between school expenditure and student test scores. This association arises due to the needs-based resource allocation to schools and cannot be interpreted as causal. Column (2) adds in the control variable set, which drives the coefficient towards zero (and insignificance) because these variables at least partially control for factors that jointly determine resource allocation and student achievement. The results in column (2) are typical of the low or insignificant coefficients that come out of OLS regressions in the literature that try to deal with the endogeneity of school resources by controlling for observable characteristics (see the literature documented by Hanushek 2003).

The second set of results (columns 3 and 4) also uses school-specific expenditures as the resource variable, but the regression controls for school-pair-by-year fixed effects, and the distance-to-boundary controls, that is, the effect of school expenditure is estimated using differences between the schools in the pairs matched across the LEA boundaries.

The estimates in both columns are again small and statistically insignificant, though switch sign when the control variables are added. In this case, the expenditure differences between matched schools on different sides of the boundary occur due to a combination of differences in the LEA grant between the adjacent LEAs and idiosyncratic variation in expenditure between the schools discussed in Sections 4.2 and 5.1. Again, these estimates cannot be interpreted as causal because the idiosyncratic, non-LEA based, sources of funding are not systematically related to geographical location nor FSM, so are not controlled by the spatial differencing and matching design. They will depend on factors such as random variation in school intake characteristics that attract additional funding (e.g., how many children are diagnosed with SEN and the proportion of ethnic minorities). Given the comparable variances of the school-specific and LEA grant components of income documented in Table 1, it is unsurprising that any causal impact from the LEA grant differences is offset by downward biases coming from other compensatory school-specific income sources. These results demonstrate that spatial differencing between closely spaced schools and controlling flexibly for spatial location is not on its own an effective strategy to deal with the endogeneity of school-level expenditures.

To overcome this problem, columns (5) and (6) implement the sharp, reduced form discontinuity design from equations (2) and (3) in which the expenditure treatment variable is the LEA-level grant rather than school-specific, level of funding per pupil per year.[20] Now, differences in funding between each of the schools in a pair are due solely to each school being on different sides of the boundary and hence exposed to a different level of LEA grant. With this set up, we need only eliminate confounders that are correlated with the LEA grant, which is determined simply by the national formula. Differencing between the FSM-matched nearest-neighbour schools and including distance-to-boundary polynomials effectively controls for these confounders. These

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20.   Specifically, this is the 4-year moving average of the mean, across schools in the LA, of the category IO1, "funds delegated by the LA" in our CFR school income and expenditure data.

results are not, however, dependent on the specific functional form of the distance controls, as we show later in Section 5.4. When we implement this reduced form, sharp discontinuity design, the coefficients for the effect of funding on ks2 test scores are positive and statistically significant. The coefficients remain relatively stable when we include the extended control variable set—including prior achievement at age 7—in column (6). The effect sizes are large, implying that a £1,000 increase in mean LEA per pupil per year expenditure during the ks2 period increases student performance at ks2 by 0.29 standard deviations.

The IV spatial discontinuity design estimates are shown in columns (7)–(10). In columns (7)–(8), we use school-specific expenditure per pupil as the treatment variable (as in columns 3–4), but instrument this with the LEA-specific grant per pupil (the treatment variable in columns 5–6) using two-stage least squares. The first-stage coefficients are around 0.8 and the F-statistics are high (85–107), which is not surprising given that the LEA grant is, by construction, a key input into the school budget. The point estimate without the extended control variables set in column (5) is larger than the reduced-form estimates at around 0.36, and this is insensitive to inclusion of the extended control variable set.

Columns (9)–(10) present the IV discontinuity estimates using our alternative instrument, the ACA component of the funding formula for the LEA grant. The key advantage of this instrument over the total LEA grant per pupil is that the ACA index is based on a labour market wage index and is intended to compensate for market prices, and prices are likely to vary more smoothly across space than other components in the LEA grant formula. Moreover, any failure in our controls for the spatial distance between schools and the boundary is likely to lead to an upward bias (thus attenuating our estimates) when we use the ACA index as an instrument, because higher wages and a higher ACA index are likely associated with educationally advantageous worker demographic characteristics. The point estimate in column (9) is in fact slightly lower than those in columns (7) and (8), but close to the reduced-form estimates in columns (5) and (6). Including the control variables in column (10) yields a very similar coefficient to that using the LEA grant instrument (i.e., in column 7).

A crucial point emerging from these results so far is that our estimates of the effects of resources are insensitive to the inclusion or exclusion of control variables for student demographics and prior achievements (namely test scores at age 7, ks1). From Table 2, it appears that the LEA grant instrument performs similarly to the ACA index in balancing, but there is much more variation in the LEA grant instrument and it gives a much stronger first stage. As noted in the discussion of the descriptive statistics, estimation from differences in the ACA index relies on relatively few LEA neighbouring pairs that have different ACA values (whereas the LEA grant always differs across neighbouring LEA pairs). The remaining results in the paper are therefore reported for the LEA grant instrument only.

Note that the reduced form sharp discontinuity estimates are best interpreted as "intent to treat" estimates of the school-level expenditure effects, given that the funding variable represents the mean per pupil grant in the LEA as a whole, not the actual expenditure per pupil in the specific school. In contrast, the IV discontinuity estimates

are best interpreted, in the context of heterogeneous causal treatment impacts, as "complier average causal effects" (Angrist, Imbens and Rubin 1996). They represent the effect of increases in resources in schools that experience higher expenditure per pupil as a consequence of being in an LEA with a high per-pupil grant. Looking across columns, the IV estimates are broadly similar given the standard errors and the reduced-form intent-to-treat estimates somewhat lower, suggesting an impact of around 0.29–0.36 standard deviations at ks2 from an increase of £1,000 per pupil per year (in each year preceding the test).

These effects are estimated from expenditure and grant differences that are averaged over 2–4 years prior to the ks2 tests that are intended to represent the average difference in expenditure in each year leading up to the tests. In Appendix C, we show that whether we use means over time or single year resource measures makes little difference, which is unsurprising given that resources are highly correlated within schools over time and the regressions are cross sectional. Because the regressions in columns (6), (8), and (9) control for ks1 tests at age 7, and we showed in Table 2 that ks1 tests were unrelated (either causally or through sorting) to the expenditure differences during ks2, the implication is that the effects on ks2 are the result of 4 years of additional £1,000 per pupil per year funding between ks1 and ks2. To understand what this means for the annual gains from a 1-year increase in expenditure, we need to know the dynamics of the education production function. If the parameter measuring persistence of achievement from 1 year to the next is ρ, then we need to reduce our estimates by a factor of $(\rho + \rho^2 + \rho^3 + \rho^4)^{-1}$ to get the implied 1-year effect (ignoring the possibility that the behavioural response to a one-off change may be different from that arising from a permanent change). Unfortunately, estimation of these dynamics is a challenge in itself and we do not attempt a thorough analysis here. Instead, we consider two benchmarks. Firstly, if achievement follows a random walk, then we simply have to divide our estimates by 4 to get the annual impacts implying that a 1 year £1,000 per pupil increase in funding would raise attainment by 0.075 to 0.090 standard deviations. Alternatively, if we estimate a mean reversion parameter by a regressing student ks2 scores on ks1 scores (standardised), with controls for student demographics and school fixed effects, we obtain an estimate of around 0.8. This implies that the autocorrelation in scores from year to year would be around $0.8^{0.25} = 0.95$ and we need to downscale our estimates by 3.5, giving estimates of between 0.08 and 0.10 standard deviations for a £1,000 expenditure difference.

A visualisation of the first-stage and reduced-form estimation is shown in Figure 3, where we replicate the kind of figures typically used in single discontinuity RDD designs. The top plot shows how school expenditure changes on average across all the boundaries in our estimation sample. The bottom plot shows the corresponding picture for ks2 scores. The dots show means in bins corresponding to decile distance bins moving away from the boundary up to 1 km either side and the curves show the fitted line and 10% confidence intervals using a cubic polynomial (the plots have the same structure as those used to show balancing in Appendix B, and discussed in Section 5.2). Evidently, there is a distinct and statistically significant jump in expenditure on average across the high/low LEA grant boundaries (the magnitude of the jump and its p-value
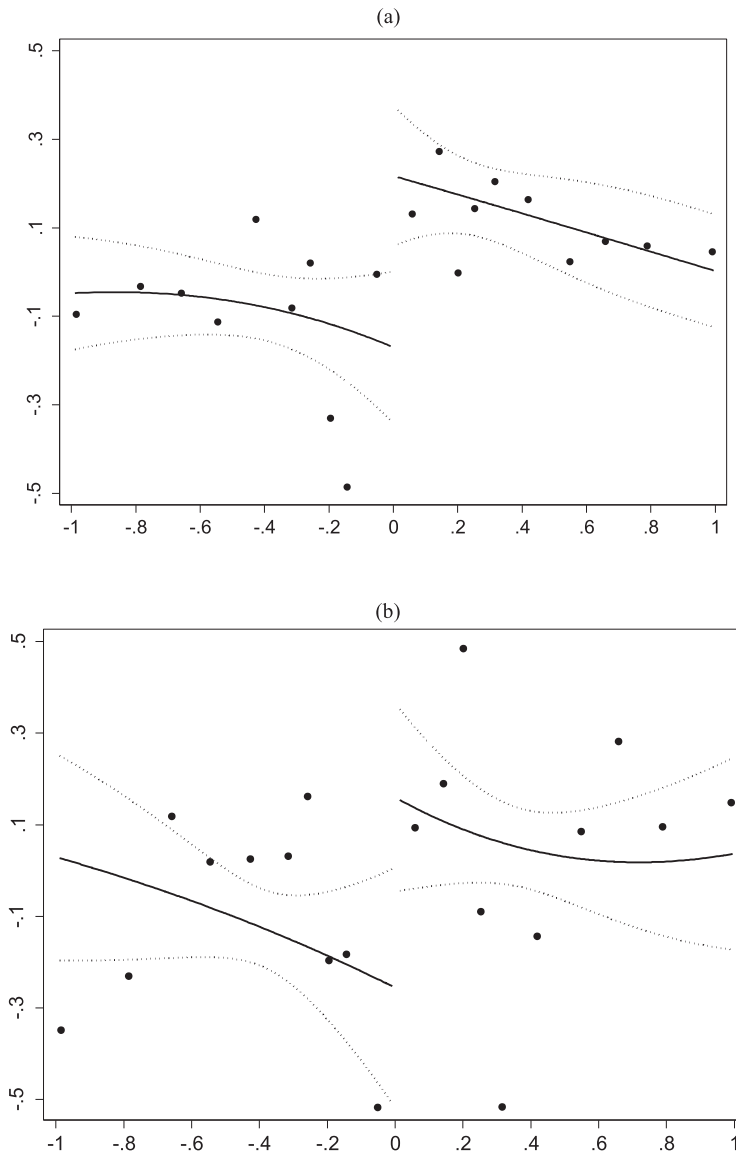
FIGURE 3. Graphical spatial RDD results for LEA grant. (a) "First stage" effects of LEA grant on school expenditure per pupil. Test for boundary difference: (standardised) b = 0.387, p = 0.024. (b) Cross-boundary age-11 ks2 score discontinuity for lower/higher LEA grant income. Positive distances indicate higher LEA grant. Test of boundary difference (standardised): b = 0.416, p = 0.07. Figures show plots of standardised variables on distance to LEA boundaries (km) amongst school pairs along LEA boundaries. Dots indicate means in distance decile bins. Curves are cubic polynomial regression predictions and 10% confidence intervals. Positive distances indicate distances from school to boundary on side of boundary with higher expenditure. Zero on *y*-axis corresponds to mean at low-funded side of each boundary. Note, these figures are analogous to the regression results in Table 3, column (7), but the regression estimation exploits variation in the magnitude of the income and ks2 differential across different boundaries, not just the sign.

is shown at the top of the figure). This jump is also clearly evident in the lower panel that shows the corresponding picture for ks2 test scores, with a 0.4 standard deviation gap across the boundaries on average (*p*-value 0.070). Note though that this mean difference in expenditures and outcomes across all boundaries is not all of the variation that is used in the estimates in Table 3, which are based on the covariance between LEA-grant differences and ks2 differences over all the LEA boundaries in our data set (there are 152 of these in our main 2 km interschool distance sample).

5.4. How is Additional Money Spent?

Our design based on LEA funding differentials does not permit us to estimate what types of expenditure are effective in raising achievements (because grants from the LEA are not designated for specific types of expenditure). However, in supplementary analysis (reported in Appendix D.1 in Appendix D) we estimate how the expenditure shares on different items vary with the LEA grant differences between the nearest-neighbour matched schools in our sample. These results suggest that schools spread additional income across most types of input (teachers, training, premises, professional services, supplies, etc.). There is a slight reduction in the share spent on teachers, with an additional £1,000 per student per year reducing the share by 4.4 percentage points (from a mean of 56.6%). There is a corresponding increase in the share spent on learning resources and Information and Communication Technology (ICT), professional services and supplies that are quite large relative to their initial shares (e.g., spending on ICT and learning resources rises from 3.8% to 4.8%). The average school in our sample has 350 in two classes of 25 in each of seven grades from age 5–11. Given 57% of any increase in expenditure is evidently spent on teachers, the overall implication is that an average school that receives and spends a £1,000 increase in total income from the LEA would increase spending on teachers by about £200,000 (=350 × 1000 × 0.57). This increase would pay for around seven teachers (one per grade in primary school) if paid at the middle of the basic class room teacher pay scale at 2009 prices (around £26,000), which is one per grade. This change is analogous to a class size reduction of 25 to 17 pupils (=50/3).

5.5. Evaluating the Identification Strategy Part 2: Robustness Tests and Other Causal Channels

Table 4 presents a range of additional robustness checks, in which we re-estimate the specification of Table 3, column (8) but with various modifications to the estimation sample and control variables. This specification uses the LEA grant as an instrument for school expenditure. This baseline result from Table 3 is repeated in Table 4, column (1) for comparison.

Column (2) takes the basic step of dropping duplicate school pairs in the estimation sample. As discussed in Section 4.5, the data structure has repeated observations for some school pairs, because two schools A and B can appear as a pair when school B is the closest school to school A in an adjacent LEA, and then again when school A

TABLE 4. Robustness checks I.

Various specification changes	(1) LEA grant instrument baseline	(2) LEA grant instrument, dropping duplicate school pairs	(3) LEA grant instrument, no boundaries that coincide with features	(4) Boundary and LEA clustering	(5) Including school mean ks2 predictions	(6) Excludes movers into school after ks1 or during ks2	(7) Movers after ks1 or during ks2
Total expenditure per pupil (£1000s)	0.360***	0.372***	0.312***	0.360***	0.357***	0.469***	0.262**
	(0.074)	(0.073)	(0.083)	(0.079)	(0.073)	(0.093)	(0.101)
First stage: F-stat	107.0	86.85	95.82	96.76	109.7	66.82	86.62
First stage coefficient	0.782	0.874	0.756	0.782	0.779	0.752	0.862
	(0.076)	(0.094)	(0.078)	(0.080)	(0.074)	(0.092)	(0.093)
Observations	379,194	275,279	328,119	379,194	379,194	245,992	133,158
School-pair-by-year fixed effects	4,540	4,536	3,915	4,540	4,540	4,402	4,537

Notes: Refer to Table 3. Column (1) repeats Table 3, column (8). All columns include control variables as in Table 3. Column (2) drops observations that correspond to duplicate pairs of schools such that if school 1 is matched to school 2 and school 2 matched to school 1, then only one of these pairs is used. Column (3) drops boundaries that approximately coincide with motorways, major roads, and railways. Column (4) applies double clustering at LEA and LEA boundary level (implemented using the STATA add-on command reghdfe). Column (5) includes the school-by-year mean predicted ks2 as a control, where the predictions are from a within-school regression of ks2 on student characteristics. Column (6) excludes students who moved into the school during ks2 either voluntarily or because they attended a ks1-only infants school. Column (7) is estimated from students who moved into the school during ks2 either voluntarily or because they attended a ks1-only infants school. **Significant at 1%; ***significant at 0.1%.

is the closest school to school B. This may raise some concerns about some school pairs being overweighted, and the standard errors being underestimated—although we cluster the standard errors on LEA boundaries to avoid the latter issue. However, as can be seen from Table 4, column (2), dropping these duplicates makes almost no difference.

A more substantive concern in boundary discontinuity designs of this type is that boundary features can coincide with geographical features. These features might split communities into different school catchment areas implying that neighbouring schools in adjacent LEAs are not comparable. To mitigate this concern, we re-estimate using boundaries that do not coincide with major roads, motorways, and railways (our main sample already excludes coastal features).[21] These results in column (3) are slightly lower than the baseline estimates but by less than one standard error, suggesting that coincidence of physical features and LEA boundaries is of little importance.

In column (4), we use double clustering on both LEA boundaries and LEAs when estimating standard errors. The concern here is that the LEA-grant variable varies by LEA and year, which can lead to biased standard errors in conjunction with intra-LEA autocorrelation in the error terms. Doing so makes almost no difference to the standard errors. Column (5) includes controls for school mean ks2 predictions (based on the predetermined pupil characteristics) as used in the balancing tests in Table 2, as a basic control for mean school composition (e.g., peer effects). This adjustment makes no difference. An alternative specification in which we control for the school means of all the student characteristics yields almost identical results. One concern might be that our estimates are driven by sorting across schools after initial entry, so in columns (6) and (7) we look at differences for school movers and stayers. The pool of students in a school at the end of ks2 consists of three groups. Around 68% of students will have been in a ks2 school from before the ks1 tests at age 7. Another 19% will have come to the school because their former infants' school only took students up to ks1/age 7 and the ks2 school may be a junior school for children aged 8 and up. Another 13% of students will have moved into the school during ks2 for other voluntary reasons. Looking at the difference between the movers and the stayers in columns (6) and (7) reveals that the effects of expenditure in the ks2 schools are substantially larger for students who have been in the ks2 school the longest, although are still evident for the movers. There is clearly no indication here that movers into high-funded schools drive our findings, and if anything our main estimates are likely to be underestimates given the substantially bigger coefficient on students who have been in the higher-funded schools the longest.

A more serious potential weakness of the overall research strategy is that LEA-level differences in funding to primary schools are very likely to be correlated with a range of other LEA-specific public finance variables and characteristics. This potential correlation occurs because similar funding rules are applied to other funding streams

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21.   We find those sections of the LA boundaries that "intersect" (i.e., share part of their length with) primary roads, railways, and motorways and drop school groups that have these boundary sections as their nearest LA boundary.

TABLE 5. Robustness checks II—other LEA policy variables.

With LEA-related policy variables	(1) LEA grant instrument	(2) LEA grant instrument, within Police Force Areas	(3) LEA grant instrument with teacher pay district dummies	(4) LEA grant instrument with control for teacher pay London weights	(5) LEA grant instrument with control for LEA mean ks2 scores
Total expenditure per pupil	0.300***	0.298***	0.320***	0.305***	0.305***
(£1000s)	(0.080)	(0.080)	(0.076)	(0.080)	(0.079)
Average council tax per household	−0.020	0.014	−0.065	−0.028	0.009
(£1000s)	(0.068)	(0.071)	(0.074)	(0.070)	(0.081)
Nonprimary school children's	−0.006*	−0.006*	−0.004	−0.006*	−0.006
Services per child (£1000s)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Social services spending per person	0.178	0.163	0.171	0.182	0.117
(£1000s)	(0.160)	(0.166)	(0.165)	(0.163)	(0.174)
Health spending per household	0.015	0.017	0.027	0.016	0.016
(£1000s)	(0.015)	(0.015)	(0.015)	(0.016)	(0.015)
Teacher pay scale weight (scale point 6)	–	–	–	−0.326	–
				(1.054)	
LEA mean ks2 score	–	–	–	–	−0.085
					(0.120)
First stage: *F*-stat	120.7	116.5	137.6	117.6	107.5
First stage coefficient	0.889	0.887	0.877	0.874	0.884
	(0.081)	(0.082)	(0.075)	(0.076)	(0.082)
Observations	379,194	348,022	379,194	379,194	379,194
School-pair-by-year fixed effects	4,540	4,179	4,540	4,540	4,540

Notes: Refer to Table 3. Column (1) repeats Table 3, column (8). All columns include control variables as in Table 3. See text of Section 5.5 for explanation of LEA spending variables. *Significant at 5%; ***significant at 0.1%.

to LEAs, and these funding streams could affect children through the quality of other services provided. The credibility of our strategy therefore requires that we demonstrate that these other aspects of local public finance do not affect student primary school test scores (conditional on primary school expenditures and the covariates and fixed effects in our regressions).

This evidence is in columns (1)–(5) of Table 5, where we repeat the basic specification but with additional controls for other aspects of LEA finance. Column (1) adds in a set of the most important LEA-related public finance variables: average council taxes per household, where council taxes are the primary source of LEA funding other than central government grants; LEA spending per student on nonprimary (nursery, higher) schooling, which might matter through spillovers from the performance of older or younger siblings; spending on social services to children and adults; and health spending per person in the local Primary Care Trust (local bodies through which the National Health Service is funded, and are generally much larger than LEAs, although their boundaries coincide). Data on police spending are not easily merged with our data, so instead column (2) keeps only matched schools that are within the same Police Force Areas (the units by which policing is funded and organised). This ensures that there are no cross–LEA-boundary differences in police funding. Column (3) goes one step further and controls for mean student performance

at age 11 in other schools in the LEA, that is, for all other potential influences on age 11 achievement at the LEA level, to rule out potential spillovers from LEA average performance to the performance of the boundary schools. In all cases, the coefficients on these LEA-level control variables are small and insignificant, and the coefficient on school expenditure is of the same order of magnitude as the previous estimates at around 0.3. Evidently, these other funding sources and other LEA-level influences are not a substantive threat to the main findings. It is also worth noting that out of this new set of LEA level variables, only nonprimary school spending and social services spending are, in practice, correlated with the LEA primary school grant (from the first stage regressions).

Lastly, in Table 5, in columns (4) and (5), we check to see whether our results could be driven specifically by teacher pay differentials rather than more general expenditure differences. Some of the boundaries between LEAs coincide with London regional boundaries that are used to define London allowances for teachers (which are intended to compensate for higher costs in London). There are four pay regions: Inner London, Outer London, London Fringe, and the rest of England. When we compare schools across these boundaries, it could be that the additional expenditure from the LEA grant goes straight into teacher pay rather than, say, paying to reduce class sizes, provide additional support staff, or buy other types of learning resources. To check for this, column (4) includes imputed regional teacher pay weights as a control variable.[22] Column (5) includes dummies for each pay region as an alternative. In both cases, the coefficient on school expenditure is only slightly reduced, in size and significance. Evidently, the effects of expenditure are not working specifically through higher teacher wages.

As a further test for a correlation between LEA-level funding differences and other sources of expenditure, we looked at whether the funding a school receives from sources other than the LEA grant are correlated with the funding they receive from their main LEA grant. Schools in our sample receive around 79% of their resources from the main LEA grant, around 8% from charitable and voluntary contributions, and the rest from various other grants from the LEA and/or central government (e.g., grants for ethnic minority achievement and SEN). In particular, we are concerned that low funding from the LEA might induce schools to raise more funds from alternative sources. Although not necessarily compromising our research design, there might be concerns that there is some general behavioural response by school leadership and staff to these funding challenges that has direct effects on achievement as well as increasing school resources. To test for these possibilities, we estimated school-by-year level regressions (with school pair fixed effects) of alternative income sources on the LEA grant per pupil. We found no large or significant association between the LEA grant and alternative funding streams in total, nor between the LEA grant and voluntary/charitable contributions specifically, again supporting the

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22.   One for England, 1.033 for London Fringe, 1.113 for Outer London, 1.153 for Inner London, based pay at point 6 over 2007–2009, a midpoint on the national teacher pay scale.

identifying assumption that the cross-boundary funding differentials are uncorrelated with cross-boundary differences in school characteristics (results not tabulated). In further robustness checks (again not tabulated), we compared schools in larger groups of 4, 6, and 8, rather than in pairs across the LEA boundaries. Extending the sample in this way results in more precise estimates, but leaves the key findings essentially unchanged.

Table 6 looks at a different issue, which is the sensitivity of our results to the restrictions used on the distance between schools, the way we control for spatial trends, and the thresholds used when matching schools on the proportion of FSM-entitled students. We show estimates with and without our extended control variable set (ks1, FSM, ethnicity, etc.). In columns (1) and (2), extending the maximum distance between schools considered in the analysis yields estimates at the lower end (0.29–0.30), whereas reducing the distance in columns (3) and (4) increases the estimates slightly (up to 0.41). Columns (5) and (6) use a common linear trend on each side of the boundary, plus a high-grant boundary side dummy. This eliminates the mean cross-boundary differences shown in Figure 3, and uses only the covariance in funding and ks2 across different boundaries. The results are unchanged. Columns (7) and (8) introduce boundary-specific linear trends instead of a general cubic polynomial for all boundaries. This gives even higher estimates without control variables, but a very similar estimate to our main specifications with controls (0.38). Columns (9) and (10) are locally weighted regressions that remove any other controls for distance and weight the regressions towards schools that are closer together (using weights $\exp(-distance)$). Results are broadly similar, though at the lower end of the range when control variables are added. Evidently, the results are not highly sensitive to differences in the spatial controls.

Columns (11)–(16) adjust the bandwidth we use when matching schools on FSM proportions, from 5 percentiles to 15 percentiles and then at 100 percentiles (i.e., ignoring FSM). Evidently, our design based on FSM matching is doing an important job in controlling for spatial trends (and any sorting) that are correlated with the LEA-grant differentials. Without additional covariates in the regression, widening the bandwidth tends to reduce the estimates while narrowing it increases them (becoming small and insignificant without any matching of schools by FSM). The results are, however, always broadly similar when linear controls for student characteristics are included.

5.6. Heterogeneity by School Characteristics and Subject

In addition to the baseline estimates of the effect of school expenditure, we offer a number of extensions that lead to additional insights into what types of student benefit and in what ways they benefit from additional funding. In particular, we are interested in whether additional funding for some students, schools, and subjects is more effective than for others. Given that we only observe funding at school level, we can only convincingly answer questions about how achievement varies with the characteristics of schools and their average student intake, because we do not have expenditure split

TABLE 6. Sensitivity to different assumptions about maximum distance between schools, free school meal matching, and controls for spatial trends.

Various matching and distance controls	(1) 2.5 km with cubic polynomial	(2) 2.5 km with cubic polynomial	(3) 1.5 km with cubic polynomial	(4) 1.5 km with cubic polynomial	(5) 2 km linear trend and hi/lo boundary side dummy	(6) 2 km linear trend and hi/lo boundary side dummy	(7) 2 km boundary specific linear	(8) 2 km boundary specific linear
Total expenditure per pupil (£1000s)	0.303***	0.290***	0.389***	0.406***	0.333***	0.354***	0.446***	0.380**
	(0.072)	(0.057)	(0.117)	(0.096)	(0.093)	(0.081)	(0.132)	(0.133)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
First stage: F-stat	96.15	123.6	53.40	51.51	68.97	101.2	38.43	32.71
Observations	566,731	566,731	186,265	186,265	379,194	379,194	379,194	379,194
School-pair-by-year fx	6714	6714	2316	2316	4540	4540	4540	4540

Various matching and distance controls	(9) 2 km, locally weighted	(10) 2 km, locally weighted	(11) 2 km, cubic, 5 pctile FSM match	(12) 2 km, cubic, 5 pctile FSM match	(13) 2 km, cubic, 15 pctile FSM match	(14) 2 km, cubic, 15 pctile FSM match	(15) 2 km, cubic, no FSM match	(16) 2 km cubic, no FSM match
Total expenditure per pupil (£1000s)	0.329***	0.287***	0.468***	0.379***	0.263***	0.297***	0.083	0.399***
	(0.082)	(0.057)	(0.097)	(0.095)	(0.065)	(0.060)	(0.117)	(0.108)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
First stage: F-stat	90.25	108.6	31.08	30.36	94.07	121.8	65.67	39.07
Observations	379,194	379,194	259,363	259,363	460,690	460,690	777,235	777,235
School-pair-by-year fx	4,540	4,540	3,079	3,079	5,465	5,465	8,204	8,204

Notes: Table shows IV regression coefficients from standardised Key Stage 2 scores on school expenditure, using LEA grant instrument. Specifications vary according to maximum distance between schools retained in sample (1.5 km, 2 km, 2.5 km), percentile bandwidth used to match schools (5,10,15, 100 percentiles), and functional form of distance to boundary trends (linear, cubic, boundary specific linear, with or without high/low LEA grant boundary side dummy). Spatial trends are always interacted with a dummy for high/low side lea grant. Locally weighted regression refers to a weighted least squares regression with weights exp(−distance) where distance is distance in kilometres between matched schools. **Significant at 1%; ***significant at 0.1%.

by subject area, and do not know on which students the money is being spent. Table 7 shows these results, reporting the effect of school expenditure on subsets of schools in our LEA boundary sample, where the sample is split by the proportion on free meals, the proportion of boys, the proportion white, student prior achievement at age 7 (ks1), and the income deprivation in the student's home neighbourhood. The split is based on whether or not a school is above or below the mean in terms of these characteristics. The regression specification is analogous to Table 3, column (8).

The overall story from looking across Table 7 is that the effects of expenditure are considerably higher and more significant in schools with more "disadvantaged" students. Expenditure appears to have had much stronger effects in schools with high proportions of students eligible for free meals, higher proportions of non-White students, lower than average mean prior achievement, and where a high proportion come from neighbourhoods with a high index of deprivation. In these demographically disadvantaged schools, £1,000 more expenditure is associated with a 0.43–0.5 standard deviation increase in test scores. Evidently, expenditure has higher returns in schools where there are greater gains to be made at school level. Interestingly, these effects seem to be based on the average characteristics of the student intake, not the type of student. If we split the sample by individual student characteristics, rather than the school characteristics (results not tabulated), there appears to be relatively little difference between the advantaged and disadvantaged groups. In other words, all types of students in the most disadvantaged schools appear to benefit from additional funding, not just the disadvantaged students, although it is hard to know what to conclude from this finding, given we have no information on how additional resources were split within schools between different student types.

Lastly, we have also split the ks2 test score into subject areas—Maths, Science, and English and estimated the effects of expenditure on these subjects separately (results not tabulated). It turns out that expenditure affects performance in all subjects, although the strongest effects are in Science (coefficient of 0.438, standard error 0.08), with English and Maths showing a more moderate, but still significant response of around 0.26–0.27 (0.07). There are few clear cut theoretical reasons to expect responses to differ across subjects, although similar variation is found in other work on English schools e.g., Machin et al. (2010) for a resource-based programme targeted at secondary schools in disadvantaged urban areas and Gibbons and Telhaj (2016) on the influence of peer group prior achievement.

6. Conclusion

We have shown that similar, closely neighbouring schools in adjoining districts in England received markedly different levels of funding from their districts during the 2004–2009 period. These differences occurred because the districts received funds from central government that compensated for differences in demographics and prices in the LEA as whole. These demographic and price differences were not representative of the intakes and inputs of closely neighbouring schools near the LEA boundaries,

TABLE 7. Heterogeneity in effect of expenditure by school characteristics.

	(1) Age-11 ks2 total score High FSM	(2) Age-11 ks2 total score Low FSM	(3) Age-11 ks2 total score More boys	(4) Age-11 ks2 total score More girls	(5) Age-11 ks2 total score High White	(6) Age-11 ks2 total score High non-White	(7) Age-11 ks2 total score Low age-7 score	(8) Age-11 ks2 total score High age-7 score
Total expenditure	0.498***	0.161	0.430**	0.325*	0.260	0.432***	0.463***	0.412*
Per pupil (£1000s)	(0.105)	(0.100)	(0.146)	(0.160)	(0.144)	(0.099)	(0.116)	(0.175)
First stage F-stat	37.47	36.84	26.37	16.76	17.79	70.87	36.93	40.43
First stage coefficient	0.749	0.789	0.860	0.723	0.742	0.772	0.715	1.006
	(0.122)	(0.130)	(0.167)	(0.178)	(0.176)	(0.092)	(0.120)	(0.168)
Observations	181,459	197,735	212,508	166,679	183,902	195,291	212,779	166,411
School-pair-by-year	2,538	2,179	3,540	3,098	2,482	2,734	3,344	2,258

Notes: Notes as given in Table 2. Regressions include control variables listed in Table 2 (except characteristic corresponding to column headings). High and Low categories refer to above and below median values of the characteristics in the column headings. *Significant at 5%; **significant at 1%; ***significant at 0.1%.

and yet LEAs did not fully redistribute funds to individual schools according to the demographics of the individual school intakes. Our analysis shows that these funding differentials caused differentials in student performance, and our findings indicate quite a strong role for general funding increases in raising achievement in urban state schools. The headline result is that a £1,000 increase in per pupil expenditure per year raises test scores at the end of primary school by around 0.30 standard deviations.

Perhaps, it should not be surprising that better resourced schools get better results. However, convincing evidence of an impact from putting more money into state schools has remained elusive. Therefore, our analysis is a useful addition to the international academic literature on the economics of schooling. The results are crucially important for higher level policy making. For example, in the English context, they help address the question as to whether school budgets should be ring-fenced from cuts to public expenditure and whether resources should be explicitly directed to pupils from lower socioeconomic groups (or more accurately, to the schools which they attend).

In order to benchmark our findings against previous work, note that a £1,000 increase in expenditure is analogous to a class size reduction 25 to 17, closely comparable to the class size reductions in the Tennessee STAR experiment (Schanzenback 2006).[23] Although this experiment was on younger children, and refers to a specific use of a resource increase, it allows a useful point of comparison for our main estimates of the effects of general expenditure. In the STAR experiment, this class size reduction in kindergarten led to a 0.19 standard deviations higher performance at the end of the year. Our estimates of the equivalent 1-year impacts on 8–11 year olds are between 0.075 and 0.100 standard deviations and are somewhat smaller than this, but of a similar order of magnitude (see Section 5.3). Note though that the accumulated effects of being in small classes over 4 years of the STAR experiment appears not to be very large, although are difficult to determine from the existing literature. Our own estimates from the STAR data suggest that the implied cumulative effect for being in a small class from kindergarten through to third grade was about 0.25 standard deviations (accumulating the annual gains for new and existing students in small classes, and taking into mean reversion of the impacts for stayers from one year to the next). Our cumulative effects of an equivalent expenditure change in England's urban schools are therefore slightly bigger, at around 0.30.

The magnitudes of the effects we estimate are also substantially larger than those reported in school or sibling fixed-effect studies that exploit small year to year changes in school expenditure, including work based on the same school system and using similar data to ours (Holmlund et al. 2010; Nicoletti and Rabe 2014). A likely explanation for the lower estimates from studies that use time series variation within schools is that schools can adjust to short run expenditure changes without making substantive adjustments to the school structure or teaching methods (e.g., small short run changes in expenditure are unlikely to shift the number of teachers). In contrast,

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23. See Section 5.4 for this calculation.

our estimates are identified from quite large and persistent cross-sectional funding differences between close-neighbour schools.

Appendix A: FSM Matching and Its Consequences

Our spatial discontinuity design involves matching on location (distance from LEA boundaries), school type (Community schools), and on a bandwidth of percentiles within the FSM distribution. The aim of matching on FSM is to control for spatial trends that cannot successfully be controlled for by spatial matching alone (see e.g., Keele et al. 2015). The justification for this prematching on FSM is illustrated in what follows. Figure A.1 shows a hypothetical example map with four schools (triangles) near a boundary between two LEAs. One (k) has a high proportion of low-income residents. The other (k') has a low proportion of low-income residents. Residents are sorted across space with low incomes in the bottom left and high incomes top right
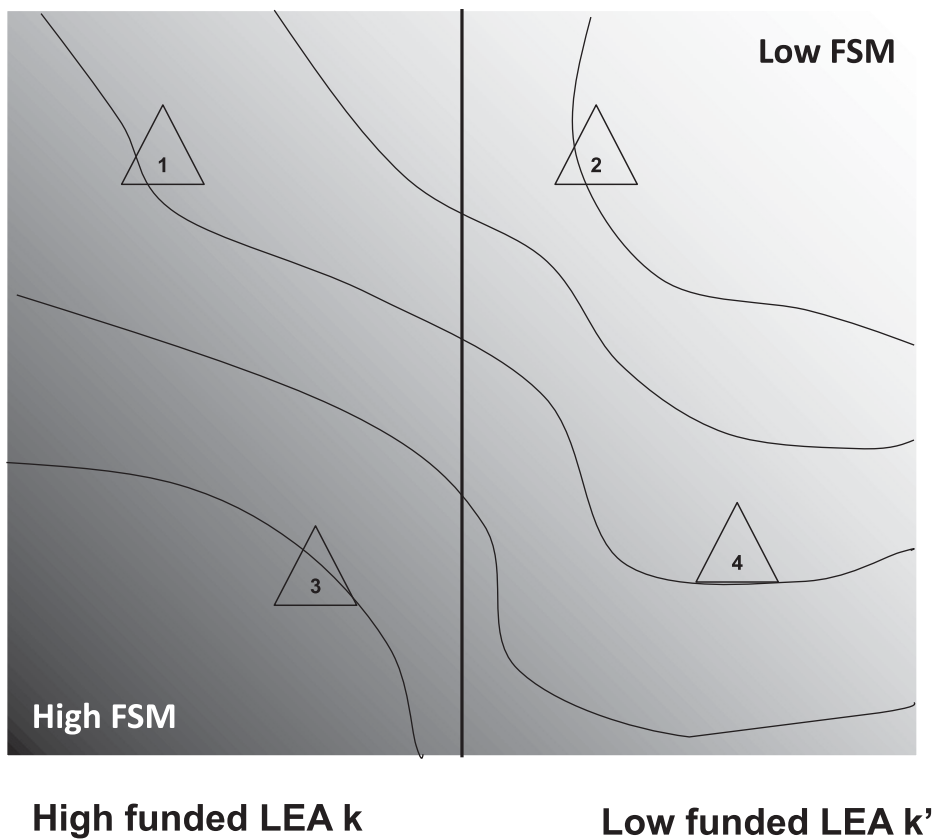
FIGURE A.1 Example of using FSM matching to control for complex spatial trends.
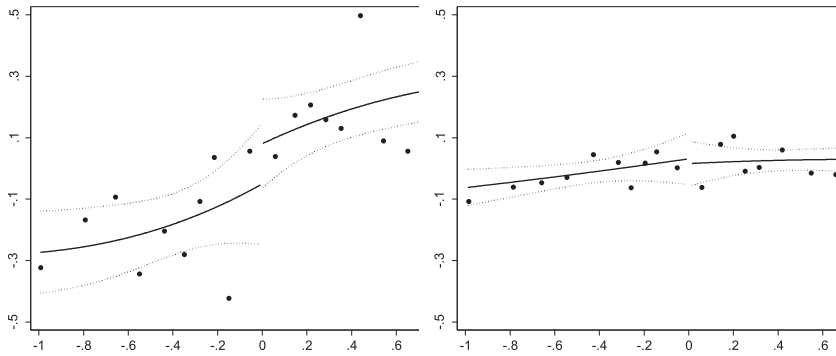
FIGURE A.2 Spatial trends in FSM in matched and unmatched samples. FSM proportion by distance and distance decile in 2 km boundary sample. FSM proportion by distance and distance decile matched using 10 percentile bandwidth.

(due to some unobserved amenity, distance to central city, etc.). Given the government compensatory funding allocation formula, LEA k receives a high per pupil grant from central government and LEA k' receives a low per pupil grant. The contours represent the levels of a random spatial field describing the resident proportions potentially entitled to FSM.

It is evident from this picture that simple spatial matching on distance to the boundary or controls for distance to boundary will be ineffective in controlling for general spatial trends of this type. School 2 and School 3 are closest to the boundary but are on different FSM contours so are poor matches in terms of intake characteristics. School 3 and School 4 are closest to each other but are also on different FSM contours. Although in this single boundary case, it might be possible to control for these spatial patterns with a high order polynomial in the geographical x and y coordinates, this will be infeasible with a high number of separate boundaries all with their own boundary specific geographical trends in socioeconomic characteristics and a small number of schools each boundary. In contrast, matching School 4 to School 1 based on similar FSM proportions provides a simple way to eliminate the differences in the socioeconomic characteristics associated with location, which are correlated with the LEA funding differences.

Figure A.2 (in what follows) illustrates this issue and the implications of the FSM matching procedure in our data. The left-hand panel shows the average FSM proportions (in standard deviations) in decile distance bins and the polynomial trend in the sample of Community schools that can be matched to a nearest Community school in an adjacent LEA within 2 km, with no restriction on FSM comparability. Positive values on the *x*-axis indicate distances (in kilometres) to boundaries on the side with higher LEA grants; negative values indicate distances to boundaries on the side with lower LEA grants. Evidently, there are strong trends (averaging across all boundaries) in FSM from the low to high funded side of the boundaries and the
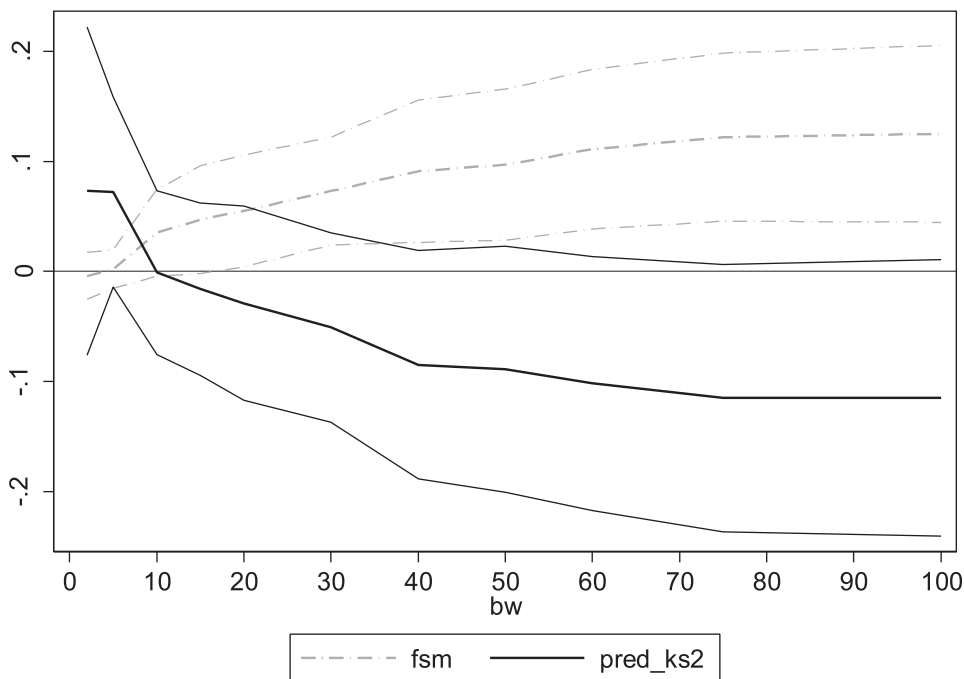
FIGURE A.3 Effect of matching by FSM on balancing of FSM and Ks2 test scores predicted from student covariates. FSM scaled as a proportion. Predicted ks2 is in standard deviations of ks2 scores in student distribution.

differential in funding between LEAs will be correlated with differences in FSM and related observables and unobservables in schools at even short distances from the LEA boundaries. The right-hand panel shows the patterns after matching each Community school to their nearest Community school with an FSM proportion within 10 percentiles in the overall distribution of FSM in the boundary schools (the matching process used in our main results). By construction, the trend across the boundary is substantially reduced and schools at any distance become more closely comparable to each other.

The relevance of this matching in terms of the overall research design is further illustrated in Figure A.3. In this figure, we show the balancing of schools in terms of FSM proportions and a linear predictor of student academic outcomes at Key Stage 2 (age 11) estimated from student demographics and prior achievement at Key Stage 1 (age 7). This linear predictor of ks2 scores is generated from coefficients estimated from a within-school regression of Key Stage 2 test scores on Key Stage 1 test point scores, and dummies for gender, age, ethnicity, English first language, and FSM entitlement, plus school cohort size. The figure shows how the balancing in terms of high- and low-funded LEA schools changes as we adjust the bandwidth used for matching schools by FSM proportion, from 5 percentiles up to 100 percentiles (unconstrained), using the data set of schools within 2 km of each other either side of the boundary. The y-axis shows the estimated coefficient (and 95% confidence intervals) from a regression

of student FSM or ks2 predicted scores on LEA-specific grant (in £000s), using the standard school-pair fixed effect design used in Table 3. The *x*-axis shows the percentile bandwidth used in matching.

Moving from right to left, it is evident that there is a strong correlation between inter-LEA funding differentials and school FSM proportions in the boundary schools without any matching on FSM. A £1,000 higher mean LEA grant is associated with an 11 percentage point higher proportion of pupils on FSM. Similarly, there is also a strong negative relationship between predicted ks2 in a school (based only on pupil demographics and prior achievement) and the level of funding, with a £1,000 extra spending per pupil per year associated with intakes with predicted scores around 12% of one standard deviation lower. Both these features are due to compensatory funding across LEAs: money follows disadvantaged pupils. As we match schools based on smaller and smaller percentile bandwidths, the mismatch in terms of FSM goes down, by construction. So too does the mismatch in predicted Ks2 scores derived from prior achievement, ethnicity, gender, age within the year, English first language, and FSM. Balancing in terms of Ks2 predictions on observables is optimised at around the 10 percentile FSM bandwidth used in our main estimation specification.

Appendix B: Discontinuity Graphs

Figure B.1 presents graphs showing average discontinuity across all boundaries. Figures show plots of standardised variables on distance to LEA boundaries (km) amongst school pairs along LEA boundaries. The plots are obtained from predictions of school-by-year level regressions of the cross-boundary difference in various dependent variables on either: (a) a dummy variable for the high-LEA grant side of the boundary and dummies for each distance-to-boundary decile up to 1 km either side, giving the means in decile bins; or (b) on a dummy variable for the high-LEA grant side of the boundary and a cubic polynomial in distance to the boundary interacted with this dummy, giving a smooth plot of the trends (and 10% confidence intervals). Dots indicate means in distance decile bins. Curves are cubic polynomial regression predictions and 10% confidence intervals. Positive distances indicate distances from school to boundary on the side of boundary with higher expenditure. Zero on *y*-axis corresponds to mean at the low-funded side of each boundary. Note, these figures are analogous to the regression results in Table 3, but the regression estimation exploits variation in the magnitude of the income and dependent variable differential across different boundaries, not just the sign. Table headings indicate mean difference in outcome (standardised) between the high and low side at zero distance and *p*-value of t-test of this difference. Again there is no evidence of any statistically significant average difference in the characteristics across boundaries. Note that in the regression analyses we can also control for these average differences across all boundaries and identify the effect of expenditure only from the covariance between funding and performance across boundaries. We do this in a robustness test in Section 5.4.
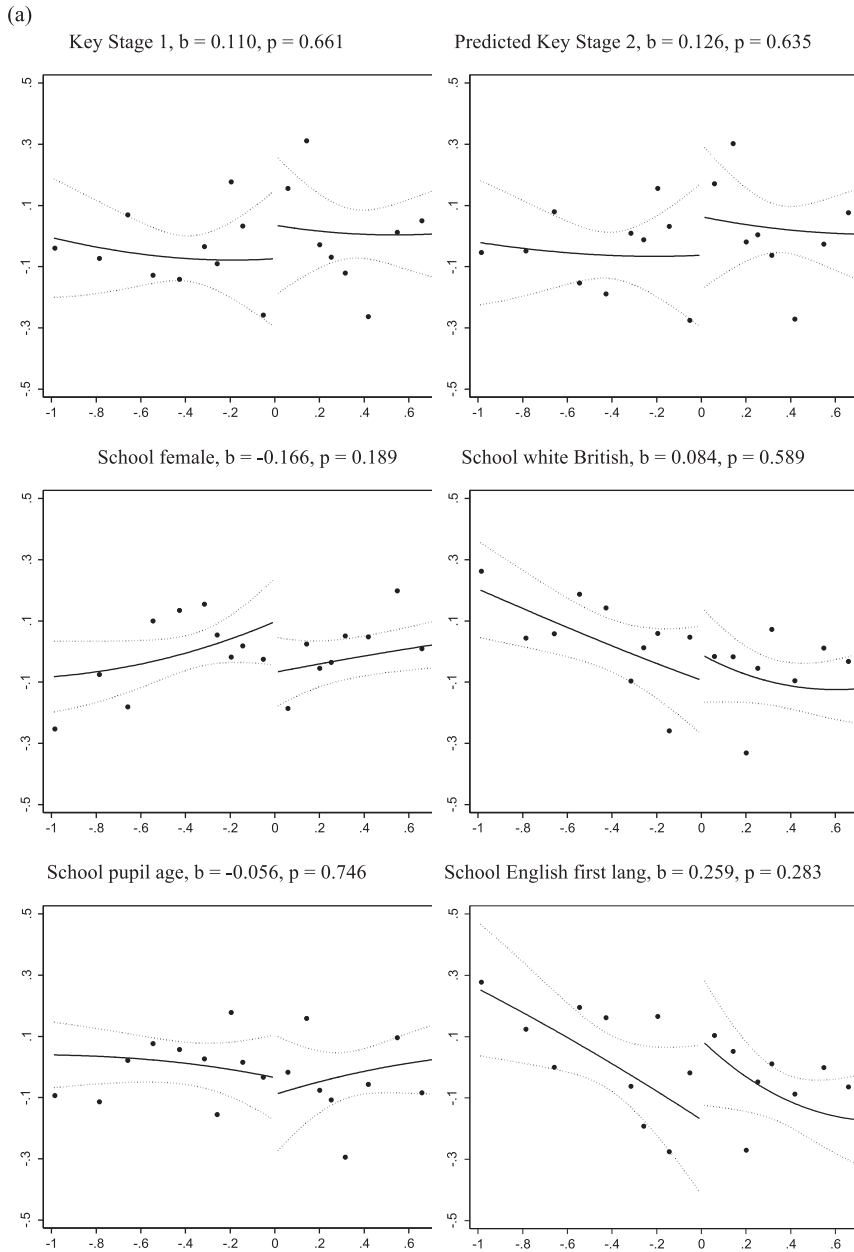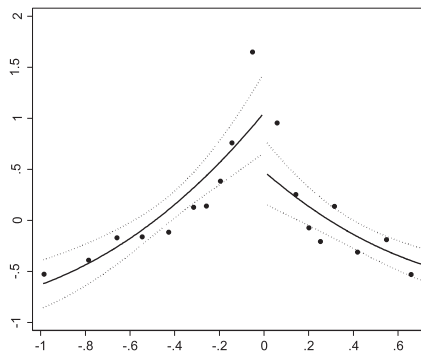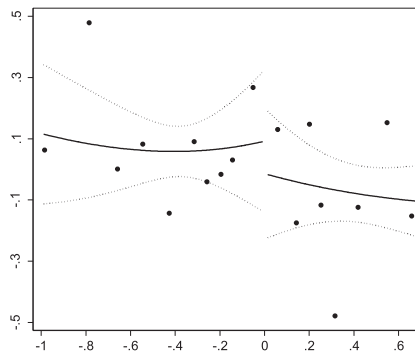
(a)

FIGURE B.1 Discontinuity graphs related to school characteristics and pupil home location characteristics. (a) School characteristics. (b) Pupil home location characteristics.
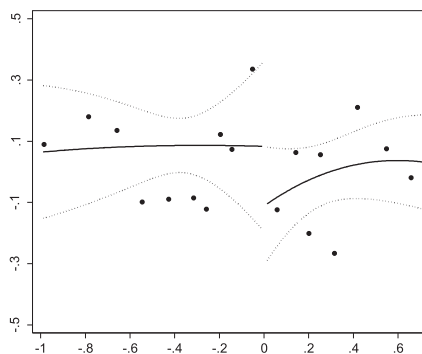
Proportion from adjacent LEA, b = -0.574, p = 0.111

Home-school distance, b=-0.106, p = 0.606
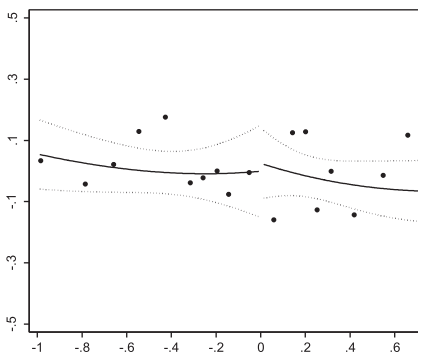

School proportion entering, b = -0.195 p =0.423

(b)

Residential house price index, b = 0.026, p = 0.844

Residential proportion high degrees, b = 0.080, p = 0.526

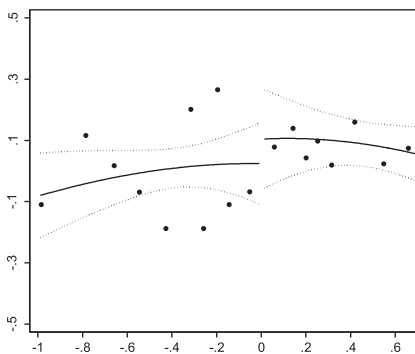FIGURE B.1 Continued.

Residential proportion unqualified, b = -0.062, p = 0.702

Proportion social renting, b = 0.161, p = 0.280

Residential proportion born UK b = 0.149, p = 0.109

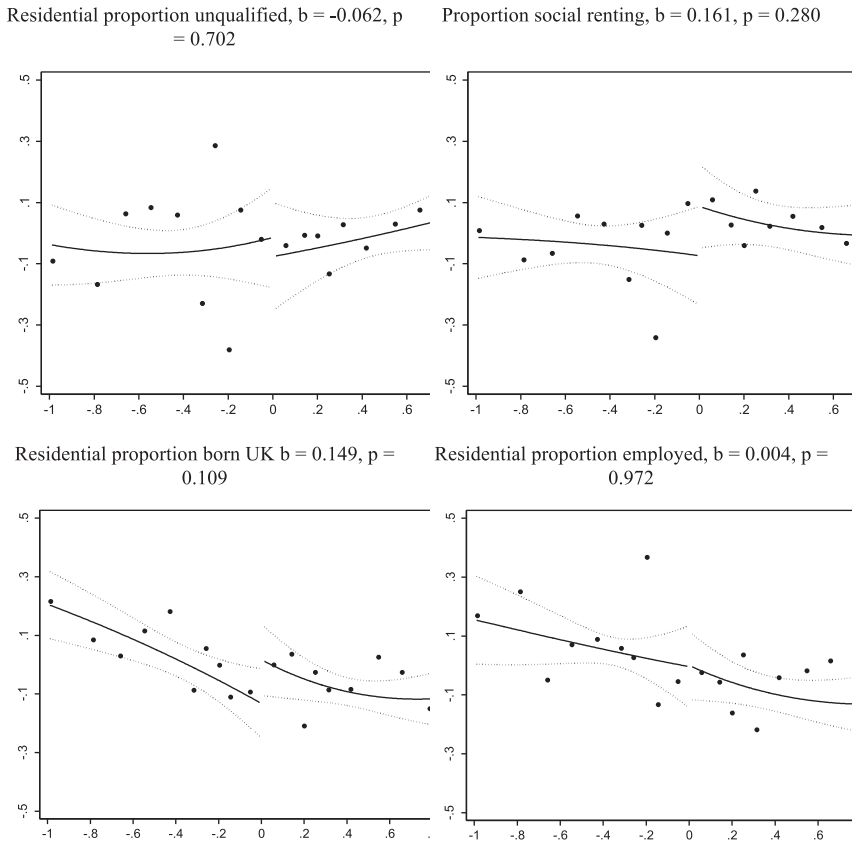Residential proportion employed, b = 0.004, p = 0.972

FIGURE B.1 Continued.

Appendix C: Timing of School Expenditure

Table C.1 shows results with various different ways of averaging expenditure and income over time. Averaging over the past 2 years or using current year expenditure gives similar results to using maximum number of years available (as in main results). Controlling for lagged 4-year expenditure (expenditure prior to Ks1) reduces the coefficient to around 0.3 and with a higher standard error (given the high correlation between lagged and current resource variables and smaller sample size due to loss of years) but is still broadly in line with the main results. This result indicates that the effects can be attributable to 4 years of £1,000 per pupil per year additional expenditure between Ks1 and Ks2.

TABLE C.1 Timing of school expenditure.

	Two-year mean expenditure (1)	One-year expenditure (2)	One-year expenditure conditional on 1-year expenditure prior to ks1 (3)
Expenditure per pupil per year (£1000s)	0.354*** (0.075)	0.342*** (0.075)	0.299* (0.135)
First stage: F-stat	94.70	83.85	26.94
Observations	379,194	379,194	174,318
School pairs	4540	4540	2215

Notes as given in Table 3, column (8). Includes control variables. *Significant at 5%; ***significant at 0.1%.

Appendix D: How schools spend additional LEA grant income

TABLE D.1 Response of expenditure shares to total expenditure per pupil. Local Education Authority grant.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Teachers	Education support staff	Other staff	Development and training	Premises	Learning resources and ICT	Professional services	Supplies	Other
Total exp pp (£000s)	−0.044***	0.002	−0.003	−0.001	−0.003	0.010*	0.016**	0.019***	0.003
	(0.012)	(0.011)	(0.005)	(0.001)	(0.005)	(0.004)	(0.006)	(0.005)	(0.004)
First stage F-stat	85.21	85.57	85.21	85.21	85.21	85.21	85.21	85.21	85.21
First stage coefficient	0.728	0.728	0.728	0.728	0.728	0.728	0.728	0.728	0.728
	(0.079)	(0.079)	(0.079)	(0.079)	(0.079)	(0.079)	(0.079)	(0.079)	(0.079)
Mean share	0.566	0.146	0.083	0.005	0.067	0.048	0.030	0.037	0.018
Observations	8,560	8,546	8,560	8,560	8,560	8,560	8,560	8,560	8,560
School-pair-by-year groups	4,280	4,273	4,280	4,280	4,280	4,280	4,280	4,280	4,280

Notes: School-by-year level regressions. Years 2004–2009. Teachers include teachers, supply, and agency teachers. Other staff includes administrative, clerical, premises, catering, and other employees. Premises includes building maintenance and improvement; grounds maintenance and improvement; cleaning and caretaking; water and sewerage energy; rates; other occupation costs. Professional services include bought in professional services—curriculum; bought in professional services—other. Supplies include administrative and catering supplies. Other includes insurance costs, loans, other financial outlays. Instrument is mean grant per pupil from LEA (mean in LEA as a whole). *Significant at 5%; **significant at 1%; ***significant at 0.1%.

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