Empirical Essays on the Economics of Education

BY

ANWEN ZHANG

BACHELOR OF ECONOMICS, JILIN UNIVERSITY (2006)
BACHELOR OF MANAGEMENT, JILIN UNIVERSITY (2006)
MSC ECONOMICS, UNIVERSITY COLLEGE LONDON (2010)

A THESIS PRESENTED TO
DEPARTMENT OF ECONOMICS
IN PARTIAL FULFILLMENT OF THE REQUIREMENTS
FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY

Lancaster University

LANCASTER, UNITED KINGDOM
FEBRUARY 2015

Declaration

I hereby declare that this thesis is my own work and has not been submitted in any form for the award of a higher degree elsewhere.

To Hui
for making the world beautiful

Acknowledgements

This thesis has benefited from the help and support of many people.

First and foremost, I am deeply indebted to my supervisors Ian Walker and Colin Green. This thesis would not have been possible without them. They have been continuously offering the best guidance and support I could ever ask for. They are always ready to share their insights, resources and time with me. I am particularly grateful for their extreme patience with me. Even though I am not the bright person I would like myself to be, they have helped me learn more than I thought possible. I count myself lucky to have had the privilege of learning from the best. I thank Ian for lending me generous support and encouragement when I applied for PhD studies at Lancaster, and for making arrangements for me to attend several training courses outside Lancaster. His advice and comments have been invaluable in the development of the research ideas in this thesis. His knowledge, wisdom and insights have inspired me in many ways. I am grateful to Colin for his constant guidance, warm encouragement, and the occasional gentle nudge to keep me on track. I have benefited immensely from his constructive and detailed comments on the drafts of this thesis, from the expression of major arguments down to the little revisions of "a" and "the". I also owe my gratitude to him for his kindness and generosity in connecting me to people and resources in the research field. As my role-model young economist, he sets an outstanding example for me to follow.

Besides my supervisors, many other members of the department have offered kind help when I knocked on their doors. A special thanks to Maria Navarro Paniagua, Geraint Johnes, Efthymios Pavlidis, Robert Simmons, and Dakshina De Silva, for their advice and discussions at various stages.

I also thank my colleagues and friends for the time we have spent together at Lancaster. Sharing an office with Peng Wang has led to countless useful discussions as well as a fruitful friendship. I have also had the pleasure of sharing the office with Rossella Iraci Capuccinello, Bankole Fred Olayele, and Colin Mang, whose company made my everyday work more productive and enjoyable. My fellow PhD students and friends, Peng Zhang, Dandan Li, Weijia Li, Rhea Goerge, and Peiran Shi, also deserve special thanks for the talks, laughs, games, and dinners we've had together.

In addition, I would like to thank many departmental visitors, seminar participants at the departmental internal seminar (2011), NWDTC PhD Conference (2012, 2013), cemmap housing workshop (2013), and IWAEE (2013) for helpful comments and conversations. Thanks are also due to Ryan Liu for his technical assistance with my earlier attempt of collecting house price data.

Financial support by Lancaster University Management School and the Department of Economics in the form of a PhD studentship is gratefully acknowledged for making my life easier as a postgraduate student.

Finally, it is the love of my family that ultimately motivates every step I have taken in my life. Without them, I could have never come this far. Thanks to my parents and my little sister, for being proud of me and being there for me; especially my mother, who has been the firmest supporter in my education from elementary school to graduate school, although she has never had a single day of formal education. Thanks are also due to my parents-in-law, for their love and support no less than my own parents. I dedicate this thesis to my wife, who has always believed in me, during easy and tough times.

Empirical Essays on the Economics of Education
by Anwen Zhang
Submitted to Department of Economics, Lancaster University in partial fulfillment of the requirements for the degree of Doctor of Philosophy

Abstract

This thesis consists of three self-contained essays on the economics of education.

Chapter 2 explores a secondary school admission policy reform and employs repeat sales data to examine the relationship between house prices and school quality. I use the reform to generate exogenous variation in school quality, and repeat sales data to eliminate time-invariant unobservable influences on house prices. There are two primary findings from this analysis. First, I find that a one standard deviation increase in school performance raises house prices by 2–2.5% for non-flat properties. Conversely, flats do not respond to school quality. Second, I show that parents value school outputs more than they do school inputs. These findings are robust to a number of alternative school quality measures and samples.

Chapter 3 provides evidence on the effectiveness of school capital investment on education outcomes by studying the short-run effect of a large school construction programme in England. Taking advantage of the phasing design that the whole programme is delivered in a sequence of waves, I apply difference-in-differences

techniques to elicit the causal effect of school capital investment on student academic achievement. I find that the programme disproportionately affects pupils from different backgrounds: academically and socioeconomically disadvantaged students enjoy large and positive test score gains, while their more advantaged counterparts do not. The overall effect remains positive but much smaller and insignificant. There is some evidence suggesting heterogeneous effects by school types, project types, and time lengths of building occupancy. I further demonstrate that these results are not driven by student selection into newly built schools.

Chapter 4 examines how income under-reporting could lead to biases in estimating returns to education for the self-employed. As the first step, I infer the true self-employment income following an expenditure-based approach. An average self-employed worker's reported earnings should be boosted by a factor of 1.4 to arrive at the actual earnings. More importantly, the degree of income under-reporting is nonlinear across the income distribution. Lower-income self-employed households under-report more heavily. In the second step, I estimate the returns to education for self-employed workers using the inferred income data. I find income under-reporting leads to a severe upward bias in estimating the returns to education. Compared to employees, the self-employed extract lower returns from education.

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Chapter 1

Introduction

Education is the most powerful weapon which you can use to change the world.

—— Nelson Mandela (1918–2013)

Our understanding of education from the economics perspective has both deepened and widened substantially over the last few decades. In this thesis, I contribute to this lively literature by presenting empirical evidence on three policy-relevant questions concerned with the provision and reward of education: How much do parents value school quality? Does school capital investment improve students' academic performance? How large are the economic returns of education for self-employed workers?

Chapter 2, "School Quality and House Prices: Evidence from a School Admission Policy Reform", aims to quantify the value parents place on school quality, and to a further extent, tries to identify what aspects of school quality parents value. There are two fundamental empirical difficulties in these questions. First, many of the factors that influence house prices are unobservable. High-performing schools tend to be located in good neighbourhoods, thus the observed house price differentials do not only reflect better school quality, but also better housing characteristics and neighbourhood traits. I seek to solve this problem by using repeat sales data. I difference out time-invariant influences of housing characteristics and neighbourhood amenities, then attribute the remaining variation in house prices to changes in school quality. Second, school quality may be driven by the characteristics of the neighbourhood, for instance parents in good neighbourhoods may input more resources in their children's education thus pushing up the neighbourhood school quality. This leads to an endogeneity issue. I address this issue by exploring a secondary school admission policy reform that exogenously changes the expected probability of securing a place at local schools.

There are two primary findings from this chapter. First, I find that a one standard deviation (SD) increase in school performance raises house prices by 2–2.5% for non-flat properties. Conversely, flats which are smaller in size and are less likely to accommodate school-age children, do not respond to school quality. Second, I show that parents value school outputs more than they do school intake. This suggests that parents care more about academic effectiveness, rather than the composition of students in the school.

Building schools is costly. Yet little is known about whether the provision of school buildings and facilities provides value for money. Causal evidence on the effect of school capital investment on education outcomes in the existing literature is scarce and mixed. It is difficult to evaluate the effect of school capital investments using observational data as government polices often target resources towards disadvantaged schools and areas, therefore simply comparing academic outcomes for schools with higher capital investments to those with lower capital investments would lead to biased conclusions. In Chapter 3, *"Better Buildings, Better Scores? The Short-Run Impact of a Large School Construction Programme"*, I seek to address this issue by exploiting the phasing design of a large school construction programme in England.

The whole programme is designed with clear-out prioritisation rules, which place the schools to receive school estate renewal investment in a sequence of waves according to their ranking of social and educational needs. The prioritisation procedure means that schools in neighbouring waves are similar, thus their performance is likely to follow similar trends over time. This enables a difference-in-differences approach to elicit the causal effect of school capital investment on student academic achievement.

I find that the programme has heterogeneous effects on pupils from different backgrounds: academically and socioeconomically disadvantaged students enjoy disproportionately large and positive test score gains, while their more advantaged counterparts do not. The overall effect remains positive but much smaller and insignificant. There is some evidence suggesting heterogeneous effects by school types, project types, and time lengths of building occupancy. I further demonstrate that these results are not driven by student selection into newly built schools.

Research has consistently documented a positive relationship between educational attainment and economic benefits. Chapter 4, "*Returns to Education for the Self-Employed: The Income Under-Reporting Bias*", studies the long-researched question of measuring economic returns to education attainment, but focuses on a group that has often been overlooked—the self-employed. The existing literature offers mixed evidence on how the returns to education for the self-employed individuals compare with returns for those in paid employment. Moreover, self-employment income reported in survey data must be treated with caution. Self-employed workers tend to under-report their income to tax authorities and survey collectors. Ignoring this feature could potentially bias the estimates for the rates of returns to education. Chapter 4 aims to fill in this gap by taking an expenditure-based approach.

Chapter 4 examines how income under-reporting could lead to biases in estimating returns to education for the self-employed using their reported earnings. As the first step, I infer the true self-employment income following an expenditure-based approach. An average self-employed worker's reported earnings should be multiplied by a factor of 1.4 to reflect their actual earnings. More importantly, the degree of income under-reporting is non-linear across the income distribution. Lower-income self-employed households under-report more heavily. In the second step, I estimate the returns to education for self-employed workers using the inferred income data. I find income under-reporting leads to a severe upward bias in estimating the returns to education for the self-employed. Compared to employees, the self-employed extract lower returns from education.

A common theme of this thesis is the effort to find and utilise natural experiments that generate exogenous sources of variation in the explanatory variables that determine the treatment assignment of various "programmes"—the school quality that a house has access to, the participation in a school capital investment programme, or an individual's schooling choice. This corresponds to a common problem that underlines empirical research using observational data, that the assignment of treatment is often correlated with unobservable characteristics of participants, thus leading to an endogeneity problem. This endogeneity problem renders unreliable the findings drawn from pure correlations between the treatment and the outcomes. Without the understanding of the extent to which these correlations reflect a causal relationship, it is impossible to evaluate the effectiveness of those "programmes" and consequently make constructive policy recommendations. To this end, the common theme of this thesis is to find credible exogenous variations in various "programmes" of interest, and bridge the gap between observational correlations and causal relationships.

All three essays are analysed in the UK context. Cautions must be exercised in extrapolating the conclusions reached in this thesis to other economies, as the analyses carried out in these essays are tied with the institutional backgrounds. That said, the findings can provide input for education policies in developed countries which exhibit similar institutions of education provision and labour market structure to the UK.

Chapter 2

School Quality and House Prices: Evidence from a School Admission Policy Reform

2.1 Introduction

Parents have strong preferences for schools' academic performance (Hastings and Weinstein 2008; Burgess et al. 2014). These preferences are very likely reflected in their residential location decisions, given that state schools often allocate places based on prospective students' residential locations. It has become common practice for property websites to provide school quality information for a number of schools surrounding the house for sale. Under location-based admission criteria, an effective way of securing a place at a sought-after school is to buy a house near the school. According to a recent online survey[1] published by Nationwide, UK's largest building society, 23% of parents with children aged 5–16 years would be prepared to pay a 2–10% premium on a new home in order to be in the catchment area of a better state

[1] Article available at `http://www.nationwide.co.uk/mediacentre/pressreleases/viewarticle.htm?id=2254`, last accessed in October 2013.

school.

This chapter aims to quantify the house price premium associated with school performance using hedonic pricing method, a workhorse model for estimating economic values of local public services and amenities in housing markets. The theoretical foundation for this method is described in Rosen's (1974) seminal work on the demand and supply of differentiated products. In the Rosen model, composite goods are viewed as a bundle of attributes and characteristics, thus the consumer choice problem is essentially to choose the optimal bundle. In equilibrium, the marginal gains from better attributes and characteristics must offset the marginal loss from higher prices, *ceteris paribus*. Thus, regressing equilibrium prices on the attributes uncovers the marginal willingness to pay (MWTP) for each attribute of the composite good.

A growing literature has been focusing on this topic.[2] The principal concern in the empirical implementation is the omitted variable bias. A house is a composite good, compromising many housing characteristics, neighbourhood attributes and local amenities, which cannot be all observed by the researcher. High-performing schools tend to be located in good neighbourhoods, leading to a strong correlation between school quality and neighbourhood attributes. Due to this strong positive correlation, omitting unobservable (by the researcher) housing and neighbourhood characteristics would yield upward-biased estimates of the valuation of school quality.

Moreover, school performance may also be affected by the composition of the neighbourhood, *i.e.*, the characteristics of people who choose to live in a neighbourhood influence the characteristics of the neighbourhood and schools. In rich neighbourhoods, parents may pay higher taxes to contribute to the school expenditures, invest more in other ways in their children's human capital, and volunteer more often at school events. This leads to an

[2]For recent summaries of the literature, see Gibbons and Machin (2008); Black and Machin (2011); Machin (2011); Nguyen-Hoang and Yinger (2011).

endogeneity issue that will bias the estimates upward.

Earlier studies try to control for as many observable house price determinants as possible, or directly model the unobservable factors.[3] This requires either the availability of ideal data, or the imposition of hard-to-justify structural assumptions.

A more innovative and credible way to deal with unobservable housing and neighbourhood characteristics is the boundary discontinuity approach, pioneered by Black (1999). The idea of this approach is to compare houses within a close range of the boundary between neighbouring school attendance zones. Given the assumption that unobservable characteristics and amenities are smoothly distributed on both sides of the boundary, restricting the sample coverage to a close range of the boundary gives rise to trivial differences in those unobservable characteristics and amenities, but leave a distinctive difference in school quality since children on different sides of the boundary attend different schools. By differencing out the unobservable influences on both sides, this approach attributes the remaining differential in house prices to the differential in school quality and identifies the implicit price of school quality. This method has been refined and implemented for many other countries.[4]

The boundary discontinuity approach relies on the assumption of smooth distribution of unobserved housing characteristics and neighbourhood amenities on both sides of the boundary to eliminate the omitted variable bias. However, this may not be valid in some cases. Catchment boundaries are not usually drawn randomly; rather, they often collide with political jurisdictions, community boundaries or main roads, causing the housing and neighbourhood characteristics to change discretely at the boundary as well as

[3]See Black and Machin (2011) for a review of papers that follow these approaches.

[4]Examples include Davidoff and Leigh 2008, for Australia; Fack and Grenet 2010, for France; Gibbons and Machin 2003, 2006, Gibbons, Machin and Silva 2013 for the UK; and Machin and Salvanes 2010, for Norway.

school quality. For instance, Kane, Riegg and Staiger (2006) show that building quality and median income still change discretely even within 400 feet of the boundary in their sample. Even if the drawing of catchment boundaries are initially exogenous, Bayer, Ferreira and McMillan (2007) argue that the differential school quality on opposite sides of the boundary would lead to residential sorting, which would consequently create discontinuities in household characteristics on opposite sides of the same boundary.

Another strand of literature utilises education policy changes as quasi-experiments, and relates changes in house prices to variation in school quality induced by those policies. Some studies look at school finance reforms to examine the effect on house prices. For instance, Dee (2000) examines court-ordered education finance reforms in California that channelled educational resources available to poor communities, and finds that the median housing values and residential rents rose by at least 8 percent in the poorest school districts. Cellini, Ferreira and Rothstein (2010) exploit a regression discontinuity created by narrowly passed or failed school bond referenda in California. Their results imply that the marginal home buyers are willing to pay $1.50 or more for each $1 of school capital spending. Other studies turn to school choice reforms for exogenous variation in school quality. Reback (2005) examines Minnesota's inter-district open enrollment program, and finds that house prices increased in school districts that allow student to transfer to preferred districts, and decreased in districts that accept transfer students. Ries and Somerville (2010) utilizes changes in the catchment areas of public schools in Vancouver and employ repeat sales methods to control for time-invariant neighborhood effects. They find only the most expensive quartile of residences are influenced by changes in school quality. Machin and Salvanes (2010) consider a school choice reform in Oslo that replaced catchment zones with open enrolment. They find this weakening of the association between houses and schools led to a fall of over 50% in house price premium.

This chapter builds on and expands this quasi-experimental literature by seeking for an education policy reform that creates exogenous variation in school quality associated with houses. I explore a school admission reform that took place in the city of Brighton and Hove, England. The reform abolished the admission rule of allocating secondary school places according to applicants' home-to-school distances, and introduced catchment areas and random allocation into the school admission process. As the first city in the UK to abolish distance-based admission rules and introduce random allocation into the process of school admission, Brighton and Hove provides a rare opportunity for economists to explore a series of issues on education. It led to a drastic change in the probability of securing a neighbourhood school place for local children. Allen, Burgess and McKenna (2013), the first to analyse the early impact of this reform, examine the post-reform school composition changes and conclude that "there are clearly winners and losers from these reforms: some students are attending less academically successful secondary schools than they might have expected to; for others the reverse is true" (p. 16).

In this chapter I provide alternative evidence on house prices and school quality using quasi-random methods, in particular in the UK context. My primary approach relates the change in school admission probabilities to changes in house transaction prices. The contributions to the literature are threefold. First, instead of relying on cross-sectional variation of school quality at the school catchment boundary, I exploit an exogenous shock induced by a school admission policy reform. This has an advantage in alleviating the endogeneity problem. The variation in the school quality associated with a house is mainly driven by an unanticipated policy change, thus it is unlikely to be correlated with the unobservable housing characteristics and neighbourhood traits. Second, the analysis goes beyond a small boundary sample, thus capturing the effect of intra-district differences in school quality as well as inter-district differences. Third, I employ repeat sales data at the dwelling level to difference out all time-invariant housing characteristics and neighbourhood traits.

There are two primary findings from this analysis. First, I find that a one *school-level* standard deviation (SD) increase in academic performance raises house prices by 2–2.5% for non-flat properties. The magnitude of these estimates is in line with previous research. These estimates are also plausible when benchmarked against alternative schooling options. The house price premium associated with a one *student-level* SD increase in academic performance is equivalent to about 3.5 years of private school fees. Conversely, flats which are smaller in size and are less likely to accommodate school-age children, do not respond to school quality. This supports the notion that school quality is perceived and valued by parents and not other house buyers.

Second, contrary to the majority of existing literature, I show that parents value school value-added. An explanation for this inconsistency of findings is that I use well-established value-added measures that are readily available to parents, whereas much of the existing literature uses self-calculated value-added measures that parents do not have access to. A further finding is that parents value school value-added more than they do school intake. This suggests that parents place higher values on school outputs than school inputs.

The rest of this chapter is organised as follows. Section 2.2 presents some background details of the reform. Section 2.3 sets out the empirical strategy. Section 2.4 describes the data, and Section 2.5 presents the results and discusses the findings. Section 2.6 concludes.

2.2 The School Admission Reform

Brighton and Hove (BH) is a seaside city in south-east England with a population of 273,400 (UK 2011 census). The local authority (LA), Brighton & Hove City Council (BHCC), maintains 8 state-funded secondary schools[5], and manages the allocation of school places in these schools[6]. In terms of the percentage of pupils achieving 5 or more A*–C GCSEs (or equivalent) including English and Maths[7], a headline measure for England secondary school performance, the LA is below the national average in England by a small margin. Figure 2.1 shows the performance of BHCC maintained schools between academic years 2004/2005 and 2011/2012. Although there is year-to-year variation in the performance for each school, the relative ranking of the schools within the LA stays roughly unchanged. Of the 8 maintained schools, 3 are on or above national average, and the rest are generally below average. The high-performing schools are all located in the central area of the city (see Figure 2.2).

Prior to 2007, BHCC administered a similar admission process with other LAs in England. Parents were allowed to name three preferences in their application for a school place. When the number of applications exceeded the number of available places at a school, all the applications would be ranked on a number of priorities to decide which applicants get a place at the school.

[5]There were 9 including East Brighton College of Media Arts (known as COMART) which was announced to close in 2004 and was officially closed in August 2005, due to poor performance and extremely high truancy rate.

[6]Cardinal Newman Catholic School is an autonomous state-funded school. It manages its own admission based on the strength of commitment to its religious faith. This school did not take part in the admission reform, and therefore is excluded from the analysis.

[7]The national curriculum in England is organised into a few Key Stages (KS). The primary school phase is split into KS1 (covers ages 5–7) and KS2 (ages 7–11), and secondary school phase is split into KS3 (ages 11–14) and KS4 (ages 15–16). Pupils are formally assessed at the end of each KS. Most KS4 pupils work towards national qualifications—usually General Certificate of Secondary Education (GCSE) in a number of subjects. The GCSE examinations are usually taken in May at the end of KS4. The pass grades are A*, A, B, C, D, E, F and G from highest to lowest. In the National Qualifications Framework, a GCSE at grades D–G is a Level 1 qualification, while a GCSE at grades A*–C is a Level 2 qualification.

Applicants in lower priorities were offered places only when applications in higher priorities were fulfilled first. The first priority was given to looked-after children[8]. The second priority was given to children with special social and medical needs. The total number of applications under these two criteria were usually very small. Children with a sibling currently enrolled at the school had the third priority. The first three priorities usually took about half of the available spaces at the school. The rest of the places were allocated based on the distance between the school and the home addresses of the applicants under priority IV. A home-to-school distance tie-breaker would be introduced when a school is oversubscribed. Only children living within the tie-breaker distance could obtain a place at the school.

The School Admissions Code 2007 permitted school admission authorities to use random allocation as the oversubscription criterion for allocating places at popular schools. As a result, from the 2008/09 admission onwards, BH became the first city to see a lottery system come into effect for determining the allocation of places at its secondary schools. This reform involved changing the allocation rules under priority IV. The home-to-school distance rule was abolished and replaced by catchment areas and random allocation. The first three allocation priorities remained unchanged.

Table 2.1 summarizes the features of the reform. Under the new admission rules, the city is divided into 6 catchment areas, with 4 single-school catchment areas and 2 two-school ones (see Figure 2.2). When the first three priorities are fulfilled, the next available places are first allocated to children living within the catchment area. When the number of applications from the catchment area exceeds the number of available places, a lottery is introduced to randomly allocate the places. The random allocation process is carried out by a software which assigns a random number to each preference

[8]Look-after children are those taken into care by the local authority.

within an oversubscribed priority category and ranks them from the smallest to the largest. The catchment boundaries are not rigid, so cross-catchment-area applications are allowed. When there are still spaces available after applications from the catchment area are fulfilled, the available places are allocated among the applicants from outside the catchment area. A second lottery is introduced if the school is oversubscribed under this priority.

Table 2.1: Summary of the secondary school admission reform

	Pre-reform	Post-reform
Catchment area	No	Yes
Priority I	Children in care	Children in care
Priority II	Social and medical reasons	Social and medical reasons
Priority III	Sibling link	Sibling link
Priority IV	Home to school distance	Within catchment area, with a random allocation tie-breaker
Priority V	N/A	Outside catchment area, with a random allocation tie-breaker

This reform essentially changes the expected school quality that can be associated with the location of a property through the school place allocation mechanism. First, it creates a shift in the "local" school. Under the new catchment rules, it is now more probable for the children to attend the catchment school than the nearest school, if the two are different. Allen, Burgess and McKenna (2013) confirm that the introduction of catchment areas leads to distinct winners and losers in terms of the quality of school attended. They note that the children who live on the far east side of Varndean and Dorothy Stringer schools were not able to get into the two popular schools under the old allocation, but now gain substantially by residing in the Varndean/Dorothy Stringer catchment area. Those who live close enough to access Dorothy Stringe, on the other hand, suffer a loss of school quality because they now have the low-performing Patcham High designated as their catchment area school.

Second, even if the "local" school remains the same, the random allocation leads to a change in the probability of obtaining a place at the school. Figure 2.3 illustrates how the reform acts as a shock to the probability of children living at different distances getting admitted into an oversubscribed school. Children living close to a high-performing school who used to enjoy an almost guaranteed place in their neighbourhood school under the distance rule, now have to compete equally via random allocation with others who live further from the school but in the same catchment area. The houses close to the school, effectively suffer from a loss in the access to high-performing schools; whereas houses which are not close to the school but lie within the catchment area, are associated with a better chance of getting children into the school under the new allocation. Children living in a low-performing school catchment area, in principle, are now also able to get into a random draw into a high-performing school. However, since high-performing schools are always oversubscribed, there are often few or even no places left for children outside the catchment area. Since the random allocation tie-breaker only comes into force when a school is oversubscribed, it's expected that children living around high-performing schools will be more affected by the policy reform, and house prices may see different changes in terms of magnitude and directions depending on the proximity to a high-performing school.

2.3 Empirical Methodology

2.3.1 The Fixed Effects Estimator

Let p_{int} denote the log transaction price of house i located in a neighbourhood n at time t. A hedonic price function relating house prices and school quality can be expressed as follows:

$$p_{int} = X_{int}\alpha + \beta q_{it} + f_i + T_t + \varepsilon_{int}, \tag{2.1}$$

where X_{int} represents housing and neighbourhood characteristics, q_{it} represents the associated school quality, f_i and T_t represent the location and time fixed effects, and ε_{it} is the error term.

The main empirical challenge is that f_i is unobservable, and it is often correlated with q_{it}. Thus ignoring f_i results in a conventional omitted variable bias. As f_i and q_{it} are often positively correlated, omitting f_i leads to an overestimation of β.[9] The boundary discontinuity approach deals with this issue by comparing two groups of houses on the opposite sides of the catchment area boundary. Let i and i' denote these two groups, then the difference between these two groups yields:

$$\begin{aligned} p_{int} - p_{i'nt} = {} & (X_{int} - X_{i'nt})\alpha + \beta(q_{it} - q_{i't}) \\ & + (f_i - f_{i'}) + (T_t - T_t) + (\varepsilon_{int} - \varepsilon_{i'nt}). \end{aligned} \tag{2.2}$$

Given the assumption that unobservables are spatially smoothly distributed, $(f_i - f_{i'})$ is approximately zero within a close range of the boundary, therefore β is identified from non-zero variation in school quality $(q_{it} - q_{i't})$ due to the assignment of different schools in the two school attendance zones.

The major identification challenge facing the boundary discontinuity ap-

[9]Note f_i and X_{int} might also be correlated, thus estimates for α could also be biased.

proach is that residential stratification at the boundary. If heterogenous households sort into opposite sides of the boundary, $(f_i - f_{i'})$ may not approach zero at the boundary. Another concern is that $(q_{it} - q_{i't})$ may still be correlated with $(\varepsilon_{int} - \varepsilon_{i'nt})$, in the sense that the differences in neighbourhood traits lead to the differences in school quality. In addition, under this approach, the sample has to be restricted to fall into a close range of the catchment boundaries, to ensure that $(f_i - f_i')$ is close to zero. So this approach exploits the inter-district differences in school quality, but leaves out the intra-district differences (Reback 2005).

We turn to an alternative strategy to identify the causal effect of school quality on house prices. We address the potential bias from omitting unobservable characteristics by using repeat sales data. Instead of differencing out the unobservable housing characteristics and neighbourhood traits by comparing two houses i and i', we eliminate the time-invariant unobservables by comparing the same house at two time periods t and t'. This essentially gives a fixed-effects estimator that can be implemented with the following equation:

$$\begin{aligned} p_{int} - p_{int'} = (X_{int} - X_{int'})\alpha + \beta(q_{it} - q_{it'}) \\ + (f_i - f_i) + (T_t - T_{t'}) + (\varepsilon_{int} - \varepsilon_{int'}). \end{aligned} \tag{2.3}$$

In the equation above, the parameter of interest β, can still be biased if $(q_{it} - q_{it'})$ is correlated with $(\varepsilon_{int} - \varepsilon_{int'})$. It is essential to find exogenous variation in $(q_{it} - q_{it'})$ for the causal identification of β. I discuss in detail how to address this issue in the following subsection 2.3.2.

2.3.2 Matching Schools to Houses

We use two methods to introduce exogenous variation induced by the school admission reform into the term $(q_{it} - q_{it'})$. As a starting point, we focus on the most probable school that parents may choose. Under the pre-reform proximity rule, the nearest school to a house is the most likely school choice, taking into account admission probability and transportation costs. This choice is changed by the introduction of catchment areas under the reform. Under the new rules, the catchment school becomes the most probable choice. In other words, the reform changes the school that is mostly likely associated with a house (I refer to this as the local school hereafter), from the nearest school, to the catchment school. Thus, we incorporate this exogenous variation in school choice into the term $(q_{it'} - q_{it})$, as the identification source for house price premium β.

This might be an over-simplification of the situation. Unlike in the U.S. or other countries (see, for example, Black 1999; Fack and Grenet 2010), catchment areas in England are usually not rigid, and cross-boundary attendance is possible, which adds to the complexity of mapping schools to houses. Although not all pupils necessarily attend their nearest school (Burgess et al. 2006; Gibbons, Machin and Silva 2008), parents do have a preference for a school near to home, even when other choices are feasible (Burgess et al. 2014).

As a second method, we follow Gibbons and Machin (2006) and match a weighted average measure of school quality to houses. A child can apply up to three different schools both before and after the reform, and these can be any three schools within the city. Therefore, house prices do not only respond to the local school quality, but are also affected by the performance of other schools even if they are not the nearest ones. To address this issue, we introduce the changes in admission probabilities caused by the admission rule reform into the school quality term. Specifically, we construct an aggregated measure of school performance q_{it} as the following:

$$q_{it} = \frac{\sum_{j \in J} \left(\frac{Pr_{ijt}}{d_{ij}} \cdot s_{jt} \right)}{\sum_{j \in J} \left(\frac{Pr_{ijt}}{d_{ij}} \right)}. \tag{2.4}$$

Here the school quality q_{it} is the average of J schools' quality s_{jt}, weighted by the ratio of the expected probability of a child living at house i getting into school j at time t, Pr_{ijt}, to the home-to-school proximity, d_{ij}. The admission probabilities, Pr_{ijt}, are calculated using the previous year's numbers of applications and offered places for each school published by the LA in advance of the admission. In the empirical analysis we examine how house prices respond to the quality of the 3 nearest schools ($J = 3$) and all 8 LA-maintained schools ($J = 8$).

These two methods explore both time-series and cross-sectional variation in school quality. In time series, the reform induces a change in the access to schools in the admission process pre- and post- 2008/09 admission. Cross-sectionally, houses are affected by the reform in different ways and by different levels.

An additional concern is that the single-year school performance, s_{jt}, could be a noisy measure of school performance that parents perceive. It is likely that parents respond to the long-term school performance, instead of the school performance for a single year. To address this concern, we conduct a robustness check by replacing the current measure s_{jt} with the past three years' average performance $\bar{s}_{jt}$ in equation (2.4).

Another way of examining the relationship between house prices and school quality is to study how the reform changes the house price response to the nearest school. Given the pre-reform proximity-based allocation system for school places, houses close to an oversubscribed school enjoy a priority over houses further away, leading to higher prices within a certain close range of the school. The replacement of the proximity rule with random allocation, erodes the link between the price of a house and the performance of the nearest school, by assigning equal admission probabilities for all houses within (or outside) the newly-drawn catchment area.

As discussed in Section 2.2, houses near high-performing schools and those near low-performing ones receive different treatments in the reform. Houses close to high-performing schools which used to enjoy guaranteed admission now lose some chance of getting admitted, either because they are allocated to a catchment area with a different designated school, or because they have to compete with those live further away in the random allocation process. In contrast, houses near low-performing schools which used to have no chance of going to a popular school far away from them, now have access to better school quality, either because they are put in a catchment area with a better school, or because they now have the chance to enter the random allocation process of schools in other areas. Either way, the feasible school choice sets are changed by the reform. This erodes the link between house prices and the performance of their nearest schools. To test how the reform changes the valuation of the nearest school, we construct a difference-in-differences type model as follows:

$$p_{int} = X_{int}\alpha + \beta_1 post_{it} + \beta_2 q_{ict} + \beta_3 post_{it} \times q_{ict} + f_i + \varepsilon_{int}, \qquad (2.5)$$

where i indexes houses, n indexes neighbourhoods, t denotes time periods, p is the log house price, X represents the house characteristics, q_{ict} denotes the quality of the nearest school c, and $post$ is a dummy indicating the post-reform period. The coefficient of interest here is β_3. If parents value school quality, as the admission reform weakens the link between the house and nearest school, post-reform they should value the nearest school performance less compared to what they did pre-reform. Following this argument, β_3 is expected to show a negative sign.

2.3.3 School Inputs and Outputs

A further question of interest is what school characteristics parents value. The literature mostly focuses on school outputs such as test scores, with some exceptions that study student composition (Weimer and Wolkoff 2001; Clapp and Ross 2004), school expenditures (Dee 2000; Bradbury, Mayer and Case 2001; Downes and Zabel 2002), and school value-added (Kane, Riegg and Staiger 2006; Brasington and Haurin 2006; Gibbons, Machin and Silva 2013; Imberman and Lovenheim 2013). Whether parents are paying for the school outputs or inputs have different policy implications. The capitalisation of test scores sheds light on the pricing of academic standards, whereas the valuing of students backgrounds or school inputs is more relevant for school segregation policies (Gibbons, Machin and Silva 2013).

To distinguish between these two channels, we follow Gibbons, Machin and Silva (2013) to estimate the following equation:

$$p_{int} = X_{int}\alpha + \gamma_1 KS2_{it} + \gamma_2 VA_{it} + f_i + T_t + \varepsilon_{int}, \tag{2.6}$$

where $KS2_{it}$ is the mean KS2 test scores at the entry of secondary phase for the matched school(s) of house i, and VA_{it} is the value-added from KS2 to KS4[10]. Here $KS2_{it}$ serves as a proxy for students' background characteristics or school inputs, while VA_{it} is an indicator for the expected school outputs.

[10]See footnote 5.

2.4 Data

The house prices data are drawn from UK Land Registry Price Paid database for the period of January 2005 to April 2013. As an administrative dataset, it covers all the sold house transactions in England and Wales, and the transaction price information is more reliable and accurate than surveys. The Price Paid data contain information on the full address of the property, the price paid for the property, the date of transfer, the property type, whether the property is new build or not, and whether the property is freehold or leasehold.

School quality information is mainly drawn from public sources. The Department for Education (DfE) publishes school performance tables around the end of every year for the previous academic year.[11] I use the headline measure for secondary school performance in England, the percentage of pupils achieving 5 or more A*–C grades in GCSEs or equivalents including English and Maths, as the indicator for school performance. A school-level value-added measure is also provided in the performance tables. I also extract school-level KS2 attainment at the entry of the secondary school phase from the National Pupil Database (NPD), the administrative database for English pupils. The previous year's application and admission numbers that I use to calculate the admission probabilities for each school are published by BHCC every year.

A note here is how to map the period of school quality to the date of house sale. I use the time when the information becomes available as the matching reference. For the single year school quality measures, I match house transactions in a calendar year, say January to December 2008, to the school quality of the academic year 2006/07, which were made available to

[11] For instance, the results for academic year 2008/09 were published around the end of 2009 calendar year.

the public around December 2007. For multiple year averages, I match 2008 transactions to the average school quality for 2004/05–2006/07. Applications for secondary school admissions are usually made in October each year for the forthcoming academic year, and decisions are made around January the following year. For instance admissions for 2008/09 academic year were made in January 2008. At the time of application, parents will have known the admission and application numbers for each school for the last admission (academic year 2007/08) published by the LA. Thus I link the admission probabilities for academic year 2007/08 to house transactions in 2008 when calculating average school quality.

To include controls for time-varying neighbourhood characteristics, I match Indices of Deprivation 2007 and 2010 (ID2007 and 2010) to the house price data at the lower layer super output area (LSOA) level[12]. These matched neighbourhood characteristics include indicators for crime rates, accessibility of local services and outdoor living environment. Crime indicator is a combined single score that measures the rate of recorded crime in an area for four major crime types (violence, burglary, theft, and criminal damage). Measure of accessability to local service consists of population weighted average road distances to a primary school, to a food store, to a post office, and to a GP surgery. Outdoor living environment include indicators for air quality and road accidents. All neighbourhood variables are standardised to facilitate comparison over time. Table 2.2 summarizes the data pre- and post-reform.

[12]Super Output Areas (SOAs) are a set of geographical areas developed for census statistics. Lower Layer Super Output Areas (LSOAs) typically have an average of roughly 1,500 residents and 650 households.

2.5 Results

2.5.1 The House Price Premium

Table 2.3 presents the results for the house price premium estimates for the local school, that is, the nearest school under home-to-school proximity rule and the catchment school under the new allocation rules. The school quality measure here is the percentage of pupils achieving 5 or more A*–C grades in GCSE and equivalent tests including English and Maths. To aid with interpretation, the school quality measures in this and the following tables are standardised using sample means and standard deviations.

Columns (1)–(3) report the OLS estimates which do not control for dwelling fixed effects. The correlations between house prices and the local school performance are strong and significant. The point estimates indicate that a one school-level SD increase in the local school quality is associated with an around 5% premium on house prices. This association is robust to the inclusion of a series of controls including housing characteristics, neighbourhood traits, and time trends. In column (1), the housing characteristics mostly show significant coefficients of expected signs. Detached houses are more expensive than the omitted terraced houses, flats are less expensive, while semi-detached houses cost about the same as terraced houses on average. New build properties and freehold tenure are also associated with higher prices. The inclusion of year-month dummies and observable neighbourhood traits in columns (2) and (3) only change the estimated house price premium associated with better local school quality by very little .

Columns (4)–(6) report the fixed-effects (FE) estimates for equation (2.3). The point estimate on local school quality falls to 2.7% once the dwelling fixed effects are controlled for in column (4). Since our estimator mainly explores the time-series variation in house prices, the 2008 financial crisis is likely to be a major confounding factor. As the financial crisis is probably a common macroeconomic shock to all houses, this can be easily dealt with by controlling for the common trends of the housing market. Column (5) includes controls for the year-month dummies, which further brings down the house price premium to 1.0% for one school-level SD increase in the local school quality. Column (6) further controls for observable neighbourhood traits. These neighbourhood attributes are mostly insignificant, except crime rates. The inclusion of these controls only changes the coefficient on school quality by very little. This lends support to the idea that the school quality variation is independent from neighbourhood attributes, and suggests that we can disentangle the WTP for school quality from the WTP for neighbourhood attributes.

Compared to the OLS results, the FE estimates are largely reduced. The associated house price premium for one school-level SD increase in school quality falls from around 5% to 1%. In monetary terms, this is equivalent to a change from £11,500 to £2,300 pounds in 2007 terms. This drop in the magnitude of the estimated school quality effects shows evidence that omitted unobservable housing characteristics and local amenities impart a large upward bias.

Similar evidence is found using alternative measures of school quality. Table 2.4 report the results for the weighted average school quality measures for the nearest 3 schools (in panel A) and for all 8 LA schools (in panel A). The FE estimates are typically a fraction of the OLS estimates. The preferred estimates in Table 2.4 column (6) using weighted average school quality are only slightly different from the results in Table 2.3 column (6) using the single local school quality. This supports the robustness of our results.

2.5.2 The Role of the Nearest School

An implication of the reform is that the nearest school carries less weight in parents' school choice set due to either the reassignment of local schools or the introduction of random allocations. Next we test the hypothesis that the nearest school is valued less after reform. We expect the relationship between house prices and the nearest school quality to be weakened by the reform. Table 2.5 summarizes the estimation results of equation (2.5). The results are broken down by whether the nearest school is a high-performing (odd columns) or a low-performing (even columns) one.

Several features can be noted from the results. First, the positive and significant coefficients on school quality suggests the pre-reform relationship between house prices and nearest school quality is very strong. Moreover, it gets stronger when the distances are shorter. This is consistent with the pre-reform allocation rule based on the home-to-school proximity.

Second, the negative coefficients on the interaction terms indicate that the house price premium associated with the nearest school reduces substantially after the reform, especially for those houses close to high-performing schools. This suggests that the replacement of proximity admission criterion by catchment areas and random allocation makes parents value their nearest school less, since they no longer have the guarantee of a secured place at that school by residing within a short distance. Those who live close to a low-performing school also observe a reduction in house price response to their nearest school quality. This may reflect the fact that the reform brings more alternative school choices to them. They now have some probability of entering a high-performing school through being included in a catchment area with a better designated school, or have a larger choice set as they can access farther schools which would not have been feasible under the proximity rule. This makes their nearest low-performing school less important in their school choice.

Third, a comparison of the results between houses close to high- and low-performing schools suggest that those who live close to high-performing schools value the school quality even more. They place a higher value on school quality pre-reform, and the reduction in their valuation post-reform is also larger.

2.5.3 Who Value School Quality?

In this section, we examine the potential heterogeneity in the school quality effect by property types. We add three interaction term of property types and school quality into equation (2.3) to examine whether flats and non-flat properties respond to school quality in different ways. Table 2.6 summarises the regression results. Columns (1)–(3) use the three alternative school quality measures for the local school, the nearest 3 schools, and all 8 LA schools respectively. The coefficients on the school quality measures estimate the effect of a one school-level SD increase in school quality on the prices of terraced houses. These estimates are very consistent across the three school quality measures, ranging from 2.1% to 2.5%, more than twice the size of the average effect found in column (6) of Tables 2.3 and 2.4. They are also statistically significant at 1% level. The coefficients on the interaction term of flat and school quality, measure the differences in the school quality effect between flats and terraced houses. These differences are highly significant at 1% level. More interestingly, the school quality effect on flats essentially amounts to close to zero. A distinctive feature of the BH housing market is that flats constitute a much larger proportion of the properties than the rest of England. In our sample, flats contribute to 54% of the transactions (see Table 2.2), while the national average for the same sample period is 20%. This means that the school quality change only effectively affect the prices of less than half of properties in our sample.

The finding that flats do not respond to the changes in school quality provides evidence that it is indeed the parents that value school quality and pay the price premium. Flats are generally smaller in size than non-flat properties. The average number of bedrooms for flats in Brighton is 1.7, whereas that figure for non-flat houses is 3.2.[13] Thus flats are far less likely to accommodate families with school-age children than houses.

A further concern is that this heterogeneity in school quality effect could be due to residential sorting. It's possible that households with higher incomes place more value on school quality. Flats are on average 30% less expensive than terraced houses in the sample. If house prices are any indication for family income, households living in terraced houses might have higher preferences for school quality and consequently value school quality more than those living in flats. However, this explanation is unlikely in our case. Detached houses are on average 23% more expensive than terraced houses. Following the same logic of heterogenous preferences, we would expect the school quality effect to be significantly larger than that on terraced houses. This is not supported by the small and insignificant coefficients on the interaction term of detached houses and school quality.

The results reported here are consistent with the existing literature in finding that school quality is capitalised in house prices. A one school-level SD increase in school quality raises the prices for non-flat properties by 2–2.5%. The magnitude of the estimates for the effect of school quality is in line with previous research (see Black and Machin 2011, for a summary of findings).

These estimates can be benchmarked against private school fees. The largest private school in BH is among the top 1% schools in England. For the period of 2008/09–2012/13, above 99% of its KS4 students achieve 5 or more

[13] Author's calculation using data from www.zoopla.co.uk, a major property website in the UK.

A*-C grades at GCSEs including English and Maths. This equals a 4.8 school-level SD increase from the mean performance of state schools in BH. If the house price and school quality relationship is taken linearly, such a substantial increase in school quality would raise non-flat house price by 10%–12%, or £30,000–36,000 in 2007 monetary terms. This is equivalent to slightly over 2 years of tuition fees of this private school[14]. This top school may be an extreme example. Benchmarked to the average private schools, the house price premium associated with a one *student-level* SD increase in KS4 performance is equivalent to about 3.5 years of private school fees.[15] These calculations suggest our estimates are plausible. The house price premia for state schools alone are not enough to drive parents to opt for private education.

2.5.4 What do Parents Value?

Next we examine what school characteristics parents value. Table 2.7 presents the estimation results for equation (2.6). OLS results in columns (1)–(3) show that the point estimates on KS2 test scores at the entry of the secondary school phase are highly significant and larger, while the coefficients on school value-added are less significant and smaller. However, the FE estimates in columns (4)–(6) exhibit quite the opposite: the estimates on school value-added are statistically significant and mostly larger, while the coefficients on KS2 attainment are less significant and smaller. The effect of a one SD change in school value-added on house prices ranges from 0.6% to 0.8%.

These results suggest that parents care more about school effectiveness than school intake. This finding is robust to different mappings of schools to

[14]The 2014/15 tuition fees for non-boarding pupils at this private school is £93,570, covering the ages 11-16. Discounted to 2007, this is about £75,000, giving an average of £15,000 per annum.

[15]A one *student-level* SD increase in KS4 performance, equivalent to 4.5 *school-level* SDs, will raise non-flat house prices by roughly 10%, or about £30,000 in 2007 monetary terms. Termly tuition fees for non-boarding pupils are £2,800 in England for 2006–2007 (Gibbons, Machin and Silva 2013).

houses. Next in Section 2.5.5 we will further demonstrate that these results are robust to alternative samples and school quality measures.

This finding regarding whether parents value school value-added is similar to Gibbons, Machin and Silva's (2013) results in the sense that school value-added is capitalised into house prices. This is different from much of the previous literature on house prices and value-added, which generally finds no effects. A reconciliation for this mixed evidence may be that parents only value some school quality of which they can access and perceive its information. Studies that find little support for the value-added model are either using self-calculated value-added measure (Dills 2004; Downes and Zabel 2002; Brasington and Haurin 2006) which is not available to parents, or information that has only been available for a short period of time (Imberman and Lovenheim 2013). From this point of view, it's likely that parents do not respond to these information because they haven't (fully) perceived it. On the other hand, like Gibbons, Machin and Silva (2013), we use well-established measures of value-added that has been published by the government for a long period, and find supporting evidence that parents values these value-added measures. Further support for this idea can be drawn from Figlio and Lucas's (2004) study, which finds that there is an independent house price effect of school grade ratings, above and beyond the effects of test scores and the other components of the school grades. As the grades are mostly some functions of test scores and other public information, this suggests that parents value new information.

That said, unlike Gibbons, Machin and Silva (2013), we find that parents' valuations for school value-added and school inputs are not similar. From this finding, we draw a different conclusion that parents place more value on academic effectiveness than school composition. A possible explanation for this difference is that this chapter focuses on the secondary school phase, while Gibbons, Machin and Silva (2013) look at the primary school phase.

2.5.5 Robustness Checks

In this section, we conduct two sets of robustness checks to gauge the strength of previous findings.

The first robustness check reruns the analysis on a balanced subsample, that is, a sample of houses with at least one pre-reform sale and post-reform sale. This is motivated by the concern that the results could be driven by some changes in school quality that is not induced by the reform. Table 2.8 compares the results from the full sample and the balanced sample. Columns (1)–(3) repeat the previous results from the full sample, and columns (4)–(6) report new results from the balanced sample. Estimates for the school performance models are organised in panel A. The two sets of results exhibit virtually no change. Both samples yield a finding of around 2% house price premium for a one SD increase in school performance, and close to zero premium on flats. Panel B present the estimates for the value-added models. Both samples yield results supporting the notion that parents value the school value-added more than they do the school inputs. A slight difference is that the results are stronger from the balanced sample. This lends further support to the identification strategy that exploits the variation in school quality induced by the reform.

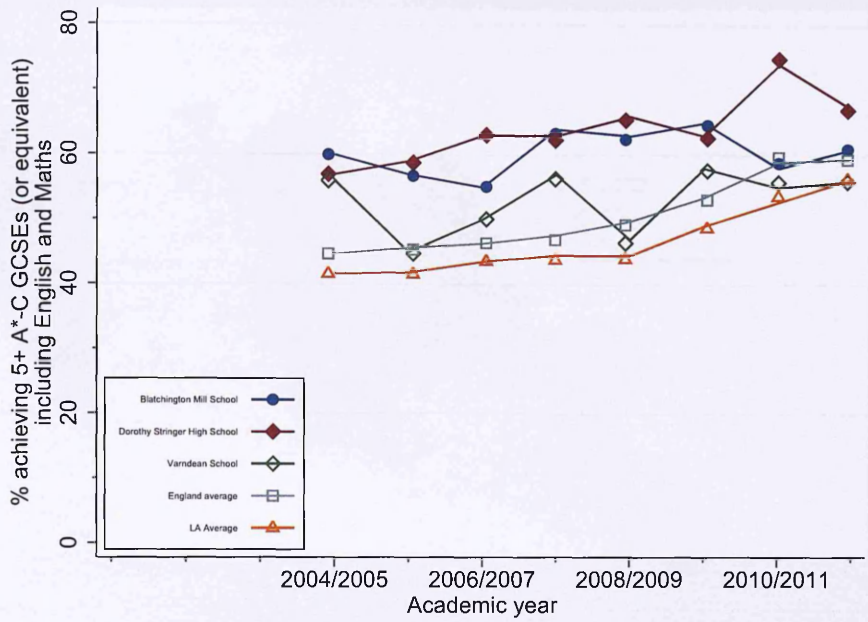
The second robustness check assesses that the results are not driven by some noisy measurement of school quality. In previous results, the school quality measures are taken from the previous year of the house sale. These single year indicators could be noisy measures of the perceived school quality by the parents. We rerun the analysis using the past three years' average as the alternative school quality measures. These new results are presented in columns (4)–(6) of Table 2.9. This change doesn't appear to matter for the school performance results in panel A. In panel B, columns (4)-(6) using the three-year average value-added and KS2 measures find larger estimates on value-added and smaller estimates on KS2 attainment at the entry of the secondary school phase, compared to previous results repeated in columns (1)-(3) that use single year measures. Therefore, this strengthens the finding that parents place higher values on school value-added than on school inputs.

2.6 Conclusion

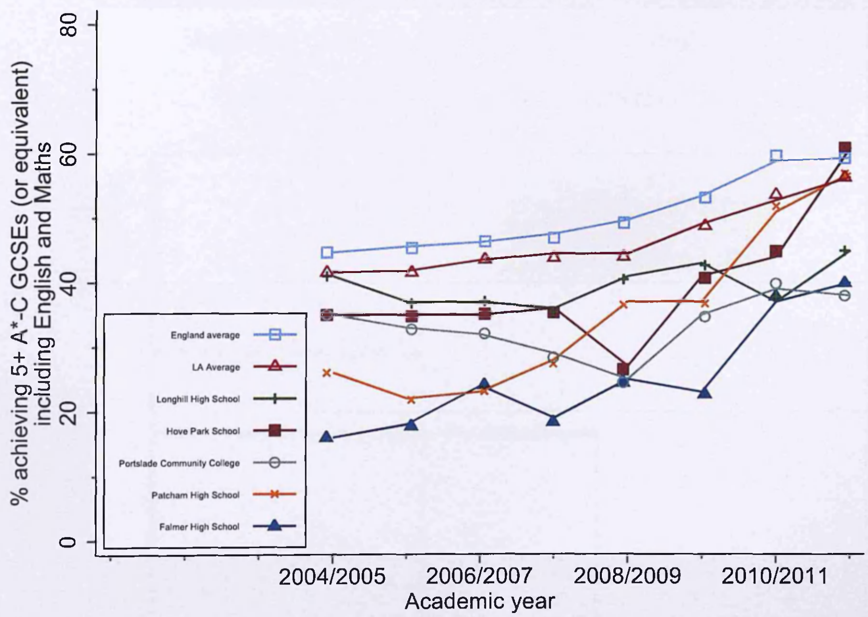
In this chapter I study the relationship between house prices and school quality by following an alternative approach to the popular boundary discontinuity design. I exploit the exogenous variation in school quality induced by a school admission reform to identify the causal effect of school quality on house prices. I use repeat sales data to eliminate all time-invariant house characteristics and neighbourhood attributes. The estimates show that a one school-level SD increase in school quality raises the house prices by 2–2.5% for non-flat properties. In monetary terms, this yields a house price premium of £6,000–7,500 in 2007 prices. Benchmarked against alternative schooling options, the house price premium associated with a one student-level SD increase in academic performance is equivalent to about 3.5 years of private school fees. In contrast, there is virtually no effect on flats. This implies that it is indeed parents that value school quality, as flats are generally too small in size to house families with school-age children. These results are robust across a number of school performance measures and samples.

One of the objectives of the reform was to promote equal educational opportunities by breaking the "selection by mortgage" in school admissions. On this front this policy may have achieved limited success. While the house price premium for the nearest school is reduced, house prices now respond to other schools that parents have access to.

A further look at what school characteristics matter reveals that parents place higher values on school outputs than on school intake. This result is different from much of the existing literature which finds little support for the notion that school value-added affects house prices. A reconciliation for this contradiction is that parents value school information that is available to them. Much of the existing literature uses self-calculated value-added measure that parents do not have access to. This finding suggests that parents place higher values on schools outputs than school intake. A policy implication from this is that investments in technologies and interventions that can boost academic effectiveness are likely to be met with high demand.



(a) High-performing schools



(b) Low-performing schools

Figure 2.1: School performance: % achieving 5+ A*-C GCSEs (or equivalent) including English and Maths

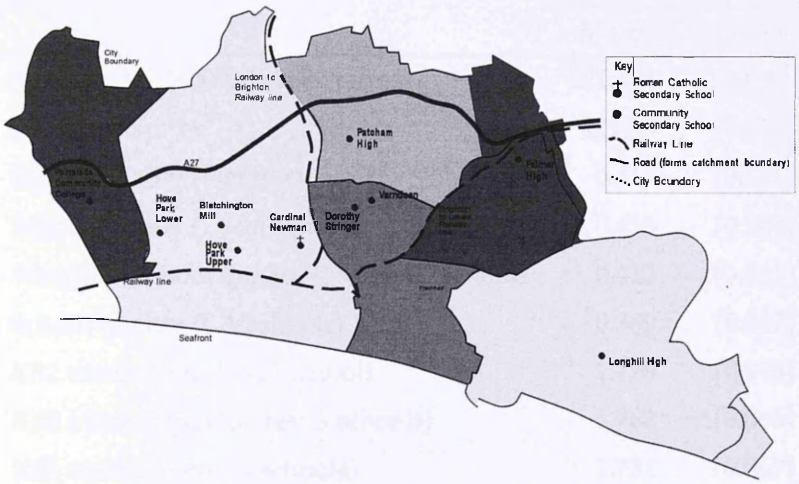


Figure 2.2: School catchment area map

Source: Brighton and Hove City Council

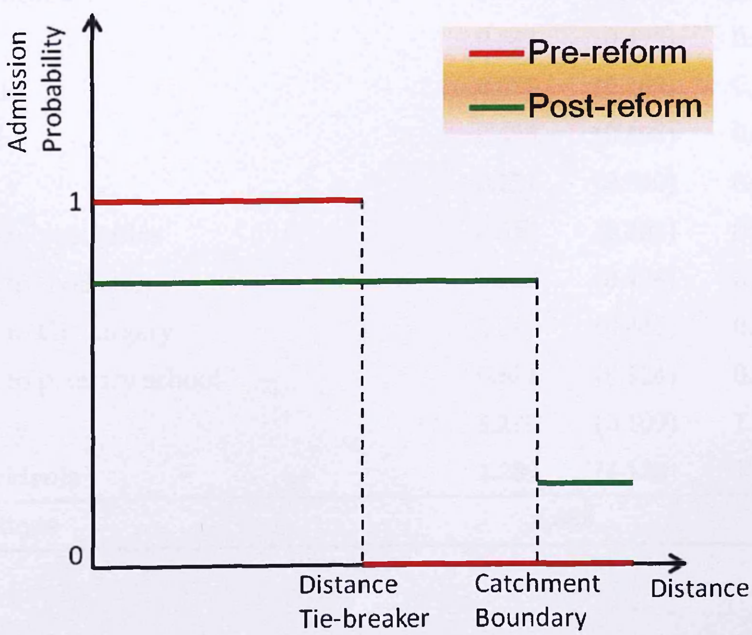


Figure 2.3: The admission probability changes caused by the reform

Table 2.2: Summary statistics

	Pre-reform		Post-reform	
	Mean	Std. Dev.	Mean	Std. Dev.
House price (£000, in 2007 terms)	221,124	83,893	235,317	90,211
Log house price	12.243	(0.350)	12.302	(0.360)
School quality (Nearest)	0.417	(0.111)	0.467	(0.137)
School quality (Local school)	0.418	(0.109)	0.488	(0.100)
Weighted school quality (Nearest 3 schools)	0.412	(0.061)	0.449	(0.104)
School quality (LA schools)	0.369	(0.047)	0.415	(0.091)
KS2 attainment (Local school)	1.779	(0.098)	1.771	(0.086)
KS2 attainment (Nearest 3 schools)	1.762	(0.066)	1.738	(0.082)
KS2 attainment (LA schools)	1.732	(0.057)	1.711	(0.063)
Value-added (Local school)	0.982	(0.018)	0.996	(0.014)
Value-added (Nearest 3 schools)	0.984	(0.010)	0.993	(0.013)
Value-added (LA schools)	0.980	(0.007)	0.992	(0.011)
Terraced	0.251	(0.434)	0.258	(0.438)
Detached	0.059	(0.236)	0.064	(0.245)
Semi-detached	0.147	(0.354)	0.139	(0.346)
Flat	0.543	(0.498)	0.539	(0.499)
New build	0.037	(0.188)	0.009	(0.093)
Freehold	0.455	(0.498)	0.457	(0.498)
Crime	0.251	(0.540)	0.029	(0.638)
Distance to post office	0.650	(0.303)	0.687	(0.306)
Distance to food store	0.668	(0.428)	0.642	(0.422)
Distance to GP surgery	0.695	(0.446)	0.725	(0.496)
Distance to primary school	0.674	(0.324)	0.686	(0.324)
Air quality	1.229	(0.109)	1.036	(0.151)
Road accidents	1.289	(1.128)	1.431	(0.930)
Observations	4,663		8,307	

Table 2.3: Hedonic regressions of house prices on the local school quality

	OLS			Fixed Effects		
	(1)	(2)	(3)	(4)	(5)	(6)
School quality	0.047***	0.051***	0.048***	0.027***	0.010***	0.009***
(Local school)	(0.003)	(0.003)	(0.010)	(0.002)	(0.002)	(0.003)
Detached	0.216***	0.220***	0.182***			
	(0.013)	(0.012)	(0.037)			
Semi-detached	−0.001	0.001	0.014			
	(0.009)	(0.009)	(0.023)			
Flat	−0.058**	−0.065**	−0.090**			
	(0.026)	(0.025)	(0.036)			
New build	0.152***	0.164***	0.161***	−0.022	0.020	0.020
	(0.026)	(0.026)	(0.046)	(0.016)	(0.016)	(0.018)
Freehold	0.313***	0.310***	0.330***	0.273***	0.264***	0.263***
	(0.026)	(0.025)	(0.037)	(0.087)	(0.088)	(0.084)
Crime			−0.003			−0.028**
			(0.023)			(0.014)
Distance to post office			−0.029			−0.019
			(0.038)			(0.017)
Distance to food store			−0.201***			−0.006
			(0.030)			(0.015)
Distance to GP surgery			0.017			0.013
			(0.028)			(0.011)
Distance to primary school			0.199***			−0.035
			(0.051)			(0.034)
Air quality			−0.049			0.009
			(0.132)			(0.057)
Road accidents			−0.000			−0.006
			(0.014)			(0.005)
Time FE	No	Yes	Yes	No	Yes	Yes
Dwelling FE	No	No	No	Yes	Yes	Yes
Cluster level	Dwelling	Dwelling	LSOA	Dwelling	Dwelling	LSOA
Observations	13,236	13,236	13,236	13,236	13,236	13,236

Notes The local school refers to the nearest school for the pre-reform period, and the catchment school for the post-reform period. School quality is measured by the proportion of pupils achieving 5 or more A*-C grades at GCSEs or equivalents including English and Maths. Terraced house is the omitted dwelling type. Time fixed effects include all year-month dummies for the sample period. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively. Cluster-robust standard errors at the dwelling or LSOA levels are in parentheses.

Table 2.4: Hedonic regressions of house prices on school quality using weighted school quality measures

	OLS			Fixed Effects		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
School quality	0.033***	0.059***	0.050***	−0.003	0.007***	0.007**
(Nearest three)	(0.003)	(0.003)	(0.011)	(0.002)	(0.003)	(0.003)
Panel B						
School quality	0.021***	0.064***	0.059***	−0.005***	0.007**	0.006
(LA schools)	(0.003)	(0.004)	(0.013)	(0.002)	(0.003)	(0.004)
House characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Neighbourhood traits	No	No	Yes	No	No	Yes
Time FE	No	Yes	Yes	No	Yes	Yes
Dwelling FE	No	No	No	Yes	Yes	Yes
Cluster level	Dwelling	Dwelling	LSOA	Dwelling	Dwelling	LSOA
Observations	13,236	13,236	13,236	13,236	13,236	13,236

Notes School quality is the average performance of nearest three schools (in Panel *A*) or all LA schools (in Panel *B*) using the product of previous year's admission probability and inverse home-to-school distance as weights. School performance is measured by the proportion of pupils achieving 5 or more A*-C grades at GCSEs including English and Maths. Terraced house is the omitted dwelling type. Time fixed effects include all year-month dummies for the sample period. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively. Cluster-robust standard errors at the dwelling or LSOA levels are in parentheses.

Table 2.5: Pre- and post-reform house price premia for the nearest school's performance

	All distances		Distance ≤ 2km		Distance ≤ 1km	
	Close to a high-performing school	Close to a low-performing school	Close to a high-performing school	Close to a low-performing school	Close to a high-performing school	Close to a low-performing school
	(1)	(2)	(3)	(4)	(5)	(6)
Post-reform	0.199***	0.100***	0.194***	0.116***	0.232***	0.128***
	(0.012)	(0.016)	(0.015)	(0.019)	(0.024)	(0.031)
School quality (Nearest)	0.095***	0.065***	0.093***	0.045***	0.110***	0.045
	(0.012)	(0.015)	(0.015)	(0.015)	(0.023)	(0.030)
Post × School quality	−0.106***	−0.040**	−0.109***	−0.022	−0.147***	−0.015
	(0.012)	(0.015)	(0.017)	(0.016)	(0.017)	(0.027)
Observations	5,812	7,424	3,160	4,818	1,132	1,638

Notes All specifications control for housing characteristics, neighbourhood traits, annual trends and month dummies, and dwelling fixed effects. School quality is measured by the proportion of pupils achieving 5 or more A*-C grades at GCSEs including English and Maths of the nearest school to the house. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively. Cluster-robust standard errors at the LSOA level are in parentheses.

Table 2.6: House price premium across property types

	(1)	(2)	(3)
School quality (Local school)	0.025*** (0.005)		
School quality (Nearest three)		0.021*** (0.005)	
School quality (LA schools)			0.023*** (0.006)
Detached× School quality	−0.005 (0.011)	−0.007 (0.013)	−0.001 (0.012)
Semi-detached× School quality	−0.005 (0.009)	0.005 (0.008)	0.001 (0.008)
Flat× School quality	−0.025*** (0.006)	−0.026*** (0.005)	−0.028*** (0.005)
Observations	13,236	13,236	13,236

Notes All specifications control for housing characteristics, neighbourhood traits, year-month dummies, and dwelling fixed effects. The local school quality refers to the performance of the nearest school for the pre-reform period, and the catchment school for the post-reform period. The weighted school quality refers to the average performance of nearest three schools (in row 2) or all LA schools (in row 3) using the product of previous year's admission probability and inverse home-to-school distance as weights. School performance is measured by the proportion of pupils achieving 5 or more A*-C grades at GCSEs including English and Maths. Terraced house is the omitted dwelling type. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively. Cluster-robust standard errors at the LSOA levels are in parentheses.

Table 2.7: House price premium for school inputs and outputs

	OLS			Fixed Effects		
	(1)	(2)	(3)	(4)	(5)	(6)
KS2 attainment	0.075***			0.006		
(Local school)	(0.010)			(0.005)		
Value-added	0.009			0.006*		
(Local school)	(0.014)			(0.004)		
KS2 attainment		0.070***			0.005	
(Nearest three)		(0.012)			(0.004)	
Value-added		0.032**			0.007**	
(Nearest three)		(0.013)			(0.003)	
KS2 attainment			0.064***			0.006
(LA schools)			(0.013)			(0.004)
Value-added			0.041***			0.008**
(LA schools)			(0.014)			(0.003)
Flat ×	−0.071***	−0.068***	−0.072***	−0.005	−0.006	−0.008*
KS2	(0.014)	(0.014)	(0.012)	(0.005)	(0.005)	(0.004)
Flat ×	−0.029**	−0.041***	−0.036***	−0.001	−0.003	−0.007*
Value-added	(0.012)	(0.012)	(0.010)	(0.004)	(0.004)	(0.004)
N	11,384	11,384	11,384	11,384	11,384	11,384

Notes All specifications control for housing characteristics, neighbourhood traits, year-month dummies, and dwelling fixed effects. The local school (for rows 1 and 2) refers to the nearest school for the pre-reform period, and the catchment school for the post-reform period. KS2 attainment is the average prior KS2 test scores for the GCSE students at the school level. VA refers to the KS2–KS4 value-added at the school level. The weighted school quality measures refer to the average quality of nearest three schools (in rows 3 and 4) or all LA schools (in rows 5 and 6) using the product of previous year's admission probability and inverse home-to-school distance as weights. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively. Cluster-robust standard errors at the LSOA levels are in parentheses.

Table 2.8: Results for the full and balanced samples

	Full Sample			Balanced Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
School quality (Local school)	0.023*** (0.004)			0.025*** (0.005)		
School quality (Nearest three)		0.022*** (0.005)			0.021*** (0.005)	
School quality (LA schools)			0.023*** (0.005)			0.024*** (0.006)
Non-flat × School quality	−0.023*** (0.005)	−0.026*** (0.005)	−0.028*** (0.004)	−0.026*** (0.005)	−0.027*** (0.005)	−0.027*** (0.005)
Observations	13,236	13,236	13,236	8,397	8,397	8,397
Panel B						
KS2 attainment (Local school)	0.006 (0.005)			0.007 (0.004)		
Value-added (Local school)	0.006* (0.004)			0.011*** (0.004)		
KS2 attainment (Nearest three)		0.005 (0.004)			0.005 (0.004)	
Value-added (Nearest three)		0.007** (0.003)			0.009** (0.004)	
KS2 attainment (LA schools)			0.006 (0.004)			0.008* (0.004)
Value-added (LA schools)			0.008** (0.003)			0.010*** (0.004)
Non-flat × KS2	−0.005 (0.005)	−0.006 (0.005)	−0.008* (0.004)	−0.003 (0.006)	−0.006 (0.006)	−0.008 (0.005)
Non-flat × Value-added	−0.001 (0.004)	−0.003 (0.004)	−0.007* (0.004)	−0.010** (0.004)	−0.012*** (0.004)	−0.014*** (0.004)
Observations	11,384	11,384	11,384	7,219	7,219	7,219

Notes Full sample includes all houses with at least two sales during the period January 2005–April 2013. Balanced sample includes houses with at least one sale pre-reform and at least one repeat sale post-reform. All specifications control for housing characteristics, neighbourhood traits, year-month dummies, and dwelling fixed effects. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively. Cluster-robust standard errors at the dwelling or LSOA levels are in parentheses.

Table 2.9: Results for previous year's and past three years' average school quality

	Single Year			Three Year Average		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
School quality (Local school)	0.023***			0.025***		
	(0.004)			(0.005)		
School quality (Nearest three)		0.022***			0.021***	
		(0.005)			(0.005)	
School quality (LA schools)			0.023***			0.024***
			(0.005)			(0.005)
Non-flat × School quality	−0.023***	−0.026***	−0.028***	−0.026***	−0.029***	−0.031***
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
Observations	13,236	13,236	13,236	13,236	13,236	13,236
Panel B						
KS2 attainment (Local school)	0.006			0.002		
	(0.005)			(0.005)		
Value-added (Local school)	0.006*			0.015***		
	(0.004)			(0.004)		
KS2 attainment (Nearest three)		0.005			−0.001	
		(0.004)			(0.005)	
Value-added (Nearest three)		0.007**			0.016***	
		(0.003)			(0.005)	
KS2 attainment (LA schools)			0.006			0.004
			(0.004)			(0.004)
Value-added (LA schools)			0.008**			0.012**
			(0.003)			(0.005)
Flat × KS2	−0.005	−0.006	−0.008*	−0.002	−0.002	−0.004
	(0.005)	(0.005)	(0.004)	(0.006)	(0.006)	(0.005)
Flat × Value-added	−0.001	−0.003	−0.007*	−0.015***	−0.020***	−0.020***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
Observations	11,384	11,384	11,384	11,384	11,384	11,384

Notes Columns (1)–(3) use school quality measures for the previous year. Columns (4)–(6) use the average school quality measures for the past three years. All specifications control for housing characteristics, neighbourhood traits, year-month dummies, and dwelling fixed effects. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively. Cluster-robust standard errors at the dwelling or LSOA levels are in parentheses.

Chapter 3

Better Buildings, Better Scores? The Short-Run Effect of a Large School Construction Programme

3.1 Introduction

Do students do better academically when placed in well-equipped classrooms at modern design schools? The answer to this question is of legitimate policy concern to the government and the public, particularly in the context where public capital expenditure on education has seen a rapid increase until recently. In England, capital spending on school buildings increased considerably by 12.9% every year in real terms from 1997/98 to 2008/2009, while current spending increased at a 5.0% annual rate.[16] In spite of this dramatic growth, the returns in academic attainment to these large capital investments remain a mystery.

The fastest-growing area in educational capital spending in England is the investment on secondary school estate through a programme named Building Schools for the Future (BSF). Launched in 2003, BSF aims to rebuild or

[16]Source: DCSF (2009), Table 8.5.

renovate all England's 3,500 secondary schools by 2020. The whole programme is due to cost £55 billion, making it the biggest single capital investment programme in 50 years. Due to its large scale, the programme is split into 15 waves, prioritising the most deprived and low-performing schools and areas.

This chapter evaluates the short-run effect of BSF on student test scores. The main objective of this chapter is to provide causal evidence on the effectiveness of school capital investment on student academic achievement. While recognising that the treatment is not randomly assigned across all schools, I adopt a difference-in-differences (DiD) empirical strategy that takes advantage of the phasing design of BSF, by comparing changes in student test scores for the schools where the policy intervention took place earlier, to changes for the schools which were rebuilt later. Though schools in the first and last waves may be systematically different, I argue that schools in earlier few waves are similar in characteristics and performance, thus more likely to follow similar trends over time. With the availability of multiple periods of pre-treatment data, I am able to assess this identification assumption by running placebo tests to investigate whether there is any significant difference between the test score trends for the treatment and control groups.

The most significant finding is that BSF produces large test score gains for disadvantaged pupils. Looking by pupil background, I find strong evidence that academically and socio-economically disadvantaged students enjoy large and positive gains, but their more advantaged counterparts do not. This leads to a positive effect on the average test scores for *all* treated pupils, but this *overall* effect is not significantly from zero. I also find evidence suggesting heterogeneous effects by school types, exposure to the treatment, and project types. Autonomous academy schools constructed under BSF have larger effects compared to non-academy BSF schools. Longer exposure to the treatment appears to generate larger effects, particularly in the third year after treatment, but these effects are not statistically significant. Different levels of renewal to the school also generate heterogeneous test score gains. However, somewhat surprisingly, minor refurbishment works and information and communications technology (ICT) installation seem to produce higher test score gains than more costly new build projects. This might reflect the fact that the more extensive new build projects lead to more disruptions to student learning during the construction. These results are not driven by selection of students into BSF schools.

The remainder of this chapter proceeds as follows. Section 3.2 relates this chapter to the existing literature. Section 3.3 provides details on the background of the programme, followed by a description of how it enables the empirical methodology outlined in Section 3.4. Next, Section 3.5 describes the data and assesses the trends in test scores for various subgroups. Section 3.6 presents and discusses the results. Section 3.7 checks the robustness of the empirical findings. Section 3.8 concludes.

3.2 Related Literature

The Coleman Report (Coleman et al. 1966) spawned the debate on the effectiveness of input-based schooling policies. It reports that school resources have little effect on pupil achievement once student background and socioeconomic status are controlled for. Since then, a large body of research has been carried out to establish the association between school inputs and educational outcomes (see Hanushek 2003 and Krueger 2003 for recent surveys and debate). Various school inputs such as class size reduction[17], pupil-

[17] Angrist and Lavy (1999); Krueger and Whitmore (2001); Fredriksson, Öckert and Oosterbeek (2012).

teacher ratio[18], and teacher quality[19], *etc.*, have been explored to assess their impact on students' educational attainment, and sometimes to a further extent, their labour market performance after schooling is completed[20]. However, with the sizable literature on school resources still growing, evidence is scarce on the effect of capital investment in school facilities.

Outside the economics discipline, a literature has been seeking to explore how and why school facilities might affect learning outcomes. The first channel is a direct effect on learning effectiveness from better physical attributes. Facilities such as air conditioning, heating, fluorescent lighting reduce the distractions from poor conditions and provide a more enjoyable learning environment. From the perspective of educational research, Schneider (2002) reviews studies on physical facility attributes of indoor air quality, ventilation, and thermal comfort, lighting, acoustics, and building age and quality. He concludes that these physical attributes obviously bear on students' ability to perform. Second, there may also exist a psychological effect where better conditions make pupils feel more valued and more motivated to learn (Woolner et al. 2007). Third, ICT equipment as a complement (or sometimes substitute) to traditional teaching, might make pupils more interested in studying. Lastly, the effect can operate through teaching. Better facilities may attract better teachers and boost their morale (Buckley, Schneider and Shang 2004; Schneider 2003), therefore adding more values to education production. While it sheds light on the possible mechanisms through which school capital inputs might affect educational outcomes, a downside of this literature is that a causal link is often missing. As school capital investments are usually not randomly allocated, finding causal evidence from observational data is often difficult.

[18]Examples include Case and Deaton (1999); Dearden, Ferri and Meghir (2002).

[19]See Hanushek and Rivkin (2006) for a review.

[20]See, for instance, Card and Krueger (1992); Duflo (2004); Chetty et al. (2011); Fredriksson, Öckert and Oosterbeek (2012).

A small literature in economics has been trying to fill this gap. Jones and Zimmer (2001) are among the first to directly assess the impact of school capital inputs on academic achievement. Without an accurate measure of school capital stock, they use the school district's bond indebtedness as a proxy, and employ a series of determinants of debt to instrument for it. They report a large positive association between bond indebtedness and academic achievement; one standard deviation (SD) increase in per-student debt is associated with 0.1–0.2 SD increase in test scores[21]. However, it may be argued that the debt determinants could be confounded with other factors at the district which also affect educational outcomes.

Duflo (2001) studies the effect of a major Indonesian primary school construction programme in the 1970s, one of the largest of its kind on record, on educational attainment and wages. Within a difference-in-differences framework, she examines the educational outcomes for pupils born in different years (which determines whether a pupil was exposed to the programme) in different regions (which determines whether the exposure to the programme was high or low), and finds that each primary school constructed per 1,000 children led to an average increase of 0.12–0.19 years of schooling. She further estimates the labour market consequences of the programme, and finds a 1.5–2.7% increase in wages for the treated. Duflo's study may be more focused on education provision than school buildings, since the Indonesian programme was mainly providing new schools instead of replacing old ones, in a country with relatively low human capital development. It's unlikely these results could be extrapolated to developed countries, where human capital development is high and attending schools are compulsory for children. In the context of developed countries, the more relevant issue is to replace or renovate deteriorating buildings and facilities with new construc-

[21]Calculated from their reported results in Tables 3 and 4 (pp.583–584).

tion and modern equipment.

Two recently studies by Cellini, Ferreira and Rothstein (2010) and Neilson and Zimmerman (2014) exploit more credibly exogenous variation in school capital spending. Cellini, Ferreira and Rothstein (2010) examine a school infrastructure investment program in California. They adopt a dynamic regression discontinuity approach to study the effect of school capital projects funded by local bonds, by comparing districts where school bond referenda passed or failed by narrow margins. They find a large and persistent effect on home prices, but weak evidence on test score gains. Test score effects are small and insignificant for the first several years after bond passage, peak during the sixth year at around one sixth of a school-level deviation and become marginally significant, and fall back to zero thereafter.

Due to the nature of regression discontinuity design, the effect Cellini, Ferreira and Rothstein (2010) find is very local in the whole distribution of school capital investment. Neilson and Zimmerman (2014) study a bigger (but lower) part of the capital investment distribution by exploring a school construction programme in a poor U.S. district, which was implemented in a wide span of 16 years. They use a DiD strategy to exploit the variation in occupancy dates of new school buildings. They also use panel data to address the possible issue of student selection into newly-built schools. Their results show large effects on both home prices and reading scores. Home prices increased by 10% due to school construction, and reading scores rose by 0.15 SD after six years of occupancy, but math scores did not exhibit similar effects. There are two missing features from their study. First, although they can observe different levels of capital investment, they do not distinguish the potential heterogeneous effects of these project types. Second, they do not investigate whether the school construction affects different socio-economic subgroups in different ways.

Besides these papers that examine general school capital investments, a few studies have specifically focused on the effect of school technology investment on education outcomes. The evidence is mixed. Angrist and Lavy (2002) follow an instrumental variable approach to exploit exogenous variation in computer use at schools generated by priorities in a funding programme in Israel. Computer use in receipt schools saw a substantial increase, but no or even a negative association with achievement was found. Goolsbee and Guryan (2006) take advantage of a regression discontinuity in ICT investments created by a government subsidy in California by comparing schools which receive and miss the funding cutoff points, they do not find a significant effect. Machin, McNally and Silva (2007) devise an instrumental variable which indexes gaining or losing from a policy change in ICT funding rules, and find a positive and significant effect for English and Science at the end of primary school, but not for Maths. As BSF also includes ICT installation, this chapter also relates to these studies in assessing the effect of ICT investments on test scores.

This chapter provides new evidence on the effectiveness of school capital investment in raising academic attainment. It moves the literature forward in a number of ways. The first contribution is that it studies a programme that enables a simple and effective identification strategy to deal with selection issues and elicit the causal effect of school capital investment on student test scores. Under the BSF programme, clear-cut prioritisation rules are set to determine the wave in which schools enter the programme. Such phasing design makes the empirical strategy feasible in controlling for the selection into treatment. The prioritisation rules imply that schools placed in neighbouring waves have similar pre-treatment characteristics and performance, thus are more likely to follow common trends.

Secondly, it adds to the literature by exploring different pupil backgrounds and treatment intensity to investigate potential heterogeneous treatment effects of school construction. It examines which groups of students benefit more from the policy intervention. As input-based policy interventions often target disadvantaged groups, the findings are useful for assessing the costs and benefits of such policies. Moreover, the dataset has an advantage in that it provides information on the levels of school renewal, which makes it possible to assess how the effects vary with treatment intensity. Additionally, this chapter also provides some dynamics in how the treatment effects emerge over time.

Thirdly, as a national programme, the scale of BSF is larger than that of city- or state-wide programmes studied in previous literature. This large scale of the programme makes the findings more representative.

3.3 The BSF programme

BSF was launched in 2003 by the then UK Labour government to support teaching and learning by providing schools with modern buildings and facilities. It aimed to rebuild or refurbish all of England's 3,500 state secondary schools by 2020, with an estimated cost of £55 billion. This would it make the single largest capital investment programme in the UK for 50 years.

In a sequence of calls for proposals, Local Education Authorities (LEAs) were invited to submit expressions of interest for inclusion in the programme. Each project usually consisted a group of schools which were geographically coherent to the LEA. The proposals were assessed by the central government education department[22], and accepted ones were placed in a sequence of waves to be carried out in sequence. The first six waves were announced

[22]Department for Education and Skills (DfES) during 2001–2007, later known as Department for Children, Schools and Families (DCSF) between 2007 and 2010, and subsequently Department for Education (DfE) from 2010 onwards.

in 2004, covering over 600 schools managed by 50 LEAs. By 2009, waves 1-15 had been announced, involving two thirds of all LEAs in England. In addition, a number of LEAs originally placed in waves 7–15 were awarded funding for a one-school pathfinder project.

The order in which school projects entered the programme was prioritised on the basis of two core criteria: social deprivation and educational need. Deprivation is measured by the percentage of pupils eligible for a free school meal (FSM)[23]. Educational need is measured by the percentage of pupils achieving 5 or more A*-C grades[24] in GCSE exams including English and Maths, a headline indicator for school performance. The two criteria are assigned equal weights and summed up to produce a final ranking score.[25] Areas and schools with higher deprivation and lower performance therefore obtained a higher score and entered the programme first.

The project implementation proved the programme too ambitious. At the time of announcement, the government expected 200 schools would be open by December 2008 (DfES 2004). The actual delivery fell behind initially planned progress. By 2010, approximately £10 billion had been spent (see Figure 3.1), but only 175 schools had their construction projects completed. Following a spending review in 2010, BSF was scrapped by the new coalition government. Planned projects for over 700 schools were cancelled, while another 600 schools which had already received capital allocation and signed procurement contracts were allowed to go ahead.

As of July 2010, construction had been completed in 175 secondary schools, of which 137 are mainstream schools[26]. Table 3.1a lists the the progress of BSF

[23]For waves 7–15, this measure is changed to Tax Credit Indicator (TCI) index.

[24]There are eight pass grades, in descending order: A*, A, B, C, D, E, F and G.

[25]To take into account different spreads of the two criteria, the two percentages are standardised using national SD. GCSE score is subtracted from 100 to achieve an under-achievement figure. Specifically, the final ranking score is calculated as: $\frac{\text{\% FSM}}{\text{national SD for FSM}} + (100 - \frac{\text{\% 5+ A*-C grades in GCSE}}{\text{national SD for \% 5+ A*-C grades in GCSE}})$.

[26]Others include special schools which provide special education, and Pupil Referral Units

till 2009/10 by year and wave. The majority of the completed schools are from Waves 1–3 and one-school pathfinder projects to be constructed around a similar time with Waves 1–3.

Schools received different levels of renewal under BSF. There are four types of construction projects: new build, which involved completely rebuilding existing schools, or making new provisions where there were no existing schools; refurbishment of existing schools; a combination of new build and refurbishment; and information and communication technology (ICT) installation only. Table 3.1b details the numbers of schools constructed by 2010 under BSF by project type. Around half of the schools (67 out of 137) were constructed as new build projects, while the other half received ICT installation only (21 schools), refurbishment (22 schools), or a mix of new build and refurbishment (27 schools).

The amount of capital investment is substantially different among the four project types. ICT installation projects are allocated funding of £1.7 thousand per pupil.[27] Building project (also including ICT) costs range from £2.5 to £31 thousand per pupil, with an average of £17 thousand per pupil[28]. The equivalents to over three years of current spending. Academies constructed under BSF cost less due to reduction in building area and specification. The average is £10 thousand per pupil.

3.4 Empirical Methodology

This section outlines the empirical framework that is used to estimate the effect of BSF on student test scores. Let T_{ist} denote the academic achievement

(PRU), which provide education students who are excluded from or unable to attend mainstream schools.

[27] Source: Partnerships for Schools (PfS).

[28] Source: UK Parliament House of Commons Hansard, HC Deb 7 Mar 2012 vol 541 cc799-800W. Available at `http://www.publications.parliament.uk/pa/cm201212/cmhansrd/cm120307/text/120307w0003.htm`, last accessed Janurary 2014.

of student i in school s at year t. We start with an OLS estimator based on the following equation:

$$T_{ist} = \lambda_t + \beta BSF_{st} + X_{ist}\gamma + T_{is,t-5}\delta_1 + T_{is,t-2}\delta_2 + \varepsilon_{ist}, \tag{3.1}$$

where BSF_{st} is a dummy variable that takes the value of 1 if school s has finished BSF construction by year t, and 0 otherwise. Controls include observable student characteristics X_{ist} and the time effects λ_t. ε_{ist} represents the error term. The parameter of interest, β, measures how BSF affects the academic achievement of the treated students.

We include the term $T_{ij,t-5}$, KS2 attainment at the end of the primary school phase, and $T_{ij,t-2}$, KS3 attainment during the secondary school phase, in the current education production. This is motivated by the fact that we do not observe historical inputs and ability endowment for the students, yet they might still have an impact on current attainment. The prior attainment acts as a proxy for historical inputs and endowed ability in the education production function. This value-added model has been widely adopted for assessing teaching quality, although recent literature has questioned the validity of the geometric decay assumption (Todd and Wolpin 2003, 2007; Rothstein 2010). On the other hand, there is also research that finds value-added models exhibit little forecast bias in the evaluation of teacher quality (Chetty, Friedman and Rockoff 2014). We employ this value-added specification to mitigate the lack of historical input data and to control for heterogeneity in student background.

The OLS estimate of β is unbiased if BSF_{st} is randomly assigned. This assumption is unrealistic since BSF clearly selects more deprived and low-performing schools for earlier intervention. To control for this selection, we include the school fixed effects α_s to construct a DiD estimator as set up in the following equation:

$$T_{ist} = \lambda_t + \alpha_s + \beta BSF_{st} + X_{ist}\gamma + T_{is,t-5}\delta_1 + T_{is,t-2}\delta_2 + \varepsilon_{ist}. \qquad (3.2)$$

The key identification assumption for the DiD estimation strategy is the common trend assumption, *i.e.*, the time paths of academic achievement, λ_t, would not differ systematically for students in earlier BSF schools and those in later BSF schools, had the intervention not taken place. Under this assumption, β retrieves the average treatment effect of BSF programme for the treated.

We are concerned that equation (3.2) may not adequately control for confounding factors that are entangled with BSF. A threat to the correct identification of β is policy interventions that took place at the same time as BSF. Two programmes are worth noting. The first is that some disadvantaged schools convert to academies as part of BSF delivery. Academies are self-governing independent schools that are funded directly by the central government rather than a local authority. Literature has documented that converting to academies improves the school's academic performance (Machin and Vernoit 2011; Machin and Silva 2013). If the conversion is positively correlated with BSF, our estimates could be biased upwards. Fortunately this confounding factor is observable, so we can directly control for it. The second is the Devolved Formula Capital (DFC) programme, which allocates capital income to schools purely on a formulaic basis. The key factor that determines the DFC allocation is the number of pupils. As the school size is relatively stable over time, this can be controlled for in the school fixed effect, so that the effect of BSF capital investments can be isolated from that of devolved capital investments.

In addition to the two confounding programmes mentioned above, we also control for per-pupil current expenditure for the school, out of the concern that the funding authorities might substitute between current and capital funding. If current spending is effective in raising academic achievement, and the balancing consideration in allocating current and capital funds exists, then failing to control for current spending will bias the estimates of BSF effects downwards.

Adding controls for confounding programmes and spending discussed above at the school level, S_{is}, leads to the following specification:

$$T_{ist} = \lambda_t + \alpha_s + \beta BSF_{st} + X_{ist}\gamma + T_{is,t-5}\delta_1 + T_{is,t-2}\delta_2 + S_{is}\zeta + \varepsilon_{ist}. \tag{3.3}$$

In addition, to allow for potential heterogeneous effect for academies, we also estimate a variation of equation (3.3) which includes a dummy for academy conversion $academy_{st}$ and an interaction term $BSF_{st} \times academy_{st}$.

The common trend identification assumption is not formally testable, but with the availability of multiple periods of pre-treatment data, we are able to assess whether the two groups follow a similar trend before the intervention took place. In order to analyse the pre-treatment trends, we augment equation (3.3) by including p leads and q lags following Autor (2003):

$$\begin{aligned} T_{ist} = \lambda_t + \alpha_s + \sum_{\tau=-p}^{-1} \beta_\tau BSF_{s\tau} + \sum_{\tau=0}^{q} \beta_\tau BSF_{s\tau} \\ + X_{ist}\gamma + T_{is,t-5}\delta_1 + T_{is,t-2}\delta_2 + S_{is}\zeta + \varepsilon_{ist}, \end{aligned} \tag{3.4}$$

where $BSF_{s\tau}$ equals 1 if current period is year τ relative to intervention, and 0 otherwise. The completion year is normalised to year 0.

This has a few advantages. First, including leads helps assess whether the pre-treatment trends are similar for both groups. This acts as a placebo test. Each lead indicator can be thought of as a placebo treatment. If the common-trend identification assumption holds, we should expect to find no effect of these placebo treatments.

Second, if it takes time for the effect to fully emerge, including lags enables us to observe the dynamics of the treatment effect. Because construction completion dates are different across treated schools, one could argue that treatment intensity is not the same across all treated students at a given time t, as the time lengths of the exposure to new buildings are different. Including lags circumvents this problem, since the dose of exposure is allowed to differ within the treated group. If the effect lasts, it can be expected that students exposed to longer period of treatment experience larger effects.[29]

Third, anticipatory behaviour could pose a threat to identification. Students in anticipation of new school buildings and facilities may be motivated to exert more effort to studies. Parents expecting new school capital investment might substitute it for less family inputs on their children's education. These anticipatory changes in inputs from the student and their parents may bias the estimates as the common-trend assumption will no longer be valid. This anticipatory effect can be examined by testing whether lead indicators are significant.

There are reasons to suspect students with different backgrounds derive heterogeneous benefits from the programme. One one hand, academically and socioeconomically disadvantaged children have fewer resources available from other sources, therefore additional school inputs may have a larger effect on them. On the other hand, advantaged children may have higher ability in utilizing available resources, so the same programme may be more effective among them. Literature offers mixed evidence on this aspect. For instance, Krueger's (1999) experimental estimates from the STAR experiment find larger effects of class size reduction on minority and free lunch students. Using the same experiment but different statistical procedures, Ding

[29]On the other hand, it could also be argued that school infrastructure wears down over time so later cohorts derive less benefit from it compared to earlier cohorts. But this case is unlikely as in this analysis the post-treatment period is short.

and Lehrer (2011) reject the evidence for larger effects on socioeconomically disadvantaged children, and find larger cognitive gains for academically advantaged children. Heterogeneity in treatment effects of BSF is of particular policy relevance, since the programme prioritises socioeconomically and academically disadvantaged schools and areas. To explore potential heterogeneous treatment effects at the individual level, we estimate the following regression equation

$$\begin{aligned} T_{ist} = \lambda_t + \alpha_s + \sum_{g} \beta^{g} BSF_{st} \times G_{ist} + X_{ist}\gamma \\ + T_{is,t-5}\delta_1 + T_{is,t-2}\delta_2 + S_{is}\zeta + \varepsilon_{ist}, \end{aligned} \tag{3.5}$$

where G_{ist} is a list of indicators for pupil academic and socioeconomic background drawn from X_{ist} , and β^{g} captures the BSF effect for subgroup g.

Lastly, to have a closer look at the effects of different project types, we expand the empirical model (3.3) by allowing for heterogeneous effect across various project types:

$$\begin{aligned} T_{ist} = \lambda_t + \alpha_s + \sum_{m} \beta^{m} BSF^{m}_{st} + X_{ist}\gamma \\ + T_{is,t-5}\delta_1 + T_{is,t-2}\delta_2 + S_{is}\zeta + \varepsilon_{ist}, \end{aligned} \tag{3.6}$$

where m indicates one of the four types of projects: new build, refurbishment, a mixture of new build and refurbishment, and ICT only.

One limitation of this study is that it only focuses on the short-run effect. As the data only cover the period up to 2009/10, it's only possible to examine the short-run effect of BSF, with the exposure to new infrastructure up to three years. This inevitably loses out some interesting dynamics in student enrollment and academic achievement.

On the other hand, focusing on the short run makes it easier to distinguish between two channels through which capital investment might raise academic performance. It's possible that there is a direct effect of capital investment on student achievement; it's also possible that the new faculties attract better students thus improving the school's performance. The second channel will likely change the school choice equilibrium in the local area. These changes could potentially give rise to different dynamics for the treated group and control group, which would cast doubt on the validity of the common trend assumption. In this case β captures an amalgamation of the direct effect of BSF on test scores and an indirect effect through the intake of better students. It would then be difficult to distinguish between the two effects if the treated schools and control schools compete for the same pool of candidates. But since our analysis is restricted to the short run, the students whose test scores we can observe at the end of secondary school are not new students admitted after the intervention, but enrolled students who have been admitted prior to the treatment.

A further possibility is that students select into BSF schools in advance based on the belief of future treatment. In this case, restricting the analysis to the short run does not separate the direct effect from the student intake effect. To examine this possibility, we provide robustness checks on cohort composition changes to investigate whether the student intake has changed before and after the BSF programme.

3.5 Data

3.5.1 Data Sources

The delivery agent of BSF, Partnerships for Schools (PfS), published the list of schools that had been constructed under the BSF programme by July 2010. The list also provides information on what type the project was, and the date when new buildings and facilities were open to students. I match this list to two external sources which provide more information at the school level. To find out the full history of the sampled schools for the analysis period 2003/04–2009/2010, I link the list to EduBase, which records all current educational establishments in England and Wales and their predecessor schools. Next I match the list to school revenue balances data, which record the annual balances of current spending account for all state-maintained schools in England, excluding academies. Each school's per-pupil current spending is calculated using the current account balance and its proportion in the total current income.

The microdata in this analysis are administrative data drawn from the National Pupil Database (NPD). The NPD keeps records of all pupils in state schools of England. The key source of the data is a school census conducted each year since 2002 (every term from 2006 onwards), providing information on school and pupil characteristics. At the pupil level, the NPD provides socio-economic background information including gender, ethnicity, having English as an additional language (EAL), special education needs (SEN), and eligibility for free school meal (FSM). The pupil-level record can be linked to the student's attainment at former Key Stages (KS)[30]. The outcome of interest in this analysis is the academic attainment at age 16, by which time secondary schools students take GCSE exams or equivalent tests in a number of subjects. I use the average GCSE and equivalents test score as the outcome for academic attainment. Details of summary statistics of the variables are available in Table 3.2.

[30]The compulsory state education system in England and Wales is divided into four stages: primary education consists of KS1 (school years 1–2 for ages 5–7) and KS2 (school years 3–6 for ages 7–11), and secondary education is split into KS3 (school years 7–9 for ages 11–14) and KS4 (school years 10–11 for ages 14–16).

3.5.2 Trends in Academic Achievement

This subsection presents some graphical evidence at aggregate levels. Figure 3.2 plots the trends of the average test scores for BSF schools and all England schools. The solid line represents the achievement for the BSF schools that had completed construction by 2009/10, the dashed line for the BSF schools that were still in progress by 2009/10, and the dotted line for the non-BSF state secondary schools in England. The "+" sign indicates the year when pupils started to benefit from BSF buildings and facilities. A few observations can be made from this graph. First, average test scores had been rising steadily during the period of analysis. The figures show that average test scores trend upwards both before and after first occupancy. This highlights the necessity of controlling for time effects in the analysis, and shows that failing to do so would bias the results upwards. Next, it is also noticeable that there's a large performance gap between BSF schools and non-BSF schools, and BSF schools fall behind non-BSF schools. This confirms the BSF prioritisation criteria which selected schools in greater social and educational need for early treatment. Third, the performance gap is narrowing over time. It starts at about a quarter of a school-level SD[31], and gradually narrows to about 0.17 SD. This suggests that non-BSF schools are not a suitable control group for the DiD analysis as the two groups seem to follow different time trends. Finally, it is worth noting that completed BSF schools follow ongoing BSF schools more closely than they do the non-BSF schools. This suggests ongoing BSF schools provide a better control group than non-BSF schools.

Figures 3.3 plots the average test score by wave. Waves 7–15 (short-dashed line) have no school construction completed under BSF during the sample period. One-school pathfinders (dash-dotted line) and Waves 4–6

[31]The national school-level standard SD of average GCSE and equivalent test scores in England for the sample period is 11.04.

(dashed line) have their first schools opened in 2008/09 and 2009/10, so they are treated with a smaller dose in terms of time length. They appear to follow similar attainment trends with non-treated Waves 7–15 throughout the sample period both pre- and post-treatment. Waves 1–3 are treated for a longer period, with the first school opened in 2006/07. During the pre-treatment period, Waves 1–3 and Waves 7–15 circle around a similar upward trend; but during the post-treatment period, Waves 1–3 seem to perform consistently higher than Waves 7–15. This suggests that a positive effect is likely to be found for Waves 1–3.

As we will explore the heterogeneity of BSF effects by academy status and project type, it is important to ensure that the consequent treatment subgroups exhibit similar pre-treatment trends with the control group as well. Figures 3.4 and 3.5 further break down the treated schools by academy conversion status and project type. Figure 3.4 illustrates that BSF hardly changes the performance of non-academy schools, but creates a break in trends for academies. For non-academies, treated and non-treated groups are hardly distinguishable in terms of performance. For academies, BSF schools fall behind non-BSF schools in a parallel fashion, and seem to catch up post-treatment. Data in Figure 3.5 are noisier, and breaks in trends are less visible.

Figures above exhibit very different patterns, but a fairly common feature is that the treatment group and control group share similar pre-treatment trends. This offers confidence in the key identification assumption that the treatment group and control group experience common time effects over the sample period.

3.6 Results

3.6.1 Baseline Results

Table 3.3 reports the baseline estimates of the impact of BSF on student average score in GCSE and equivalent tests. Columns (1)–(3) present the results for the OLS estimator from equation (3.1), which does not control for school fixed effects and simply compares the students from treated schools with those from non-treated schools; columns (4)–(6) report the results of the DiD estimator from equations (3.2) and (3.3), taking into account the pre-treatment differences between schools. Standard errors in all specifications allow for arbitrary correlation within the same school. This is motivated by a concern raised by Bertrand, Duflo and Mullainathan (2004) that serial correlation within the same group will produce incorrect standard errors and cause overrejection of the null hypothesis of no effect in DiD estimations.

Column (1) only controls for pupil characteristics, including number of GCSE and equivalent test entries, gender, IDACI score of the pupil's home area, having SEN or not, EAL, FSM eligibility, race, and prior attainment on English, Maths, and Science at KS3 and KS2. It shows that BSF treatment is associated with a large and significant increase of 2.19 points in the KS4 average test score, or 0.20 SD. Students who enter more GCSE and equivalent tests tend to do better, possibly because they are more motivated and exert more effort on studies; the negative quadratic term shows the rate of increase diminishes with the number of test entries. Conditional on other factors, girls perform better than boys. Students coming from a poor family background, proxied by the IDACI[32] score of their living area, having SEN and eligibility for FSM, suffer a disadvantage compared to their counterparts with better

[32] Income Deprivation Affecting Children Index (IDACI) is an index of deprivation, which measures the proportion of children under the age of 16 that live in low income households in a local area.

socioeconomic background. Ethnic and language minorities seem to do better in our sample. Students who speak English as an additional language at home score higher than native speakers. Broken down the scores by race, the results reveal that all other ethnicity groups perform better than the omitted white British group.

Column (2) further controls for prior attainment at KS3 and KS2. We notice that previous test scores strongly predict current pupil performance, and the coefficients on some family background proxy variables such as IDACI, SEN, and FSM are reduced, but still remain highly significant. As the inclusion of previous test scores has already incorporated the effects of historical family inputs on current attainment, any residual effect of the family background variables must be contemporaneous effect. In connection with the discussion in Todd and Wolpin (2003), this result suggests that measures of contemporaneous family inputs should not be left out from the value-added specification. The change in point estimates on BSF is very small. This suggests not taking into account historical inputs might not bias the estimates on current inputs by much. The precision of the estimates on BSF and covariates improves. Thus we control for prior attainment throughout the following specifications.

Column (3) then controls for year fixed effects on top of pupil characteristics and prior attainment. Due to an upward trend over the sample period, the BSF coefficient becomes significantly lower. The point estimate is reduced to 0.30, only a slight proportion of the previous estimates, and becomes only borderline significant at 10% level. This implies that if one simply compares the post- and pre-treatment academic attainment for the students in treated schools, the estimate of the BSF effects will be largely biased upwards, because the treated schools are performing better over time even without the BSF intervention.

The next three columns report the DiD estimates, adding controls for school fixed effects. Column (4) corresponds to the results for equation (3.2). Comparing columns (3) and (4) reveals two observations. First, the point estimate on BSF now becomes statistically insignificant. Second, this estimate only changes by a small amount. This suggests that within the sample, the selection into treatment is not severe. The point estimate reveals that the average treatment effect of studying in a BSF school is 0.22 point scores, or 0.02 SD.

The last two columns deal with policy interventions that took place around similar time as BSF, as laid out in equation (3.3). Column (5) controls for academy conversion, and allows for heterogeneous effects for BSF academies. In the absence of BSF, academy conversion gives rise to a significant improvement in pupil performance. This finding is consistent with Machin and Vernoit (2011). On top of this, BSF has a significantly larger effect on academies relative to non-academy schools. The difference is 1.53 points, or 0.15 SD. Despite this large difference, the estimates on BSF change very little between Columns (4) and (5), mainly because academies only take up a small proportion of the sample.

Column (6) adds controls for per-pupil current spending for the three recent years. This applies to non-academy schools only, as current spending data on academies are not available for this period. The BSF effect is comparable to those obtained in columns (4) and (5). The coefficients on recent current spending for the last three years are of similar magnitude, but not all three are significant. It's worthwhile to point out that this association between current spending and test scores is not necessarily causal.

How large are the BSF effects? In a review of recent evidence on the effect of school resource expenditures, Gibbons and McNally (2013) summarise that small effects in the literature are in the order of 0.02–0.05 SD for a 30% increase in expenditures, whereas large effects are in the order of 0.25–0.30 SD. At first glance, our estimates appear very small, considering the average cost per school under BSF is three times the annual current operational spending. But this would not be comparing like with like, as BSF investments are large initial capital expenditure, with a stream of potential future returns. So without knowing how long the returns will last, it's impossible to benchmark these results with studies on current spending by spending terms. Comparison with similar studies on school construction is more plausible. In terms of average investment per school, BSF is similar in the order of magnitudes with the school construction programme in Neilson and Zimmerman (2014). In this analysis, the average time length of treatment under BSF is 1.3 years, so it makes sense to compare our estimates with their results for one year after treatment. They find a 0.046 SD positive effect on reading scores, but no effect on maths, after one year of occupation in new school buildings. Based on an assumption of equal weights, the gains on average test score would be 0.023 SD. Our results for overall effects (around 0.02 SD) are very similar in size to this figure, but not statistically significant. Benchmarked to this figure, the 0.17 SD effects on academies are large.

3.6.2 Heterogeneity by Dose and Placebo Tests

BSF schools differ in the doses of treatment exposure, as the construction work was finished at different times across schools. During a given year, earlier finished schools received longer treatment than later finished schools. In addition, anticipatory effects could obscure estimates for the treatment effects. In order to accommodate the potential dynamic and anticipatory effects, Table 3.4 reports the estimates for equation (3.4), which is augmented by including leads and lags of the treatment. Specifically, indicator variables are added for years 1–4 prior to BSF project completion, and years 0–3 after the competition. Year 5 and before is the omitted category. Ongoing schools are also left in the omitted category as we not able to determine their year of treatment. Column (1) reports the estimates ignoring time-variant school-level variables. Columns (2)–(3) further control for academy conversion and current spending respectively. To aid with illustration, the estimated coefficients and 95% confidence intervals from column (2) are plotted in Figure 3.6.

We make two observations from Table 3.4. First, across all three specifications, the coefficients on lead indicators of years 1–4 prior to project completion are close to zero. This shows that there is little evidence of anticipatory effects. It also suggests that pre-treatment trends are not significantly different between the treatment group and control group.

Second, the point estimate on year 3 lag indicator are very large compared to year 1 and year 2. Based on Column (3) results, students exposed to three years of treatment score 1.43 points (or 0.13 SD) higher, although this is not significantly differently from zero at 10% level. Thus this only offers weak evidence that longer exposure creates a bigger effect.

3.6.3 Heterogeneity by Pupil Background

Next we examine whether the treatment effects are heterogeneous by pupils' academic and socioeconomic background by estimating equation (3.5). Specifically, we add interaction terms of BSF treatment and pupil background variables, including prior attainment at KS2, IDACI score, having SEN or not, and FSM eligible or not. For simplicity of interpretation, interaction terms between BSF and continuous variables (KS2 attainment and IDACI score) are constructed as BSF multiplied by three mutually exclusive dummy variables indicating low (within the lower quartile of each sampled cohort), mid (within second and third quartiles), and high (within the upper quartile) range of each continuous variable. The results are reported in Table 3.5. For each measure of background, two specifications are estimated, controlling for academy conversion and per-pupil current spending respectively.

The results paint a clear picture of disproportionate gains by academic and socioeconomic background. In general, all the coefficients on interaction terms between BSF and poor background are positive and statistically significant at 1% level, whereas the estimates on interactions between BSF and better background are close to zero or negative. This demonstrates that BSF is more effective in improving academic achievement for disadvantaged children. In fact, they seem to be the only subgroup that gain from BSF. The effects on students with mid or high KS2 attainment are close to zero and insignificant at 10% level; in contrast, the effects on students with low KS2 attainment are very large and significant at 1% level. The size of the effects on low KS2 students are around 0.14 SD. Students in rich areas tend to have a negative effect about the size of 0.04 SD, whereas those in poor areas experience a gain by 0.09 SD. SEN students or FSM students gain by roughly the similar magnitude, in the range of 0.07–0.09 SD.

These larger effects of school capital investment for disadvantaged students are in line with previous literature that generally finds increases in school current inputs to be more effective for disadvantaged schools and/or students (Gibbons and McNally 2013). This finding supports BSF's phasing strategy that prioritises socially and academically disadvantaged schools and areas. More generally, this leads to a policy implication that targeting school resources towards disadvantaged students could be more efficient.

3.6.4 Heterogeneity by Project Type

In recognition of potential heterogeneous effects due to that different levels of renewal as set out in equation (3.6), Table 3.6 breaks down the BSF treatment into four categories: ICT only, new build, mixture of new build and refurbishment, and refurbishment only. Column (1) reports the DiD estimates of the four treatment variables ignoring time-variant school-level controls S_{st}, whereas columns (2) and (3) further control for academy conversion and per-pupil current spending respectively. Across the three specifications, BSF generally does not have a significant effect among non-academy schools, except ICT projects in column (2). Besides this, comparison of point estimates on the four treatment variables seems to suggest that less costly project types, ICT and refurbishment, have bigger effects than more costly new build and mixture projects. Considering an average building project cost ten times as an ICT only project in terms of per-pupil funding, this result is somewhat surprising.

A possible explanation for these seemingly counter-intuitive results is that different project types create distinct levels of disturbance during the construction period. Major renewal projects involved more construction work and could cause more disturbance to teaching and learning. Construction work could take one to three years to finish. Some schools placed students in temporary classrooms during the construction work, and some continued on the original site with construction work took place nearby. The disturbance could have a negative impact on short-term academic outcomes, thus reducing the estimated treatment effects on major projects. In comparison, ICT or refurbishment projects might have caused less disturbance, as it took less time to carry out the work, or was done outside term-time.

Another finding from Table 3.6 is that new build academies constructed under BSF have larger effects. Column (2) finds that new build projects generate a significantly larger gain of 1.96 points (0.18 SD) for academies than for non-academies. ICT projects also have larger effects on academies, but only weakly so.

3.7 Robustness Check on Cohort Composition Change

In the BSF programme, a school's selection into treatment is partly determined by its past performance. If this past low performance is transitory, this will give us a concern that the treated group might have experienced Ashenfelter's (1978) dip before treatment. Ashenfelter's dip is a regularity often found in labour market training programmes (Heckman and Smith 1999; Heckman, Lalonde and Smith 1999), where participants experience a temporary earnings shock prior to programme entry. Formally, this means the idiosyncratic error term ε_{ist} has different expected values for the treatment and control groups before the treatment takes place. Earnings for the treated group would grow more quickly even in the absence of the programme due to their mean-reverting tendency. Thus the DiD estimator is likely to overestimate the treatment effect. In the context of BSF, if treated schools are selected because of the low performance of a particular pre-treatment cohort, we might suspect they would follow a faster growth track because later cohorts would perform better even without the programme. We might suspect this composition change creates different dynamics for the treated and control groups, thus differencing out fixed effects is not enough to deal with the selection bias, and the causal interpretation of the DiD estimates will be in doubt. On the other hand, if the pre-treatment low performance is permanent and cohort composition is stable before and after the treatment, the performance gap between the treated and control groups will be picked up by the school fixed effects, so the treatment effect is correctly identified by the DiD estimator.

To detect whether Ashenfelter's dip exists in this analysis, now we turn to examine the pre- and post-treatment cohort composition change. Specifically, we estimate equation (3.4) at the school cohort level using prior attainment at KS2 (English, Maths, and Science) and indicators of disadvantage (IDACI, SEN, and FSM) as dependant variables. Table 3.7 reports the results. Two points are worth noting here. First, the point estimates are mostly insignificant, indicating little change in cohort composition before, during and after the programme for the treated. Second, the signs of the estimates on different attainment and characteristics variables are well mixed[33]. Thus positive changes in some dimensions of cohort quality are likely to be balanced out by negative changes on other measures. In summary, we interpret these evidence as suggesting stable cohort composition in BSF schools pre- and post-treatment for the sample period.

3.8 Conclusions

School infrastructure investment is costly. Yet there is little evidence that such investments are effective in improving learning outcomes. In this chapter, I utilise the phasing design of BSF and apply difference-in-differences methods to estimate the effect of this large school construction programme on average test scores. BSF Schools are ranked based on their social and educational needs for capital investment, and constructed in a number of sequential waves according to their ranking. This phasing design implies pre-treatment similarity in schools between neighbouring waves.

The most important finding is that BSF has heterogeneous effects on pupils from different backgrounds. I find strong evidence that BSF has large effects on disadvantaged students. As a matter of fact, only students from the low end of academic and socioeconomic backgrounds benefit from the programme. Depending on the measure for pupil background, BSF raises the average test scores of the bottom quartile students by 0.07–0.14 SD. Spreading the test score gains over all treated students, the overall effects become

[33]Note that for a positive change in cohort quality, we expect positive signs for point estimates on KS2 attainment, and negative signs for indicators of disadvantage.

much smaller (0.02 SD) and insignificant. These disproportionate gains suggest that BSF has achieved some success in reducing educational inequality. For the implication on policy-making, targeting resources on the disadvantaged group may prove more effective.

There is some evidence that the BSF effects vary with school types, exposure to the treatment, and levels of investment. Autonomous academy schools experience larger gains than non-academy schools. Longer exposure to new school buildings and facilities generate larger effects, particularly after three years of occupancy, although these effects are not statistically significant. Somewhat surprisingly, more costly new build projects do not warrant larger returns. Instead, the effects of new build projects on test scores are very close to zero, and smaller relative to the effects of minor renovation or ICT only projects. A plausible explanation is that new build projects take more construction work and cause more disturbances to the students.

I emphasise that these findings apply to the very short run. As the whole BSF programme has not been completely finished, it is not clear how long it will take before the full effects emerge, and how long these effects will last. With a longer time horizon in mind, future work might consider medium-run and long-run evaluations of this programme on a range of outcomes, such as school choices, academic attainment, neighbourhood house prices, and labour market performance. Although the majority of later waves are cancelled, the completion of ongoing projects will see over 20% of all England's secondary schools rebuilt or renovated. Considering this large scale, it would be interesting to investigate the long-term effects of BSF on educational attainment and labour market performance in a general-equilibrium framework.

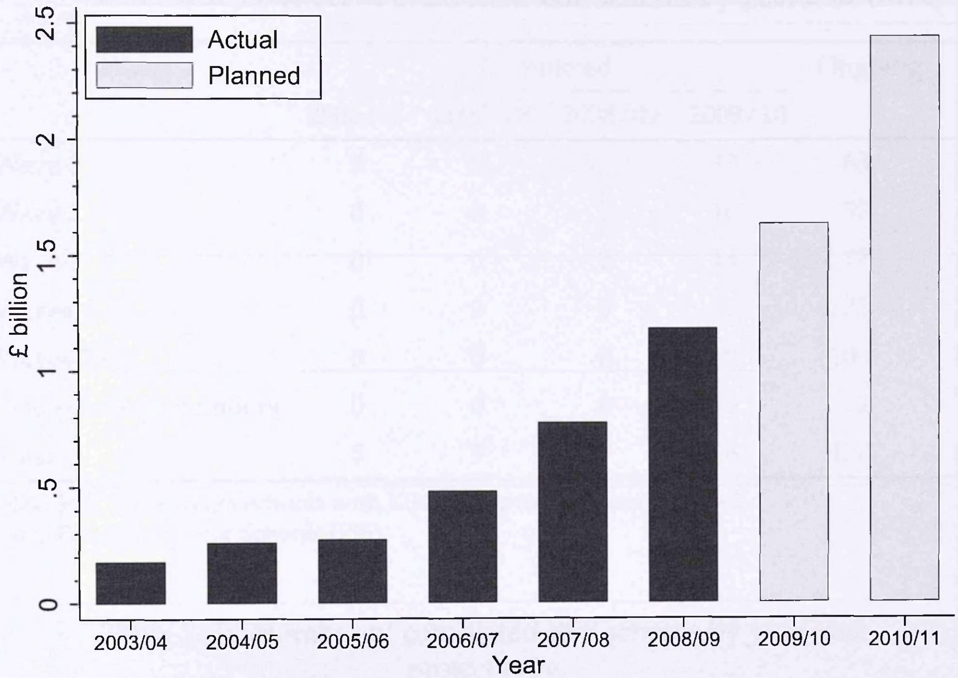


Figure 3.1: Public spending on BSF 2003/04–2010/11

Notes Figures do not include private finance initiative (PFI) credits.
Source DCSF (2009).

Table 3.1a: Number of completed BSF schools by year and wave

Wave	Completed				Ongoing	Total
	2006/07	2007/08	2008/09	2009/10		
Wave 1	5	9	40	33	68	155
Wave 2	0	0	2	10	57	69
Wave 3	0	0	2	14	77	93
Waves 4–6	0	0	0	5	173	178
Waves 7–15	0	0	0	0	100	100
One-school pathfinders	0	0	3	14	2	19
Total	5	9	47	76	477	614

Notes Only mainstream schools with KS4 stage provision are included.
Source Partnerships for Schools (PfS).

Table 3.1b: Number of completed BSF schools by year and project type

Project type	Year open				Total
	2006/07	2007/08	2008/09	2009/10	
ICT only	0	5	7	9	21
New build	0	3	28	36	67
Mixture	1	0	7	19	27
Refurbishment	4	1	5	12	22
Total	5	9	47	76	137

Notes Only schools with KS4 stage provision are included.
Source Partnerships for Schools (PfS).

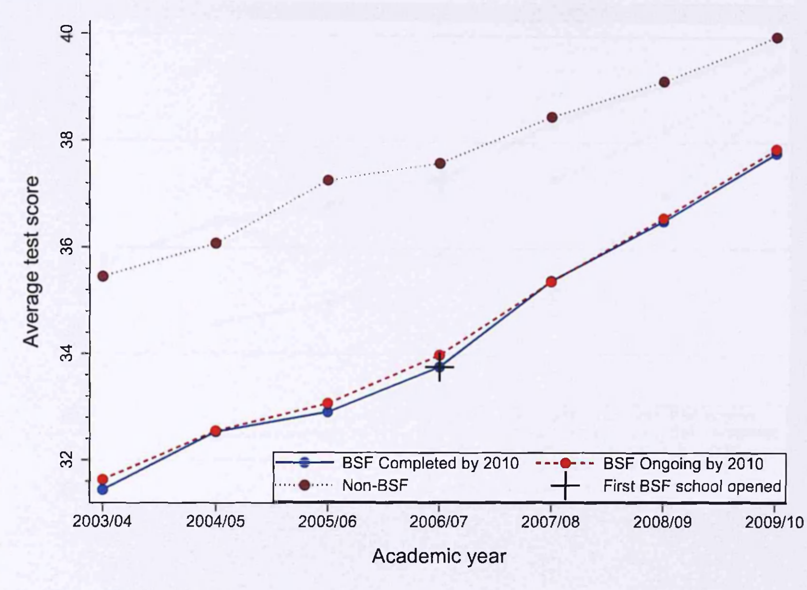


Figure 3.2: Trends in average GCSE and equivalent test score for BSF schools and all England schools

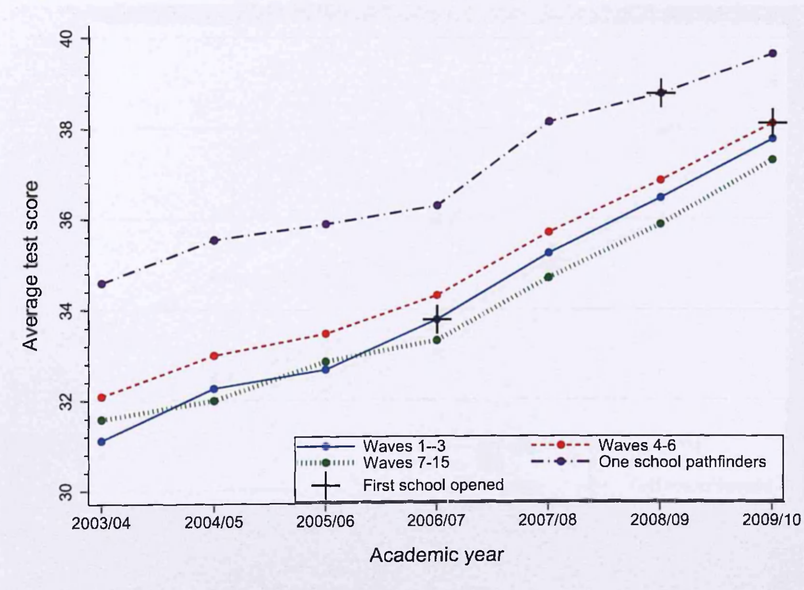


Figure 3.3: Trends in average GCSE and equivalent test score by BSF wave

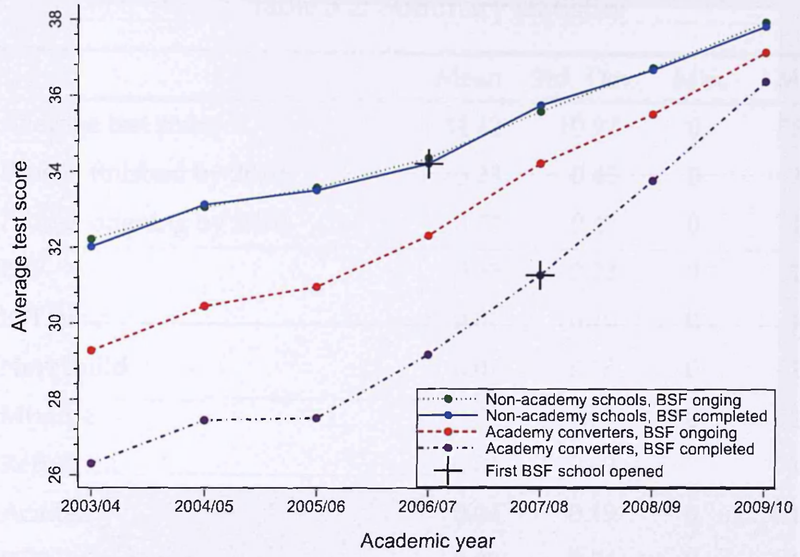


Figure 3.4: Trends in average GCSE and equivalent score by academy conversion status

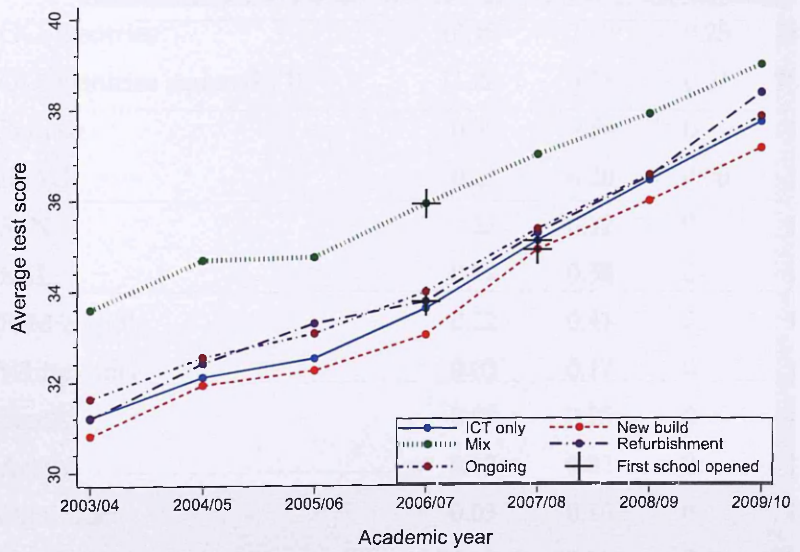


Figure 3.5: Trends in average GCSE and equivalent score by BSF project type

Table 3.2: Summary statistics

	Mean	Std. Dev.	Min.	Max.
Average test score	34.42	10.97	0	58
Project finished by 2010	0.23	0.42	0	1
Project ongoing by 2010	0.77	0.42	0	1
BSF	0.05	0.22	0	1
ICT only	0.01	0.10	0	1
New build	0.02	0.15	0	1
Mixture	0.01	0.09	0	1
Refurbish	0.01	0.09	0	1
Academy	0.04	0.19	0	1
BSF × academy	0.00	0.06	0	1
ICT only × academy	0.00	0.02	0	1
New build × academy	0.00	0.05	0	1
Per-pupil current spending (£000)	5.40	1.22	1.54	14.50
GCSE entries	10.18	2.89	0.25	28.00
GCSE entries squared./10	11.20	5.73	0.01	78.40
Female	0.50	0.50	0	1
IDACI	0.32	0.20	0.00	1.00
SEN	0.23	0.42	0	1
EAL	0.17	0.38	0	1
FSM eligible	0.22	0.41	0	1
White other	0.03	0.17	0	1
Black	0.07	0.25	0	1
Asian	0.12	0.33	0	1
Other race	0.03	0.16	0	1
KS3 English	36.62	20.16	0	99
KS3 English missing	0.10	0.29	0	1
KS3 Maths	69.70	26.89	0	149
KS3 Maths missing	0.05	0.23	0	1
KS3 Science	87.32	33.24	0	174
KS3 Science missing	0.06	0.23	0	1
KS2 English	49.07	21.81	0	109

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	Mean	Std. Dev.	Min.	Max.
KS2 English missing	0.10	0.30	0	1
KS2 Maths	54.10	26.16	0	100
KS2 Maths missing	0.09	0.28	0	1
KS2 Science	50.21	19.54	0	80
KS2 Science missing	0.08	0.27	0	1
Academic year 2003/2004	0.14	0.35	0	1
Academic year 2004/2005	0.14	0.35	0	1
Academic year 2005/2006	0.14	0.35	0	1
Academic year 2006/2007	0.15	0.35	0	1
Academic year 2007/2008	0.15	0.35	0	1
Academic year 2008/2009	0.14	0.35	0	1
Academic year 2009/2010	0.14	0.35	0	1
Observations		784,632		

Source National Pupil Database 2003/04–2009/10.

Table 3.3: The effect of BSF on average GCSE and equivalents test score

	OLS			DiD		
	(1)	(2)	(3)	(4)	(5)	(6)
BSF	2.19***	2.12***	0.30*	0.22	0.21	0.24
	(0.24)	(0.16)	(0.16)	(0.19)	(0.19)	(0.20)
Academy					1.24***	
					(0.31)	
BSF × academy					1.53**	
					(0.60)	
Per-pupil current spending (£000)						0.15**
						(0.06)
Lag 1 per-pupil current spending (£000)						0.10
						(0.07)
Lag 2 per-pupil current spending (£000)						0.13*
						(0.08)
GCSE entries	3.76***	2.83***	2.93***	2.86***	2.86***	2.84***
	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
GCSE entries squared./10	−1.02***	−0.84***	−0.95***	−0.88***	−0.88***	−0.87***
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Female	1.44***	1.06***	0.96***	0.93***	0.93***	0.94***
	(0.08)	(0.05)	(0.04)	(0.03)	(0.03)	(0.03)
IDACI	−7.22***	−2.79***	−2.98***	−2.83***	−2.83***	−2.88***
	(0.37)	(0.20)	(0.19)	(0.09)	(0.09)	(0.09)
SEN	−5.08***	−0.46***	−0.85***	−0.98***	−0.98***	−1.02***
	(0.10)	(0.07)	(0.07)	(0.05)	(0.05)	(0.05)
EAL	0.54***	1.84***	1.77***	1.56***	1.55***	1.60***
	(0.15)	(0.11)	(0.10)	(0.06)	(0.06)	(0.07)
FSM eligible	−2.19***	−1.02***	−0.92***	−0.97***	−0.97***	−0.99***
	(0.07)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)
White other	1.70***	1.63***	1.39***	0.87***	0.87***	0.85***
	(0.22)	(0.15)	(0.14)	(0.09)	(0.09)	(0.09)
Black	1.90***	2.08***	1.97***	1.30***	1.30***	1.32***
	(0.19)	(0.13)	(0.12)	(0.08)	(0.08)	(0.09)
Asian	2.03***	1.51***	1.47***	1.42***	1.43***	1.40***
	(0.21)	(0.13)	(0.13)	(0.09)	(0.09)	(0.09)
Other race	2.52***	1.86***	1.66***	1.10***	1.10***	1.10***
	(0.20)	(0.14)	(0.14)	(0.08)	(0.08)	(0.09)
KS3 English		0.22***	0.21***	0.20***	0.20***	0.20***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

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		Naïve			DiD	
	(1)	(2)	(3)	(4)	(5)	(6)
KS3 English missing		4.01***	3.91***	3.71***	3.71***	3.74***
		(0.12)	(0.11)	(0.10)	(0.10)	(0.10)
KS3 Maths		0.04***	0.04***	0.04***	0.04***	0.04***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
KS3 Maths missing		3.56***	3.40***	3.30***	3.31***	3.39***
		(0.13)	(0.12)	(0.10)	(0.10)	(0.11)
KS3 Science		0.03***	0.03***	0.03***	0.03***	0.03***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
KS3 Science missing		2.17***	2.13***	2.10***	2.10***	2.06***
		(0.12)	(0.11)	(0.10)	(0.10)	(0.10)
KS2 English		−0.00	0.03***	0.03***	0.03***	0.03***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
KS2 English missing		0.49***	1.53***	1.50***	1.51***	1.51***
		(0.10)	(0.09)	(0.08)	(0.08)	(0.08)
KS2 Maths		0.06***	0.06***	0.06***	0.06***	0.06***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
KS2 Maths missing		2.37***	2.19***	2.09***	2.09***	2.11***
		(0.08)	(0.08)	(0.07)	(0.07)	(0.08)
KS2 Science		0.07***	0.05***	0.05***	0.05***	0.05***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
KS2 Science missing		3.76***	3.17***	3.13***	3.13***	3.15***
		(0.12)	(0.12)	(0.10)	(0.10)	(0.11)
Year FE	No	No	Yes	Yes	Yes	Yes
School FE	No	No	No	Yes	Yes	Yes
Number of schools	614	614	614	614	614	605
Number of pupils	784,632	784,632	784,632	784,632	784,632	714,816
Adj. R^2	0.430	0.646	0.662	0.683	0.683	0.684

Notes Heteroskedasticity and cluster-robust standard errors at the school level are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% levels respetively. White British is the omitted race group.

Table 3.4: The estimated impact of BSF on average test score before, during, and after intervention

	(1)	(2)	(3)
Year −4	−0.09	−0.09	−0.10
	(0.22)	(0.22)	(0.24)
Year −3	−0.17	−0.17	−0.11
	(0.23)	(0.23)	(0.24)
Year −2	−0.22	−0.22	−0.24
	(0.25)	(0.24)	(0.25)
Year −1	0.04	0.06	−0.03
	(0.28)	(0.27)	(0.28)
Completion year	0.02	0.01	0.02
	(0.29)	(0.28)	(0.29)
Year +1	0.37	0.43	0.47
	(0.36)	(0.36)	(0.38)
Year +2	0.50	0.43	0.20
	(0.59)	(0.50)	(0.54)
Year +3	1.04	1.26	1.43
	(0.78)	(0.78)	(0.89)
Academy		1.26***	
		(0.31)	
BSF × academy		1.53**	
		(0.60)	
Per-pupil current spending (£000)			0.14**
			(0.06)
Lag 1 per-pupil current spending (£000)			0.10
			(0.07)
Lag 2 per-pupil current spending (£000)			0.13*
			(0.08)
Number of schools	614	614	605
Number of pupils	784,632	784,632	714,816
Adj. R^2	0.683	0.683	0.684

Notes All specifications include year and school fixed effects, pupil characteristics and prior attainment. Heteroskedasticity and cluster-robust standard errors at the school level are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% levels respetively.

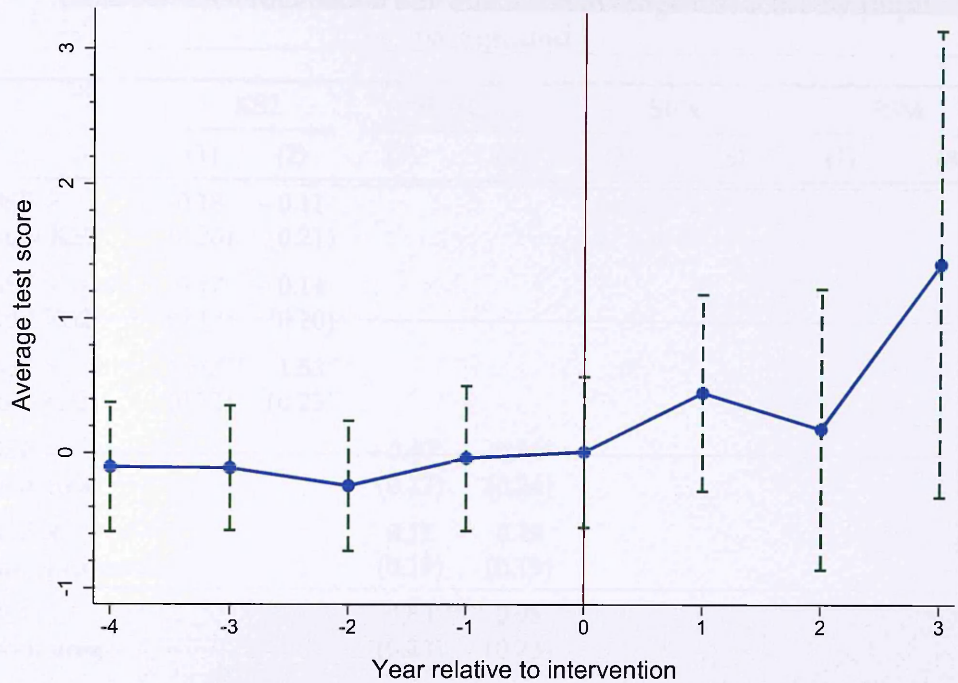


Figure 3.6: The dynamics of BSF effect on average test score before, during, and after the intervention

Table 3.5: Heterogeneous BSF effects on average test score by pupil background

	KS2		IDACI		SEN		FSM	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BSF × high KS2	-0.18	−0.11						
	(0.20)	(0.21)						
BSF × mid KS2	-0.17	−0.14						
	(0.19)	(0.20)						
BSF × low KS2	1.56***	1.53***						
	(0.23)	(0.25)						
BSF × rich area			−0.40*	−0.44*				
			(0.22)	(0.24)				
BSF × mid-income area			0.17	0.19				
			(0.19)	(0.19)				
BSF × poor area			0.83***	0.98***				
			(0.23)	(0.23)				
BSF × non-SEN					−0.03	−0.01		
					(0.19)	(0.20)		
BSF × SEN					0.80***	0.85***		
					(0.24)	(0.26)		
BSF × non-FSM							0.02	0.03
							(0.19)	(0.20)
BSF × FSM							0.87***	0.99***
							(0.24)	(0.23)
Schools	614	605	614	605	614	605	614	605
Pupils	784,632	714,816	784,632	714,816	784,632	714,816	784,632	714,816
Adj. R^2	0.683	0.685	0.683	0.684	0.683	0.684	0.683	0.684

Notes All specifications include year and school fixed effects, pupil characteristics and prior attainment. In each panel, the first model controls for academy conversion and its interaction with BSF, and the second model controls for per-pupil current spending for the most recent three years. The low KS2 category is defined as achieving KS2 attainment in the lowest quartile of the sampled cohort, the high KS2 category is defined as achieving KS2 attainment in the highest quartile, and the mid KS2 category is the rest. Same categorisation applies for low, mid, and high IDACI score, except higher IDACI score indicates poorer areas. Heteroskedasticity and cluster-robust standard errors at the school level are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively.

Table 3.6: The influence of BSF on average test score by project type

	(1)	(2)	(3)
ICT only	0.63	0.68*	0.51
	(0.38)	(0.38)	(0.37)
New build	0.09	−0.09	−0.04
	(0.25)	(0.25)	(0.28)
Mixture	0.01	0.19	0.47
	(0.43)	(0.43)	(0.45)
Refurbish	0.39	0.55	0.40
	(0.52)	(0.52)	(0.48)
Academy		1.24***	
		(0.31)	
ICT only × academy		0.44	
		(1.89)	
New build × academy		1.96***	
		(0.58)	
Per-pupil current spending (£000)			0.15**
			(0.06)
Lag 1 per-pupil current spending (£000)			0.10
			(0.07)
Lag 2 per-pupil current spending (£000)			0.13*
			(0.08)
Schools	614	614	605
Pupils	784,632	784,632	714,816
Adj. R^2	0.683	0.683	0.684

Notes All specifications include year and school fixed effects, pupil characteristics and prior attainment. Heteroskedasticity and cluster-robust standard errors at the school level are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% levels respetively.

Table 3.7: Cohort composition change pre- and post-intervention

	KS2			Characteristics		
	MAT	ENG	SCI	IDACI	SEN	FSM
Year −4	0.693**	0.186	0.150	−0.003	−0.001	0.000
	(0.297)	(0.215)	(0.239)	(0.003)	(0.010)	(0.006)
Year −3	0.256	0.054	−0.044	0.000	−0.014	−0.003
	(0.324)	(0.231)	(0.242)	(0.003)	(0.010)	(0.007)
Year −2	0.466	0.091	−0.188	−0.004	−0.009	−0.003
	(0.325)	(0.242)	(0.261)	(0.004)	(0.012)	(0.007)
Year −1	0.562*	0.060	0.060	−0.007	0.002	−0.003
	(0.325)	(0.253)	(0.252)	(0.005)	(0.014)	(0.007)
Completion year	0.169	0.319	−0.411	−0.005	0.011	0.003
	(0.361)	(0.279)	(0.282)	(0.005)	(0.016)	(0.008)
Year +1	−0.114	−0.510	−0.712**	−0.009	0.019	−0.004
	(0.473)	(0.360)	(0.357)	(0.005)	(0.020)	(0.009)
Year +2	−0.158	−0.761	−0.663	−0.009	0.001	0.017
	(1.136)	(0.974)	(0.846)	(0.010)	(0.039)	(0.018)
Year +3	0.588	1.205	0.030	−0.012	0.066	0.007
	(0.971)	(0.791)	(0.625)	(0.020)	(0.096)	(0.015)
Schools	614	614	614	614	614	614
Observations	4,267	4,267	4,267	4,267	4,267	4,267
Adj. R^2	0.882	0.895	0.869	0.968	0.589	0.914

Notes All specifications include year and school fixed effects. Heteroskedasticity and cluster-robust standard errors at the school level are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% levels respetively.

Chapter 4

Returns to Education for the Self-Employed: The Income Under-Reporting Bias

4.1 Introduction

Economic returns to investment in education have been studied by economists over decades for many countries,[34] particularly since the classic work of the human capital theory (Becker 1962, 1964; Schultz 1963) and human capital earnings function (Mincer 1974). While there is a vast literature estimating rates of private returns to education for wage- and salary-earning employees, there is relatively little evidence for self-employed workers.

Yet self-employment constitutes a significant part of the economy. Self-employed workers on average take up over 15% of total civilian employment in OECD countries, with Turkey at the top, reaching over 50% in 2000 (see Figure 4.1 for details). And the employment status composition of the

[34]For instance, see Card (1999, 2001), Harmon, Oosterbeek and Walker (2003) for reviews on theoretical framework and empirical strategies of estimating private returns to education; Ashenfelter, Harmon and Oosterbeek (1999) for a meta-analysis of the estimates; and Psacharopoulos (1985, 1994), Trostel, Walker and Woolley (2002), Psacharopoulos and Patrinos (2004), for updates on cross-country comparisons.

economy has been changing in many countries. Most OECD countries have experienced a fall in self-employment ratio from 2000 to 2012. UK, on the other hand, has been witnessing an upward trend in both the number and the proportion of self-employed workers since 2000 (see Figure 4.2). Studying the economic returns to education for the self-employed workers may help to understand these changes.

Previous literature has discussed various issues with respect to the estimation of returns to education for self-employed workers. But to my best knowledge, the data quality issue for self-employment earnings has not been explored. Surveys are widely used for estimations of returns to education, but reliable survey data on earnings, particularly for the self-employed, are scarce. It has been long noticed in the literature that self-employed workers under-report their income to tax authorities for tax evasion reasons[35]. They lack the incentive to behave otherwise to data collectors. First, they might not be confident in the confidentiality of surveys and worry about incriminating themselves if they report higher income to survey collectors but lower income to tax authorities (Pissarides and Weber 1989). Second, it takes considerably more time and efforts for self-employed worker to account for their income than for employees, and it's much easier to simply report the same accounts to survey collectors (Hurst, Li and Pugsley 2014).

Incorrectly-reported income data, could bias the estimates of returns to education. Income under-reporting leads to a downward bias in estimating the earnings-age profile. More importantly, the estimates of rate of returns to education could be biased as well, if the degree of under-reporting is different across the income distribution or education levels. For instance, if self-employed individuals with lower education under-report their income more heavily, then using these ill-measured data will lower the bottom end

[35]See, for example. Slemrod (1985); Andreoni, Erard and Feinstein (1998); Slemrod (2007).

of the earnings-education profile and raises the slope, therefore the estimates of returns to education will be biased upwards.

In this chapter I examine these potential biases by comparing the estimates using reported income data and the estimates correcting for income under-reporting. As the first step, I infer the true self-employment income following an expenditure-based approach pioneered by Pissarides and Weber (1989). I estimate an Engel curve using food expenditure and household income from employee households, and apply this Engel curve to self-employed households to impute their actual income. Next, I estimate the returns to education for self-employed workers using the inferred income data. In this step, I adopt an Instrumental Variable (IV) approach to address the endogeneity issue of self-selection into education and self-employment. The instruments used in this step include early smoking behaviour and parental education qualification.

I find evidence that supports income under-reporting from self-employed households. On average, reported income of households with at least one self-employment worker should be multiplied by a factor of 1.18-1.26 to reflect their true income. At the individual level, an average self-employed worker's reported earnings should be boosted by a factor of 1.4. More importantly, I present a new finding to the literature that the degree of income under-reporting is heterogenous across the income distribution. Lower-income self-employed households under-report more heavily. This leads to a severe upward bias in estimating the returns to education for the self-employed. While using reported self-employment income leads to the finding of a 11.0% (using early smoking as the instrument) or 5.9% (using parental qualification as the instrument) rate of returns for one additional year of schooling, these estimates are reduced to 5.1–7.2% (early smoking) and 3.0–3.2% (parental qualification) when income under-reporting is corrected for. Compared to the 13.9–15.2% returns for employees, the results suggest that the self-employed extract lower returns from education. However, this comparison does not necessarily support the screening view (Spence 1973; Weiss 1995), namely education acts as a signalling device for ability instead of enhancing ability, as this assumes that individuals make the decisions of becoming self-employed and investment in education around the same time. This assumption does not appear to be supported by the data.

The remainder of this chapter proceeds as follows. Section 4.2 reviews previous literature on income under-reporting and returns to education for the self-employed; Section 4.3 presents the empirical methodology; Section 4.4 describes the data; Section 4.5 reports and discusses the results; Section 4.6 offers concluding remarks.

4.2 Literature Review

The economic literature has long suspected households of under-reporting their income to tax authorities, and has developed various theories and methods to explain and test the existence of tax evasion. Allingham and Sandmo (1972), adapting Becker's (1968) economics of crime model, develop a theoretical model of household choice of tax evasion under uncertainty, in which the household maximise their expected utility and choose such degree of tax evasion that expected utility gain equals expected utility loss of detection and penalty. The theoretical model has since been extended in a number of ways.[36] Under this framework, many empirical papers have tried to estimate the magnitude of tax evasion.[37] Some particular findings (Slemrod 2007) of interest for this chapter are: income under-reporting accounts for over 80% of individual tax evasion; and the magnitude of tax evasion varies sharply

[36] See Andreoni, Erard and Feinstein (1998) and Sandmo (2005) for surveys.
[37] See Alm (1999); Slemrod and Yitzhaki (2002); Slemrod (2007) for reviews of evidence.

with sources of income: only 1% of wages and salaries are under-reported, whereas self-employment business income is associated with a much higher noncompliance rate—nonfarm proprietor income is under-reported by 57%[38]; Slemrod (2007) further calculates that self-employment tax is under-reported by 52%.

Pissarides and Weber (1989) corroborate this striking dissimilar tax noncompliance pattern between employed and self-employed workers by designing an expenditure-based approach, to estimate the size of black economy in the UK. They assume correct reporting of food expenditures for self-employment and employment income groups, correct reporting of employment income, and under-reporting of self-employment income. They estimate the differences in the Engel curve function between the two groups. They attribute the differences to two components: income under-reporting of self-employed workers, and transitory income fluctuations over time. They impose further assumptions on the distributions of these two components, and obtain an interval estimate of degree of income under-reporting. Using data from Family Expenditure Survey 1982 for the UK, they find average true self-employment income is 1.55 times reported self-employment income. This implies that the size of the black economy is about 5.5% of UK's GDP.

This approach has been applied and refined by several other researchers. Lyssiotou, Pashardes and Stengos (2004) expands this approach from using a single Engel curve to a complete demand system. Kim, Gibson and Chung (2009) makes use of panel data to pin down an exact estimate of the degree of underreporting rather than just an interval estimate. Tedds (2010) proposes a nonparametric approach which avoids imposing a functional form of the Engel curve *a priori*. Recent work by Hurst, Li and Pugsley (2014) finds that self-employed workers systematically under-report their incomes by 25% on

[38]Federal tax gap estimates by US Internal Revenue Service (IRS).

average in two U.S. household surveys. They argue that failing to account for such income under-reporting leads to biased estimates in various settings. For example, they find that 10-15% of the decline in earnings between the ages 45-65 can be attributed to income under-reporting.

Other than to show the biases in estimating returns to education self-employment due to income under-reporting, my second interest in this chapter is to compare the returns to education for the self-employed and employees. This is motivated by the mixed evidence in the existing literature. In a meta-analysis of empirical studies,Van der Sluis, Van Praag and Vijverberg (2005) reviews 20 studies for industrial industries,[39] and finds mixed evidence on the relative magnitude of returns to education for the self-employed and employees. Some studies find the self-employed extract higher or equal returns from education relative to employees, while others find the reverse is true. Perhaps a more consistent pattern is that studies that use US data often support the former conclusion, while studies for Europe support the latter. This chapter provides one more piece of evidence to this comparison.

This chapter builds on and links the two strands of literature on income under-reporting and returns to education for the self-employed. I start with estimating a semiparametric food Engel Curve without imposing functional form restrictions, and recover that the Engel curve can be closely approximated by a simple linear or quadratic functional form. Previous work on income under-reporting has been focusing on estimating the mean degree of income under-reporting based on expenditures. I bring the literature forward by assessing the income under-reporting across the income distribution, and examine the consequences of income under-reporting in the setting of estimating returns to education for the self-employed.

[39]See their Table 5.

4.3 Methodology

4.3.1 Correcting for Income Under-Reporting

Let k denote the household type, where $k = EE$ for an employee household, and $k = SE$ for a self-employed household. We define the employee household as a household with at least one member in paid employment and none in self-employment, and the self-employed household as one with at least one self-employed member. Without imposing a specific function form, we assume that household preferences generate a partially linear Engel curve of the following form:

$$\ln C_i^k = f^k(\ln Y_i^k) + W_i^k \phi + \xi_i^k, \tag{4.1}$$

where C_i is the expenditure of household i on food, Y_i is the household income, W_i is a vector of controls, and ξ_i is the unobserved random error.

We suspect that the self-employed household income $\ln Y_i^{SE}$ may be misreported in our data. Our aim is to infer the true income for from the inverse Engel curve function. We make three assumptions: (a) employee households report both income and food expenditure correctly; (b) self-employment households report their food expenditure correctly; (c) the food Engel curve is identical for employee and self-employment households.

We choose food expenditure as we believe the assumptions are more reasonable for food consumption. First, unlike consumer durables, food consumption usually takes up a small proportion of household income, so there is little incentive for households to under-report food expenditure for the purpose of concealing income information. On the other hand, consumer durables usually involve large purchases, so correctly reporting consumer durables expenditure and under-reporting income may seem inconsistent.

From this viewpoint, self-employed households who choose to under-report their income are likely to under-report large expenditures as well. Second, as food is a daily necessity for all households, conditional on household demographics, its consumption is unlikely to be systematically different by employment status.

Given these assumptions, we first estimate the food Engel curve using reported information from employee households:

$$\ln C_i^{EE} = f^{EE}(\ln Y_i^{EE}) + W_i^{EE}\phi + \xi_i^{EE}, \tag{4.2}$$

to obtain the estimated parameters $\hat{\phi}$ and the nonparametric fit $\hat{f}^{EE}(\cdot)$.

We then run a similar regression on self-employed households using their reported income $\ln Y_i^{SE,reported}$:

$$\ln C_i^{SE} = f^{SE}(\ln Y_i^{SE,reported}) + W_i^{SE}\phi + \xi_i^{SE}, \tag{4.3}$$

to obtain residuals $\tilde{\xi}_i^{SE}$. We use this as a proxy for unobservable determinants of food consumption for self-employed households.

Next, we plug in the estimated parameters $\hat{\phi}$ and function $\hat{f}^{EE}(\cdot)$ from the employee household equation (4.2), and the residuals $\tilde{\xi}_i^{SE}$ from equation (4.3), into the Engel curve equation to establish a relationship between the food expenditure and true income for self-employed households:

$$\ln C_i^{SE} = \hat{f}^{EE}(\ln Y_i^{SE}) + W_i^{SE}\hat{\phi}^{EE} + \tilde{\xi}_i^{SE}, \tag{4.4}$$

where $Y_i^{SE,true}$ is the self-employed household income to be inferred.

Let $f_{-1}(\cdot)$ denote the inverse function of $f(\cdot)$. Solving equation (4.4) yields the inferred income for self-employed households:

$$\ln Y_i^{SE,true} = \hat{f}_{-1}^{EE}(\ln C_i^{SE} - W_i^{SE}\hat{\phi}^{EE} - \tilde{\xi}_i^{SE}). \tag{4.5}$$

In order for it to be invertible, $f(\cdot)$ must be monotonic within a reasonable income range. Intuitively, this means food is a normal good, and higher-income households spend more on food than lower-income households, conditional on household demographics.[40] We will demonstrate that this is supported by the data.

To simplify the imputation, we check how we can approximate $f(\cdot)$ with a simpler functional form. If $f(\cdot)$ is close to a linear function in log income, we can reduce the Engel curve to the following form:

$$\ln C_i^k = \alpha \ln Y_i^k + W_i^k \phi + \xi_i^k, \tag{4.6}$$

In this case, the self-employment household income can be inferred as:

$$\ln Y_i^{SE,true} = \frac{1}{\hat{\alpha}^{EE}}(\ln C_i^{SE} - W_i^{SE}\hat{\phi}^{EE} - \tilde{\xi}_i^{SE}), \tag{4.7}$$

If the Engel curve resembles a quadratic form[41] as follows,

$$\ln C_i^k = \alpha_1 \ln Y_i^k + \alpha_2(\ln Y_i^k)^2 + W_i^k \phi + \xi_i^k, \tag{4.8}$$

theoretically this yields two solutions for the self-employment income

$$\ln Y_i^{SE,true} = \frac{-\hat{\alpha}_1^{EE} \pm \sqrt{(\hat{\alpha}_1^{EE})^2 - 4\hat{\alpha}_2^{EE}(W_i^{SE}\hat{\phi}^{EE} + \tilde{\xi}_i^{SE} - \ln C_i^{SE})}}{2\hat{\alpha}_2^{EE}}. \tag{4.9}$$

[40]Strictly speaking, $f(\cdot)$ can also be invertible if food is an inferior good. This means higher-income households spend less on food. While it does not affect the feasibility of our imputation, this possibility is not supported by the data.

[41]For instance, see Banks, Blundell and Lewbel (1997) for evidence that supports quadratic Engel curves.

In this scenario, we impose a reasonable income range (for instance, 1.5 times the sample income range) to select the solution that is realistic.

4.3.2 Returns to education

To estimate the returns to education, we specify a Mincerian earnings function (Mincer 1974):

$$\ln y_j = \beta S_j + X_j \gamma + \varepsilon_j, \tag{4.10}$$

where y_j represents log hourly earnings for individual j, S_j is the number of years of education, X_j is a vector of controls, and ε_j is the unobservable error term. The parameter of interest, β, measures the returns to education for one additional year of schooling.

Estimating equation (4.10) by least squares is problematic if education is endogenous, $E(S_j \varepsilon_j) \neq 0$. A prevalent argument is that individuals with higher ability acquire more education as the cost is lower for them, thus omitting ability counts the reward for higher ability towards the returns to education, consequently leads to an upward bias in β. Another issue is S_j is often measured with error in survey data. This will lead to a downward attenuation bias in least squares estimation.

We seek to address these issues with instrumental variable (IV) methods. The first stage is given by:

$$S_j = \pi Z_j + X_j \delta + u_j, \tag{4.11}$$

where Z_j is the instrumental variable that is correlated with education (the relevance restriction), $E(S_j Z_j) \neq 0$, but not correlated with the error term from the earnings equation (the exclusion restriction), $E(Z_j \varepsilon_j) = 0$.

We use two instruments that are available in the data. The first one is early smoking behaviour proposed by Evans and Montgomery (1994). The intuition is that smoking proxies for an individual's rate of time preference. Smoking behaviour at an early age thus affects the investment decisions in education. Smokers, having higher discount rates, will invest less in education. Dickson (2013) estimates the rates of returns to education for male employees using early smoking and Raising of School Leaving Age (RoSLA) in the UK (Harmon and Walker 1995; Oreopoulos 2006) as instruments for education. As a state policy, RoSLA generates more credible exogenous variation in education. Unfortunately, we can not use RoSLA as an instrument for education in our analysis, as it does not have enough predictive power for the education of the self-employed, thus will lead to severe weak instrument bias. However, it's likely that this instrument will generate similar results to early smoking. Dickson (2013) finds that the rate of returns for one additional year of education is 12.9% using early smoking as the instrument, and 10.2% using RoSLA. His finding suggests that the results obtained from these two instruments are not dissimilar.

A second instrument is parental qualification. It is documented in the literate that parental education is strongly correlated with education (Card 1999). A major criticism for using family background as instruments for education is that it may have a wealth effect that directly affects earnings (Card 1999; Trostel, Walker and Woolley 2002; Psacharopoulos and Patrinos 2004), thus violating the exclusion restriction. However, recent research by Hoogerheide, Block and Thurik (2012) explores the potential biases of family background variables, and finds that a moderate violation of the exclusion restriction does not distort the results by much. In our analysis, we control for parental occupations to alleviate the potential direct effect of parental education on earning.

4.4 Data

The analysis in this chapter uses data from the British Household Panel Survey (BHPS) 1991–2008 and the Family Expenditure Survey (FES)[42] 1994–2009. Each dataset has its own advantages. The FES provides more reliable information on expenditures, whereas the BHPS has richer information on individual characteristics.

The FES is a continuous annual survey that collects information on household expenditures and income. Food expenditure is recorded under a diary system. Interviewed households are asked to keep a 14-day diary which details every single purchase. The BHPS is a longitudinal dataset which follows a nationally representative sample individuals in 1991 each year for 18 waves. It records food expenditure at household level in every wave. In wave 1, respondents are asked to think out their "weekly food bills" and report approximately the weekly figure the household spends on food and groceries to the nearest pound. From wave 2 onwards, this question is rephrased where respondents are asked to choose their answers from a showcard which lists 12 intervals ranging from under £10 to £160 or over. I take the mid-point value of each interval as the measure of the household's food expenditure.

Table 4.1a presents some summary statistics in the two datasets by household type. The statistics show that the two datasets share similar demographic patterns. In both datasets, self-employed households report slightly lower income than employee households, but spend more on food. Self-employed households tend to have more members, more children, and more non-working members, and the heads of the self-employed households tend to be older.

[42]There have been a few changes in the name of this survey. The FES ran from 1961 to 2001. From 2001, the Expenditure and Food Survey (EFS) replaced FES and the National Food Survey (NFS). From 2008, EFS was renamed the Living Costs and Food Survey (LCF) and included as part of the Integrated Household Survey (IHS). FES data prior to 1994/1995 did not contain some characteristics on central heating.

I carry out the individual-level analysis for the returns to education in the BHPS. This is because the instrumental variables, early smoking and parental qualification are not available in the FES. I restrict the individual sample to be full-time workers in their working ages[43]. The top and bottom 1% of the earnings distribution are trimmed off. In the BHPS, years of education is not directly recorded. Instead, respondents are asked to report their school leaving age and further education leaving age where applicable. I calculate years of education as education leaving age subtracted by five, the usual age to start compulsory schooling in the UK. However, this calculation does create a measurement error for those who return to education with certain years of work experience. I drop a small proportion of observations that report leaving full-time education at age 27 or later. This leads to a 2% reduction in the sample size.[44]

Table 4.1a presents the summary statistics at the individual level. Self-employed workers appear to work longer hours at lower hourly earnings than employees. They tend to have less education, more likely to be male and older. They are about equally likely to have smoked at age 14. Their parents are less likely to have further education qualifications or above.

4.5 Results

4.5.1 The Food Engel Curve

We first assess the shape of the Engel curve by estimating a semiparametric relationship between food expenditure and household income as set up in equation (4.1). This procedure is carried out using a Robinson's double residuals estimator (Robinson 1988). We first take the expected values

[43] 18–65 for males, and 18-60 for females.

[44] Results are robust if these observations are coded as having 21 years of education.

of food expenditure and demographic controls conditioning on household income, then partial out these expected values from food expenditure and demographic controls to obtain the residuals. The parameters ϕ on demographic controls are identified by regressing food expenditure residuals on demographic residuals. We then partial out the parametric fit from food expenditure to obtain the part that can be explained by household income. Finally, we estimate this part nonparametrically by using a local polynomial fit estimator. Figure 4.3 plots the nonparametric part of equation (4.1) for employee and self-employed households separately using the BHPS and FES samples.

We make three observations from the nonparametric Engel curves in Figure 4.3a using the BHPS sample. First, for the most part of the household income distribution (plotted on the right axis), the Engel curve for self-employed households (dashed line) lies above the Engel curve for employee households (solid line). This shows that given the amount of food expenditure, self-employed households report lower income than employee households. Second, the gap between the Engel curves for the two groups is larger for the bottom half of the income distribution, and diminishes quickly with reported income for the upper half of the income distribution. If both groups share the same food Engel curve, and employee households correctly report their income, this means lower-income self-employed households under-report more heavily than higher-income self-employed households. Third, except for the curvature at both ends of the income distribution due to thin data, the slope of the Engel curve for employee households stays roughly constant as income increases. In fact, a simple regression reveals that a linear function can explain 99% of the nonparametric Engel curve. This suggests that the Engel curve can be approximated closely by a linear (or quadratic, to avoid missing the slight curvature in the middle) parametric functional form. The

FES sample presents similar features in Figure 4.3b.

Table 4.2 reports the linear and quadratic parametric estimates of the food Engel curves for the employee households using BHPS and FES samples. All specifications control for household composition (household size, number of children, number of non-working members), a quadratic term in household head's age, region dummies and year dummies. Columns (1) and (3) present the results for the linear Engel estimation. In both samples, the results suggest that log household income strongly predicts log food expenditure. Consistent with intuition, the results also suggest that larger households spend more on food, whereas households with more children and non-working members spend less. The household head's age is correlated with food expenditure, but seemingly in different patterns in the two samples.

Moving to the results for a quadratic Engel curve in columns (2) and (4), the results show two things. On one hand, the quadratic term of household income is statistically significant in both samples, suggesting there is some curvature in the Engel curve. On the other hand, judging from the changes in R-squared, the quadratic term adds very little to the Engel curve estimation in explaining additional variation in food expenditure. This suggests the improvement in fitting the data towards the Engel curve from a quadratic functional form to a linear one might be marginal. We conduct the following analyses using inferred income from both linear and quadratic Engel curves to check for robustness.

4.5.2 Income Under-Reporting

Next we examine the extent to which self-employed households misreport their income. We impute the income for self-employed households using equations (4.7) and (4.9) based on the estimates in Table 4.2. As the quadratic Engel curves are monotonic over the sample income distribution, we drop the one root from equation (4.9) that falls far outside the sample income range. All income data are deflated to 2008 terms.

Table 4.3a presents the summary statistics on reported and inferred monthly income at the household level. Panel A reports the results from the linear Engel curve, and Panel B reports the results from the quadratic Engel curve. The first three columns present the results using BHPS data. BHPS self-employed households report an average monthly household income of £3,120. Our imputation using the linear food Engel curve suggests that the true average monthly household income is £3,938. This means that the average self-employed household's reported income has be to multiplied by a factor of 1.26 to arrive at the actual income. This ratio is much similar to 1.25 found in two US surveys by Hurst, Li and Pugsley (2014). In Panel B, using a quadratic Engel curve does not seem to generate very different results. The under-reporting factor is 1.24 in this case. In both the linear and quadratic cases, quartile differences (the difference between reported income quartile and inferred income quartile) vary across the three quartiles, suggesting the actual income distribution is quite different from the reported income distribution. Results in the next three columns using FES data offer further support that self-employed households under-report their income. A light difference is that the degree of under-reporting seems to be lower. The under-reporting factor at the mean is 1.18–1.19 in FES.

Table 4.3b presents the summary statistics on income under-reporting at the self-employed individual level using BHPS data. We calculate the difference between inferred and reported household income and add this to the self-employed household member's reported earnings to achieve their true earnings. In cases where there are more than one self-employed member in the household (this applies to 11% of the self-employed households), we divide the total unreported household income by number of self-employed members, and add this average to each self-employed member's earnings. Our results suggest self-employment income should be boosted by a factor of about 1.4 at the individual level to reflect their actual earnings.

We plot the ratio of the imputed income to the reported income for self-employed households in BHPS against the imputed income in Figure 4.4. We also plot a local mean polynomial fit across the income distribution. We make three findings for these plots. First, self-employed households generally under-report their income. Unlikely some previous studies (Pissarides and Weber 1989; Johansson 2005; Kim, Gibson and Chung 2009), we do not impose the assumption that self-employed household under-report income , but find this is supported by the data. Second, the extent of under-reporting is higher for lower-income households, and lower for higher-income households. This heterogenous under-reporting pattern can lead to biased conclusions for research that relies on the distribution of self-employment income. Third, the linear Engel curve seems to capture the food expenditure-income relationship very well, as inference using the quadratic Engel curve does not seem to generate very different results. Figure 4.5, using FES data, presents a much similar picture in supporting these findings.

4.5.3 Returns to Education

Table 4.4 reports the OLS estimates of the returns to an additional year of education for employees and the self-employed. In all specifications, we control for a quadratic term in age, sex, a quadratic term in year of birth, region dummies and year dummies. The dependent variable is log hourly earnings. Column (1) presents the estimates for employees using their reported wages. The coefficient on years of education suggests that one additional year of schooling is associated with a 4.3% increase in hourly wage. Column (2) presents the estimates for the self-employed using their reported earnings. By contrast, the rate of returns to one additional year of education for the self-employed is 2.1%, about half of that for employees. In the next two columns, we use the inferred earnings for the self-employed from the linear and quadratic Engel curves respectively. The returns to education become lower, reduced to 1.5% and 1.2%.[45] This reflects our previous findings that lower-education self-employed workers under-report their income more heavily, thus using reported income leads to an upward bias in the slope of the earnings-education profile.

The OLS estimates may suffer from ability bias and measurement error attenuation bias. We turn to IV estimation to address these two issues. Table 4.5a reports the IV estimates using early smoking as the instrument. Columns (1) and (2) presents the results obtained from using the employee subsample. The first stage results in column (2) show that early smoking strongly predicts years of completed education. Employees who smoke at age 14 complete 1.1 fewer years of education than those who do not. The *F*-statistic on the instrumental variable is well above the rule-of-thumb critical value of 10 (Staiger and Stock 1997; Stock and Yogo 2005), therefore we are not concerned about the weak instrument bias. Compared to the results in Table 4.4 column (1), the size of the IV estimate is about three times that of the OLS estimate. This estimate is much similar to the 12.9% returns found by Dickson (2013) for male employees in the BHPS.

Columns (3) and (4) reports the IV estimates for the self-employed using reported earnings. The instrument gets weaker in the first stage, but the *F*-statistic are still well above conventional critical values. The estimated rate of returns to education is 11.0%. The magnitudes of the coefficients on early smoking and years of education in the first and second stage equations for the self-employed are comparable to those for employees. Using reported

[45]The estimates hardly change (not reported here) when the sample is restricted to observations with non-missing values of the early smoking and parental qualification variables.

income, these results suggest the self-employed slightly lower returns from education than employees.

Column (5) reports the IV estimates for the self-employed using inferred earnings. The rate of returns of education is 7.2% in this estimation. Comparing this number to 15.2% returns for employees in column (1), the difference is now more visible. The lowering estimates from column (3) to column (5) suggest that using misreported self-employment income data can lead to serious upward biases. Taken the estimates literally, this introduces an upward bias by over 50%.

Table 4.5b reruns the estimations using the inferred income from the quadratic Engel curve. Column (5) shows that using this imputed information leads to consistent findings with Table 4.5a. Estimates for the self-employed using inferred income are smaller in size and less significant. A difference is that the estimated rate of returns to education for the self-employed becomes even lower using inferred income from the quadratic Engel curve. The point estimate is 5.1%, about one third of the size of the estimate for employees, and is not statistically significant.

A clear message from the analysis above is that income under-reporting distorts the earnings-education profile, and biases the estimation for returns to education in self-employment upward. To further support this finding, we next rerun the estimations using an alternative instrument, parental qualification. We control for parental occupation to address the issue that parental education might have an effect on their children's earnings through family wealth, thus violating the exclusion restriction of IV estimators. Table 4.6a presents the results using this instrument. First stage results in columns (2) and (4) for employees and the self-employed respectively suggest that this is an instrument with strong predictive power on education. Column (1) reports that the estimated returns to education for employees are 13.9%, very close to the estimates using early smoking. By comparison, the returns to education for the self-employed in column (3) are lower than for employees using reported information. This is also consistent with results using early smoking as the instrument. We find similar results in Table 4.6b column (5) which uses inferred income from the quadratic Engel curve.

A concern is that the subsample analysis above is not valid if individuals with higher or lower education systematically select into self-employment. We check if this is the case by estimating the impact of education on the self-employment status using OLS and IV. Table 4.7 presents the results. The coefficients on education is small and insignificant in all specifications. These results do not support the notion that education has an effect of increasing the propensity to become self-employed. This is in line with the evidence in existing literature that the impact of education on selection into self-employment is insignificant (see Van der Sluis, van Praag and Vijverberg 2008, for a survey of findings).

One reason previous literature distinguishes between employed and self-employed workers is that it may shed light on the screening hypothesis, an alternative to human capital theory for the explanation of the association between earnings and education (Spence 1973; Weiss 1995). The strong screening hypothesis argues that education, does not enhance productivity, but signals it. A weaker version of the screening hypothesis concedes that education may augment productivity beyond its primary role signalling. Previous literature argues that self-employment provides a setting for testing the screening hypothesis, as at least in some occupations, education has little signalling value since self-employed workers know their own productivity.[46] Thus the difference in the rates of returns to education between employed and self-employed workers may be seen as the signalling value of education. This

[46]It can be argued that education can still be of signalling value to customers, particularly in professional occupations like lawyers and accountants.

test hinges on an assumption that individuals make the choice to be self-employed at the same time as their education decision (Chevalier et al. 2004). Therefore, individuals who plan to become self-employed do not have as large an incentive to invest in education as those who plan to find employed work (Brown and Sessions 1999). However, as we have demonstrated, education does not appear to affect entry into self-employment in our data. Thus, this is likely to suggest that self-employment may not be the ideal setting for testing the screening hypothesis.

4.6 Conclusions

Self-employment is an important part of the economy. UK in particular has seen a surge in self-employment since 2000. Yet our understanding of self-employment regarding its causes and consequences remains limited. In part, this is due to the lack of reliable self-employment income data. Previous literature has offered evidence that self-employed workers under-report income to tax authorities and survey collectors. But it was less known how income under-reporting varies with household and individual characteristics, and what implications this has on related questions. This chapter takes a step in filling in this gap by making a connection between two strands of literature on income under-reporting and returns to education.

Using data from two UK surveys, I infer self-employment income from the food Engel curve. Consistent with previous literature, I find self-employed households under-report their income. On average, the self-employment income at the individual level should be multiplied by a factor of 1.4 to arrive at their actual earnings. More importantly, I find that the extent of under-reporting varies across the income distribution. This is a feature that has been missing in the literature. Lower-income households under-report their income more heavily than higher-income households. The results are robust to linear and quadratic functional forms of the Engel curve and are consistent in both surveys.

Applying the inferred income information to estimating returns to education, I find using reported self-employment income from surveys severely biases the results upwards. In the instrumental variable (IV) estimation using early smoking as the instrument, reported earnings lead to an estimated rate of returns of 11.0% for an additional year of schooling. Corrected for income reporting, the estimates fall to 5.1–7.2%. In another IV estimation using parental qualification as the instrument, the estimates are also reduced using inferred income. The comparison between the self-employed and employees suggest the self-employed extract lower returns from education. However, these relatively lower returns for the self-employed do not necessarily provide support for the screening hypothesis, as investment in education and entry into self-employment do not seem to be correlated.

Data are an indispensable ingredient to empirical research. A broader conclusion is that we as researchers should be more aware of unreliable information in survey data. Other than income information, respondents may also provide biased responses in other situations. Data collectors should try to devise more incentive mechanisms to obtain reliable data.

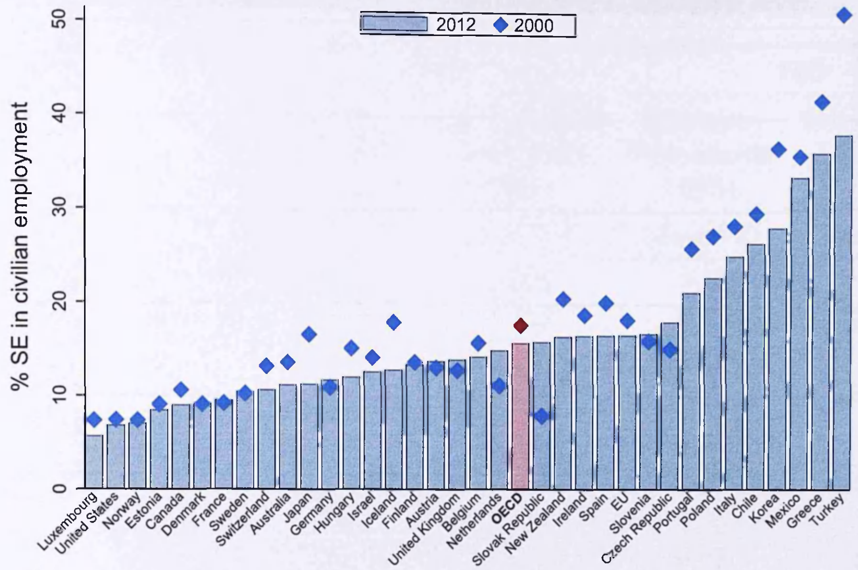


Figure 4.1: Proportion of self-employment in civilian employment: OECD coutries, 2000 and 2012

Source: OECD Annual Labour Force Statistics

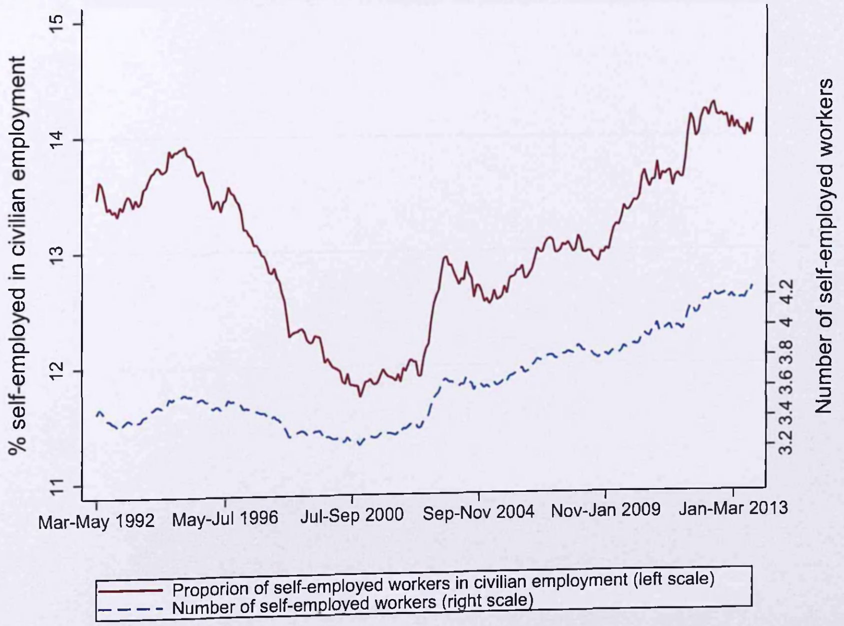


Figure 4.2: Trend in self-employment: UK, 1992–2013

Source: UK Office for National Statistics, Labour Market Statistics, September 2013

Table 4.1a: Summary statistics at the household level

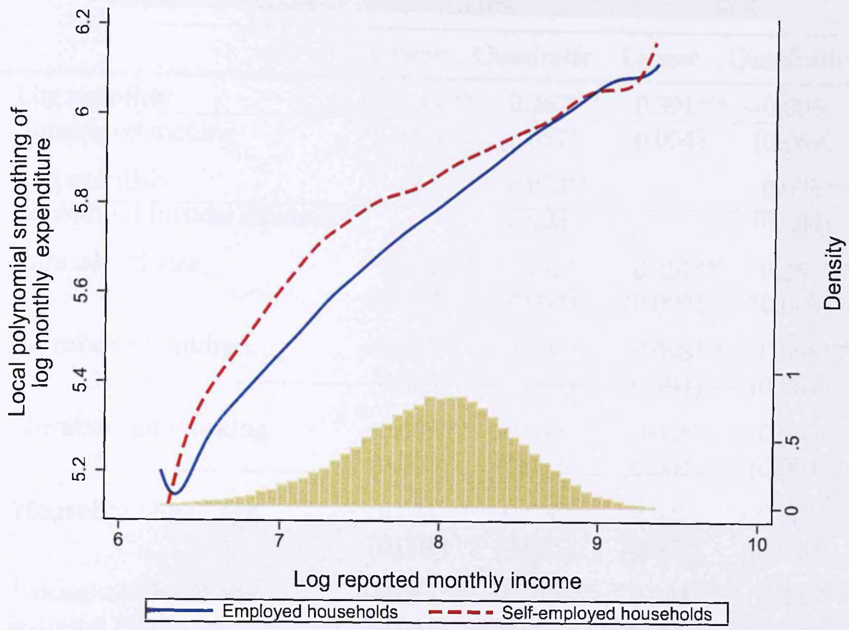
	BHPS		FES	
	Employee Households (82%)	Self-employed Households (18%)	Employee Households (83%)	Self-employed Households (17%)
	Mean/SD	Mean/SD	Mean/SD	Mean/SD
Log monthly household income	7.902	7.872	7.992	7.980
	(0.546)	(0.616)	(0.552)	(0.609)
Log monthly food expenditure	5.754	5.902	5.820	5.972
	(0.480)	(0.456)	(0.566)	(0.551)
Household size	2.867	3.155	2.742	3.007
	(1.275)	(1.366)	(1.295)	(1.312)
Number of children	0.705	0.825	0.679	0.767
	(0.985)	(1.103)	(0.987)	(1.049)
Number not working	1.126	1.245	1.082	1.122
	(1.205)	(1.318)	(1.195)	(1.225)
Household head age	42.230	45.346	35.411	37.693
	(12.271)	(11.658)	(18.289)	(19.400)
Observations	63,546	13,659	54,088	10,724

Note Standard deviations are in parentheses.

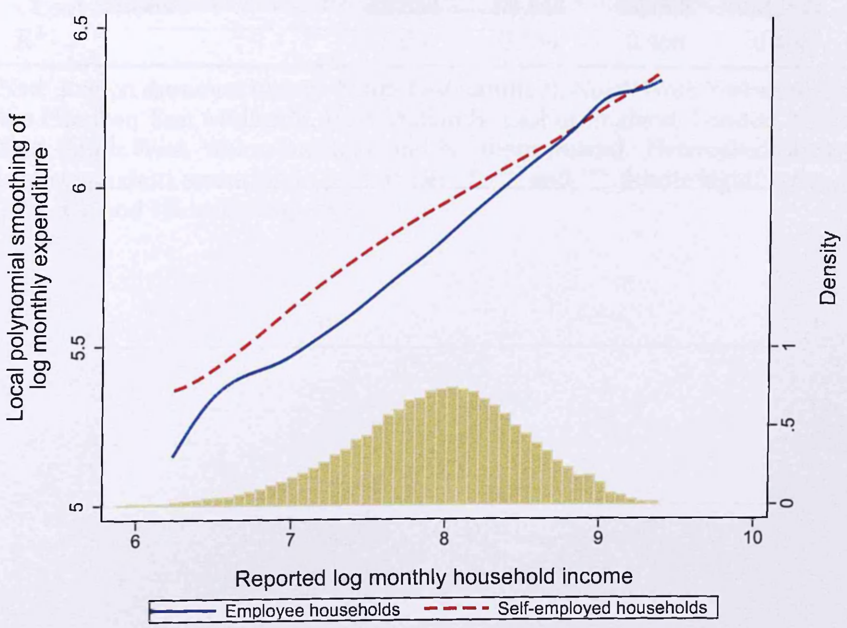
Table 4.1b: Summary statistics at the individual level: BHPS 1991–2008

	Employee (91%)		Self-employed (9%)	
	Mean	SD	Mean	SD
Log hourly pay	2.214	(0.445)	2.023	(0.581)
Weekly working hours	43.103	(8.794)	49.067	(13.161)
Years of education	12.979	(3.010)	12.667	(2.976)
Male	0.589	(0.492)	0.847	(0.360)
Age	38.925	(11.071)	43.252	(10.624)
Smoking at 14[1]	0.130	(0.336)	0.141	(0.349)
Parent further education and above[2]	0.629	(0.483)	0.582	(0.493)
Observations	73,475		7,385	

Note Standard deviations are in parentheses.
[1] Numbers of observations for this variable are 56,246 for employees and 5,825 for the self-employed respectively.
[2] Numbers of observations for this variable are 56,224 for employees and 5,657 for the self-employed respectively.



(a) BHPS 1991–2008



(b) FES 1994–2009

Figure 4.3: Nonparametric estimates of the food Engel curve for employed and self-employed households

Table 4.2: Food Engel curve estimation using employee household data

	BHPS		FES	
	Linear	Quadratic	Linear	Quadratic
Log monthly household income	0.243***	−0.262***	0.391***	−0.008
	(0.003)	(0.052)	(0.004)	(0.069)
Log monthly household income squared		0.033***		0.025***
		(0.003)		(0.004)
Household size	0.195***	0.193***	0.257***	0.257***
	(0.002)	(0.002)	(0.003)	(0.003)
Number of children	−0.006**	−0.006*	−0.098***	−0.098***
	(0.003)	(0.003)	(0.004)	(0.004)
Number not working	−0.009***	−0.008***	−0.006	−0.005
	(0.003)	(0.003)	(0.004)	(0.004)
Household head age	0.021***	0.020***	−0.001*	−0.001
	(0.001)	(0.001)	(0.000)	(0.000)
Household head age squared/100	−0.015***	−0.015***	0.003***	0.003***
	(0.001)	(0.001)	(0.000)	(0.000)
Region dummies	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Observations	63,544	63,544	54,088	54,088
R^2	0.457	0.458	0.468	0.468

Note Region dummies include North East (omitted), North West, Yorkshire and the Humber, East Midlands, West Midlands, East of England, London, South East, South West, Wales, Scotland, and Northern Ireland. Heteroskedasticity-robust standard errors are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively.

Table 4.3a: Income under-reporting from self-employed households

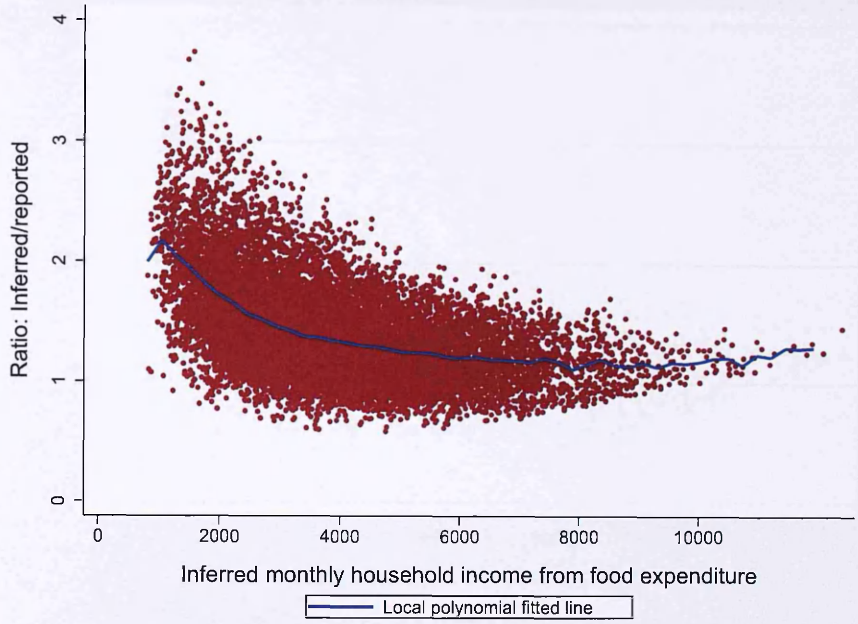
	BHPS			FES		
	Reported	Inferred	Difference	Reported	Inferred	Difference
Panel A: Inferred from linear Engel Curve						
Mean	3,120	3,938	818	3,484	4,161	676
Standard deviation	(1,798)	(1,678)		(2,080)	(2,003)	
1st quartile	1,785	2,701	454	1,966	2,686	454
Median	2,760	3,676	844	2,981	3,776	700
3rd quartile	4,066	4,897	1,265	4,498	5,202	961
Observations	13,659	13,658	13,658	10,724	10,724	10,724
Panel B: Inferred from quadratic Engel Curve						
Mean	3,120	3,867	747	3,484	4,118	634
Standard deviation	(1,798)	(1,443)		(2,080)	(1,797)	
1st quartile	1,785	2,814	421	1,966	2,779	418
Median	2,760	3,745	849	2,981	3,905	722
3rd quartile	4,066	4,792	1,251	4,498	5,209	1,004
Observations	13,659	13,658	13,658	10,724	10,724	10,724

Note All income data are deflated to 2008 terms.

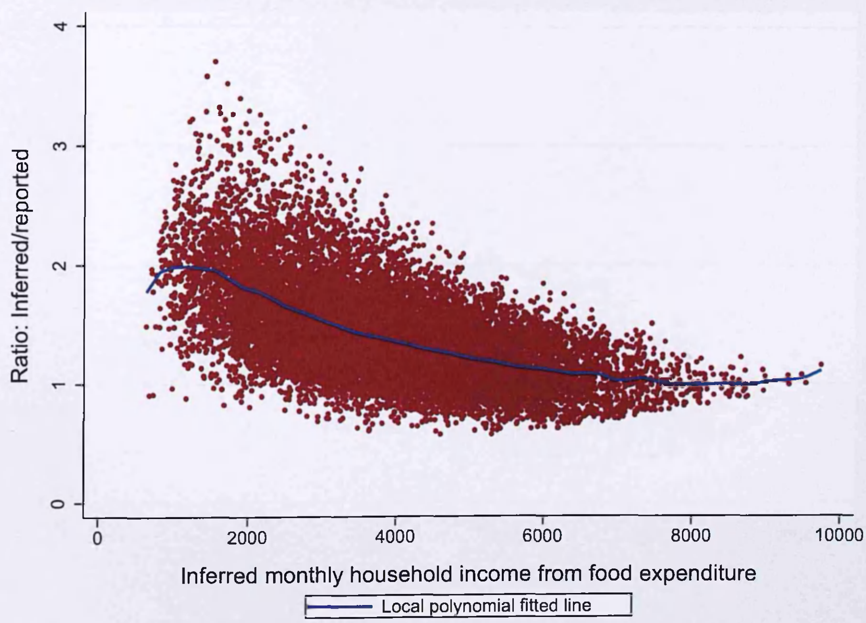
Table 4.3b: Income under-reporting from self-employed individuals

	Linear			Quadratic		
	Reported	Inferred	Difference	Reported	Inferred	Difference
Mean	1,849	2,640	791	1,849	2,577	728
1st quartile	(1,156)	(1,261)		(1,156)	(1,165)	
	1,042	1,732	351	1,042	1,742	323
Median	1,530	2,420	772	1,530	2,411	762
3rd	2,368	3,284	1,235	2,368	3,210	1,216
Observations	7,385	7,385	7,385	7,385	7,385	7,385

Note All income data are deflated to 2008 terms.



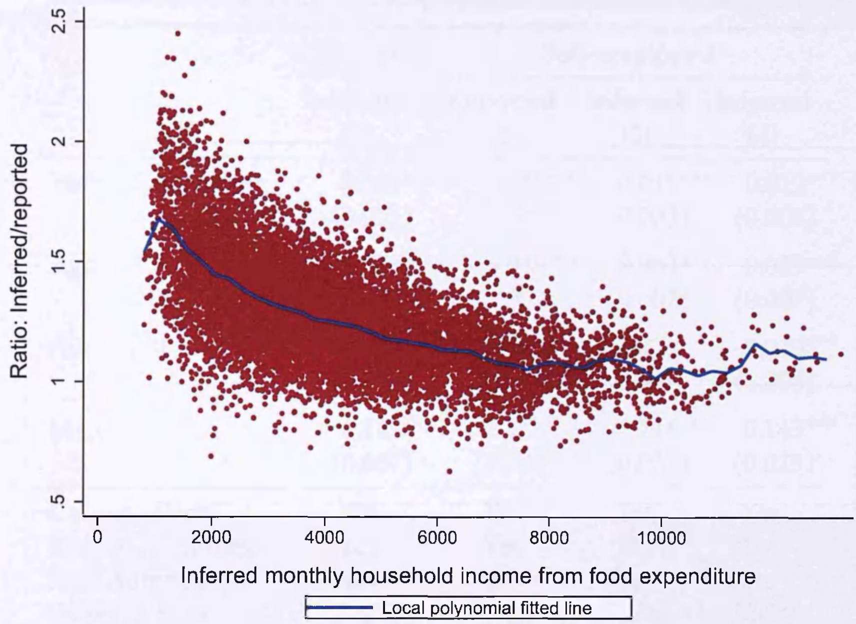
(a) BHPS, imputed using linear Engel curve



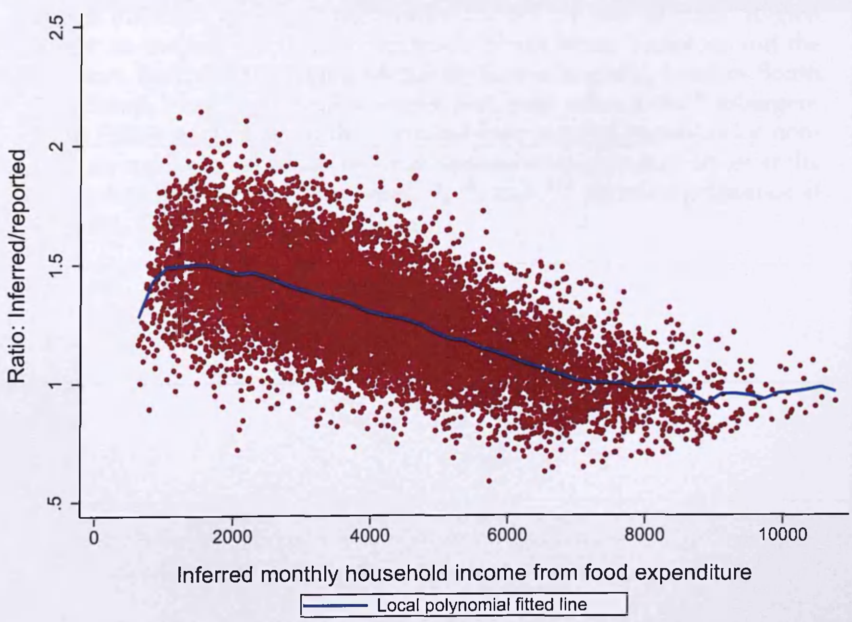
(b) BHPS, imputed using quadratic Engel curve

Figure 4.4: Income under-reporting gap for the self-employed: BHPS 1991–2008

Note Solid lines are the local mean polynomial fit using the Epanechnikov kernel with a rule-of-thumb bandwidth.



(a) FES, imputed using linear Engel curve



(b) FES, imputed using quadratic Engel curve

Figure 4.5: Income under-reporting gap for the self-employed: FES 1994–2009

Note Solid lines are the local mean polynomial fit using the Epanechnikov kernel with a rule-of-thumb bandwidth.

Table 4.4: OLS estimates of returns to education by employment status

	Employee	Self-employed		
	Reported	Reported	Inferred	Inferred
	(1)	(2)	(3)	(4)
Years of education	0.043***	0.021***	0.015***	0.012***
	(0.001)	(0.004)	(0.003)	(0.003)
Age	0.073***	0.038***	0.032***	0.029***
	(0.002)	(0.008)	(0.007)	(0.007)
$Age^2/100$	−0.072***	−0.041***	−0.020***	−0.020***
	(0.003)	(0.009)	(0.007)	(0.008)
Male	0.157***	0.164***	0.116***	0.143***
	(0.007)	(0.029)	(0.025)	(0.025)
Cohort effects	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Observations	73,475	7,385	7,385	7,385

Note Columns (1) and (2) use reported income. Column (3) uses inferred self-employment income from a linear Engel curve. Column (4) uses inferred self-employment income from a quadratic Engel curve. Cohort effects include a quadratic polynomial control for year of birth. Region dummies include North East (omitted), North West, Yorkshire and the Humber, East Midlands, West Midlands, East of England, London, South East, South West, Wales, and Scotland. *F*-statistic refers to the Kleibergen-Paap (2006) rk-statistic on the excluded instrumental variables for non-i.i.d. errors. Heteroskedasticity- and cluster-robust standard errors at the individual level are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively.

Table 4.5a: IV estimates of returns to education by employment status using early smoking as the instrument and inferred self-employment income from linear Engel curve as the dependent variable

	Employee		Self-employed		Self-employed (Inferred)	
	IV	First stage	IV	First stage	IV	First stage
	(1)	(2)	(3)	(4)	(5)	(6)
Years of education	0.152***		0.110**		0.072**	
	(0.016)		(0.044)		(0.034)	
Smoking at 14		−1.089***		−0.927***		−0.927***
		(0.104)		(0.276)		(0.276)
Age	0.063***	0.115***	0.041***	0.035	0.024***	0.035
	(0.004)	(0.018)	(0.012)	(0.060)	(0.009)	(0.060)
$Age^2/100$	−0.060***	−0.132***	−0.048***	0.028	−0.022**	0.028
	(0.004)	(0.022)	(0.012)	(0.070)	(0.009)	(0.070)
Male	0.191***	−0.158*	0.196***	−0.334	0.128***	−0.334
	(0.013)	(0.082)	(0.047)	(0.262)	(0.036)	(0.262)
Cohort effects	Yes	Yes	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	56,246	56,246	5,825	5,825	5,825	5,825
F-statistic		890.81		68.00		68.00

Note Cohort effects include a quadratic polynomial control for year of birth. Region dummies include North East (omitted), North West, Yorkshire and the Humber, East Midlands, West Midlands, East of England, London, South East, South West, Wales, and Scotland. F-statistic refers to the Cragg-Donald F-statistic on the excluded instrumental variable. Heteroskedasticity- and cluster-robust standard errors at the individual level are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively.

Table 4.5b: IV estimates of returns to education by employment status using early smoking as the instrument and inferred self-employment income from quadratic Engel curve as the dependent variable

	Employee		Self-employed		Self-employed (Inferred)	
	IV	First stage	IV	First stage	IV	First stage
	(1)	(2)	(3)	(4)	(5)	(6)
Years of education	0.152***		0.110**		0.051	
	(0.016)		(0.044)		(0.032)	
Smoking at 14		−1.089***		−0.927***		−0.927***
		(0.104)		(0.276)		(0.276)
Age	0.063***	0.115***	0.041***	0.035	0.023***	0.035
	(0.004)	(0.018)	(0.012)	(0.060)	(0.009)	(0.060)
$Age^2/100$	−0.060***	−0.132***	−0.048***	0.028	−0.021**	0.028
	(0.004)	(0.022)	(0.012)	(0.070)	(0.009)	(0.070)
Male	0.191***	−0.158*	0.196***	−0.334	0.156***	−0.334
	(0.013)	(0.082)	(0.047)	(0.262)	(0.033)	(0.262)
Cohort effects	Yes	Yes	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	56,246	56,246	5,825	5,825	5,825	5,825
F-statistic		890.81		68.00		68.00

Note Cohort effects include a quadratic polynomial control for year of birth. Region dummies include North East (omitted), North West, Yorkshire and the Humber, East Midlands, West Midlands, East of England, London, South East, South West, Wales, and Scotland. F-statistic refers to the Cragg-Donald F-statistic on the excluded instrumental variable. Heteroskedasticity- and cluster-robust standard errors at the individual level are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively.

Table 4.6a: IV estimates of returns to education by employment status using parental qualifications as the instrument and inferred self-employment income from linear Engel curve as the dependent variable

	Employee		Self-employed		Self-employed (Inferred)	
	IV	First stage	IV	First stage	IV	First stage
	(1)	(2)	(3)	(4)	(5)	(6)
Years of education	0.139***		0.059**		0.032	
	(0.022)		(0.028)		(0.022)	
Parent further education and above		0.631***		1.108***		1.108***
		(0.092)		(0.223)		(0.223)
Age	0.059***	0.157***	0.038***	0.092	0.017*	0.092
	(0.005)	(0.018)	(0.011)	(0.060)	(0.009)	(0.060)
$Age^2/100$	−0.054***	−0.185***	−0.039***	−0.041	−0.015	−0.041
	(0.006)	(0.022)	(0.011)	(0.065)	(0.009)	(0.065)
Cohort effects	Yes	Yes	Yes	Yes	Yes	Yes
Parents occupation	Yes	Yes	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	56,224	56,224	5,657	5,657	5,657	5,657
F-statistic		527.11		170.68		170.68

Note Cohort effects include a quadratic polynomial control for year of birth. Parents' occupation dummies include father's and mother's 10 occupational classes when the respondent was aged 14: not working or not present (omitted group), managers and administrators, professional, associate professional and technical, clerical and secretarial, craft and related, personal and protective service, sales, plant and machine operatives, and other occupations. Region dummies include North East (omitted), North West, Yorkshire and the Humber, East Midlands, West Midlands, East of England, London, South East, South West, Wales, and Scotland. F-statistic refers to the Cragg-Donald F-statistic on the excluded instrumental variable. Heteroskedasticity- and cluster-robust standard errors at the individual level are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively.

Table 4.6b: IV estimates of returns to education by employment status using parental qualifications as the instrument and inferred self-employment income from quadratic Engel curve as the dependent variable

	Employee		Self-employed		Self-employed (Inferred)	
	IV	First stage	IV	First stage	IV	First stage
	(1)	(2)	(3)	(4)	(5)	(6)
Years of education	0.139***		0.059**		0.030	
	(0.022)		(0.028)		(0.022)	
Parent further education and above		0.631***		1.108***		1.108***
		(0.092)		(0.223)		(0.223)
Age	0.059***	0.157***	0.038***	0.092	0.015	0.092
	(0.005)	(0.018)	(0.011)	(0.060)	(0.009)	(0.060)
$Age^2/100$	−0.054***	−0.185***	−0.039***	−0.041	−0.014	−0.041
	(0.006)	(0.022)	(0.011)	(0.065)	(0.009)	(0.065)
Cohort effects	Yes	Yes	Yes	Yes	Yes	Yes
Parents occupation	Yes	Yes	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	56,224	56,224	5,657	5,657	5,657	5,657
F-statistic		527.11		170.68		170.68

Note Cohort effects include a quadratic polynomial control for year of birth. Parents' occupation dummies include father's and mother's 10 occupational classes when the respondent was aged 14: not working or not present (omitted group), managers and administrators, professional, associate professional and technical, clerical and secretarial, craft and related, personal and protective service, sales, plant and machine operatives, and other occupations. Region dummies include North East (omitted), North West, Yorkshire and the Humber, East Midlands, West Midlands, East of England, London, South East, South West, Wales, and Scotland. F-statistic refers to the Cragg-Donald F-statistic on the excluded instrumental variable. Heteroskedasticity- and cluster-robust standard errors at the individual level are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively.

Table 4.7: OLS and IV estimation of education on selection into self-employment

	Early Smoking		Parental Qualification	
	OLS (1)	IV (2)	OLS (3)	IV (4)
Years of education	−0.001	0.005	−0.001	0.003
	(0.001)	(0.009)	(0.001)	(0.004)
Age	−0.008	−0.009	−0.007	−0.008
	(0.014)	(0.014)	(0.014)	(0.005)
$Age^2/100$	−0.003	−0.002	−0.003	−0.003*
	(0.002)	(0.003)	(0.002)	(0.002)
Cohort effects	Yes	Yes	Yes	Yes
Parents occupation	No	No	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Observations	51,122	51,122	50,956	50,956

Note Cohort effects include a quadratic polynomial control for year of birth. Region dummies include North East (omitted), North West, Yorkshire and the Humber, East Midlands, West Midlands, East of England, London, South East, South West, Wales, and Scotland. Parents' occupation dummies include father's and mother's 10 occupational classes when the respondent was aged 14: not working or not present (ommited group), managers and administrators, professional, associate professional and technical, clerical and secretarial, craft and related, personal and protective service, sales, plant and machine operatives, and other occupations. Heteroskedasticity-robust standard errors are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% levels respectively.

Chapter 5

Concluding Remarks

In this thesis I have addressed three separate yet related topics on the economics of education. Chapter 2 estimates the price premium parents place on school quality through the housing market, and tries to pin down which aspects of school quality parents value. Chapter 3 seeks to understand how effective school capital investments are in raising student academic achievement. Chapter 4 extends the literature on the economic returns to education in the labour market by focusing on the self-employed, with an emphasis on correcting for income misreporting in survey data that could lead to biased conclusions.

In Chapter 2, I find that a one school-level standard deviation increase in school quality raises non-flat house prices by 2.1%–2.5%. This implies one student-level standard deviation increase in academic performance will raise house prices by roughly 10%. This estimate appears large, but is plausible. This is equivalent to about 3.5 years of private school fees. As the compulsory secondary school phase lasts 5 years in the UK, this house price premium alone is not enough to drive parents to opt for private schools.

One policy implication is that school choice programmes are unlikely to achieve equal education opportunities for disadvantaged students, as long as there remains some link between school admission and the students' residential locations. This "selection by mortgage" is driven by parents' preferences for better school quality. As long as these preferences exist, parents will compete for better schools one way or another.

Another finding is that parents value school academic effectiveness more than they value school composition. The estimates could provide some reference for the cost-benefit analysis of education policies that aim to raise academic standards.

Chapter 3 evaluates the short-run effect of a large school construction programme, BSF, on student academic achievement. I find strong evidence that BSF has large effects on disadvantaged students and no effect on more advantaged students. This speaks to education polices in the sense that targeting resources on the disadvantaged group could prove more effective.

Chapter 3 is limited by the time scope of the study. Future work might consider the medium-run and long-run effects of school capital investments. Considering the large scale of the programme, it will be interesting to consider the long-run effects in a general equilibrium framework.

Chapter 4 confirms the previous findings that the self-employed under-report their income in surveys, and presents a new finding to the literature that the extent of under-reporting varies across the income distribution. Lower-income households under-report their income more heavily than higher-income households. This speaks to research that relies on self-employment income data. In the analysis of returns to education for the self-employed, I show that the estimates are severely biased upwards due to income under-reporting.

Future work might consider accounting for income under-reporting in other settings than returns to educations. More broadly, it is also important to pay attention to other survey responses that might be systematically biased.

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