

# **Evaluation of an OFT intervention**

Independent fee-paying schools

May 2012

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# 1 EXECUTIVE SUMMARY

- 1.1 The Office of Fair Trading (OFT) has a public commitment to evaluate each year at least one of its previous interventions. These evaluations help us to understand whether and how our projects have achieved the desired impact, and whether the outcomes could be further improved. The OFT relies on findings from such evaluations to learn lessons that can be applied to future comparable interventions.
- 1.2 In this context, the OFT's evaluation team has evaluated the impact of the intervention addressing the anti-competitive practice of 50 independent fee-paying schools in the setting of fees during academic years 2001/02 to 2003/04. This research has been carried out by OFT economists and independently reviewed by Professor Stephen Davies.<sup>1</sup>
- 1.3 The main aim is to understand whether the OFT intervention had an impact, and to estimate this impact in terms of reduced school fees. To do so we have collected data on the evolution of school fees and other variables before and after the OFT's intervention.

## Background

- 1.4 For the academic years 2001/02 – after the Competition Act came into force – to 2003/04, the OFT held that the exchange of future pricing information between the Sevenoaks Survey schools 'had as its **object** the distortion of competition within the United Kingdom'.<sup>2</sup> It was not necessary therefore for the OFT to come to a conclusion as to whether the information exchange had an anti-competitive effect.
- 1.5 The schools concerned had exchanged information relating to their intended fee increases and fee levels for boarding and day pupils in relation to the academic years 2001/02, 2002/03 and 2003/04. The

<sup>1</sup> Stephen Davies is Professor of Economics at the University of East Anglia

<sup>2</sup> OFT (2006) *'Exchange of Information on Future Fees by Certain Independent, Fee-Paying Schools'*, (See paragraph 1402)

information was exchanged through a survey, known as the 'Sevenoaks Survey'. Between February and June of each year, the schools concerned gave details of their intended fee increases and fee levels for the academic year beginning in September. Sevenoaks then collated that information and circulated it, in the form of tables, to the schools concerned. The information in the tables was updated and circulated between four and six times each year as schools developed their fee increase proposals in the course of their annual budgetary processes.

1.6 The key features of the infringement that were instrumental in the OFT's assessment of the information exchange as an object offence included:

- The information that was exchanged related to future intentions of price, and was confidential and not publicly available.
- It was done on a regular and highly systematic basis, and for a number of years.
- The timing of the exchange corresponded with the timing in which school fees for the following year were set.

1.7 The economic literature suggests that the exchange of future confidential pricing intentions is likely to result in harm for consumers and reduce overall welfare. The primary way in which this can happen is through the facilitation of coordination. Market participants coordinating either explicitly or tacitly over prices may be able to charge higher prices for the goods and services they supply.

1.8 In theory, the exchange of confidential future information about changes in school fees may have resulted in higher school fees. If the OFT's intervention in stopping the information exchange did have an impact, we would expect this to be manifest in a reduction of school fees compared with the counterfactual of no OFT intervention. Therefore, the primary focus of the analysis in this report is on identifying whether there is evidence that school fees have fallen as a result of the intervention.

## Methodology

- 1.9 This evaluation has relied principally on data on school fees, rankings and other characteristics from online 'FT 500'<sup>3</sup> annual rankings and from the 'Best-Schools'<sup>4</sup> datasets.
- 1.10 We have analysed fees data over time, looking at the evolution of fee levels and annual fee changes for the schools involved in the Sevenoaks Survey ('SS Schools') before and after the OFT's intervention. To control for other factors that may also have had an impact on the determination of fee setting, most notably the economic downturn, we have been able to compare fees to those set by a sub-group of schools not involved in the Sevenoaks Survey ('Non-SS Schools'). Further, we have used 'difference-in-differences' econometric analysis to exploit the Non-SS Schools as a control group in testing whether the intervention had a statistically significant impact on the fees that the SS Schools charged.

## Empirical findings

- 1.11 The key finding of the econometric analysis is that since OFT intervention, boarding fees have been on average 1.6 per cent lower, and day fees 1.5 per cent lower, than we would expect in absence of intervention. The result for boarding fees is statistically significant at the 95 per cent level, and robust to a range of sensitivity checks. The result for day fees was less statistically significant<sup>5</sup> and less robust to sensitivity checks.
- 1.12 We have used the results of the econometric analysis to present best estimates of benefits to the consumer arising from lower school fees.

<sup>3</sup> FT annual rankings are found for instance here: [www.ft.com/reports/schools2006](http://www.ft.com/reports/schools2006)

<sup>4</sup> See [www.Best-Schools.co.uk](http://www.Best-Schools.co.uk)

<sup>5</sup> The boarding fees findings are statistically significant at the 95 per cent level (meaning there is less than a five per cent probability that there was no effect). The day fees findings are only statistically significant at the 90 per cent level, and unlike the boarding fees results are not robust to sensitivity tests.

Total discounted savings to the consumer<sup>6</sup> can be estimated (in 2010 prices, discounted to the present) as

- £85m in boarding fees
- £20m in day fees.

1.13 Given the high degree of confidence placed in the findings in relation to boarding fees, the analysis therefore suggests that OFT intervention has realised savings of at least £85m to the consumer in the post-intervention period 2004/05 to the present, equating to average savings of £495 per boarder per annum.

1.14 There are a number of reasons for regarding the estimate of consumer savings as conservative. The first is that, in using the Non-SS Schools as a control group for the econometric analysis, we have implicitly assumed that this group was not impacted by the OFT intervention. We consider that this assumption supports the conservativeness of the estimate on the grounds that the OFT intervention is likely to have had an indirect impact on the Non-SS Schools, as we would expect the majority of them to be in competition with SS Schools. To the extent that the OFT intervention led to a reduction in SS Schools fees, we would expect competition between SS Schools and Non-SS Schools to create incentives for Non-SS Schools – and other independent fee-paying schools not considered in this analysis – to respond by in turn lowering their own fees. A second reason is that to the extent that OFT intervention had an industry-wide effect, the difference in differences methodology will underestimate the impact of intervention in lowering fees of SS Schools.

1.15 In relation to the two reasons above, we note that average annual real fee changes decreased significantly and steadily following OFT intervention for both SS Schools and Non-SS Schools, from approximately seven per cent during the infringement to four per cent in

<sup>6</sup> See Annexe F for calculation of point estimate and range of consumer benefits.

the four-year period immediately after intervention (before the onset of the financial crisis and recession). While this may be explained by other factors, it would also be consistent with a broad cross-industry impact of OFT intervention.

- 1.16 The third reason is that the analysis does not attempt to quantify the deadweight loss averted of OFT intervention,<sup>7</sup> focusing only on the transfer of surplus back from schools to the consumer. The final reason is that the estimate does not take account of the wider deterrence effects of this competition enforcement action across the economy. In this respect, the OFT has published previous research which finds evidence of significant deterrent effects.<sup>8</sup>

<sup>7</sup> See Annexe F for discussion of estimation of deadweight loss averted.

<sup>8</sup> OFT (2011) 'The Impact of Competition Interventions on Compliance and Deterrence' [www.offt.gov.uk/shared\\_offt/reports/Evaluating-OFTs-work/oft1391.pdf](http://www.offt.gov.uk/shared_offt/reports/Evaluating-OFTs-work/oft1391.pdf). The research identifies and quantifies the wider benefits and costs associated with deterrence resulting from enforcement activities undertaken by the OFT. For every case investigated it estimates deterrence ratios of 40 in relation to 'other commercial agreements' (including information exchange).



## 2 INTRODUCTION

### Objectives of the research

- 2.1 This report considers the impact of the OFT intervention addressing the anti-competitive conduct of 50 independent fee-paying schools, who shared information concerning the setting of fees during academic years 2001/02 to 2003/04. The objective of the evaluation is to understand whether and how the intervention had an impact, and to ensure that lessons are learned for future interventions. The paper focuses predominately on the impact of the OFT's investigation on prices ('fees').

### Background

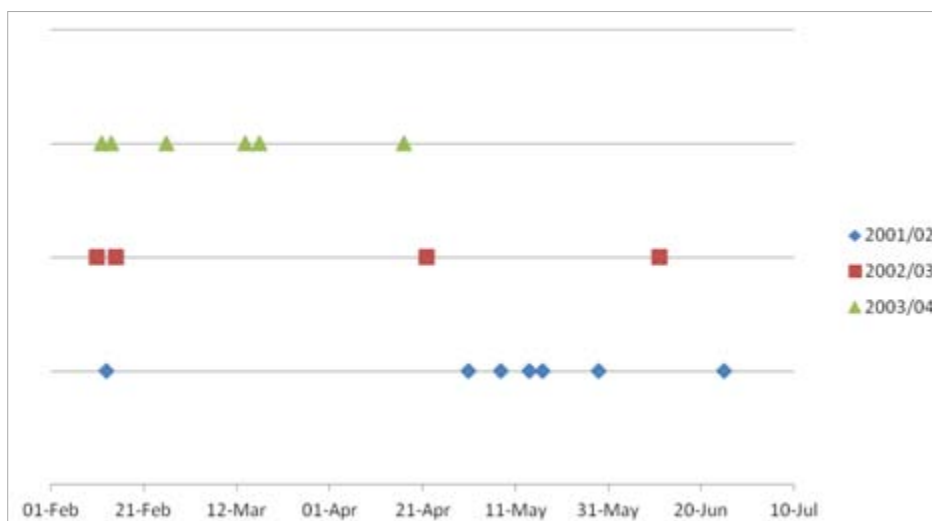
- 2.2 The OFT held that the exchange of future pricing information between the Sevenoaks Survey schools for the academic years between 2001/02 – after the Competition Act came into force – and 2003/04, 'had as its **object** the distortion of competition within the United Kingdom'.<sup>9</sup> It was not necessary therefore for the OFT separately to assess and come to a conclusion as to whether the information exchange had an anti-competitive effect.
- 2.3 The schools concerned had exchanged information relating to their intended fee increases and fee levels for boarding and day pupils. The information was exchanged through a survey, known as the 'Sevenoaks Survey'.
- 2.4 Each year from 1997 until June 2003, the Sevenoaks bursar requested and received fee increase information from up to 49 other independent fee-paying schools (see Annexe A for a table of Participant schools). The information, which was collated into the format of a spread-sheet, listed individual schools' fee increases and fee levels for the coming academic year, identifying them as 'fixed' or 'estimated'.

<sup>9</sup> See paragraph 1402 of the Decision

2.5 Between February and June of each year, the schools concerned gave details of their intended fee increases and fee levels for the academic year beginning in September. Sevenoaks then collated that information and circulated it, in the form of tables, to the schools concerned. The information in the tables was updated and circulated between four and six times each year as schools developed their fee increase proposals in the course of their annual budgetary processes. Additional circulations took account of revised or new submissions from the Participant schools as their proposed fee calculations evolved during each of their respective budgetary processes. Typically, a final iteration was circulated in May or June, just as schools were finalising fees.

2.6 Figure 1 shows the recorded circulation dates of the survey in each year of the infringement. It shows that the survey was circulated at least four times each year in the run-up to the setting of fees for the academic years 2001/02 to 2003/04. This compares with the 1997/98 to 2000/01 period when it is reported there were usually two circulations per academic year, one in February and one in May.<sup>10</sup>

**Figure 1: Recorded circulation dates of the Sevenoaks Survey**



<sup>10</sup> See Paragraph 320 of the Decision

2.7 The key features of the infringement that were instrumental in its assessment as an object offence include:

- The information that was exchanged related to future intentions of price, and was confidential and not publicly available. In this respect the OFT noted in particular 'it is hard to envisage what legitimate purpose could be served by the exchange of such information, in particular in circumstances where the information remains otherwise confidential and is not shared with customers'.
- It was done on a regular and highly systematic basis, and for a number of years.
- The timing of the exchange corresponded with the timing in which school fees for the following year were set.

### 3 THEORY, METHODOLOGY AND DATA

#### Theory

- 3.1 Profit maximising firms have an incentive<sup>11</sup> to coordinate across the industry on a jointly profit maximising price structure: by restricting competition and raising prices across the industry towards the monopoly profit maximising level, participating firms can extract greater rents.<sup>12</sup> However, as the effect of such coordination is a loss of welfare to the consumer – these practices are for this reason deemed anti-competitive and prohibited by the Competition Act 1998.
- 3.2 The economic literature suggests that the primary way in which the exchange of certain types of information between competitors is likely to result in harm for consumers and reduce overall welfare is through the facilitation of coordination. Information exchange makes it easier to establish and sustain tacit or explicit coordinated behaviour, and so may allow for higher prices to be charged for the goods and services.
- 3.3 The extent to which information exchanges can lead to adverse effects for consumer and welfare depends both on the form of the information exchanged and the frequency of the exchange.
- 3.4 In relation to the form of information exchanged:
- Individualised information exchange facilitates coordination by enabling monitoring and identification of deviations. In contrast, exchange of aggregated information does not allow for such exact monitoring, and is therefore less likely to facilitate such coordination.

<sup>11</sup> The incentive to coordinate is influenced by market conditions, such as concentration, symmetry, product homogeneity, buyer power, ease of entry, and how parties discount the future.

<sup>12</sup> Charities may be subject to the same incentives to the extent that they aim to generate a surplus when setting prices.

- Exchange of private information has more potential for harm than public, as public data is already available to all market players and is less likely to be of strategic nature.
- Exchange of information on future intentions carries the risk – more so than exchange of historic information – of helping firms to arrive at future focal points, by diminishing the risk of losing sales when enacting them.
- Exchange of individualised, future pricing information is more likely to lead to collusive outcomes than say exchange of cost information, which could even promote the sharing of cost efficiencies.

3.5 In the case of the Sevenoaks Survey, the information exchanged related to future fee intentions which were confidential and not publicly available. This form of information exchange is particularly useful for facilitating coordination over prices and so is most likely to be harmful to consumers.

3.6 The harm associated with the frequency of the information exchange is related to the frequency with which prices are set and the length of contracts. Information that is exchanged sufficiently frequently to enable coordination in each iteration of the price-setting process has more potential for facilitating coordination than a frequency that does not. As frequency is further increased, this enables further fine-tuning in the coordinated price setting process, and further potential for harm. In the case of the SS schools, the exchange was done on a highly systematic and regular basis at the time when schools were to set fees for the next school year.

3.7 A principal way in which the sharing of strategic pricing information can facilitate coordination and thereby engenders harm is through helping

market participants to reach a focal point.<sup>13</sup> It can do this by allowing competitors to communicate where they would like to be, without actually having to commit to the price.<sup>14</sup> For example, firm A might want to increase prices but is unsure how firms B and C will react. By communicating the intended price increase, and observing firm B and C's intentions, firm A can determine whether firms B and C will follow. This is important for firm A because it would risk losing market share and profits when it implemented the price increase if firm B (and/or C) did not increase prices.

- 3.8 Emails in the Sevenoaks Survey suggest that focal points – if they existed – may have included the average increase in fees of the participant schools.<sup>15</sup> In internal emails, the Decision shows at least eight different schools referring to the average increase in fees across the surveyed schools in the process of setting their own fees. Minutes of an internal meeting of school Governors reproduced in the OFT Decision offer a vivid example of how the exchange of such information may have led to higher fees than would otherwise have been the case.

'The Governors reviewed the Sketch budget for the financial year ending 30 June 2002. The major assumptions underpinning the budget were the fee and salary increases.

**The fee assumption was for an increase in the School of 5.5%. Since this assumption was made, it had become apparent that other schools were considering significantly higher increases, certainly in the range 6–8%, this largely engendered by above inflation increases**

<sup>13</sup> See for example Stigler, G (1961) 'A Theory of Oligopoly' *Journal of Political Economy* 72(1), pp 44, 45–46, and Scherer, F, and Ross, D (1990) *Industrial Market Structure and Economic Performance* (Boston, Houghton Mifflin Company, 3rd edn), pp 265

<sup>14</sup> Bennett et al, (2010) 'The Law and Economics of Information Sharing: The Good, the Bad and the Ugly', *European Competition Journal*

<sup>15</sup> See the Decision

in the State sector coupled to the introduction of performance related pay...

... Decisions. It was agreed that:

a. The budget for 2001/2002 must be referred back to the F&GPC at their meeting on 18 May but in the meantime, the Chairman should write to parents to say that the **termly boarding fee for the next academic year would be of the order of £5,900, ie 8.2%**. This would give the F&GPC some room for manoeuvre when addressing the budget...<sup>16</sup>

3.9 Besides making it easier to establish coordinated behaviour in the first place, economic theory predicts that, where a certain extent of coordination is already present, information sharing can

- facilitate internal stability of coordination, by enabling the monitoring of competitors and formulation of precise punishments
- facilitate external stability of collusion by helping to detect new market entrants and coordinate against them.

3.10 These latter aspects are less relevant in this case. The ability to monitor was already enabled by the fact that actual fees were publicly observable once set, and the threat from any new entrant was relatively weak in such an un-concentrated market. They are therefore not discussed in more detail.<sup>17</sup>

3.11 To summarise, the economic literature suggests that the exchange of confidential future information may result in higher prices than would otherwise be the case. Therefore, if the OFT's intervention in stopping the information exchange was effective, we would expect this to be

<sup>16</sup> See paragraph 977 of the Decision (our emphasis added)

<sup>17</sup> For a discussion see Bennett, M et al, (2010) 'The Law and Economics of Information Sharing: The Good, the Bad and the Ugly', *European Competition Journal*

manifest in lower school fees. Therefore, the primary focus of the subsequent analysis is on identifying whether there is evidence that school fees have fallen as a result of the intervention compared to the counterfactual of 'no OFT intervention'.

## Methodology

3.12 The primary focus of the analysis is on whether the OFT intervention had an impact on the level of SS School fees. As a first step in this analysis, we can simply compare fees before and after the OFT intervention at the participating schools. Any reduction in the fee levels we observe may be explained (at least partly) by the OFT's intervention. To facilitate this, we define two time periods:

- pre-OFT intervention: academic years 2001/02 to 2003/04, and
- post-OFT intervention: academic years 2004/05 to 2011/12.

3.13 This before and after comparison considers both differences in fee levels pre and post intervention and differences in annual fee changes. As noted above, given the nature of the exchange, annual fee increases appear more likely to have constituted focal points than the fee levels themselves.

3.14 To control for other factors that may also have had an impact on the determination of fee setting, we have been able to compare fees to those set by a sub-group of schools not involved in the Sevenoaks Survey ('Non-SS Schools'). Non-SS Schools are the 178 schools from the Best-Schools dataset from 2006/07 that were not party to the infringement. Non-SS Schools are listed in Annexe A.

3.15 We have used 'difference-in-differences' econometric analysis using the Non-SS Schools as a control group to test whether the intervention had a statistically significant impact on the fees that the SS Schools charged. This technique builds on the assumption that while all of these schools are likely to share broadly the same exogenous influences over time, only SS Schools were subject to the OFT intervention. Difference in differences analysis allows for an assessment of the impact of the



intervention by considering the difference between how the treatment group (SS Schools) and the control group (Non-SS schools) have changed fee-setting after the OFT intervention. It is discussed in more detail in Annexe D.

3.16 Difference in difference analysis relies on two key assumptions. The first is that schools in the control group were not affected by the OFT intervention. This may be an appropriate starting point, but has the potential to be misleading in the event that either of the following effects materialise

- Indirect impact: we would expect the majority of non-SS schools to be in Bertrand (price) competition with SS Schools. This could entail for example SS Schools serving as price leaders by acting as reference points to Non-SS Schools. If the OFT intervention prompted SS Schools to reduce fees, we would expect competition between SS and Non-SS schools to create incentives for Non-SS Schools also to reduce fees.
- Direct impact: hypothetically, OFT intervention could have stimulated compliance more broadly across the rest of the market.

3.17 To the extent that the control group is directly or indirectly affected by the intervention, as through the possible mechanisms outlined above, the difference in differences technique will, if anything, underestimate the impact of the intervention. Thus, any impact identified by this analysis may be considered as erring on conservative.

3.18 The second assumption – necessary for the causal identification of the intervention impact – is that Non-SS Schools are not subject to different trends and influences that are not accounted for in the econometric specification. This encompasses an assumption that variations in economic growth over time do not impact the two groups differently over time, which in turn largely depends on potential differences in income elasticity of demand between SS Schools and Non-SS Schools. Important considerations in this respect include that

- On average SS Schools charge higher fees than Non-SS Schools,<sup>18</sup> and as a result may price at the more elastic end of the demand curve (if linear). If so, an economic down-turn for instance could be expected to have a greater impact on SS Schools than Non-SS Schools.
- SS Schools include many of the UK's most prestigious schools, and so could plausibly face the less price sensitive portion of demand. This would translate into lower price elasticity for SS Schools compared to Non-SS Schools. If so, an economic down-turn for instance could be expected to have a lesser impact on SS Schools than on Non-SS Schools.

3.19 This analysis takes as a starting point that these considerations – should they materialise – may effectively counterbalance each other out. Sensitivity tests are conducted later to probe the robustness of the results when gross disposable income per head is allowed to influence SS Schools and Non-SS Schools differently.

3.20 This study first employs descriptive analysis to give an introductory overview of the data, before proceeding to more rigorous econometric techniques. The descriptive analysis facilitates visual inspection of the data. The econometric analysis tests the effect of the OFT intervention, controlling for the presence of other factors. This allows for a more rigorous identification of the impact of the OFT's intervention. In particular the analysis seeks to identify an impact of intervention on

- fee levels
  - of SS Schools – whether they fall after OFT intervention
  - of the difference between SS School fees and Non-SS Schools – whether this difference falls after OFT intervention

<sup>18</sup> SS Schools average fees were for instance £1000 higher per term for boarders in 2003/04 than for Non-SS Schools

- annual fee increases
  - of SS Schools – whether they fall after OFT intervention
  - of the difference between SS Schools and Non-SS Schools – whether this difference falls after OFT intervention.

## Data

- 3.21 To ensure comprehensive and accuracy, fees data for schools were compiled from two sources 'FT 500'<sup>19</sup> annual rankings and the 'Best-Schools'<sup>20</sup> datasets. Potential concerns that spurious results could be driven by differences in measurement between the datasets were allayed by sensitivity tests – restricting the time period so that only FT datasets were used – which showed that findings of the analysis were not sensitive to the merging of the two datasets.
- 3.22 Merging the FT and Best-Schools datasets necessitated restricting the sample of Non-SS Schools to the sample of the smaller dataset.<sup>21</sup> Fees data were thus collected for all 50 SS Schools, and for 178 Non-SS Schools.<sup>22</sup> FT data were also collected for other variables for each school, including FT rank, number of pupils, proportion of boarders and region.<sup>23</sup> This was supplemented with GDP deflator data<sup>24</sup> to allow fees

<sup>19</sup> FT annual rankings are found for instance here: [www.ft.com/reports/schools2006](http://www.ft.com/reports/schools2006)

<sup>20</sup> See [www.Best-Schools.co.uk](http://www.Best-Schools.co.uk)

<sup>21</sup> It was felt that this need not raise sample selection issues, as the Best Schools dataset is composed of SS Schools and (arguably) their chief competitors, thereby aiding the process of identifying the appropriate counterfactual group.

<sup>22</sup> See Annexe B for overview of independent schools market

<sup>23</sup> Not accounting for gaps, it is estimated that raw data points in the dataset exceed 25,000.

<sup>24</sup> [www.hm-treasury.gov.uk/data\\_gdp\\_fig.htm](http://www.hm-treasury.gov.uk/data_gdp_fig.htm)

to be adjusted for inflation, as well as gross disposable income per head data<sup>25</sup> for sensitivity analysis.

3.23 There were, however, many gaps in the data. This meant that in each year, information on some schools was lacking, and that for many schools a complete time series of fees covering the entire eleven academic years was not available. In the analysis therefore where data were lacking for any variable during any given year, that data point was excluded from the analysis.<sup>26</sup> This problem, common in panel estimation, need not cast doubts concerning the reliability of the results.<sup>27</sup>

<sup>25</sup> National and regional gross disposable income per head were obtained from [www.ons.gov.uk/ons/rel/regional-accounts/regional-household-income/march-2011/stb-regional-gdhi-march-2011.html](http://www.ons.gov.uk/ons/rel/regional-accounts/regional-household-income/march-2011/stb-regional-gdhi-march-2011.html)

<sup>26</sup> We assume in the analysis that data gaps are random, uncorrelated with fees or any other variable, and therefore will not lead to selection bias. This assumption seems reasonable given the correlation coefficient between average boarding (day) fee over the time period and number of fee data-points is 0.09 (0.06) for SS Schools and 0.15 (-0.09) for Non-SS Schools.

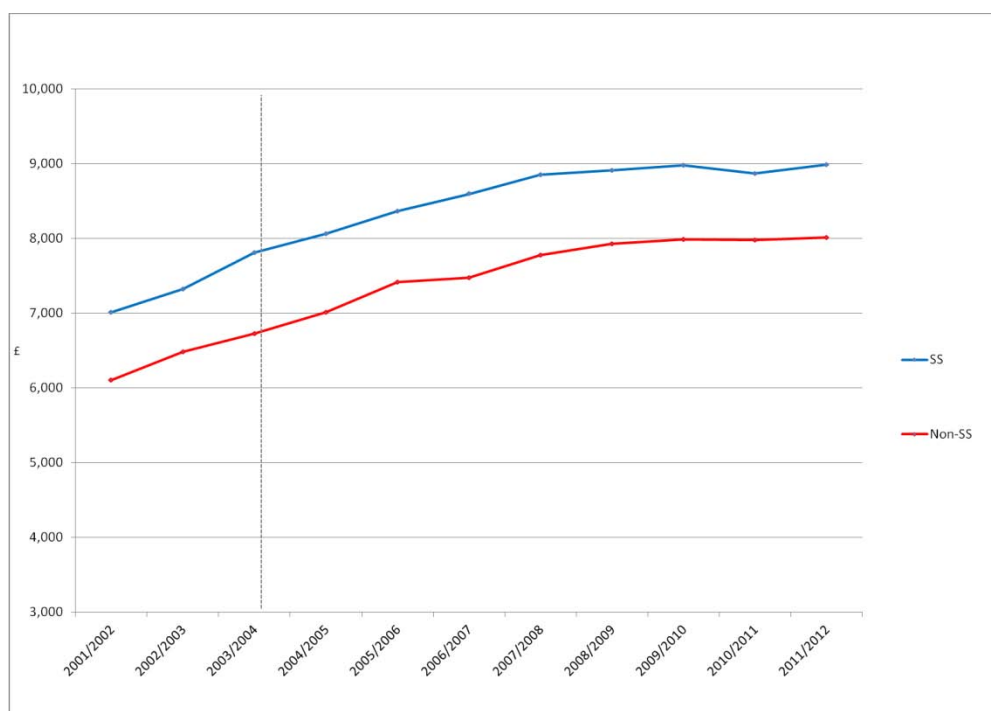
<sup>27</sup> The missing values are uncorrelated with any of the measured variables and hence the results are not directly impacted. See Annexe D for more detail.

## 4 DESCRIPTIVE ANALYSIS

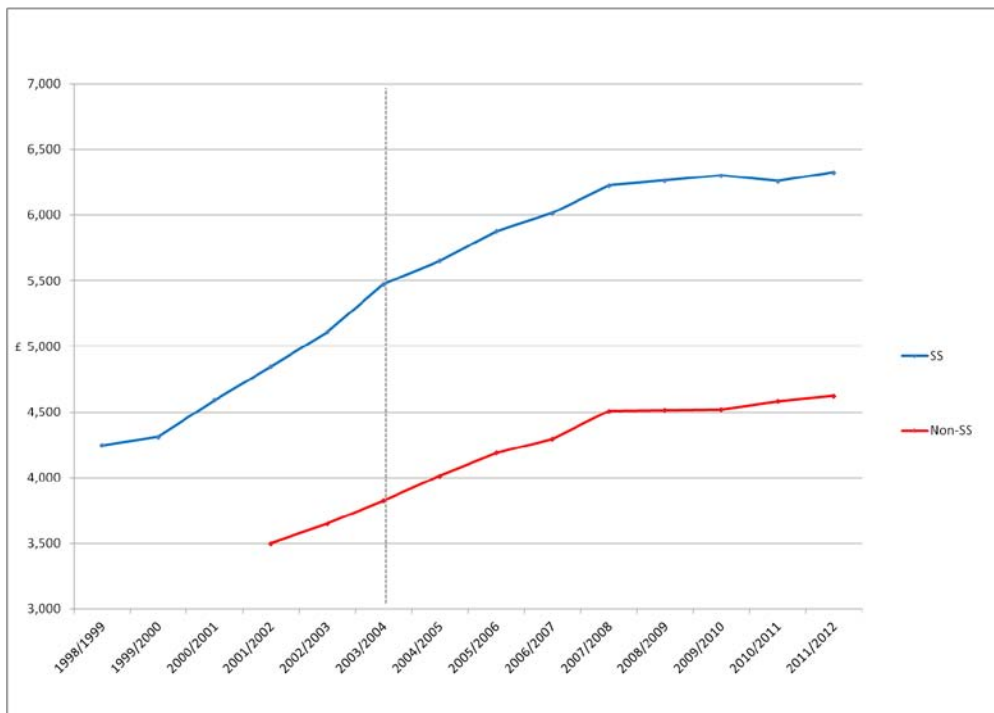
### Fee levels and annual fee increases

4.1 Figure 2 and Figure 3 plot average deflated fees per term for SS and Non-SS Schools for boarders and day pupils over time. For both charts, the blue line shows the evolution of fees over 2001/02 to 2011/12 for SS Schools. The red line shows the evolution of fees for Non-SS Schools over 2001/02 to 2011/12. The grey vertical line marks the OFT intervention.

Figure 2: Average fees per term (boarding, deflated)



**Figure 3; Average fees per term (day, deflated)**

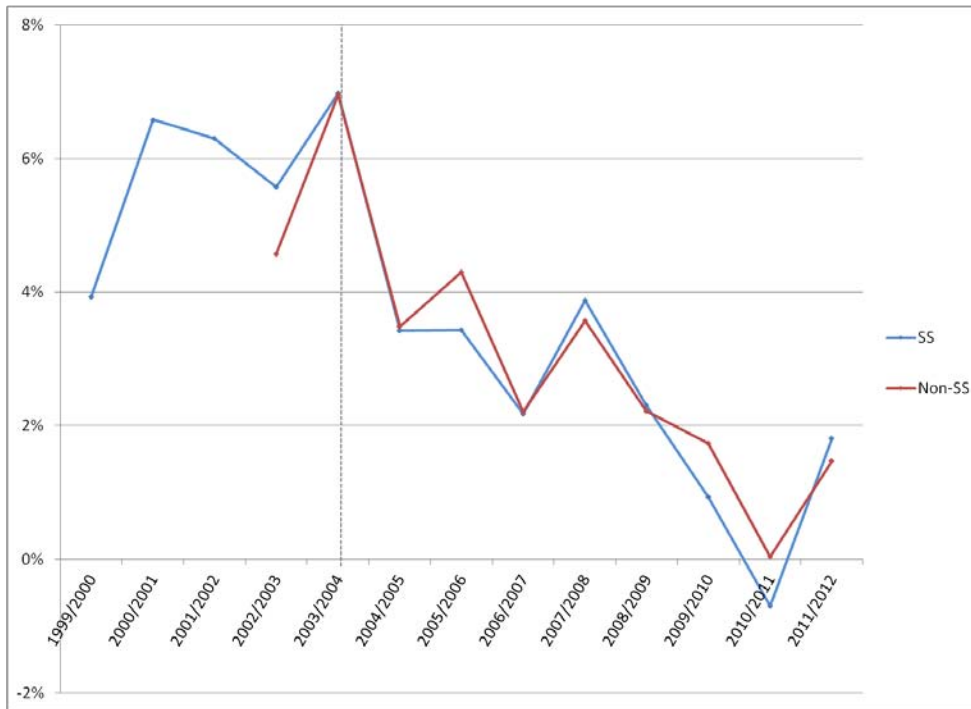


4.2 Figure 2 and Figure 3 show that

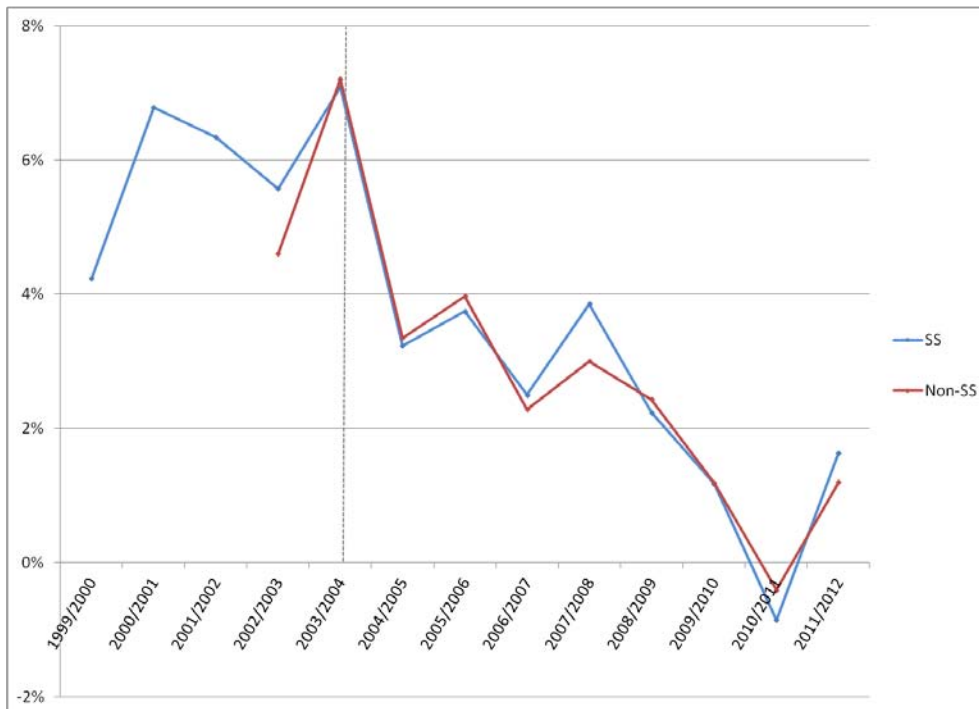
- Average boarding prices per term are typically about £1,000 higher for SS Schools than for Non-SS Schools. The difference is £1,500-£2,000 for day prices.
- Boarding prices are typically between £2,000-£4,000 higher than day prices.
- Visual inspection does not enable detection of a change in the difference in price levels between SS Schools and Non-SS Schools post-intervention.
- There is no discernible change in the trend of price levels for either group following the OFT intervention.

4.3 Figure 4 and Figure 5 below plot the average annual increase in deflated fees per term for boarding and day pupils. The grey vertical lines mark the OFT intervention. SS Schools data pre-2001 is presented to provide further context.

**Figure 4: Average annual increase in deflated boarding fees per term (per cent)**



**Figure 5: Average annual increase in deflated day fees per term (per cent)**



4.4 Figure 4 and Figure 5 show that

- Annual price increases decreased significantly after the OFT intervention for both SS and Non-SS Schools.
- Annual price increases are remarkably similar between SS and Non-SS Schools over time. It is hard to discern a clear impact of the OFT intervention on SS Schools relative to Non-SS Schools by mere visual inspection. Price increases could be grouped into three periods:
  - pre-intervention by OFT, when price increases were typically around six to eight per cent
  - in the four years immediately after OFT-intervention, when price increases were typically around three to five per cent
  - the most recent four years – corresponding with the financial crisis – when price increases reduced significantly, falling below zero in 2010/11 before recovering the following year.

4.5 The sudden drop in fees inflation of both SS Schools and Non-SS Schools immediately after OFT intervention is a reminder of the possibility that an impact may have been felt across the whole industry – including both SS Schools and Non-SS Schools. As previously outlined, this could occur for instance if the re-invigoration of competition among SS Schools were to stimulate competitive responses across other schools. Descriptive analysis however cannot assess the importance of the OFT intervention in stimulating this reduction: further econometric analysis would be required.<sup>28</sup> This paper therefore does not draw any firm conclusions from the notable drop in fees inflation in 2004/05, but notes that to the extent OFT intervention may have stimulated a broader cross-industry impact, the difference in difference analysis that follows may underestimate the impact of the intervention, and could therefore be considered a conservative estimate of impact.

<sup>28</sup> And not difference in differences.



## 5 ECONOMETRIC ANALYSIS

- 5.1 Econometric analysis, using difference in differences methodology, allows for more rigorous interrogation of the data. The model and findings are outlined below. See Annexe D for a technical overview.

### Model

#### General approach

- 5.2 Regression analysis is a statistical method for understanding the relationship between two or more variables. Multiple regression analysis concerns a variable to be explained, the dependent variable, and one or more explanatory variables that are thought to produce or be associated with changes in the dependent variable. Regression analysis allows the effect of a change in an explanatory variable on the dependent variable to be measured, while controlling for the influence of the other explanatory variables. For example, in this context, we can find the impact of the intervention, while controlling for other relationships present in the data.
- 5.3 Panel data is produced when a number of individual units (schools in this case) are observed over a period of time. The advantage of panel data regression models is that they can exploit both the variation across time periods, for a given unit, and variation across individual units for each given point in time.
- 5.4 The panel structure of the data set can be exploited using different assumptions concerning the distribution of the error term. In general, practitioners can choose between unstructured methods, such as pooled cross-section, where no specific assumptions are made, and more structured ones, such as fixed effect and random effects.
- 5.5 The advantage of fixed and random effects models, compared to cross-sectional approaches, is that they reduce to the minimum the amount of information required to control for differences across SS Schools. Random effects models rely on strict assumptions, which would not be met in this case. Therefore a fixed effect model is used. A fixed effects

model estimates the impact of the explanatory variables by relying exclusively on the co-variation between the fees and these explanatory variables over time within schools. See Annexe D for further detail.

## Econometric model

5.6 This analysis uses a panel of yearly, school-level data on fees to estimate a fixed effects model. The below econometric model is estimated:

$$\log(Fee_{it}) = \beta_0 + \beta_1 \cdot boarder\%_{it} + \beta_2 \cdot ranking\%_{it} + \beta_3 \cdot \log(Pupils_{it}) + \beta_4 \cdot year_t + \lambda \cdot postintervention_t + \delta \cdot infringe.post_{it} + S_i + \varepsilon_{it}$$

5.7 In short, this model assumes that fees in school  $i$  in year  $t$  depend on the percentage of boarding students, the ranking relative to other independent schools, the number of students, the year, whether or not it is the post intervention period, whether the school was a party to the collusive conduct, and whether or not the observation relates to a participating school in the post-intervention period. It estimates the impact of each of these factors on fees. As the purpose of the analysis is to test whether the intervention had an effect on fees in SS Schools, the key variable of interest is  $\delta$ . Specifically, we are interested in whether or not it is significantly different from zero. Annexe D contains further detail on the model. The next section has more detail on the variables.

## Variable

- $Fee_{it}$ , the dependent variable, is the fee for school  $i$  in year  $t$ . Inflation adjusted day and boarding fees are used as the dependent variable.
- $\beta_0$  is a constant.
- $S_i$  is the average value of the fixed effects for each school  $i$ . It accounts for any school specific time invariant factors that

distinguish schools from the average<sup>29</sup>. It can be thought of a school-specific adjustment to the constant.

- $Boarder\%_{it}$  is the percentage of boarders in school  $i$  in year  $t$ . For example, a school with 75 per cent boarders would have a value of 0.75.
- $Ranking\ \%_{it}$  is the percentile in the Financial Times school rankings for school  $i$  in year  $t$ . For example, if a school had a ranking in year  $t$  which put them at the 80<sup>th</sup> percentile this variable would equal 0.8.
- $Pupils_{it}$  is the number of pupils in school  $i$  in year  $t$ .
- $Year_t$  is the relevant year and accounts for any linear trend in fees.
- $postintervention_t$  indicates whether or not the observation comes from the post-intervention period and allows for the trend, for all schools, to differ before and after the intervention.
- $infringe.post_{it}$  indicates whether or not the observation is from an SS School in the post intervention period.<sup>30</sup> Under specific assumptions concerning the scope and the duration of the anti-competitive agreement, the estimated result for this variable can provide a basis on which to estimate the impact of the OFT intervention. This is the pivotal variable in the difference in difference approach. A negative and statistically significant coefficient would suggest, consistent with theory that the intervention led to a reduction in fees.

<sup>29</sup> This may capture for instance historical factors or any other factor affecting the average level of the fee observed for each school. It will also include whether the school was among the group engaged in the collusive practice, as this is also time invariant.

<sup>30</sup> For simplicity, the two SS Schools which were not found to have breached the law during the academic year 2002/03 are modelled as being party to the collusive conduct this year.

- $\varepsilon_{it}$  is the random error associated with school  $i$  at time  $t$ .

## Assumptions

5.8 Key assumptions of the fixed effects model and our application of difference in differences methodology are

- The impact of the OFT intervention – if there is one – is best modelled as a percentage reduction in fees, beginning in 2004/05.
  - On timing, it is not straightforward to predict whether and at what point in time any impact of the OFT intervention would occur. It has been argued that cartels can continue as ‘well-established’ rules even after detection. This suggests that an impact might take time to be felt, particularly when practices are ingrained, such as through use of rules of thumb or focal points which may still be adhered to post-intervention. While the assumption of an immediate impact in our model is offered some support by evidence to suggest that when the number of parties exceeds two, that post-intervention, pre-cartel pricing is quickly resumed,<sup>31</sup> we nevertheless conduct tests to explore whether the impact of the intervention is gradual.
- In absence of OFT intervention, information exchange and its impact would have persisted to the present day (2011/12)

<sup>31</sup> Kovacic, W, et al (2007) conduct an empirical analysis of a US vitamins cartel and find that the number of conspirators may be important in influencing whether prices return to pre-conspiracy levels, and how quickly this may happen. They find that in the post collusion period, products with two conspirators continue to be priced as if explicit conspiracy never stopped, but that products with three or four participants quickly return to pre-conspiracy pricing or lower.

See Kovacic, W, et al (2007) ‘Lessons for Competition Policy from the Vitamins Cartels’ in Ghosal, V, and Stennek, J, *The Political Economy of Antitrust*, Volume 282, pp149-176

See also Sabbatini P. (2008) ‘Assessing the impact of antitrust intervention by the Italian Competition Authority’, *De Economist*, 156, pp491-505

- The FT rank variable is a proxy for the absolute quality of each school in a given year (FT score was not used because the score methodology changed in 2007, meaning it cannot be used to track quality over time<sup>32</sup>)
- Schools other than SS Schools did not engage in similar information exchange practices in the first place
- There are no omitted variables, or other endogeneity issues, which would lead to biased results
  - This assumption implies that the only factor that might be expected to impact the two groups differently is the targeted OFT intervention itself, as any other important differences are controlled for with the measured explanatory variables
    - As income may be one such variable that may influence fee setting differently between SS Schools and Non-SS schools, sensitivity tests are performed to explore this
  - In reality, the extent to which schools other than SS Schools engaged in information exchange pre- and post-OFT intervention is unknown, and, by implication, speculative. Given that the OFT did not make a finding on the behaviour of schools other than the SS Schools, however, this simplifying assumption may be considered a suitable starting point. To the extent that Non-SS Schools may have lowered prices in response to OFT intervention – influenced

<sup>32</sup> See Annexe C for average FT score and average FT rank over time for SS Schools and Non-SS Schools. FT rank is defined by the FT as the position of the school, compared to its peers, calculated by the FT (based on the FT score). The FT score is defined as a combination of the points per candidate in core subjects (to measure the quantity of work), and the points per entry in core subjects (to measure the quality). We converted this into a percentile to allow for changes in the number of schools ranked over time.

by the extent to which OFT intervention enhances competition from SS Schools – the difference in differences technique may understate the impact of the OFT intervention.

## Findings

5.9 Results of the econometric difference in differences analysis are presented in Table 1 below. Although the modelling was performed with both real and nominal fees, Table 1 only reports the findings for real fees, as there is no substantive difference in the equivalent results for nominal fees.<sup>33</sup> The left-most column in the table lists the names of the coefficient and the columns to the right show the estimated coefficients for two versions of the fixed effect model.<sup>34</sup> Significance is indicated by asterisks (\*) and crosses (+) as explained at the bottom of the table. The bracketed figures below the coefficients are the standard errors.

5.10 The key finding is that following OFT intervention, annual school fees of infringing schools fell by 1.6 per cent for boarders and 1.5 per cent for day pupils compared with the counterfactual of 'no OFT intervention'. Some confidence may be placed in these results.<sup>35</sup> The findings support our expectations, based on economic theory, that OFT intervention drove a reduction in fees of infringing schools by alleviating the harm imposed on the consumer by the original information exchange infringement. The analysis suggests therefore that in absence of OFT intervention fees would have been 1.6 per cent and 1.5 per cent higher

<sup>33</sup> Real fees are preferred as they allow for the time-varying effects of inflation to be accounted for.

<sup>34</sup> In order to test robustness two versions of the fixed effects model were run. The first is Ordinary Least Squares, and the second is heteroscedasticity and auto-correlation robust (HAC) standard errors in order to check for heteroscedasticity and autocorrelation.

<sup>35</sup> The findings are statistically significant at the 95 per cent level (boarding) and 90 per cent level (day).

for boarding and day pupils of SS Schools each academic year from 2004/05 onwards.

**Table 1: Regression results**

Dependent Variable	Log(Real Day Fees)		Log(Real Boarding Fees)	
	Fixed Effect (OLS SEs)	Fixed Effect (HAC SEs)	Fixed Effect (OLS SEs)	Fixed Effect (HAC SEs)
Boarder%	0.0773*** (0.018)	0.0773+ (0.051)	0.0367 (0.030)	0.0367 (0.029)
Ranking%	-0.0147 (0.015)	-0.0147 (0.019)	0.00396 (0.015)	0.00396 (0.015)
Log(Pupils)	0.0247+ (0.015)	0.0247 (0.033)	0.0291* (0.017)	0.0291+ (0.021)
Year	0.0698*** (0.001)	0.0698*** (0.004)	0.0709*** (0.001)	0.0709*** (0.004)
Post intervention	0.0750*** (0.005)	0.0750*** (0.027)	0.0674*** (0.006)	0.0674*** (0.022)
<b>Post intervention and Infringer (DiD)</b>	<b>-0.0149** (0.007)</b>	<b>-0.0149** (0.007)</b>	<b>-0.0162** (0.007)</b>	<b>-0.0162*** (0.005)</b>
<i>N</i>	1829	1825	1317	1311
<i>R</i> <sup>2</sup>	0.949	0.949	0.957	0.957

Standard errors in parentheses

+  $p < 0.2$ , \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## Sensitivities and robustness

5.11 Diagnostic and sensitivity tests are conducted to explore the robustness of and determine how confident we can be with these findings. In particular to assess the robustness of the findings, various diagnostics and sensitivity tests were used to assess the validity of assumptions about the modelling of the error term, whether an explanatory variable was correlated with the error term, whether the results were impacted if regional and national disposable incomes were included as explanatory variables and if the results held for sub-samples of the data. Annexe D

presents technical details in depth. The results of these tests underline the robustness of the findings in relation to boarding fees, showing

- The impact cannot be explained by a hypothesised difference in impact on SS Schools and Non-SS Schools of the financial crisis and ensuing recession, as the findings are broadly unchanged when the time period is restricted to academic years 2001/02 to 2007/08.<sup>36</sup> This suggests that asymmetric impacts of the recession cannot account for the findings.
- The impact does not depend on the inclusion of the Best-Schools dataset: the findings are broadly unchanged when the dataset is restricted to the FT dataset by adjusting the time period to academic years 2001/02 to 2005/06. This suggests findings are not driven by problems with merging the two datasets.
- The impact is not sensitive to exclusion of the 10 per cent most expensive or 25 per cent least expensive schools.
- The impact is not sensitive to exclusion of the top 50 ranking schools or bottom 200 ranking schools.
- The impact is not found to be sensitive to inclusion of a disposable income variable (regional and national). However, we are unable to assess the sensitivity of the results to inclusion of a variable that allows gross disposable income per head to impact SS Schools and Non-SS Schools differently. See Annexe D.

5.12 The findings lose their statistical significance when sensitivity tests are conducted to exclude Greater London and the South-East (about half of total schools sampled). When the sample was restricted only to London and the South East, boarding fees were again highly statistically

<sup>36</sup> Academic fees were set for academic year 2007/08 before the financial crisis began.



significant (day fees were not), with an estimated effect of a 2.4 per cent reduction in fees.<sup>37</sup>

5.13 We also conducted tests – interacting the time trend and difference in difference variable – to explore whether the impact of the intervention is gradual or immediate. Results suggest the impact of the intervention may be gradual, but the average effect is not materially different.

#### **Key findings**

- Following OFT intervention, SS School boarding fees fell by an estimated 1.6 per cent per annum relative to what we would expect had the OFT not intervened.
- This estimate is highly statistically significant (at the 95 per cent level), and robust to a number of different specifications and sensitivity tests, and therefore presents strong evidence that OFT intervention has driven a reduction in consumer harm.
- The impact for SS School day fees is estimated at 1.5 per cent per annum. This finding, although statistically significant at the 90 per cent level, is not as robust as for boarding fees.
- The findings control for the influence of other factors – for instance quality, to the extent that this is captured by the variable ‘FT rank’ – and are likely to represent a lower bound of impact given the potential for broader impact across the market.

<sup>37</sup> The difference between the coefficients obtained on the restricted sample and on the full sample however was not statistically significant. Note that although the result indicates that London and the South-East play a strong role in driving the result, this does not mean that using the average estimated effect across all areas to estimate the consumer detriment avoided would lead to inaccuracies in estimating the total national effect.

## 6 CONCLUSION AND ESTIMATE OF CONSUMER BENEFITS

6.1 The analysis has presented evidence that suggests OFT intervention lowered boarding fees of SS Schools in the post-intervention period (2004/05 to the present) by 1.6 per cent. It presented evidence of a 1.5 per cent reduction in day fees. However, only the findings in relation to boarding fees were robust to sensitivity and diagnostic testing.

6.2 Total discounted savings to the consumer were estimated (in 2010 prices, discounted to the present) at

- £85m in boarding fees<sup>38</sup>
- £20m in day fees.

6.3 Given the high confidence with which the findings are held in relation to boarding fees, this analysis has therefore presented evidence that OFT intervention has realised savings of an estimated £85m to the consumer. This equates to an estimated £495 per boarder per annum.<sup>39</sup>

6.4 We consider the total estimated savings of £85m to be conservative for the following reasons

- The analysis does not attempt to quantify the deadweight loss averted by the OFT intervention,<sup>40</sup> focusing only on the transfer of surplus back from schools to the consumer.
- The analysis does not account for the possibility of broader impact where competitors to SS Schools respond to the reinvigoration of SS Schools price competition – as spurred by OFT intervention - by lowering their prices. In this regard, it is notable that patterns of

<sup>38</sup> See Annexe F for calculation of point estimate and range of consumer benefits.

<sup>39</sup> £165 per boarder per term, in 2010 prices, discounted to the present. See Annexe F

<sup>40</sup> See Annexe F for discussion of estimation of deadweight loss averted.

annual fee increases show a sizeable reduction in fee inflation immediately after OFT intervention – as observed in Figure 4 and Figure 5 – consistent with an industry-wide effect in terms of reduced fees across both SS Schools and Non-SS Schools.

- The analysis does not consider any impact the OFT intervention may have had in stimulating compliance more broadly across the rest of the market.
- The analysis does not take account of the wider deterrence effects of this competition enforcement action across the economy. In this respect, the OFT has published previous research which finds evidence of significant deterrent effects of OFT investigations.<sup>41</sup>

<sup>41</sup> OFT (2011) *The Impact of Competition Interventions on Compliance and Deterrence*, ([www.offt.gov.uk/shared\\_offt/reports/Evaluating-OFTs-work/oft1391.pdf](http://www.offt.gov.uk/shared_offt/reports/Evaluating-OFTs-work/oft1391.pdf)) finds during 2003-2011 for every 'other commercial agreement' (including information exchange) investigated by the OFT, 40 such agreements are deterred in the rest of the economy.

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## A LIST OF SS SCHOOLS AND NON-SS SCHOOLS

**Table 2: SS Schools**

Ampleforth College	Gresham's School	Sedbergh School
Bedford School	Haileybury School	Sevenoaks School
Benenden School	Harrow School	Sherborne School
Bradfield College	King's School Canterbury	Shrewsbury School
Bromsgrove School	Lancing College	Stowe School
Bryanston School	Malvern College	Strathallan School
Canford School	Marlborough College	Tonbridge School
Charterhouse School	Millfield School	Truro School
Cheltenham College	Mill Hill School	Uppingham School
Cheltenham Ladies' College	Oakham School	Wellington College
Clifton College	Oundle School	Wells Cathedral School
Cranleigh School	Radley College	Westminster School
Dauntsey's School	Repton School	Winchester College
Downe House School	Royal Hospital School	Woldingham School
Eastbourne College	Rugby School	Worth School
Epsom College	St Edward's School Oxford	Wycombe Abbey School
Eton College	St Leonards-Mayfield School	

A.1 Table 3 below lists the Non-SS School monikers used in the dataset, in many instances shortened versions of the full school name.

**Table 3: List of Non-SS Schools**

Abbey School Reading	Kent College Pembury	Rossall
Abbots Bromley	King Edward's School Bath	Royal School
Abbotsholme	King Edward's School	Royal Ballet School
Adcote	King Edward VI High Girls	Royal Grammar School
Albemarle	King Edward VI School	Royal Grammar School
Alleyn's	Kingham	Royal Masonic School
Ardingly	King's College	Royal Russell School
Arts Educational	King's High School	Royal Wolverhampton
Badminton	King's School Ely	School Of St Helen & St Katharine
Barnard	Kingswood	Seaford
Bedstone	Lady Eleanor Holles	Shebbear
Blundell's	Langley	Sherborne School for Girls

Bootham	Latymer	Shiplake
Bosworth	Leighton	Shrewsbury High
Box Hill	Licensed Victualler	Sidcot
Bradford Grammar	Lime House School	Solihull
Bredon	Llandoverly	South Hampstead High
Bristol Grammar	Lord Wandsworth	St Antony's
Bromley	Loughborough High	St Bede's
Bruton	Magdalen College	St James's School
Bury Grammar Girls	Malvern Girls'	St Mary's Hall
Casterton	Manchester Grammar	St Albans High
Channing	Manchester High	St Albans School
Chetham's	Maynard	St Bees School
Christ College Brecon	Merchant Taylors' Hertfordshire	St Catherine's School
Christ's Hospital	Merchiston	St Edmund's College
City of London School for Boys	Milton	St George's School
City of London School for Girls	Moira	St John's School
Clayesmore	Monkton	St Lawrence College
Cobham	Monmouth	St Mary's School
Concord	Moreton	St Mary's School Ascot
Culford	Mount School York	St Mary's School Calne
Dean Close	Northampton High	St Paul's Girls' School
Denstone	Norwich School	St Peter's School
Dover	Notting Hill	St Swithun's School
D'Overbroeck's	Nottingham High School	St Teresa's School
Duke Of York	Nottingham High Girls	Stonar
Ellesmere	Oratory	Surbiton
Eltham	Oswestry	Sutton Valence
Exeter	Oxford High School	Talbot
Felsted	Pangbourne	The Leys School
Fettes	Park School Ilford	Tormead
Framlingham	Perse School for Boys	Trinity School Devon
Fyling	Perse School for Girls	Tudor
Giggleswick	Portsmouth Grammar	Twycross
Godolphin	Princess Helena	University
Grange	Prior Park College	Warminster
Guildford High	Prior's Field School	Wentworth
Haberdashers Boys	Purcell	Westonbirt
Haberdashers Girls	Putney High	Whitgift
Hammond	Queen Anne's	Wimbledon High

Hampton	Queen Elizabeth's Hospital	Windermere
Harrogate	Queen Ethelburga's College	Wispers
Headington	Queen Margaret's	Withington
Heathfield School Ascot	Queen's College Somerset	Worksop
Highgate	Queenswood	Wrekin
Hurtwood	Read School	Wychwood
James Allen's Girls' School	Redland	Wycliffe
John Lyon School	Rendcomb	
Kent College Canterbury	Roedean	

## **B MARKET FOR INDEPENDENT SCHOOLS**

B.1 The UK independent sector educates around 628,000 children in around 2,600 schools. The independent sector educates around 6.5 per cent of the total number of schoolchildren in the UK.<sup>42</sup> Of the pupils in Independent Schools Council (ISC) schools – accounting for around 80 per cent of the total number of pupils in independent schools in the UK – it is estimated that

- pupils aged 11-19 account for 70 per cent of ISC pupils
- 68,102 are boarders and 438,398 are day pupils. Boarders thus represent about 13 per cent of ISC pupils
- girls represent 49 per cent of ISC pupils
- 24,554 pupils are non-British with parents living overseas, representing over one third of ISC boarder pupils
- 45 per cent of ISC schools are in Greater London or the South East.

<sup>42</sup> Independent Schools Council [www.isc.co.uk/FactsFigures\\_PupilNumbers.htm](http://www.isc.co.uk/FactsFigures_PupilNumbers.htm).



## C FT SCORE AND RANK

Figure 6: Average FT score

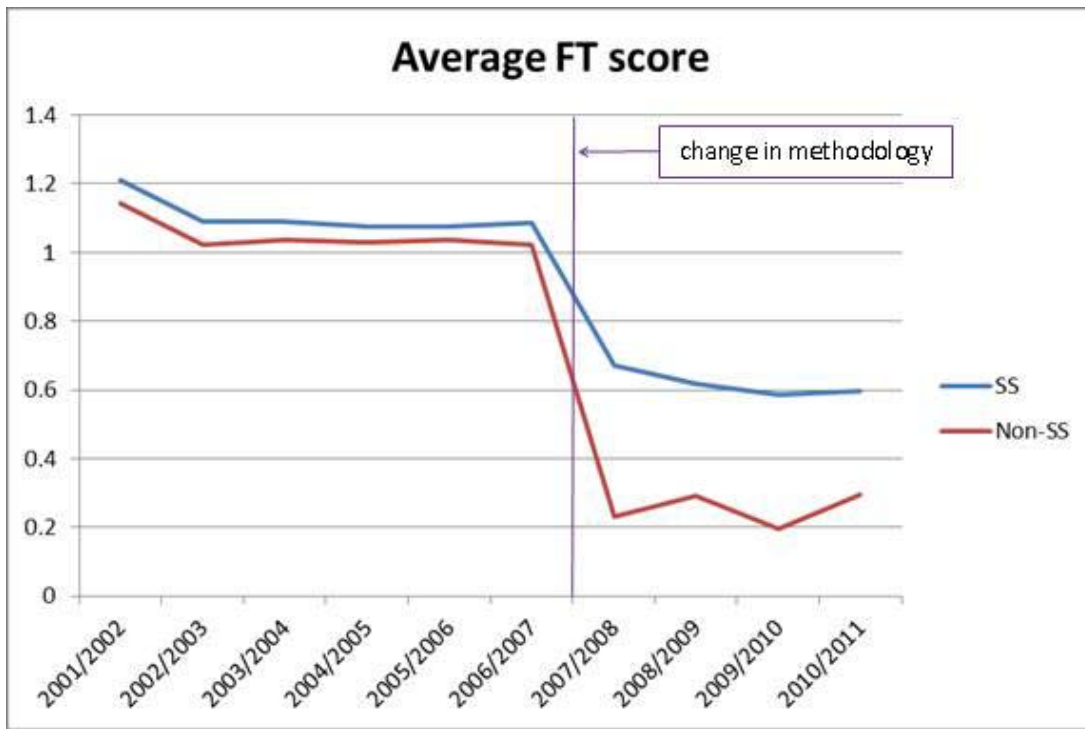
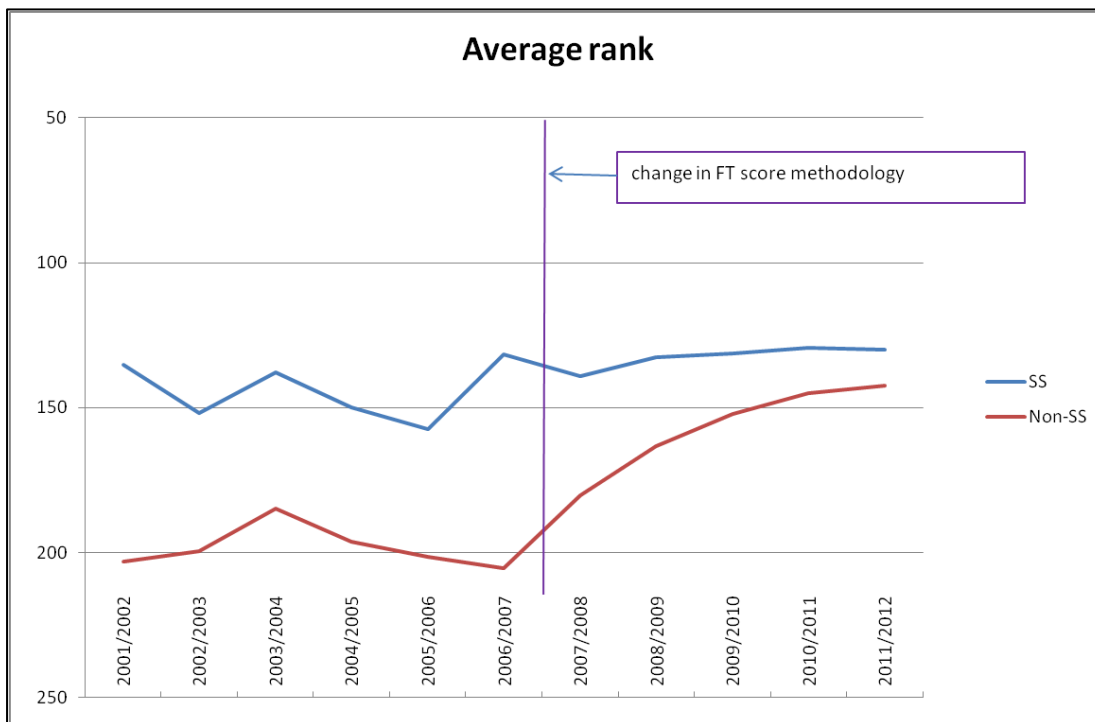


Figure 7: Average FT rank



- C.1 Note that the driving factor behind the improvement in the average Non-SS rank post-2007 is that many of the lower ranking Non-SS Schools drop out of the ranking from this point.

## D ECONOMETRIC ANALYSIS

### Summary

D.1 Difference-in-difference (DiD) analysis is used to estimate the impact of the OFT's intervention on boarding and day fees in SS schools. Our analysis suggests that as a result of the OFT's intervention, the real boarding and day fees in SS schools were about 1.6 per cent and 1.5 per cent lower in the post intervention period respectively, than would have otherwise been expected. Sensitivity testing and diagnostics suggest that the results regarding day fees are less robust than the results regarding boarding fees.

### Data

D.2 The dataset has a panel structure, covering about 230 schools between 2001 and 2011. It contains:

- day and boarding fees by school charged by each school, either SS or non-SS, on a yearly basis
- scores and rankings from the Financial Times annual comparison of independent secondary schools<sup>43</sup> in the United Kingdom
- rankings based on those scores
- the location of schools
- the proportion of boarding students in each school.

D.3 There are some missing values in the dataset, both in the dependent variables – day and boarding fees – and in some of the explanatory variables. As these are assumed to be distributed randomly, and in

<sup>43</sup> See the main text for more details.

particular uncorrelated with any of the measured variables, the estimation and the results presented below are not directly impacted.<sup>44</sup>

## **Econometric methodology<sup>45</sup>**

- D.4 Regression analysis is a statistical method for understanding the relationship between two or more variables. Multiple regression analysis concerns a variable to be explained, the dependent variable, and one or more explanatory variables that are thought to produce or be associated with changes in the dependent variable. Regression analysis allows the effect of a change in an explanatory variable on the dependent variable to be measured, while controlling for the influence of the other explanatory variables. For example, in this context, we can find the effect of the intervention, while controlling for other relationships present in the data.
- D.5 Panel data is produced when a number of individual units (schools in this case) are observed over a period of time. The advantage of panel data regression models is that they can exploit both the variation across time periods, for a given unit, and variation across individual units for each given point in time. This analysis uses a panel of yearly, school-level data on fees to estimate a fixed effects model.
- D.6 The panel structure of the data set can be exploited using different assumption concerning the distribution of the error term. In general, practitioners can choose between unstructured methods, such as pooled

<sup>44</sup> Specifically, there is assumed to be no sample selection problems resulting from the missing data. Sample selection issues arise when the probability of individuals – schools in the present exercise – being part of the sample, or missing data, is related to one or more of the variables we want explain. This leads to a non-random, non-representative sample from which unbiased results cannot be drawn under the current methodology. For more information, see a standard econometrics textbook such as Greene (2008) or Kennedy (2008).

<sup>45</sup> Excel 2007 was used to conduct the initial data management and Stata 11 was used to conduct the econometric analysis (xtreg for standard FE regression and xtivreg2 for FE regression with HAC errors).

cross-section, where no specific assumptions are made, and more structured ones, as fixed effect and random effects.

- D.7 In this case fixed effect panel estimation was used. This model estimates the impact of the explanatory variables on fees by exclusively exploiting the co-variation between the fees and these explanatory variables over time within schools.<sup>46</sup> The advantage of fixed effects models, compared to cross-sectional approaches, is that it minimises the information required to control for differences across schools. In the absence of an adequate control for the heterogeneity between schools present in the data, this reduces the risk of obtaining biased results. This advantage can come at a cost. Fixed effects models are suitable only if there is sufficient time series variation in the data to permit precise estimates of the relationship between fees and the explanatory variables. In this case both regular and robust standard errors are estimated to control for the presence of heteroscedasticity and serial correlation in the error term.
- D.8 Finally, the estimation relies on the assumption that there is no selection bias, that is no correlation between the within schools' variation in fees – the variable of interest – or any other regressor and the probability of a school being part of the SS group. This is a necessary condition for DiD methodology to produce unbiased results. Diagnostic tests on the variables used allow us to exclude the possibility that selection bias may arise.

## **Econometric model**

- D.9 The below econometric model has been designed and estimated.

<sup>46</sup> An often preferable alternative to the fixed effects model is the random effects model, which also takes into account the co-variation of these variables across, as well as within schools. A random effects model however relies on stronger assumptions than a fixed effect model. A Hausman test can be used to formally test whether these assumptions hold, and therefore if a random effects model is appropriate. In this case, the Hausman Test indicates these assumptions do not hold and that a random effects model is inappropriate.

- $\log(\text{Fee}_{it}) = \beta_0 + \beta_1 \cdot \text{boarder}\%_{it} + \beta_2 \cdot \text{ranking}\%_{it} + \beta_3 \cdot \log(\text{Pupils}_{it}) + \beta_4 \cdot \text{year}_t + \lambda \cdot \text{postintervention}_t + \delta \cdot \text{infringe.post}_{it} + S_i + \varepsilon_{it}$

where

- $\text{Fee}_{it}$ , the dependent variable, is the fee for school  $i$  in year  $t$ . Inflation adjusted day and boarding fees are used as the dependent variable. Nominal fees were deflated with the GDP deflator,<sup>47</sup> and 2010 prices are used as the base year. The natural log of fees is used as this functional form lends itself to a more straightforward interpretation in terms of the explanatory variables. Regressions were replicated on non-deflated day and boarding fees as a robustness test.
- $\beta_0$  is a constant.
- $S_i$  the average value of the fixed effects for each school  $i$ . The fixed effect is the time invariant difference between school  $i$  and an average school present in the sample. This controls for both the impact of observed, time invariant differences, such as location and whether the school was among the infringing group,<sup>48</sup> and unobserved time-invariant differences between the schools such as historical factors. It can be thought of a school-specific adjustment to the constant.
- $\text{Boarder}\%_{it}$  is the percentage of boarders in school  $i$  in year  $t$ . For example, a school with 75 per cent boarders would have a value of 0.75.  $\beta_2\%$  is the percentage increase (decrease) in fees that could be expected from a one per cent increase (decrease) in the number of boarders. For example, if  $\beta_1 = 0.07$  then an increase of one per cent in boarders should lead to 0.07 per cent increase in fees.

<sup>47</sup> [www.hm-treasury.gov.uk/data\\_gdp\\_fig.htm](http://www.hm-treasury.gov.uk/data_gdp_fig.htm)

<sup>48</sup> When a fixed effects model is used to conduct DiD analysis, the indicator for the treatment group is subsumed into the fixed effect as it is time-invariant.

There is no economic presumption concerning the sign of this coefficient.

- *Ranking*  $\%_{it}$  is the percentile in the Financial Times school rankings for school *i* in year *t*. For example, if a school had a ranking in year *t* which put them at 80<sup>th</sup> percentile this variable would equal 0.8. For consistency across time, it is assumed that there are 500 comparable schools per year.<sup>49</sup>  $\beta_2\%$  is the percentage increase (decrease) in fees that could be expected from a one per cent increase (decrease) in the number of boarders. We expect that a higher ranking is associated with a higher fee charged by the schools.
- *Pupils*  $_{it}$  is the number of pupils in school *i* in year *t*.  $(\beta_3 \times 100)\%$  is the percentage increase (decrease) in fees that could be expected from a one per cent increase (decrease) in the number of pupils. As with the percentage of boarders, it is not clear whether or not a higher number of pupils would be associated with higher or lower fees.<sup>50</sup>
- *Year*  $_t$  is the relevant year and accounts for any linear trend in fees. It can be considered to incorporate the composite effect of any relevant national-level trends over the period.  $(\beta_4 \times 100)\%$  is the percentage change in fees that can be expected each year. Note this specification assumes a constant, linear trend. For example, the effect on fees of moving from 2002/3 to 2003/4 is assumed to be the same as moving from 2004/5 to 2005/6.
- *postintervention*  $_t$  is a dummy variable indicating whether or not an observation is in the post intervention period. This variable allows

<sup>49</sup> Therefore the variable is constructed by dividing ranking by 500.

<sup>50</sup> Including contemporaneous volume in a reduced-form regression which has price as the dependent variable could, in theory, could lead to endogeneity. However, as discussed in a later section of the text, diagnostic testing indicates endogeneity is not present in this case.

for the trend, for all schools, to differ before and after the intervention. Specifically,  $\lambda$  is the average difference in fees between all schools in the pre and post intervention period, controlling for all other explanatory variables, including the linear time trend.

- $infringe.post_{it}$  is a dummy variable equal to one when the observation is from an SS school in the post intervention period.  $(\delta \times 100)\%$  can be interpreted as the average percentage difference in fees between the SS schools and non-SS schools in the post-intervention period, controlling for all other variables in the model including the time trend, observed time-variant, school-specific characteristics, and observed and unobserved time invariant fixed effects. Under specific assumptions concerning the scope and the duration of the anti-competitive agreement, the estimated result for this variable can provide a basis on which to estimate the impact of the OFT intervention.
- $\varepsilon_{it}$  is the random error associated with school  $i$  at time  $t$ .

## Hypothesis of interest

- D.10 We are interested in whether or not the intervention had an impact on fees when the explanatory variables and time invariant observed and unobserved factors are controlled for. In particular, we are interested in testing whether or not the intervention had an impact on the fees charged by SS schools.

## Results

- D.11 The results are summarised in Table 4. The top row shows the dependent variable, the second row shows the estimation method and standard errors used. We discuss standard errors further in the next section. Only the result for real fees are reported. There is no substantive difference in the equivalent results for nominal fees, and it is the real fees which are used to estimate consumer detriment avoided. The left-most column in the table lists the names of the coefficient and the columns to the right show the estimated coefficients for each model.



Significance (two-sided difference from zero) is indicated by asterisks (\*) and crosses (+) as explained at the bottom of the table. The bracketed figures below the coefficients are the standard errors.

**Table 4: Regression results**

Dependent Variable	Log(Real Day Fees)		Log(Real Boarding Fees)	
	Fixed Effect (OLS SEs)	Fixed Effect (HAC SEs)	Fixed Effect (OLS SEs)	Fixed Effect (HAC SEs)
Boarder%	0.0773*** (0.018)	0.0773+ (0.051)	0.0367 (0.030)	0.0367 (0.029)
Ranking%	-0.0147 (0.015)	-0.0147 (0.019)	0.00396 (0.015)	0.00396 (0.015)
Log(Pupils)	0.0247+ (0.015)	0.0247 (0.033)	0.0291* (0.017)	0.0291+ (0.021)
Year	0.0698*** (0.001)	0.0698*** (0.004)	0.0709*** (0.001)	0.0709*** (0.004)
Post intervention	0.0750*** (0.005)	0.0750*** (0.027)	0.0674*** (0.006)	0.0674*** (0.022)
<b>Post intervention and Infringer (DiD)</b>	<b>-0.0149** (0.007)</b>	<b>-0.0149** (0.007)</b>	<b>-0.0162** (0.007)</b>	<b>-0.0162*** (0.005)</b>
<i>N</i>	1829	1825	1317	1311
<i>R</i> <sup>2</sup>	0.949	0.949	0.957	0.957

Standard errors in parentheses

+  $p < 0.2$ , \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ , robustness discussed below

D.12 The large  $R^2$  in the final row of Table 4 indicates that the models capture much of the variation in the data.

D.13 The point estimate of the impact of the intervention on is 1.5 per cent for day fees and about 1.6 per cent for boarding fees. For both boarding and day fees the DiD coefficient is significantly different from zero at the five per cent level, regardless of whether regular or robust standard errors are used. However, diagnostics and sensitivity testing suggest the DiD coefficient for day fees is not robust to sub-sampling of the data according to the geographical location of the schools. This has no material impact on the reliability of the results and is discussed further in

Annexe D. While the estimated impact of the intervention may seem small, when applied to the relevant schools in the sector the associated consumer detriment avoided is economically significant.

### **Robustness Tests – Heteroscedascity and autocorrelation**

- D.14 Auto-correlation is of particular concern in DiD analysis.<sup>51</sup> Diagnostic tests indicate heteroscedascity and auto-correlation were both present within the model for day and boarding fees. In order to prevent any bias in the standard errors, we used heteroscedasticity and auto-correlation robust (HAC) standard errors.<sup>52</sup> Table 1 lists the HAC standard errors and shows that the DiD coefficients retain significance when these standard errors are used.
- D.15 A further robustness check, to control for the presence of autocorrelation, is to collapse the data for each school into pre and post intervention periods, re-estimate the model<sup>53</sup> and see whether  $\delta$  remains significant. With this approach the estimated DiD coefficient was not significantly different from zero for either day or boarding fees.
- D.16 We find it unsurprising that the impact is not robust to the exclusion of the yearly linear trend. DiD analysis exploits variation over time to identify the effect of the intervention. Collapsing the data discards much of the variation which can be used to achieve identification. Therefore, this modification does not constitute a parsimonious use of all the information available in the data.
- D.17 A complementary explanation for this change in significance is that it takes time for the intervention to have an impact, and by collapsing the

<sup>51</sup> Bertrand, Duflo and Mullainathan (2004) show many studies which employ difference-in-difference find spuriously significant results because they fail to properly manage serial correlation.

<sup>52</sup> Specifically, the standard errors are Driscoll-Kraay (1998) standard errors robust to heteroscedascity and correlation in the error terms across time and cross-sections.

<sup>53</sup> The year id variable was excluded.

data we are not capturing the longer run effect of the intervention. For example, the previously co-ordinated price increases could have constituted a focal point<sup>54</sup> for further price increases for some time after the point of intervention. We can test this explanation by including an interaction term between the time trend and the difference-in-difference dummy in the main model. This allows for the impact of the intervention to happen gradually. When the model is re-estimated with this interaction term, there is evidence that the effect of the intervention does occur gradually, but the resulting average effect is not significantly different<sup>55</sup> from the one obtained without interacting the variable.

- D.18 We can therefore conclude that the loss of significance when the data are collapsed is due to the loss of the variation in the time dimension which is necessary to achieve identification of the DiD coefficient, and not due to the absence of the hypothesised effect.

### **Robustness Tests – Endogeneity**

- D.19 Pupils can be interpreted as the quantity of educational services provided for a given fee. Including quantity among the explanatory variables when price is the dependent variable may raise fears of endogeneity. A Hausman test can be used to test whether there is endogeneity. If the number of pupils from the previous year is used as, and assumed to be an acceptable instrument for the number of pupils in any given year, the null hypothesis of no correlation between the explanatory variables and the error is not rejected. This suggests endogeneity of this variable is not

<sup>54</sup> Motta (2002), for example, discusses the use of focal points, historical or otherwise, in sustaining tacit collusion.

<sup>55</sup> The purpose of this regression analysis is to estimate the average effect of the intervention, so that it can be used to estimate the consumer detriment avoided. This is not changed by including the interaction term. Additionally, this expanded model and its interpretation are more complicated than original model. In particular, the calculation of the average effect is more complex and less transparent. For these reasons the interaction term is excluded from the main model.

a problem. This is not very surprising, as the coefficient is of marginal to no significance in the various specifications.

## **Robustness Tests - Effect of disposable income variables**

D.20 Variation in regional and national disposable income may also affect fees. Ideally, these variables would be included in the model. Average regional and national disposable income statistics were not available for the final two years of the sample at the time the analysis was undertaken.<sup>56</sup> Therefore the inclusion of these covariates would come at the cost of a reduction in the number of observations available for each school. When the regional and national disposable income variables were included in a regression analysis on a shorter sample which excluded the final two years, only one the measure included, the national disposable income was significant, with a positive sign.<sup>57</sup> This inclusion did not materially affect the estimated impact of the intervention, which was still significant and no different in statistical terms to original estimate.<sup>58</sup> In addition, the inclusion did not materially improve the fit of the model. Finally, comparisons indicated the reduction in the size of the coefficient of interest was largely due to the reduction of the sample used in the estimation, rather than to the inclusion of the additional variables. As explained above, this is consistent with the fact that the intervention had a gradual effect. Therefore removing the last two years from the sample would highly likely underestimate the impact of the intervention. For this reason, we relied for the computation of savings on the model

<sup>56</sup> Source: Office of National Statistics [www.ons.gov.uk/ons/rel/regional-accounts/regional-household-income/march-2011/stb-regional-gdhi-march-2011.html](http://www.ons.gov.uk/ons/rel/regional-accounts/regional-household-income/march-2011/stb-regional-gdhi-march-2011.html)

<sup>57</sup> An alternate model was estimated which included an interaction between the disposable income variables and whether or not the school was an infringer. This model should have allowed for disposable income to have a different effect on the fees in infringing and non-infringing schools. Unfortunately, collinearity between the national disposable income and the time trend meant it was not possible to effectively differentiate between infringing and non-infringing schools.

<sup>58</sup> The estimated impact was marginally lower, 0.013 (versus 0.016 in the original model). This difference was not statistically significant.

which estimated the effect of the intervention on the full time series, excluding the disposable income. The fact that the inclusion of additional controls, such as the national and regional income, does not materially affect the results is a further proof of the robustness of the general results of the analysis.

## **Robustness Tests – Sub-sampling**

D.21 To ensure the robustness of the results and to explore what drives them, we re-estimated the model using seven sub-samples of the data which each excluded one of the following categories of data.

- Schools with an average ranking of worse than 200.
- Schools with an average ranking of better than 50.
- Schools whose average real fees are in the top 10 per cent. When day fees are the dependent variable, day fees are used, when boarding fees are the dependent variable, boarding fees are used.
- Schools whose fees are in the lower 25 per cent and are not an SS school. Again, when day fees are the dependent variable, day fees are used, when boarding fees are the dependent variable, boarding fees are used.
- Schools in London and the South East.
- 2006/07 and subsequent years.
- 2008/09 and subsequent years.

D.22 The first four sub-samples should give some indication of whether schools with high, or low, fees or rankings are driving the results. The exclusion of schools in London and the South East should indicate if they are driving the results. The exclusion of data from 2006/7 will help to assess whether there is any effect from a change in the source of fee data in 2006/7. The exclusion of data from 2008/9 will help to assess whether there is any 'recession effect'.

D.23 The DiD coefficient for real day fees did not consistently retain its significance in these sub-samples. The DiD coefficient for boarding fees was more robust to sub-sampling, and when the HAC standard error was used, it retained its significance in all but the sub-sample which excludes London and the South East.<sup>59</sup> Based on these findings, the estimated effect of the intervention on boarding, but not the estimated effect on day fees, is considered sufficiently robust enough to be used in estimation of consumer detriment avoided.

<sup>59</sup> This indicates that London and the South-East play a strong role in driving the result, but it is do not mean that using the average estimated effect across all areas to apportion out the consumer detriment avoided would lead to inaccuracies in estimating the combined national effect.

## E DISPERSION ANALYSIS

### Theory

- E.1 Information exchange also has the potential to reduce the dispersion of fees.<sup>60</sup> However, analysis focused solely on the change in fee dispersion before and after the cessation of an exchange of information cannot clearly identify whether it had an anti-competitive impact. If the information exchange had led to the development and use of focal points then variation around these focal points may be limited. If this was the case for the SS Schools we might expect the OFT intervention to lead to an increase in dispersion and a reduction in fees (or in fee changes). However, information exchange may also have the potential to limit dispersion for the different reason that competitors are able to use information on each other's pricing to better understand demand and to respond more easily to one another's price changes. If this was the case for the SS schools we might expect the intervention to lead to a reduction in dispersion though not necessarily to a decrease in fees (or fee changes).
- E.2 In the analysis that follows when considering fee dispersion we therefore note that while a reduction in fee dispersion suggests an impact of the OFT intervention, it does not in itself indicate whether this impact was due to the cessation of collusive activity and so pro-competitive. In order to assess whether the intervention had a pro-competitive impact we need to establish whether there is evidence that fees were reduced as a result. This shows the limitations in drawing conclusions on the impact of intervention from price dispersion analysis alone. It shows that theory is less clear-cut regarding the expected impact of intervention on dispersion than it is on price levels and price increases, and underpins

<sup>60</sup> If focal points relate to fee levels, then this may limit dispersion of fee levels. If focal points relate to percentage annual increase in price then this may limit dispersion of annual price increases. The latter seems the more likely in the context of the fee-setting example in Paragraph 31 above, which identifies a possible focal range of six to eight per cent in annual fee increases.

the greater weight placed by this evaluation on analysing price levels and increases than on dispersion.

## Methodology

E.3 Descriptive analysis – presenting average deflated fee levels and changes for the control and treatment groups – is used to examine dispersion of tuition fees within groups, in particular to explore the impact of intervention on

- dispersion of fee levels<sup>61</sup>
  - of SS Schools – whether this falls after OFT intervention
  - of the difference between SS Schools and Non-SS Schools over time – whether this falls after OFT intervention
- dispersion of annual increases in fees<sup>62</sup>
  - of SS Schools – whether this falls after OFT intervention
  - of the difference between SS Schools and Non-SS Schools – whether this falls after OFT intervention

## Descriptive Analysis

E.4 Figure 8 and Figure 9 depict the dispersion<sup>63</sup> of fees around the mean for boarders and day pupils. The grey vertical lines mark the formal end of the anti-competitive practices by SS Schools as a result of OFT intervention.

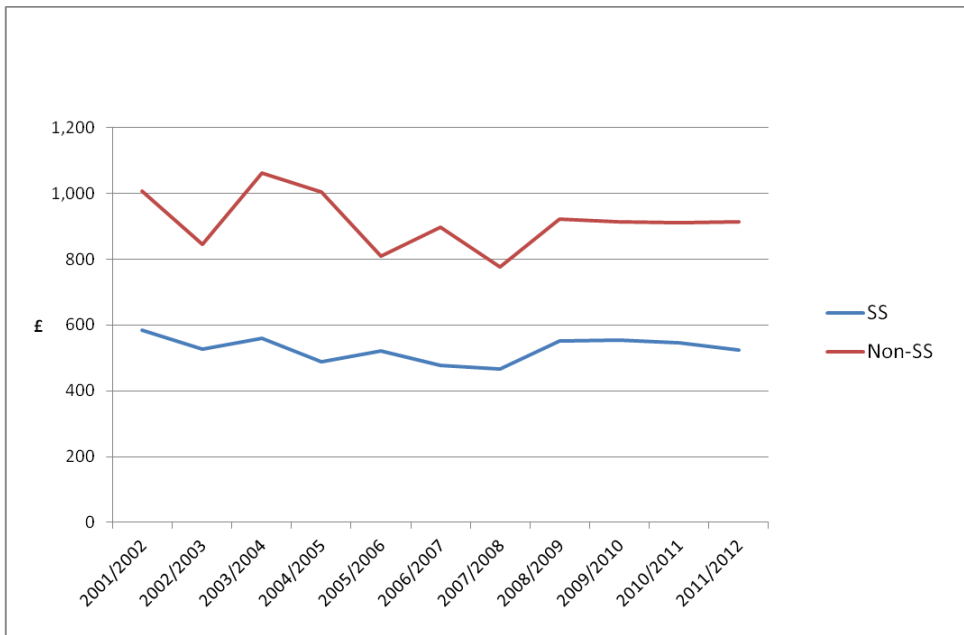
<sup>61</sup> Noting the caveat outlined above

<sup>62</sup> Noting the caveat outlined above

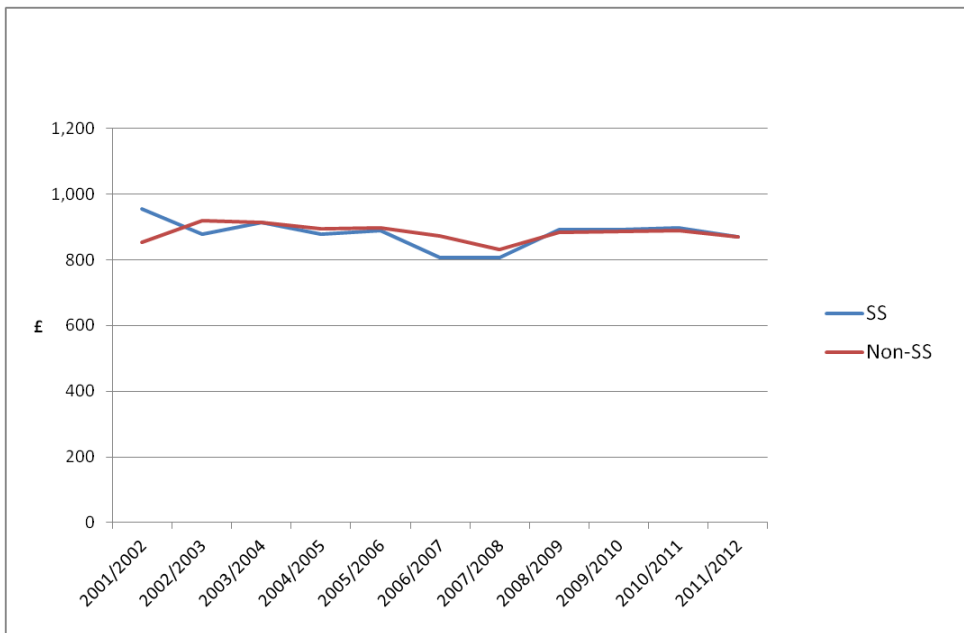
<sup>63</sup> Dispersion is corrected by the factor scaling the mean in any year to the mean in 2001/02



**Figure 8: Standard deviation of boarding fees (deflated, per term, mean corrected to 2001/02)**



**Figure 9: Standard deviation of day fees (deflated, per term, mean corrected to 2001/02)**



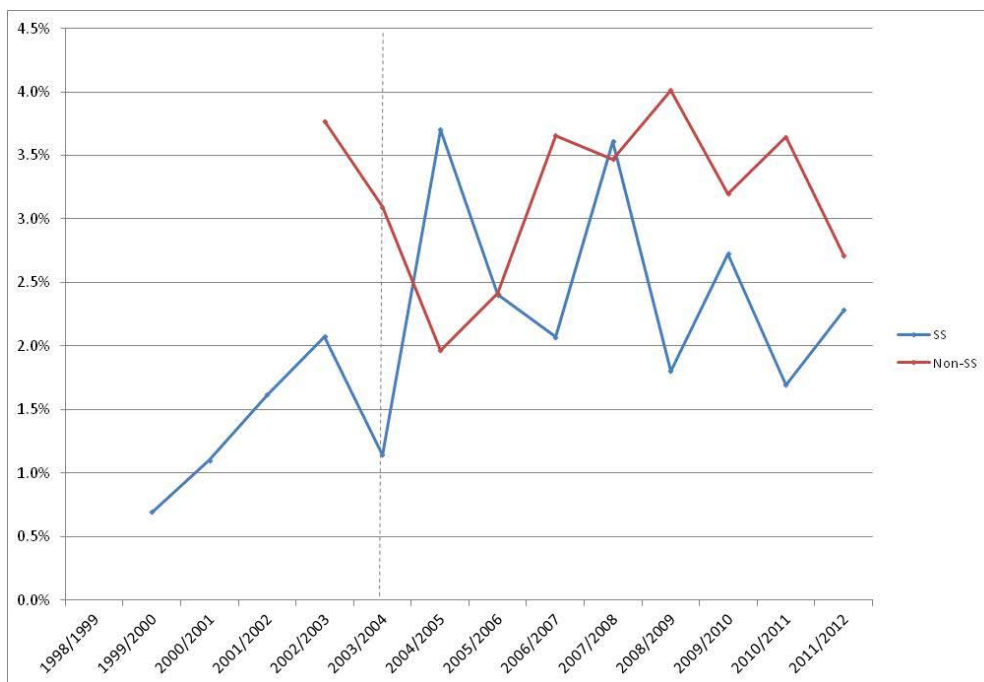
E.5 Figure 8 and Figure 9 show that

- Variation in dispersion of SS School boarding and day fees does not fall dramatically after OFT intervention.

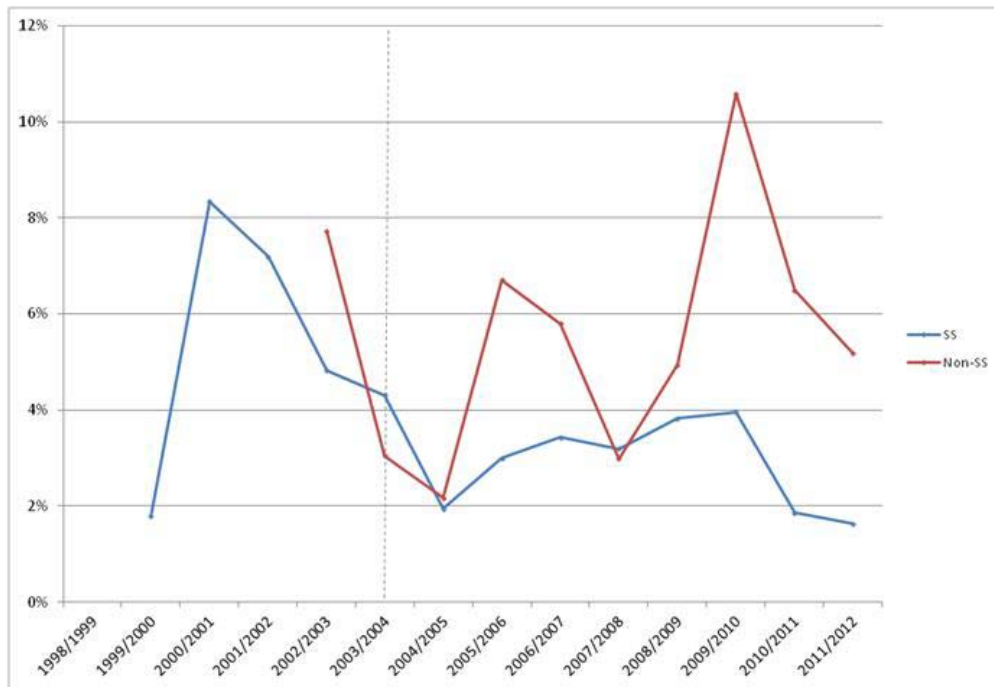
- There is not strong evidence that the difference in dispersion between the SS Schools and Non-SS Schools falls following OFT intervention.
- For boarding pupils, dispersion is consistently lower for SS Schools than Non-SS Schools. The continued low variation in SS Schools fees after OFT intervention suggests that if OFT intervention was effective in enhancing competition, it is not immediately obvious that this was effected by a change in dispersion (though econometric analysis might shed more light in this regard).

E.6 Figure 10 and Figure 11 depict the dispersion of annual fee increases around the mean for boarders and day pupils. SS Schools data pre-2001 is presented to provide further context. The grey vertical lines mark the OFT intervention.

**Figure 10: Standard deviation of annual percentage increase in deflated boarding fees**



**Figure 11: Standard deviation of annual percentage increase in deflated day fees**



E.7 Figure 10 and Figure 11 show that

- The biggest jump in the dispersion of SS School annual boarding fee increases is immediately after OFT intervention, from 1.2 per cent to 3.6 per cent. The dispersion for SS Schools is generally much lower in the years prior to intervention than after. Both of these observations are consistent with an effect of OFT intervention on dispersion.
- While boarding dispersion for SS Schools peaks immediately after OFT intervention, for the comparison group – the Non-SS Schools – dispersion is at its lowest in the two years after OFT intervention. Given that in the rest of the time period, dispersion for Non-SS School is generally higher than for SS Schools, the increase in dispersion of SS Schools immediately after intervention is all the more striking. This is consistent with OFT intervention in particular having an effect in driving an increase in SS School dispersion immediately after intervention. Two caveats are noted however. The first is that such an increase in dispersion immediately post-intervention is also consistent with the OFT intervention removing a

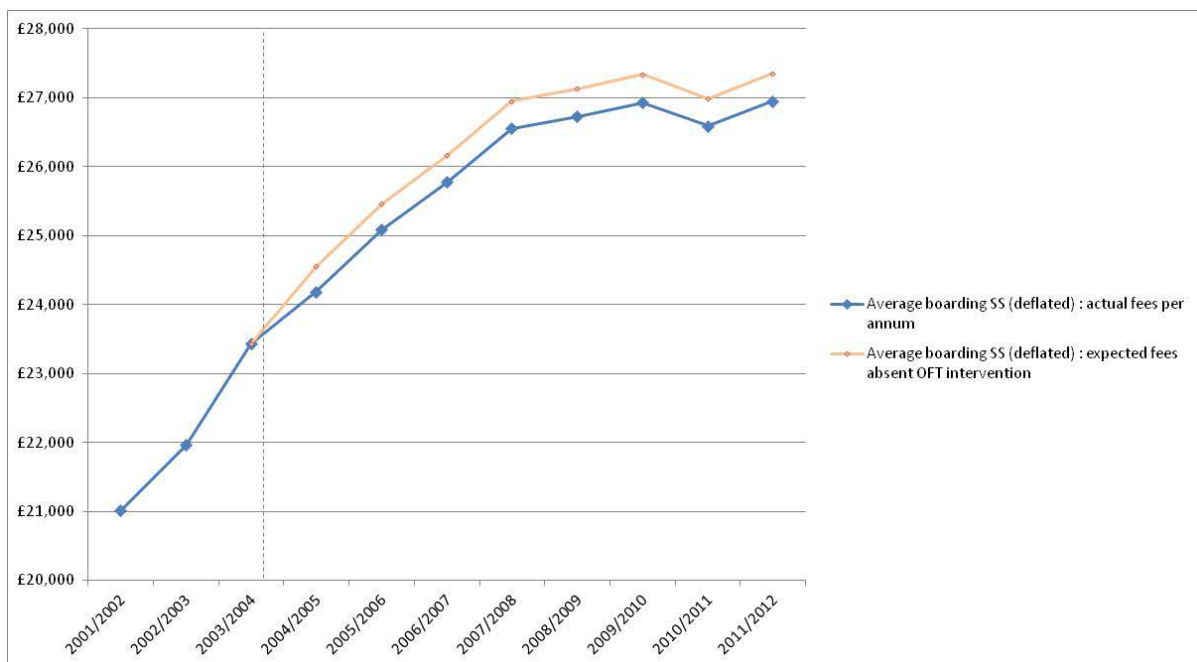
mechanism – information exchange – that allowed for greater transparency and a more similar understanding of demand. In this interpretation, the increase in dispersion need not be accompanied by an improvement in consumer welfare. The second caveat is that the great deal of variation in dispersion observed year on year over the broader time period suggests caution – based on dispersion analysis alone – in attributing with much confidence an effect on or through dispersion of OFT intervention.

- In relation to day fee annual increases, fee dispersion for SS Schools falls after OFT intervention, and remains lower in the post-intervention period than in the pre-intervention period.
- Dispersion of SS Schools day fee annual increases also remains lower than for Non-SS Schools in the post-intervention period. It is therefore harder for day fees than for boarding to identify a potential effect of OFT intervention in driving greater variation in dispersion of annual fee increases.

## F CONSUMER DETRIMENT AVERTED

F.1 The analysis has suggested that OFT intervention lowered boarding fees by 1.6 per cent in the period from academic year 2004/05 to the present. Figure 12 depicts the evolution of actual boarding fees against expected evolution of boarding fees had the OFT not intervened, for illustrative purposes assuming an immediate and constant effect of OFT intervention. The grey line marks the OFT intervention.

**Figure 12: Average annual boarding fees of SS Schools: actual and expected in absence of OFT intervention**



### Surplus – transfer from schools to the consumer

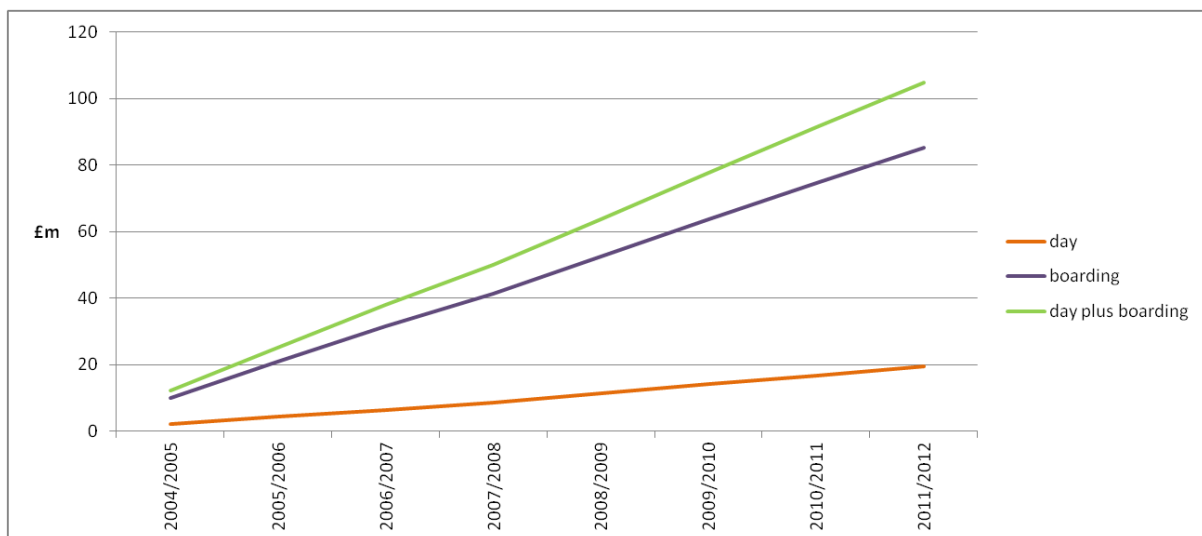
F.2 The transfer of surplus from schools to the consumer is estimated in point estimates of a 1.6 per cent reduction in boarding fees, and a 1.5 per cent reduction in day fees of SS Schools since OFT intervention. This is computed keeping levels of demand unchanged, that is, assuming that the number of pupils enrolling was not affected by the level of the fee. For simplicity, an immediate and constant impact of OFT intervention is assumed. Cumulative savings from 2004/05 to the present are calculated as: £85m in boarding fees, and £20m in day fees. This estimates the transfer of surplus from schools to the consumer as a

result of the elimination of anti-competitive practice following OFT intervention (and does not include deadweight loss averted – see ‘Deadweight loss averted’ in Paragraph F.7 below).

F.3 Owing to missing data points, the process of calculating fee revenues likely underestimates fee revenues by an estimated 10 per cent. However the process may also overstate fees revenues by omitting the effect of bursaries and scholarships and the potential for lower fees for younger students. By not attempting to correct for these opposing effects, the analysis assumes that fees revenues are reduced to 90 per cent of what they would be in absence of bursaries etc, that is, that these effects balance each other out.

F.4 Figure 13 below shows estimated cumulative consumer savings over time.

**Figure 13: Cumulative savings in fees to the consumer from OFT intervention, 2010 prices, £m, discounted to present**



F.5 Cumulative savings, for a range equating to a 95 per cent confidence interval (two standard errors each way), are estimated as lying between £12m and £159m for boarding fees. The equivalent range for day fees is between £1m to £38m for day fees, and between £13m and £197m in total.

- F.6 Dividing total annual savings in boarding fees by the number of boarders gives an **average saving of £495 per boarder per annum** (2010 prices, discounted to present).

### **Deadweight loss averted**

- F.7 The £85m of savings in boarding fees is an estimate of the consumer surplus that following OFT intervention is no longer diverted to SS Schools by the anti-competitive practice. This figure does not account for the deadweight loss – the extent to which higher prices force some consumers out of the market – of the anti-competitive practice. Given likely price inelastic demand and the capacity-constrained nature of SS Schools, the deadweight loss is likely to be small – particular relative to the transfer of consumer surplus, but could nevertheless be in the millions of pounds, again underlining the conservative nature of the evaluation.