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BETTER SCHOOLS OR BETTER PUPILS?

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Abstract

We estimate the causal effect of attending a state Faith school on primary education achievement in England using administrative student-level data and implementing various strategies to control for students' selection into Faith-schooling. Our regressions control for fixed-effects in prior achievement and residential postcode to compare pupils who are close residential neighbours and have identical observable ability. We also use information on future school choices to control for preferences for Faith schooling. Results show that pupils progress faster in Faith primary schools, but all of this advantage is explained by sorting into Faith schools according to pre-existing characteristics and preferences.

1. Introduction

Quality of childhood schooling is increasingly seen as important for life chances, because adults' success in the labour market is closely linked to early educational attainments (Carneiro et al., 2007, Cunha et al., 2006, Cunha and Heckman, 2008 and Heckman, 2000). However, economic and educational research has had little success in identifying resource-based interventions that are effective in raising school standards (Hanushek, 2003 and 2006). Government policy in many countries now favours reforms based on incentives, governance, increased choice and competition[1]. In England, this idea has become linked with the expansion of the state-funded Faith-school sector because it symbolises choice and diversity in the education system and embodies the kind of practice in governance that policy makers wish to promote, and – crucially – because it is claimed that Faith schools offer higher educational standards (DfES White Paper, 2005; DfES Education Bill, 2006). This case is, however, a difficult one to assess, because pupils that choose and get chosen by Faith schools differ from the population of pupils in ways that are correlated with their educational achievement.

Most of the existing research into the effectiveness of Faith schools is based on US Catholic schools, which are all private. Therefore previous studies do not distinguish the specific effects of religious affiliation on academic achievement from the effects of a private education. In contrast, most Faith schools in England are state funded and part of the mainstream state system. This makes England a more attractive setting for studying the specific effects of religious affiliation. In addition, it makes this country an interesting framework for assessing the effectiveness of polices aimed at improving state-funded education through the expansion of choice and alternative institutional arrangements. However, there is almost no evidence for England that makes any attempts to separate

[1] See LeGrand (1991), Le Grand and Bartlett (1993), and Machin and Vignoles (2005) for a review of the English experience, and Hoxby (2004) for an analysis of US based evidence.

out the causal effect of attending a Faith school on educational achievement from pure sorting and selection.

In this paper we present new evidence on this issue and tackle estimation of the impact of religious education under conditions in which the process of selection into Faith schools is uncertain and complex. We provide insights into whether the performance gap between Faith and Secular schools is driven by better schooling in the Faith sector, or by the fact that Faith schools admit 'better' pupils to start with. The ultimate aim of the empirical work is to compare standard national test scores at age-11 for students who attended Faith primary schools with near-identical students in the Secular (non-Faith) primary sector. To do so, we use data from an administrative census of primary school pupils in England which contains longitudinal detail on their tests at various ages, history of schools attended, and places of residence.

Previous (mainly US) research has made use of instruments for Faith school attendance such as family religion, neighbours' religion and other characteristics of place of residence. However, we agree with claims that these instruments are not credible when the point of the exercise is to purge estimates of family background and ability-related effects (Altonji et al. 2005a, 2005b). We argue that these instruments are generally inappropriate, because family religion is correlated with other background characteristics, and because families choose where to live for reasons often related to the school they wish their children to attend. In our context there are no credible instruments for Faith school attendance and it is difficult to draw firm conclusions about a unique parameter capturing the average causal effect of religious schooling. Instead, we use a variety of empirical strategies to bound the magnitude of the causal effect of attending a Faith primary school on educational attainment at the end of primary schooling (age 11). These strategies are based on our knowledge of the institutional and admissions arrangements in English schools.

To assist in reading the paper, we present next a brief 'road map' of the methods and results, which proceed in the following sequential fashion:

(a) We first exploit the sheer size and high level of detail in our dataset to control for prior, age-7 achievement and family background using prior subject-by-achievement-level fixed-effects and home-postcode fixed-effects (plus other control variables).

(b) We next take advantage of the fact that selection into Faith schooling occurs twice, once at the primary phase and again at the secondary phase. We therefore go on to include secondary-type-by-postcode fixed-effects (where 'type' is Faith or Secular schooling) or secondary-school-by-postcode fixed-effects in our regressions. This will be an effective strategy if there is comparable selection into both primary and secondary Faith schooling based *solely* on advantageous pupil and family background characteristics. By this method, we mitigate as much as possible the influence of confounding unobservables on pupil attainment at age 11. When we follow this route, we find only a small advantage from Faith primary schooling, worth about 1 percentile on age-11 test scores.

(c) However, controlling for secondary-school or secondary-school-type fixed-effects is problematic if there is selection into Faith secondary schooling according to Faith primary attendance, or on the academic outputs of primary schooling, in conjunction with selection on unobserved pupil and family background characteristics. Therefore, we go on to present a bounding exercise in which we estimate the Faith-primary school effect in two pupil sub-groups, namely: *(i)* the *stayers*, who attend the same school type in both the primary and secondary phases; and *(ii)* the *switchers*, who attend different school types in each of these phases. For the *stayers*, we compare the primary school achievements of pupils who attend both a Faith primary and Faith secondary with the primary school achievements of those who attend a Secular school in both phases. In the *switchers*' case, we compare the primary school achievements of students who attend a Faith primary school but a Secular secondary, with the primary school achievements of pupils who attend a Secular primary and Faith secondary school. Under the assumption that there is positive selection into Faith-secondary schools based on pupil and family background, there will be more positive sorting into Faith-primary schools in the sample of stayers than in the sample of switchers. Thus regression estimates based on stayers provide an

upper bound to the Faith-primary school effect, whilst regression estimates based on switchers provide a lower bound or a close-to-unbiased estimate of the causal effect of Faith primary schooling. Following the reasoning in Altonji et al. (2005b) – who suggest that the amount of selection on observables provides guidance on the amount of selection on unobservables – we assess the credibility of these arguments by looking at the degree of sorting on observable characteristics for these two sub-groups. We find that Faith-sector stayers have very different observable characteristics from Secular-sector stayers, whereas switchers exhibit much less observable sorting into Faith-primary schooling. In parallel, we find that Faith school stayers have an age-11 test score advantage of up to 2.7 percentiles, while Faith school switchers experience zero or negative gains. In practice, estimates based on switchers are likely to be a much better guide to the causal effects of primary Faith schooling than the estimates based on stayers, given that sorting into Faith primary schooling is much lower in the switchers' group.

(d) To formalize these intuitions, we conclude our analysis using the method developed in Altonji et al. (2005b) to explicitly assess the robustness of our findings to varying degrees of selection on unobservables. Our findings suggest that with moderate degrees of selection on unobservables the estimated advantage of attending a Faith school vanishes. More importantly, we find that when we constrain the amount of selection on unbservables to equal the amount of selection on observables, our estimates become zero or negative-insignificant. This is true for pupils in our full sample, as well as for the subgroups of stayers and switchers. In a nutshell, our results suggest that all of the advantage of Faith schooling can be explained by differences between pupils who attend Faith schools and those who do not.

The rest of paper has the following structure. The next section critically reviews the literature on the topic and its methods. Section 3 explains the different types of schools that exist in England and the data that we use. Section 4 sets out our empirical methods in more detail, and Section 5 discusses the results that arise from these approaches. Section 6 concludes.

2. A review of the methods used in previous studies

Evidence on the academic impact of Faith schools in England is limited and based on shaky foundations. Schagen et al. (2002) show that pupils in Faith secondary schools progress faster in English (but not in Maths and Science), and also seem to pass more subjects overall in their age-16 exams. Benton et al. (2003) report that pupils in schools affiliated with non-mainstream Christian (i.e. not Catholic or Church of England) and Jewish denominations show faster progress between age 11 and 16. Finally, in a study of two London boroughs with only 7 religious schools, Prais (2005) finds quite strong Faith school advantages in Maths, particularly amongst weakest pupils. However, none of these studies takes any steps to control for pupil background or otherwise deal with selection on unobservable characteristics that influence educational progress.

In contrast, a fairly large literature on the topic has emerged in the US and has focussed on private Catholic schools – largely springing from the influential work of Coleman (1982)[2]. Overall, this research finds that attendance at a Catholic school raises students' graduation rates and test scores, though there is variation across different demographic and geographical groups, and across subject areas. The following discussion will focus on whether there is anything we can learn from the methodological approaches used in the US and international literature, rather than the results per-se. As noted above, an important difference between the US and England is that Faith schools in the US – mainly Catholic – are private-sector schools whereas Faith schools in England – mainly Catholic and Church of England – are part of the state-school system. Nevertheless, we face the same empirical issue as US researchers, namely that there is non-random sorting of pupils into Faith schools, such that religious school attendance is correlated with educationally-advantageous pupil and family characteristics. Therefore, researchers interested in the causal effect of religious education have to look for a source of random variation in the probability of Faith school attendance that can be used as an instrument. Disappointingly, many of the instrument choices do not seem credible, and the

[2] See Dronkers (2004) for a review of the European evidence on educational and behavioural effects of attending a Faith school, and a detailed analysis of the historical and institutional determinants of religious education in Europe.

evidence in Altonji et al. (2005a) and (2005b) is not supportive of any of those that are commonly used.

One commonly used instrument is family religion, because being Catholic is a strong determinant of attendance at a Catholic school (see Noell, 1982, Evans and Schwab, 1995 and in part of Neal, 1997). However, a priori, it is hard to believe that 'religiosity' is randomly assigned so that it does not correlate with unobservables that affect family attitudes towards education, school achievements and economic outcomes. In fact, recent studies show that a wide range of outcomes are correlated with religiosity, including income, disability, marriage, divorce (Gruber, 2005), economic growth (Weber, 1905, Barro and McCleary, 2003), and attitudes that are conducive to positive economic outcomes (Guiso et al., 2003). On balance, we are not convinced that family religion is a useful instrument for Faith school attendance.

Other instruments exploit information on the local 'supply' of Faith schooling based on the geographical density of Catholic schools (Neal, 1997, Figlio and Stone, 1999, Grogger and Neal, 2000). We find this idea unconvincing theoretically. Families surely decide to live near Catholic schools in part because they want their children to go to Catholic schools. In fact, a lot of existing evidence highlights the role of schools in housing choices (Black, 1999, Gibbons and Machin, 2006, Kain, Staiger and Reigg, 2005). Therefore, instrumenting religious school attendance with distance to/density of Catholic schools is unlikely to fix problems related to the endogeneity of Catholic school choice. For similar reasons, local demographic characteristics, such as the proportion of Catholics in the neighbourhood, are inappropriate instruments if families have made the choice to live in this type of community. Others have tried to use interactions of these variables as instruments, whilst controlling for their levels (e.g. Sander, 1996), but the testing in Altonji et al. (2005a) suggests this approach is not fully satisfactory.

In short, our argument is that previous research has neglected the problem of sorting induced by residential choice, which implies that many geographical variables are inappropriate instruments for school choice. To evaluate our critique, we partly replicated this type of empirical strategy on our

English data (described later) using instruments similar to those in Neal (1997).[3] Although the instruments are powerful in the first-stage equation, they are also significantly related to a variety of neighbourhood characteristics, including local average house prices, local unemployment rates and the fraction of adults with higher levels of education in the local population. Additionally, the IV coefficients are always above OLS estimates implying strong negative selection into Faith schools such that pupils with educationally disadvantageous characteristics choose Faith schooling. This negative sorting is implausible in the English school system, as we show later. In conclusion, these instruments are likely to be invalid and simply magnify the effect of unobservable parental preferences and school-side selection in our context.

Given the weaknesses in the IV approach, other researchers have tried alternative methods. Jepsen (2003) uses value-added models to control for pupil background characteristics and finds no impact of Catholic schools on test scores. In a different approach, Altonji et al. (2005a) and (2005b) infer the degree of selection bias in the Catholic school effect from the extent of selection on observable pupil characteristics, and conclude that whilst there is an impact on high school and college graduation rates, there is no influence on test scores. Our approach is closer to these more recent US studies, but we have the advantage of a dataset on the population of students in England, containing information on pupil’s prior attainments, demographics and details on precise geographical location. Before discussing our methods, we outline the institutional context for Faith schooling in England, and the details of the dataset we will use.

3. Institutional context and data

3.1. School governance

Primary schools in the state-sector in England fall into four categories that differ in terms of governance, ownership of the school buildings, employment of teaching staff and control over pupil

[3] Results are not tabulated for space reasons. However, a selection of our findings and some further discussions are presented in the on-line appendix.

admissions[4]. The key differences between these school types – Community, Foundation, Voluntary Aided and Voluntary Controlled – are set out in Table 1. The most relevant distinctions for our analysis relate to the composition of the governing board and to pupil admissions.

State schools receive nearly all their funding from central government, through Local Authorities (LAs) that are responsible for schools in their geographical domain. Schools, other than Community schools, are linked to a Faith or other local charitable organisation. All schools are run by a Governing Body composed of members elected from amongst parents and staff (Parent Governors and Staff Governors), appointed by the LA (Representative Governors), appointed by the Faith or charitable organization that owns the school premises (Foundation/Partnership Governors), and appointed from the community (e.g. local businesses) by the Governing Body. The Governing Body sets the strategic direction of the school, draws up school policies, sets targets and monitors performance, although the day-to-day running is down to the head-teacher and his or her leadership team. The constitution of the Governing Body is important because it determines how much influence various 'stakeholders' have in the way the school is run – in particular, the balance of influence by the LA and by the Faith or charitable foundation/partnership.

3.2. Pupil admissions

In principle, admission to schools is based on parental preference, but in practice demand often outstrips the supply of places in popular schools and places are rationed on the basis of various school-specific oversubscription criteria. For the period covered by this research, these criteria conformed to a central government Code of Practice on Admissions (and today, these criteria are regulated by law). According to these rules, primary schools should not select by aptitude. Instead, for Faith-based schools, one of the overriding considerations for admission is typically attendance at a local church or a recommendation from a local minister. For Secular schools, the distance between

[4] In addition there is a small private, fee-paying sector, which we do not consider here. This educates around 6-7% of pupils in England as a whole (even though they tend to be over-represented at the leading universities in the UK).

a child's home and school is one of the key considerations. Because of these criteria and the constraints of travel costs, residential choice and school choice decisions are very closely linked. Even so, most households can choose between two or more schools from where they live, and have a choice between a Secular and Faith school.

An important distinction relevant to our study is that Voluntary Aided and Foundation schools are responsible for their own pupil admissions and hence have more flexibility in deciding which pupils will be enrolled when the school is oversubscribed. In fact, during the period under analysis, some Faith schools and other schools that we class as 'autonomous' (see Section 3.1 below), were allowed to interview or otherwise screen families – ostensibly to determine their religious or other ethical convictions. However, it has long been suspected that this resulted in covert selection based on parental and pupil characteristics that are correlated with pupil ability[5]. In other school types, the LA is the admissions authority, which will apply much more mechanical rules when deciding which pupils go to which schools.

3.3. School classifications in our empirical work

Given our emphasis on religious education and the heterogeneity in terms of governance and admissions arrangements, we re-arrange schools into four categories. The breakdown is as follows:

- *Secular-non-autonomous*: includes schools that have no religious affiliation and are Community or Voluntary Controlled.
- *Secular-autonomous*: includes schools that have no religious affiliation but are Foundation or Voluntary Aided.
- *Faith-non-autonomous*: includes schools that have a religious affiliation and are Voluntary Controlled.

[5] West (2005) and West and Hind (2003) present detailed qualitative evidence on this issue, while Allen (2007) provides some statistical analysis. Even recent news investigations have reported that Faith schools implicitly make admissions conditional on 'voluntary' church donations (The Times, 2008)

- *Faith-autonomous*: includes schools that have a religious affiliation and which are Foundation or Voluntary Aided.

Note that the aim of this breakdown is twofold. On the one hand, we want to highlight the religious affiliation of some state-sector schools in England. On the other hand, we want to emphasize the fact that some religious school, as well as some Secular schools, have more independent governance structure and control over their admissions. These 'autonomous' arrangements might: *(i)* affect the way in which schools admit pupils; and *(ii)* affect the way in which the 'ethos' of the religious (or other) charity influences their daily functioning and strategic leadership. Our empirical work will explore differences in achievements of pupils in these schools, and consider to what extent these are caused by their religious character, as opposed to their autonomous arrangements[6]. First, however, we explain the way attainment is assessed in English primary schools, and describe the data we will use.

3.4. National curriculum and assessment

Compulsory education in England is organised into five stages referred to as Key Stages. In the primary phase, pupils enter school at age 4-5 in the Foundation Stage and then move on to Key Stage 1 (*ks1*), spanning ages 5-6 and 6-7. At age 7-8 pupils move to Keys Stage 2 (*ks2*), sometimes – but not usually – with a change of school[7]. At the end of *ks2*, when they are 10-11, children leave the primary phase and go on to secondary school where they progress through Key Stages 3 and 4. At the end of each Key Stage, pupils are assessed on the basis of standard national tests, and progress

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[6] Almost all Faith schools in England are either Church of England or Catholic schools. Only a minority of schools (enrolling less than 1% of pupils) is associated with other Faiths (e.g. Jewish or Muslim). Catholic schools account for 53% of the Faith-autonomous sector, while nearly all schools in the Faith-non-autonomous sector are affiliated to the Church of England. In earlier versions of this work, we studied whether there is any heterogeneity in our conclusions along the dimensions of different religious affiliation, but failed to find any. Results are available from the authors.

[7] In few cases there are separate Infants and Junior schools (covering Key Stage 1 and 2 respectively) and a few LAs operate a Middle School system (bridging the primary and secondary phases). We do not consider these schools here.

through the phases is measured in terms of Key Stage Levels. A point system is applied to convert these levels into scores that represent about one term's (10-12 weeks) progress.

3.5. The data

The UK's Department for Children, Families and Schools (DCSF) collects various data on school and pupils for funding and administrative purposes and to compile school performance tables. This National Pupil Database (NPD) holds information on each pupil's assessment record in the Key Stage tests throughout their school career. Since 2002, the data also includes information on pupils' school, gender, age, ethnicity, language skills, special educational needs or disabilities, entitlement to free school meals and other characteristics. Importantly, the data also includes home-postcode, which typically encompasses 15 contiguous housing units, allowing us to control very carefully for residential location. The NPD therefore provides us with a uniquely large and detailed dataset on pupil characteristics and their test histories with details on the Levels reached in Maths, English and Science (although Science is not tested at *ks1* so we do not make use of it). Additionally, for *ks2* and beyond, the data also contain raw scores in the various component tests.

In our analysis, we use information on two cohorts: those aged 10-11 and sitting their *ks2* tests in 2002 and 2003, who took their *ks1* tests in 1998 and 1999 respectively. We can further observe which school these pupils go to when they move on to secondary school in 2003 and 2004. Various other data sources can also be merged in at school level – in particular each school's religious affiliation and the institutional types described above in Section 3.1. We will use this large and complex combined data set – which gives us information on around 1 million pupils in over 14,000 primary schools in England – to estimate the influence of Faith schools on pupil achievement at the end of *ks2* (age 11), conditional on achievement at *ks1* (age 7). In the next section we set out the empirical strategy more precisely.

4. Empirical Model and Strategy

4.1. Basic model

As discussed above (see Section 2 and 3.2), Faith school choice is potentially endogenous to pupil achievement. Firstly, families with a preference for religious schools may have characteristics that directly influence academic progress in their children. This pupil-side 'selection' (sorting) into Faith schooling is the main threat to the identification of the causal effect of Faith schooling relative to Secular schooling. Secondly, in the English context, Faith schools at both primary and secondary level have much greater control over their own pupil admissions than do Secular schools. Hence, admission to a Faith school may be partly determined by educationally relevant attributes that schools can observe – but which we cannot – which adds to the identification problems[8]. As outlined in Section 2, we do not believe there are any credible instruments that would solve this identification problem. Nevertheless, we do have a wealth of information on pupil characteristics, records of achievement, residential location and history of school attendance which can be used to bound the estimates of the effect of attending a Faith primary school.

The basic model that we estimate is a standard pupil-level value-added model of achievement, which measures the association of various pupil and school characteristics with test scores at *ks2*, conditional on test scores at *ks1*. In our two-period empirical setup, the subscript *2* denotes the Key Stage 2 phase, and achievement of pupil i in Maths and English at *ks2* (ks_{i2}) builds on prior attainment in these subjects at *ks1* (ks_{i1}), and is modified by school-type factors (β_{j2}, a school effect that is identical for different pupils in school type j at Key Stage 2), observable personal/family

[8] In the US private Catholic school setting, these factors are theoretically related to the benefits of choosing a Faith school, since attendance at a private school rather than a pubic school imposes financial costs. In England, conditional on place of residence, admission to a state Faith school does not incur high additional costs relative to a non-Faith school. The only likely cost is the effort of demonstrating some religious commitment through church attendance.

characteristics (x_i', mainly ethnicity, free meal entitlement, gender, language) and unobserved pupil/family characteristics (η_i, with $E(\eta_i)=0$):

$$ks_{i2} = \beta_{j2} + x_i'\gamma + g(ks_{i1}) + \eta_i + \varepsilon_{i2} \qquad (1)$$

Note that x_i', ks_{it} and ε_{i2} (other unobservables) all include school and school-type specific components, but for simplicity we suppress any school or school-type subscripts on these variables. The parameter of interest is β_{j2}, i.e. the primary school-type effect for the various school types discussed in Section 3.3. The essential empirical problem is that family-side selection of schools and school-side selection of pupils before *ks2* imply that primary school choice is endogenous to the unobserved pupil/family characteristics η_i: the unobservables that influence the rate of progress during *ks2* also influence school choice probabilities, so that $E(\eta_i \mid ks_{i1}, x_i, j=k) \neq 0$. Estimates of β_{j2} that do not control for $E[\eta_i \mid j]$ are biased estimates of the expected impact of Faith school attendance.

4.2. Prior achievement and home-postcode fixed-effects

Most previous research on Faith school effects had to make-do without information on prior achievement. In contrast, we can control for prior achievements in a highly flexible fashion by adding to our specifications a large number of fixed-effects (183 dummies) for combinations of the levels in Reading, Writing and Maths in *ks1* tests. Even so, conditioning on prior test scores and observable pupil characteristics is most likely an incomplete way of controlling for pupil selection into different school types (see Manning and Pischke, 2006). So we must take further steps to control for sorting into Faith schooling.

Previous literature has used geography-based instruments to deal with the problem of selection on unobservables into Faith schooling. However, basic theories of urban economics suggest that households with different incomes and preferences sort into communities according to the benefit they can derive from local amenities and the income available to pay for housing or taxes (Tiebout, 1956). Given that residential choice and school choice decisions are closely linked, we believe it must be preferable to use place of residence to *control* for the endogeneity (via η_i) of school choice, rather than use it as an instrument. Our data is ideal for this purpose, because the geographical detail and density of pupils means we can identify groups of pupils who live in the same postcode (typically 15 contiguous housing units), but attend different schools. We therefore include home-postcode fixed-effects in our models to partially condition out η_i , i.e. unobservable family income, background and preferences-related factors that are linked to residential choice.

4.3. Secondary school-type and school fixed-effects

Controlling for place of residence will still not be enough to eliminate selection into Faith schooling on the basis of unobservable factors in (1) if Faith school attendance is linked to different preferences or unobserved attributes even amongst pupils living in the same postcode. To tackle this issue, we take advantage of the fact the selection into Faith schools occurs twice – once at primary and once at secondary school – and assume that choice of secondary school conveys information about selection at the primary stage, including family preferences for Faith schooling and school preferences for different types of family. In practise, the same types of school found at the primary phase (Table 1) are also found in the secondary phase, and we exploit this fact to control for components of η_i that jointly influence primary and secondary school choices. We implement this idea by adding secondary-school-type × home-postcode fixed-effects or secondary-school × home-postcode fixed-effects to our value-added model of equation (1)[9]. Identification comes from the comparison of pupils who attend a different primary school type, but are otherwise 'matched' in the sense that they live in the same postcode and attend the same secondary school/type. Put simply, our argument is

[9] The use future information to control for unobservables is similar to Grogger (1995) on the effects of arrest on labour market outcomes. The author compares the pre-1984 earnings of a sample of individual arrested in 1984 or earlier – the 'treated' group – to the pre-1984 earnings of a sample of individuals whose first arrest occurred after 1984 – the 'control' group.

that pupils from Secular-primary schools are likely to be better comparators for pupils from Faith-primary schools when they are in the same secondary school/type.[10]

Under plausible assumptions, estimates of the Faith-primary effect that control for secondary-school/type will be less biased than those that only control for prior achievement and home-postcode fixed-effects (plus other controls). Specifically, these assumptions are that: *(i)* selection into primary and secondary school depends on the same unobservable pupil/family attributes η_i, and selection on η_i operates in the same direction at both phases; and *(ii)* other unobservable factors that determine Faith-school attendance at either primary or secondary phase are uncorrelated with η_i, or otherwise with *ks2* achievement (conditional on observable pupil attributes).

However, controlling for secondary school/type will not reduce the bias in our estimates of β_{j2} if there is only weak correlation between the unobservables that influence Faith-primary attendance and the assignment to secondary school. In this case, secondary school choice conveys no information about selection into primary schools.

More importantly, there are scenarios where controlling for secondary school/type fixed-effects could make our estimates more biased than they would be without these controls. These arise when there is *differential* selection at the secondary level between pupils coming from Faith primary and those coming from Secular primary schools, and this differential selection is strong relative to selection into Faith primary schooling. One such case occurs when there is positive selection on unobservables η_i into Faith-secondary schools, but Faith-secondary schools prefer pupils coming from Faith-primary schools (i.e. they set a lower threshold for η_i), conditional on all other characteristics. A second possibility is that Faith-primary schools tend to raise student achievement,

[10] Note that controlling for secondary-school fixed-effects might be problematic because of what Angrist and Pischke (2009) call the ‘bad controls’ problem (pp. 64-68). For example, if faith primary schooling is *not* endogenous in the first place, then controlling for a characteristic (secondary schooling) that is potentially determined by faith primary schooling and correlated with achievements will make the bias worse than if the control had not been included. See further discussions in the body text and Appendix A for details.

and Faith-secondary schools screen pupils on the basis of value-added (i.e. pupil *ks2* scores, conditional on background and *ks1* scores). Both these cases involve *negative* selection of Faith-primary pupils by η_i *within* secondary schools and secondary school types. This negative selection occurs because only the highest-η_i pupils from Secular primary schools are selected into Faith secondary schools, whilst middle-to-high-η_i pupils from Faith primary schools are selected into Faith secondary schools. At the same time, the lowest-η_i pupils from Faith secondary schools are selected into Secular secondary schools, whilst the low-to-middle-η_i pupils from Secular primary schools end up in Secular secondary schools. As a consequence, Secular-primary pupils could be bad comparators for Faith-primary pupils in the same secondary school/type leading to a downward bias in the estimate of the effect of attending a Faith primary school.

Note however, that this is the case only if selection into Faith primary schools is relatively weak. On the other hand, if there is strong positive selection into Faith primary schooling – as we claim – this potential differential selection into Faith secondary schools that favours Faith primary pupils tends to work in our favour, and including secondary school/type fixed-effects reduces the bias. This is because the low-η_i pupils from Faith primary schools who are forced out into the state secondary sector are better comparators for the low-to-middle-η_i pupils from the distribution in the Secular primary sector. Similarly the high-η_i pupils drawn into the Faith sector from Secular primary schools will be better compartors for the middle-high-η_i pupils from Faith primary schools who stay in the Faith sector (more details are provided in Appendix A).

In conclusion, the institutional arrangements in England mean that models with secondary school/type fixed-effects are likely to improve on OLS or specifications with postcode of residence fixed-effects. This is so for a number of reasons. Firstly, casual observation and the results we present later indicate that there is strong positive selection into Faith schooling at both phases, and that the main determinants of Faith school attendance at both education phases are persistent aspects of family background and preference. Indeed, one of the main criteria for admission into Faith schools is some demonstration of religious commitment, which is unlikely to change between primary and secondary phases. Similarly, it is unlikely that there is any differential selection at the secondary phase by primary school-type of origin, because secondary schools do not screen either on primary school attendance or on *ks2* achievements. In fact, neither primary nor secondary schools have any reliable measure of pupils' prior academic progress at the time when they admit them. Primary schools admit pupils before any testing has taken place, whereas pupils apply to and receive admission offers from secondary schools before they have taken their *ks2* tests. In addition, formal links between primary and secondary schools are rare in England. Finally, West (2005) and West and Hind (2003), report that the vast majority of (covertly) selective schools screened their pupils on the basis of background criteria such as: children of employees; children of former pupils; children with family connections to the school; children with specific talents in music, dance or arts. The authors do not report any evidence of selection based on general academic progress during primary education, or on the type of primary school attended.

4.4. Bounding the effects using switchers and stayers

The possibility of differential selection into secondary schools according to Faith primary status leads us to devise a bounding exercise that exploits the movements in and out of Faith schooling at the transition between primary and secondary phases. To implement this exercise, we estimate the primary school-type effects β_{j2} in two pupil sub-groups, namely: *(i)* the *stayers*, who attend the same school type in both the primary and secondary phases; and *(ii)* the *switchers*, who attend different school types in each of these phases. As we illustrate in Appendix A, regression estimates based on the comparison of Faith-primary-Faith-secondary stayers with Secular-primary-Secular-secondary stayers provide an upper bound to the impact of Faith-primary schools on achievement. In contrast, estimates based on the comparison of Faith-primary-Secular-secondary switchers with

Secular-primary-Faith-secondary switchers provide a lower bound.[11] These claims hold true if: *(i)* there is positive selection into Faith-secondary schools on the basis of pupil/family background η_i; and *(ii)* Faith secondary schools do not discriminate *in favour* of Secular primary pupils, i.e. Faith secondary schools either do not discriminate on the basis of the primary school-type, or they are more likely to admit Faith primary pupils than Secular primary ones, conditional on η_i.

The explanation for these claims about the bounds provided by the stayers and switchers is as follows. Suppose that Faith secondary schools admit Faith primary school pupils whose η_i falls above some threshold τ, whereas they only admit Secular primary pupils above a higher threshold τ'. The distribution of η_i amongst Secular-primary-Secular-secondary pupils is right truncated at τ', whereas the distribution of η_i amongst Faith-primary-Faith-secondary pupils is left truncated at $\tau < \tau'$. The difference in mean η_i between Faith-primary-Faith-secondary pupils and Secular-primary-Secular-secondary pupils in these truncated distributions is therefore necessarily greater than the difference in means between the un-truncated distributions of Faith-primary and Secular-primary pupils. As a consequence, the stayers' comparison provides an upward biased estimate of the effect of Faith schooling. In contrast, the distribution of η_i amongst Secular-primary-Faith-secondary switchers is left truncated at τ', whereas the distribution of η_i for Faith-primary-Secular-secondary pupils is right truncated at τ. Thus, the difference in mean η_i between Faith-primary-Secular-secondary pupils and Secular-primary-Faith-secondary pupils in these truncated distributions is necessarily negative (even if $\tau = \tau'$), so that the switchers' comparison provides a downward biased estimate of the effect of Faith schooling.

Before moving on, two further cases are worth discussing. Firstly, if switching in and out of Faith schooling during the primary-to-secondary transition is random (conditional on η_i), then the comparison of Faith-primary-Secular-secondary pupils with Secular-primary-Faith-secondary pupils

[11] In practise, when applying this method, we have four school types (two Faith and two Secular), but label as stayers pupils who stay in the same school type in both phases, and as switchers those moving between any of these four types.

provides an unbiased estimate of the causal Faith-primary effect on achievement. This is because in this case switchers-in and switchers-out of Faith schooling are likely to be well matched in terms of unobservable characteristics that are relevant to primary school achievements, since they are simply 'marginal participants' in Faith education. As detailed in Section 4.3, we argue that this is the most likely scenario: Faith-school selection is based mainly on persistent pupil/family background characteristics η_i, but for 'marginal participants' the decision to exit or enter Faith schooling on transition from primary to secondary school is more likely to be related to the quality of local secondary schools or to changes in family circumstances, neither of which affects *ks2* achievements. In our empirical evidence below, we will back up this claim by showing that switchers exhibit much less sorting into Faith-primary schooling by observable characteristics than do pupils in the whole population or in the stayers' group. Secondly, families might choose secondary school type taking into account the 'quality of the match' between the child and the sector in the primary stage as represented by ε_{i2} in equation (1), with a low realisation of ε_{i2} inducing families to switch school types. The rationale for doing so is not self-evident because families cannot observe the counterfactual match in the sector they did not attend, and the transition from primary to secondary school involves leaving the school that disappointed them in any case. Nevertheless, even under this scenario, the stayers' comparison provides an upper bound to the causal effect of Faith primary schooling, whereas the switchers' analysis yields estimates that are always less biased than in the full population of pupils and – under the most plausible assumptions – close to unbiased. Full details are provided in Appendix A.

In conclusion, it is useful to summarise our arguments about estimating the causal effect of Faith-primary schooling using either specifications that include secondary school/type fixed-effects, or exploiting the switchers' and the stayers' comparisons. We have argued that:

(a) Secondary school/type fixed-effects reduce the bias in our estimates if there is no differential selection by family/pupil background (η_i) into Faith secondary schools according to Faith primary status; or if any differential selection by family/pupil background into Faith

secondary schools according to Faith primary status is weak relative to selection by family/pupil background into Faith primary schools.

(b) Under all plausible scenarios, the stayers' comparison provides an upper bound, and the switchers' comparison either a lower bound or a close-to-unbiased estimate of the parameter of interest. This is so unless Faith secondary schools discriminate *against* Faith primaries and/or considerations about 'quality of the match' at primary school (ε_{i2}) are paramount when choosing secondary school type, which we believe is very unlikely.

(c) Under the conditions which we believe prevail in England, results based on the sample of switchers yield close-to-unbiased estimates of β_{j2}.

Although we cannot directly test these conjectures relating to unobservable selection, we draw on Altonji et al. (2005b) who suggest that it is likely that the amount of selection on unobservables 'tracks' the amount of selection on observables. Therefore the degree of selection on unobservable characteristics η_i across pupil sub-groups can be explored by looking at the degree of selection on observable characteristics ks_{i1} and x_i'. In practice, this means that we study how the estimates $\hat{\beta}_{j2}$ change as we move from the whole population of pupils to the subgroups of stayers and switchers, and that we compare these changes with the degree of sorting on observable characteristics $E[ks_{i1} \mid j]$ and $E[x_{i2} \mid j]$ as we move across the groups. By this method, we can gauge the extent to which differences in $\hat{\beta}_{j2}$ in these sub-samples might arise through differences in selection on unobservable factors $E[\eta_i \mid j]$.

4.5. Calibrating selection on unobservables to selection on observables

To formalize these intuitions, we conclude our analysis using the methods in Altonji et al. (2005b) to explicitly assess the robustness of our estimates to varying degrees of selection on unobservables. In particular, we investigate: *(i)* how much positive selection on unobservables we need in order to drive our estimates of the Faith primary effect to zero; and *(ii)* how robust our estimates are to the assumption that there is an equal amount of selection on observables and unobservables. Once again, this exercise is carried out for all pupils in the sample, as well as separately for switchers and stayers.

The results from this investigation will serve two purposes. First, we will use them to show that the estimated Faith-primary advantage vanishes when we constrain the amount of selection on observables to equal the amount of selection on observables. Secondly, we will use this method to directly validate our intuition that Faith school stayers are more positively sorted into religious schools in terms of both their observables and unobservables, than switchers. This will further lend support to our claims that estimates of the effect of attending a Faith primary school for switchers and stayers help us bound the causal impact of religious education, and that estimates based on the sample of switchers are closer to what the 'true' causal impact of Faith schooling would be in the absence of sorting and selection. Full details of our implementation of the Altonji et al. (2005b) methodology are provided in Appendix B.

5. Results and discussion

5.1. Descriptive statistics

The basic facts about the association of pupil *ks2* attainments and the type of primary school attended are summarised in Table 2. The school categories were explained in Section 3 above. The table shows the means and standard deviations of pupil *ks2* test scores, where the raw test scores are converted into percentiles. Notice that in all the empirical analysis that follows we will work with the average of one pupil's percentile in the Maths and English distribution because we found no interesting differences between these two subjects. Summary statistics are shown in Row 1 for the whole sample, and then split by broad school type. The figures show the key feature that we wish to analyse: pupils emerging from primary schools that are classified as Faith schools under our definitions have higher levels of attainment than those emerging from Secular schools. The difference is about 4.75 percentiles in the pupil test score distribution.

Splitting this gap into the finer school classifications defined above, we see that the apparent Faith-primary effect in Row 2 is more specifically associated with Faith schools that we classify as autonomous. Secular schools with comparable institutional arrangements similarly show higher average test scores than other Secular-non-autonomous schools. Next we consider to what extent these higher scores are attributable to differences in the characteristics between pupils who enter these schools and those who do not.

5.2. Regression estimates of sorting on pupil background and ks1 scores

Firstly, we show that there are important and significant differences between school types in terms of the observable characteristics of pupils at the beginning of the Key Stage 2, i.e. the age 7-11 phase. Table 3, Row 1 reports overall means and standard deviations of *ks1* attainment and background characteristics. Table 3, Rows 2-5 report results from regressions of these characteristics on school-type dummies (with Secular-non-autonomous schools as the baseline). The pupil characteristics included in our analysis are: *ks1* level points (here averaged across Reading, Writing and Maths); eligibility for free school meals; special educational need status (SEN); White ethnic origin; and English as a first language.

It is evident from this table that autonomous types of schools (Faith and Secular) are at an advantage over standard non-autonomous Secular primary schools, both in terms of *ks1* achievement and background characteristics usually associated with educational disadvantage. Pupils start off in autonomous schools in Key Stage 2 with *ks1* test scores that are, on average, 1.2 to 1.7 Level points (1 Level point is equivalent to one term) ahead of their counterparts in Secular-non-autonomous schools. This is around 15% of one standard deviation and about the same as the advantage in terms of final scores at age-11 reported in Table 2. Certainly, this difference in *ks1* between pupils in different school types may partly emerge because some pupils have already gained or lost out from the time spent in their respective primary schools. However, pupils in Faith-autonomous schools are also much less likely to be on a low income that entitles them to free school meals, more likely to be White and more likely to have English as their first language. The advantage of these schools in terms of lower free school meal entitlement also amounts to 15-20% of one standard deviation, and it is impossible that these differences in background can be a *consequence* of Faith school attendance.

Some of these disparities can be explained by differences in geographical setting, but not all: Columns 6-10 report the same regressions once we include home-postcode fixed-effects, and show that many differences persist even across pupils who live in the same postcode, but attend different types of school. These differences are less marked in terms of ethnicity and languages, but still strong in terms of free school meal entitlement and prior attainment. Pupils in Secular-autonomous and Faith-autonomous schools still start the Key Stage 2 phase with *ks1* scores that are 0.7 to 1.2 points ahead of Secular-non-autonomous pupils who live in the postcode, and are between 2.5 and 4 percentage points less likely to be eligible for free meals (on a base of 20 percent).

5.3. Regression estimates of progress between ks1 and ks2

Next we turn to regression estimates of the model in equation (1). Results from our first set of exercises are shown in Table 4. The dependent variable is the pupil-mean of the Maths and English test percentile scores described in Table 2. Column (1) provides information on the raw differences between school types (similar to those in Table 2) by regressing *ks2* test scores on school-type dummies (and academic year dummies).

In Column (2) we control for *ks1* achievement using 183 dummies for combinations of levels in Maths, Reading and Writing attainment groups. Adding these controls more than halves the differences between mean *ks2* scores of pupils attending different types of school. Even then, pupils in Faith schools and autonomous schools still appear to do better despite starting from the same *ks1* base. By the time they take their *ks2* tests, students in Faith-autonomous schools are nearly 2.5 percentiles above pupils in non-autonomous Secular schools who were in the same *ks1* achievement group[12].

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[12] Note that we have tried other specifications of the value-added model. A common alternative approach assumes that $ks_{it} = (\beta_j + x'_i \gamma + \varepsilon_i)t$, so that $ks_{i2} - ks_{i1} = \beta_j + x'_i \gamma + \varepsilon_i$, in which case we can just regress the difference between pupil's *ks2* and *ks1* point scores on school type dummies and other background characteristics. The results from this exercise convey a similar message to that in Table 4. They are available upon request.

Column (3) introduces the school and pupil level controls detailed in Appendix Table A1 alongside home-postcode fixed-effects to control for unobserved family/pupil background effects that are common to close neighbours. The gap between Secular-non-autonomous schools and other school types closes.[13] We now find no evidence of an advantage for pupils in non-autonomous Faith schools over Secular schools. However, pupils emerge with a slightly higher *ks2* scores from autonomous schools – both Faith and Secular – than they do from non-autonomous schools. We suspect that this advantage has still in part to do with selection on pupil characteristics that are correlated with progress between *ks1* and *ks2* since the evidence in Table 3 suggests that pupils attending different school types from the same postcode are still not balanced on observables. However, we cannot rule out real academic advantages from the more autonomous governance structures of Faith-autonomous (Voluntary-Aided) and Secular-autonomous (Foundation) schools.

We next estimate equation (1) allowing for secondary-school-type × home-postcode fixed-effects (Column (4)) or secondary-school × home-postcode fixed-effects (Column (5)), following the strategy described in Section 4.3. The results in both specifications reveal a very small residual gap between pupils emerging from autonomous primaries and those from baseline schools – at around 0.8-1 percentile of the pupil distribution or 0.02-0.04 of the standard deviation in test scores. A striking finding emerging from the results so far is that Faith affiliation is not, in itself, an indicator of higher educational standards. Faith-autonomous schools have mean attainments that are only 0.15 percentiles higher than Secular-autonomous schools (in Column (5)), and not significantly so (the F-test for equality of the two parameters has a p-value of 0.7236). Moreover, pupils from Faith schools

[13] Identification with home-postcode fixed-effects requires multiple school types per postcode, so we restrict the sample to postcodes where this condition holds, leaving us with a much smaller sample. Note that we have re-estimated the specification of Column (2) on the sample of Column (3) to check that the documented attenuation in the coefficients is not attributable to a change in the pupil composition arising from the reduced sample. We found this was not the case. For example, the effect of attending a Faith-autonomous school was estimated to be 2.107 (s.e. 0.182) when using the specification in Column (2) on the sample of Column (3).

seem to do slightly worse than pupils from Secular schools when admissions are not under their control[14]. This indicates that religious character is not, on its own, linked to better school performance.

Note that although the estimated *ks2* Faith-primary effect is very modest compared to some of the previous findings in the literature, it could still be upward biased by selection into Faith-primary schools amongst pupils who live in the same postcode and go on to attend the same secondary school: the last five columns of Table 3 suggest that, even conditional on secondary-school × home-postcode fixed-effects, pupils attending autonomous primary schools are associated with educationally advantageous observable characteristics. However, in Section 4.3, we have also emphasised that estimates using secondary-type or secondary-school fixed-effects could be downward biased by differential selection into secondary schools on the basis of Faith-primary attendance or *ks1*-to-*ks2* value added. We therefore next proceed to bound our estimates using the strategy described in Section 4.4.

5.4. Regression estimates on sub-samples of stayers and switchers

In this section, we implement the strategy described in Section 4.4 that compares estimates for two subgroups of pupils, namely: *(i)* the *stayers*, who attend the same school type in both the primary and secondary phases; and *(ii)* the *switchers*, who attend different school types in each of these phases. As outlined in Section 4.4, under plausible conditions the stayers' estimates will provide an upper bound, and the switchers' estimates a lower bound to the causal effect of Faith-primary schooling on achievement. However, we also argued that estimates based on the switchers' group are much more likely to be close to the causal Faith-primary effect than those based on the sample of stayers or on the population of pupils, since we expect sorting to be much weaker among switchers. To support this conjecture, we begin by presenting estimates of the degree of selection into Faith-primary

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[14] The null hypothesis that the coefficients for Faith-autonomous schools and Secular-autonomous schools are equal to the parameter for Faith-non-autonomous schools is rejected with p-values of 0.000 and 0.037 p-value, respectively.

schools based on observable characteristics in the stayers' and switchers' groups separately. We then go on to estimate the Faith-primary effect on *ks2* in these two groups separately.

Descriptive statistics on school type transitions between primary and secondary phases are shown in Appendix Table A2. About 77.5% of pupils in Secular-non-autonomous primary schools transits to Secular-non-autonomous secondary schools. Similarly, 54.8% of those attending a Secular-autonomous primary transit to a Secular-autonomous school for their secondary education. Together, this implies that 54% of our sample stays in the same type of Secular school in both phases, with just over half (52%) in Community schools controlled by the Local Education Authority (and approximately 66% staying in the Secular sector overall). Looking at Faith-school pupils, more than 50% of pupils in Faith-autonomous primary schools (about 10% of our sample) stay within the Faith-autonomous sector during secondary education (while only 2% stay in the Faith-non-autonomous sector in both phases). On the other hand, some 170,000 pupils (18%) switch out of the Faith sector at the secondary phase, and around 39,000 (4.2%) switch into it. If our conjectures about the relationship between school choice and family background are correct, we would expect to find very different estimates of the Faith-autonomous primary school effect amongst these different groups, and very different patterns of sorting along observable lines into Faith-autonomous schools.

Estimates of the degree of observable sorting into Faith-primary schools within the stayers' and switchers' sub-samples are presented in Table 5. In the top panel, we present the results from regressions of individual observable characteristics on school-type dummies and home-postcode fixed-effects for the sub-sample of stayers (i.e. pupils in all the diagonal cells of Appendix Table A2). This exercise is similar to the one presented in Table 3. Individuals who stay in Faith-autonomous and Secular-autonomous schools over both phases of education have higher *ks1* achievement than pupils in Secular-non-autonomous schools. They are also less likely to be eligible for free school meals and to have special education needs. They are more likely to be of White origin and to speak English as their first language. Importantly, the differences are larger for the sub-sample of stayers considered here than for the population as a whole (compare with Columns 6 to 10 of Table 3).

Consider now the sample of switchers (i.e. we exclude from our sample pupils in the diagonal cells of the transition matrix of Appendix Table A2). Results about observable sorting within this group are presented in the lower panel of Table 5. Although there are still some significant differences between pupils attending Faith-autonomous schools and those attending Secular-non-autonomous schools in the primary phase, these are much lower in magnitude than we found in the population (Table 3), or in the stayers' sample (Table 5, top panel). More generally, the differences in background characteristics between Faith-primary and Secular-primary pupils in the switchers' group are much smaller than differences for pupils in the population or the sample of stayers.

These findings support our conjecture that there is much less sorting into Faith-primary schools within the switchers' group than among the stayers. Under the assumption that the amount of selection on observable characteristics is a guide to the amount of selection on unobservables (Altonji et al., 2005b), this evidence also supports our argument that stayers and switchers can be used to bound the estimates of the Faith-primary effect on achievement at *ks2*. Moreover, the low level of sorting in the switchers' group relative to the stayers implies that estimates based on the former will be much closer to the unbiased causal impact of Faith-primary schooling.

Our results on the effect of primary school types on *ks2* achievement for stayers and switchers are presented in Table 6. The regressions use the same specification as in Table 4, Column (3), i.e. they are conditional on highly flexible controls for *ks1* scores, home-postcode fixed-effects and other control variables (see note to the table for details). We focus our discussion on the estimated effect of autonomous schools, since we have already shown that it is only in these schools that there appear to be significant educational advantages.

For the sample of stayers, attending Faith-autonomous schools in both phases, we find a *ks2* test scores advantage of 2.7 percentiles, relative to pupils attending Secular-non-autonomous schools in both phases; for Secular-autonomous school pupils this figure is around 2.2 percentiles. These estimates are 60-100% higher than those found for the full sample in Table 4. On the other hand, we find that the *ks2* performance gap of Faith-autonomous schools for the sub-sample of switchers is below zero, although not significant. Stated differently, pupils who attended a Faith-autonomous school up to age 11, but move to a Faith non-autonomous or a Secular secondary school, perform no better – if not marginally worse – at *ks2* than pupils who attend a Secular-non-autonomous primary school up to age-11, but go on to attend either an autonomous or a Faith secondary school thereafter. A similar pattern emerges when considering the effect of attending a Secular-autonomous primary school for the sample of switchers. These findings count against better quality in either Faith-autonomous or Secular-autonomous schools being the driving factor behind the better test results presented earlier: the estimated advantage of Faith- and Secular-autonomous school attendance closely tracks the degree of selection on observable characteristics, which makes pupil sorting a more plausible explanation for any difference in average pupil performance[15].

We can further see this by considering a few simple descriptive statistics. Consider first the full sample of pupils. Looking at Table 3 (Columns (6) to (10)), it can be seen that pupils in Faith-autonomous schools are about 11-12% of one standard deviation above pupils in Secular-non-autonomous schools in terms of observable characteristics that are associated to higher *ks2* test scores (e.g. *ks1* test scores and free-meal eligibility). In turn, they have a value-added advantage of around 6% of one standard deviation (see Table 4, Column (3)), about half the advantage in terms of predetermined observable characteristics. Next, for the sub-sample of stayers, the advantage in terms of observable characteristics is up to 20% of one standard deviation (see top panel of Table 5), while the advantage in value-added is about 10% (as from Column (1) of Table 6). On the other hand, for the sample of switchers, the *disadvantage* in *ks1* attainments and free-meal entitlement (the most important drivers of *ks2* test scores in our models) is about 2.5% of one-standard deviation (see Panel B of Table 5), while the *disadvantage* in value-added, although insignificant, is about 1% of one-standard deviation (Column (2) of Table 6). There is clearly a close relationship between selection on observables into Faith-autonomous schools and the magnitude of our estimates of their *ks2*

[15] Incidentally, our evidence does not point to any beneficial impact of attending a Faith primary school for more disadvantaged pupils, like those eligible for free school meals. This is at odds with most of the US-based evidence.

advantage, which once again suggests that most of the documented effects of Faith-autonomous schooling are likely to be driven by pupil sorting and school-side selection.

5.5. Robustness to varying degrees of selection on unobservables

To conclude our analysis, we formalize the arguments presented above using a variant of the Altonji et al. (2005b) procedure outlined in Section 4 and Appendix B. In particular, we investigate: *(a)* how much positive selection on unobservables we need in order to drive our estimates to zero; and *(b)* how robust our estimates are to the assumption that there is an equal amount of selection on observables and unobservables. We study these issues for all our pupils in the sample, and separately for stayers and switchers. Note that, for computational feasibility, we use regression specifications in which we replace home-postcode fixed-effects with 150 LA dummies, so our baseline estimates are higher than the comparable specifications in Table 4 and Table 6[16]. We also consider only pupils choosing Faith-autonomous or Secular-non-autonomous schools, since we need a dichotomous 'treatment' variable. Our estimates are presented in Table 7, where ρ in the column heading shows the constraint that we impose on the 'strength' of selection on unobservables (see Appendix B for details).

The top panel of the table reports our findings for the full sample of pupils. In the first column we present results when we constrain our model to have no selection on unobservables. This estimate is used as a benchmark and is comparable to that in Column (3) of Table 4. As soon as we allow for some selection on unobservables, the positive effect of attending a Faith-autonomous primary school is eroded. For example, with $\rho = 0.060$, the effect is about 0.6 of a percentile, while it turns completely insignificant and very small (0.06 of a percentile) when $\rho = 0.080$. Importantly, the value of $\rho = 0.080$ also corresponds to the point at which the amount of selection on unobservables equals the amount of selection on observables in the full sample. Given the importance of selection

[16] We use a modified version of the two-step `treatreg` command in Stata 11. We need to exclude the postcode fixed-effects, because the model requires a probit first-stage and there are too many postcodes for feasible estimation.

on observables (note the R-squared's in Table 4), the value of $\rho = 0.080$ is arguably an upper limit to the amount of selection on unobservables in our setting. If this is the case, then the estimate reported in the last column of Table 7 can be interpreted as a lower bound to the effect of attending a Faith-autonomous primary school[17]. On the other hand, we have not controlled as well for other aspects of family background here as we did in our main regressions, because it was infeasible to include home-postcode fixed-effects. As a result, our findings might still overstate the benefits of attending a religious autonomous primary school.

Next we repeat this analysis for the sample of stayers. Results are presented in Panel B of Table 7. Once again we find that the estimates of the effect of attending a Faith-autonomous primary school are very sensitive to sorting on unobservables. The estimated coefficient becomes statistically insignificant as we raise selection on unobservables to $\rho = 0.100$. Further, when we impose an equal amount of selection on observables and unobservables $(\rho = 0.123)$, the estimated Faith-primary school effect becomes negative at -0.379, although statistically insignificant.

Finally, in Panel C of Table 7, we present our findings for the sub-group of switchers. In this case, even with extremely small degrees of selection on unobservables $(\rho = 0.020)$ or with equal selection on observables and unobservables $(\rho = 0.019)$, our estimate becomes statistically insignificant and very close to zero (0.230-0.254 of a percentile).

All in all, this analysis reveals some patterns that support our previous intuitions and evidence about sorting for the sub-samples of switchers and stayers. Firstly, we confirm that the sample of stayers is significantly more positively selected into Faith-primary schools than other groups $(\rho = 0.123)$. However, an equal amount of selection on observables and unobservables drives our estimates of the Faith-primary effect down below zero. This finding squares well with our simple

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[17] Obviously, if we forced ρ to assume negative values, i.e. if we assumed negative selection on unobservables, then we would find estimates that lie above those documented in Table 4 and Table 6. However, negative sorting is unlikely in England given the structure of the school system and the evidence provided above.

account presented above, where we highlighted that the positive gap in achievements at *ks2* for stayers in Faith-autonomous schools is in fact smaller than the gap in their educationally advantageous characteristics. Secondly, and more importantly, for the sub-sample of switchers, we find a very small degree of selection into religious schooling $(\rho = 0.019)$ based on observable characteristics, which we argue is likely to provide an upper bound to the amount of selection on unobservables for this group too. This provides strong support for our claim that we are more likely to identify the causal effect of attending a Faith primary school in the absence of selection and sorting by focussing on the sample of switchers. To reiterate, in our richest specifications in Column 2 of Table 6 we find that the effect of Faith-primary schooling for switchers is not statistically different from zero.

One concern over the application of the Altonji et al. (2005b) methodology in our context is the use of prior achievements (*ks1*) in the selection equation. This is because *ks1* test scores are very highly correlated with *ks2* achievements, and so tend to dominate the set of explanatory factors in the achievement equation (1). As a robustness check, we replicated our analysis using an alternative value-added specification in which we replace the dependent variable with the outcome (*ks2* – *ks1*), and exclude *ks1* from both the main and selection equations (but otherwise keep the same set of controls as in Table 7; see footnote 12 for more details about this alternative specification). Results obtained following this approach confirm our previous conclusions: we still find that the sample of stayers is more positively selected ($\rho = 0.085$) than the sample of switchers ($\rho = 0.006$), and that for both groups of pupils the effect of attending a Faith-autonomous school is no longer positive and significant once we impose an equal amount of selection on observables and unobservables. In particular, for the group of switchers this effect stands at 0.51 with a standard error of 0.25.

6. Conclusions

We have provided a number of estimates of the effect of attending a Faith school in England on pupils’ educational progress between ages 7 and 11. Our approach has deliberately avoided instrumental variable strategies adopted by previous work in the field, because we do not believe (at least for the English setting) that there are any credible instruments for Faith school attendance that are uncorrelated with family background, either directly or through residential sorting. Instead we have exploited the fact that we have around one million pupils in our database, which, in conjunction with precise details about place of residence, academic record and future school choice, allows us to carefully control for factors that influence the propensity to attend Faith schools.

We make no claim to have put a precise number on the causal impact of Faith school attendance and have demonstrated that the magnitude of any difference between Faith school pupils and Secular school pupils depends substantially on the way we cut the sample. What then are we to make of these results? One thing that seems clear is that, Faith schools – and other schools that have autonomous admissions and governance arrangements – tend to admit 'better' pupils, and there is no unambiguous performance advantage that cannot be attributed purely to pupil-side sorting into these schools, or to school-side selection of pupils likely to show the fastest progress. Pupils who attended Faith or autonomous schools at primary phase, but not at the secondary phase, do no better in primary school than pupils who attend Faith or autonomous schools at the secondary phase, but not at the primary phase. The Faith or autonomous-school gap in attainments at primary phase seems largely attributable to differences between those pupils who choose to attend such schools at *any* stage in their educational careers, and those who choose never to do so or are excluded from doing so by school selection procedures.

In any case, we find no evidence that Faith affiliation lies behind the test-score advantage commonly attributed to Faith schools in England. A generous reading of the results suggests that pupils in schools that have more autonomous governance and admissions structures – a set that includes Faith schools – do progress marginally faster. A pupil starting in an autonomous school at age 7 could expect to be one percentile higher in the distribution of pupil attainments by age 11 than a comparable pupil attending a standard Secular-non-autonomous school, even when these two pupils live in the same postcode and go on to choose the same secondary school. Our upper bound estimates put this figure at 2.7 percentiles.

To put this in perspective, we draw on the results in Machin and McNally (2004) that report labour market returns to age-10 reading tests, based on the 1970 British Cohort Study[18]. Their figures (reported in their Table 7) indicate that the labour market return to a one percentile move up the attainment distribution at age 10 was around 0.0042%, conditional on family background. In other words, the labour market impact of these small school quality differences seems very slight. Certainly, the cumulative effect over 12 years of compulsory schooling could be more substantial than this would suggest, and there may be other impacts from schooling of religious ethos – on staying on rates and child wellbeing for example – that are outside the scope of this study. However, pupils in Faith schools that are under close Local Authority control do not progress any faster than similar pupils in comparable Secular schools. Any performance impact from 'Faith' schools in England seems to be closely linked to their autonomous governance and admissions arrangements, and not to religious character.

[18] The 1970 British Cohort Study follows a cohort of children born in one week in 1970 through to adulthood. The reading tests were administered in 1980 when the children were aged 10.

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Appendix A: Formalising the school selection processes

6.1. A simple model of selection

Suppose attainments at Key Stage 2 are determined by the type of school attended (*j*), unobserved family characteristics η_i and a random error term ε_{i2} as in equation (1) in the main text:

$$ks_{i2} = \beta_{j2} + x_i'\gamma + g\left(ks_{i1}\right) + \eta_i + \varepsilon_{i2} \tag{A1}$$

Consider two school types, Secular (*j = 0*) and Faith (*j = 1*) schools, and denote the primary phase by *t = 2*, and the secondary phase by *t = 3*. Selection into school type in the primary phase is determined family background and preferences η_i, with a parameter δ that takes on positive values:

$$\Pr\left(j_2 = 1\right) = \Pr\left(\delta\eta_i - \upsilon_{i2} > 0\right) \tag{A2}$$

Where υ_{i2} is a finite variance, random shock that is uncorrelated with η_i or ε_{i2}, and affects choice over school type. At the secondary stage, we assume that η_i remains the dominant factor determining Faith school choice. However, families may also take into account the quality of the match between the child and the sector in the primary stage as represented by ε_{i2}, with a low realisation of ε_{i2} inducing families to switch school types. Further, Faith secondary schools may engage in covert selection that favours children from the Faith primary sector, which we model as a differential 'switching' threshold for children from the Faith as opposed to children from the Secular sector. These considerations imply the following selection probabilities at the secondary stage:

$$\Pr(j_3 = 1 \mid j_2 = 0) = \Pr(\delta\eta_i - \sigma\varepsilon_{i2} - \upsilon_{i3} - \tau > 0) \tag{A3}$$

$$\Pr(j_3 = 1 \mid j_2 = 1) = \Pr(\delta\eta_i + \sigma\varepsilon_{i2} - \upsilon_{i3} + \tau > 0)$$

In these expressions υ_{i3} is another shock[19] uncorrelated with η_i or ε_{i2}, and σ is a parameter representing the strength of (self-) selection on the revealed sector-pupil match. Note that $\sigma\varepsilon_{i2}$ enters

[19] This might include random events such as unforeseen home moves, emerging travel constraints, changes in preferences, unexpected changes to school capacity (limiting place availability), or changes to aspects of teaching practices of a given sector that are uncorrelated with KS2 achievements (e.g. too much or too little religious education).

into the selection equations for Secular primary pupils with the opposite sign to Faith primary pupils, because a revealed high quality match (high ε_{i2}) *discourages* switching to the Faith sector. Finally, $\pm\tau$ is a selection threshold above which Faith secondary schools admit pupils, with preference for Faith primary pupils (threshold $-\tau$) against Secular primary students (threshold τ). In reality, in the English institutional context, both σ and τ are likely to negligible (see discussion in Section 4.3), but we introduce them here in order to consider their potential implications. In particular, there is little rationale for switching on ε_{i2} because families cannot observe the counterfactual match in the sector they did not attend, and the transition from primary to secondary school involves leaving the school that disappointed them in any case.

OLS estimation of the school-type effect β_{j2} in Equation (A1) is biased, with the bias in the estimate of $\beta_{12}-\beta_{02}$ in the population determined by:

$$E[\eta_i+\varepsilon_{i2} \mid \delta\eta_i > \upsilon_{i2}] - E[\eta_i+\varepsilon_{i2} \mid \delta\eta_i \le \upsilon_{i2}] = \mu > 0 \qquad \text{(A4)}$$

However, we argue that the Faith-Secular primary school comparison within various secondary school groups is informative about the size of this bias, and that some within-group comparisons provide less biased estimates. Specifically, the comparisons we consider and the associated biases are:

- *Within Faith secondary schools :-*

$$E[\eta_i+\varepsilon_{i2} \mid \delta\eta_i > \upsilon_{i2}, \delta\eta_i+\sigma\varepsilon_{i2}+\tau > \upsilon_{i3}] - E[\eta_i+\varepsilon_{i2} \mid \delta\eta_i \le \upsilon_{i2}, \delta\eta_i-\sigma\varepsilon_{i2}-\tau > \upsilon_{i3}] \qquad \text{(A5)}$$

- *Within Secular secondary schools :-*

$$E[\eta_i+\varepsilon_{i2} \mid \delta\eta_i > \upsilon_{i2}, \delta\eta_i+\sigma\varepsilon_{i2}+\tau \le \upsilon_{i3}] - E[\eta_i+\varepsilon_{i2} \mid \delta\eta_i \le \upsilon_{i2}, \delta\eta_i-\sigma\varepsilon_{i2}-\tau \le \upsilon_{i3}] \qquad \text{(A6)}$$

- *Within-switchers:-*

$$E[\eta_i+\varepsilon_{i2} \mid \delta\eta_i > \upsilon_{i2}, \delta\eta_i+\sigma\varepsilon_{i2}+\tau \le \upsilon_{i3}] - E[\eta_i+\varepsilon_{i2} \mid \delta\eta_i \le \upsilon_{i2}, \delta\eta_i-\sigma\varepsilon_{i2}-\tau > \upsilon_{i3}] \qquad \text{(A7)}$$

- *Within-stayers:-*

$$E[\eta_i+\varepsilon_{i2} \mid \delta\eta_i > \upsilon_{i2}, \delta\eta_i+\sigma\varepsilon_{i2}+\tau > \upsilon_{i3}] - E[\eta_i+\varepsilon_{i2} \mid \delta\eta_i \le \upsilon_{i2}, \delta\eta_i-\sigma\varepsilon_{i2}-\tau \le \upsilon_{i3}] \qquad \text{(A8)}$$

Our main claims regarding the bias in the estimate of the impact of attending a Faith primary school for these groups are made on the basis of empirical observations on the degree of sorting along the lines of family background within these groups in conjunction with our knowledge of current English educational context. However, it is worth highlighting some theoretical considerations which support our claims.

6.2. Analytical results when σ and τ are equal to zero

Assume first that σ and τ are zero – or small relative to the selection on η_i. In this case, the bias terms in (A5) and (A6) are likely to be less than or equal to the bias in the population (A4), whereas (A7) is unbiased and (A8) is upward biased.

To see this, consider first (A7). Assuming υ_{it} are random variables with identical distributions, the selection process on η_i at both phases is identical in the Faith-Secular group and the Secular-Faith group (just in a different order), and the bias in (A7) is zero, i.e.:

$$E\left[\delta\eta_i \mid j_{i2}=1, j_{i3}=0\right]=E\left[\delta\eta_i \mid j_{i2}=0, j_{i3}=1\right] \tag{A9}$$

In contrast, the stayers' group comparison (A8) yields an upward biased estimate, given that the Faith-Faith stayers are those for whom η_i is sufficiently high such that $\delta\eta_i > \upsilon_{i2}$ and $\delta\eta_i > \upsilon_{i3}$, whereas the Secular-Secular stayers are those for whom η_i is sufficiently low so that both $\delta\eta_i \le \upsilon_{i2}$ and $\delta\eta_i \le \upsilon_{i3}$.The within-Faith-secondary and within-Faith-primary biases are harder to determine in the general case, but our conjecture is that they will generally be less than the population bias μ in (A4), and this can be shown by simulation.

For one analytical example, consider the symmetric case when: *(i)* the probability *p>0.5* of attending a Faith secondary school, conditional on attending a Faith primary school, equals the probability of attending a Secular secondary school, conditional on attending a Secular primary school; and *(ii)* the within-Faith-secondary and within-Faith-primary biases are equal, i.e.:

$$\begin{aligned}&\left\{E\left[\delta\eta_i \mid j_{i2}=1, j_{i3}=1\right]-E\left[\delta\eta_i \mid j_{i2}=0, j_{i3}=1\right]\right\}\\&=\left\{E\left[\delta\eta_i \mid j_{i2}=1, j_{i3}=0\right]-E\left[\delta\eta_i \mid j_{i2}=0, j_{i3}=0\right]\right\}\end{aligned} \tag{A10}$$

In this case:

$$
\begin{aligned}
&E[\delta\eta_i \mid j_{i2}=1]-E[\delta\eta_i \mid j_{i2}=0] \\
&= p\{E[\delta\eta_i \mid j_{i2}=1, j_{i3}=1]-E[\delta\eta_i \mid j_{i2}=0, j_{i3}=0]\} \\
&+(1-p)\{E[\delta\eta_i \mid j_{i2}=1, j_{i3}=0]-E[\delta\eta_i \mid j_{i2}=0, j_{i3}=1]\}
\end{aligned} \tag{A11}
$$

Note also that, given (A9):

$$
\begin{aligned}
&\{E[\delta\eta_i \mid j_{i2}=1, j_{i3}=1]-E[\delta\eta_i \mid j_{i2}=0, j_{i3}=0]\} \\
&=\{E[\delta\eta_i \mid j_{i2}=1, j_{i3}=1]-E[\delta\eta_i \mid j_{i2}=0, j_{i3}=1]\} \\
&+\{E[\delta\eta_i \mid j_{i2}=1, j_{i3}=0]-E[\delta\eta_i \mid j_{i2}=0, j_{i3}=0]\}
\end{aligned} \tag{A12}
$$

From (A11) and (A12), and using (A4), (A9) and (A10) it follows that the within-Secular-secondary bias and the within-Faith-secondary bias are equal to:

$$
\begin{aligned}
&\{E[\delta\eta_i \mid j_{i2}=1, j_{i3}=0]-E[\delta\eta_i \mid j_{i2}=0, j_{i3}=0]\} = \\
&\{E[\delta\eta_i \mid j_{i2}=1, j_{i3}=1]-E[\delta\eta_i \mid j_{i2}=0, j_{i3}=1]\} = \frac{\mu}{2p}
\end{aligned} \tag{A13}
$$

Given the assumption *p>0.5,* the within-secondary bias is less than that in the population (A4).

Further insights can be gained by looking at the effects of truncating the distributions of $\delta\eta_i$ for pupils originating in Faith primaries, $f(\delta\eta_i \mid j_{i2}=1)$, and in Secular primaries, $f(\delta\eta_i \mid j_{i2}=0)$, assuming these distributions are of similar shape and variance, but mean-shifted with $E[\delta\eta_i \mid j_{i2}=1]-E[\delta\eta_i \mid j_{i2}=0]=\mu>0$. For simplicity of notation, assume that $E[\delta\eta_i \mid j_{i2}=0]=0$ and consider that for any realisation of υ_{i3} we have:

$$
E[\delta\eta_i \mid j_{i2}=1, \delta\eta_i > \upsilon_{i3}] = \mu + E[\delta\eta_i \mid j_{i2}=0, \delta\eta_i > \upsilon_{i3}-\mu] \tag{A14}
$$

$$
E[\delta\eta_i \mid j_{i2}=1, \delta\eta_i \le \upsilon_{i3}] = \mu + E[\delta\eta_i \mid j_{i2}=0, \delta\eta_i \le \upsilon_{i3}-\mu] \tag{A15}
$$

Therefore, for given υ_{i3}, the biases are as follows:

- *Within-Faith-secondary :-*

$$
0 \le \mu + E[\delta\eta_i \mid j_{i2}=0, \delta\eta_i > \upsilon_{i3}-\mu] - E[\delta\eta_i \mid j_{i2}=0, \delta\eta_i > \upsilon_{i3}] \le \mu \tag{A16}
$$

- *Within-Secular-secondary :-*

$$
0 \le \mu + E[\delta\eta_i \mid j_{i2}=0, \delta\eta_i \le \upsilon_{i3}-\mu] - E[\delta\eta_i \mid j_{i2}=0, \delta\eta_i \le \upsilon_{i3}] \le \mu \tag{A17}
$$

- *Within-switchers :-*

$$\mu + E\left[\delta\eta_i \mid j_{i2} = 0, \delta\eta_i \le \upsilon_{i3} - \mu\right] - E\left[\delta\eta_i \mid j_{i2} = 0, \delta\eta_i > \upsilon_{i3}\right] < 0 \quad \text{(A18)}$$

- *Within-stayers :-*

$$\mu + E\left[\delta\eta_i \mid j_{i2} = 0, \delta\eta_i > \upsilon_{i3} - \mu\right] - E\left[\delta\eta_i \mid j_{i2} = 0, \delta\eta_i \le \upsilon_{i3}\right] > \mu \quad \text{(A19)}$$

Note that (A16) and (A17) follow because the difference between the second two terms is less than zero, but greater than $-\mu$. Also, (A18) holds because the difference between the second two terms is less than $-\mu$, and (A19) holds because the difference between the second two terms is greater than zero.

The assumption that the distributions $f\left(\delta\eta_i \mid j_{i2} = 1\right), f\left(\delta\eta_i \mid j_{i2} = 0\right)$ differ only in their means follows from (A2) (assuming $E\left[\upsilon_{i2} = 0\right]$). However, in our empirical work, only 30% of pupils are enrolled in the Faith primary sector so it might be argued that the two distributions differ in their variance, with $Var\left[\delta\eta_i \mid j_{i2} = 0\right] > Var\left[\delta\eta_i \mid j_{i2} = 1\right]$. This difference in variance reduces the bias implied by (A16) and increases the bias implied by (A17). Assuming for simplicity that $\mu = 0$, then:

$$E\left[\delta\eta_i \mid j_{i2} = 1, \delta\eta_i > \upsilon_{i3}\right] - E\left[\delta\eta_i \mid j_{i2} = 0, \delta\eta_i > \upsilon_{i3}\right] < 0 \text{ if } Var\left[\delta\eta_i \mid j_{i2} = 0\right] > Var\left[\delta\eta_i \mid j_{i2} = 1\right] \quad \text{(A20)}$$

$$E\left[\delta\eta_i \mid j_{i2} = 1, \delta\eta_i \le \upsilon_{i3}\right] - E\left[\delta\eta_i \mid j_{i2} = 0, \delta\eta_i \le \upsilon_{i3}\right] > 0 \text{ if } Var\left[\delta\eta_i \mid j_{i2} = 0\right] > Var\left[\delta\eta_i \mid j_{i2} = 1\right] \quad \text{(A21)}$$

Clearly there is no way to determine the precise outcome of the within-secondary-school comparison without making specific distributional assumptions, although under the hypothesis specified here above, the potential biases in the within-Faith (A20) and within-Secular (A21) comparisons tend to cancel out.

On the other hand, even with $Var\left[\delta\eta_i \mid j_{i2} = 0\right] > Var\left[\delta\eta_i \mid j_{i2} = 1\right]$, the switchers' comparison in (A18) remains downward biased (for given υ_{i3}) because $E\left[\delta\eta_i \mid j_{i2} = 0, \delta\eta_i > \upsilon_{i3}\right]$ increases with $Var\left[\delta\eta_i \mid j_{i2} = 0\right]$ and $E\left[\delta\eta_i \mid j_{i2} = 1, \delta\eta_i \le \upsilon_{i3} - \mu\right]$ decreases with $Var\left[\delta\eta_i \mid j_{i2} = 1\right]$. Similarly, the upward bias in the stayers' comparison (A19) also holds because $E\left[\delta\eta_i \mid j_{i2} = 0, \delta\eta_i \le \upsilon_{i3}\right]$ decreases with $Var\left[\delta\eta_i \mid j_{i2} = 0\right]$ and $E\left[\delta\eta_i \mid j_{i2} = 1, \delta\eta_i > \upsilon_{i3} - \mu\right]$ increases with $Var\left[\delta\eta_i \mid j_{i2} = 1\right]$.

As it turns out, comparing the Faith-primary and Secular-primary sectors, we find similar standard deviations of the *ks2* test scores in the two school types (s.d.=26.17 and 26.66, respectively). Similarly, we find negligible differences across the two groups when we look at *ks2* predictions based on a regression of test scores on pupil and family characteristics (s.d.=21.01 and 21.26), as well as when focussing on the residuals from this regression (s.d.= 15.80 and 16.10). This suggests that the equal variance assumption that underlies the expressions for the biases in (A16) to (A19) may be quite reasonable in practice.

6.3. Analytical results with σ and τ different from zero

Note however that if σ and τ are non-zero, the bias from the within-secondary-school-type comparisons (A5) and (A6) could be larger or smaller than the bias from the Faith-Secular primary school comparison in the population in (A4).

To begin with, consider the case when $\sigma > 0$ (and $\tau = 0$). The within-Faith-secondary-schools estimator now compares very-high $\delta\eta_i$/high $\sigma\varepsilon_{i2}$ pupils from Faith primaries with mid-range $\delta\eta_i$/low $\sigma\varepsilon_{i2}$ pupils from Secular primaries so the bias is positive. The within-Secular-secondary school estimator compares mid-range $\delta\eta_i$/low $\sigma\varepsilon_{i2}$ pupils from Faith primaries with very-low $\delta\eta_i$/high $\sigma\varepsilon_{i2}$ from Secular primaries, so the bias is ambiguous. On the other hand, the within-switchers' estimator compares mid-range $\delta\eta_i$/low-ε_{i2} Faith-Secular switchers with mid-range $\delta\eta_i$/low-ε_{i2} Secular-Faith switchers, while the within-stayers estimator compares very high $\delta\eta_i$/high $\sigma\varepsilon_{i2}$ Faith-Faith stayers with very low $\delta\eta_i$/high ε_{i2} Secular-Secular stayers. Therefore the within-switchers comparison is still less biased, and the within-stayers comparison more biased than the population comparison in (A4).

Consider finally the case when $\tau > 0$ (and $\sigma = 0$). The within-secondary biases implied by (A5) and (A6) are attenuated, because $\tau > 0$ implies that Faith primary pupils have relatively low $\delta\eta_i$ when compared with their Secular primary counterparts in the same secondary school type. However, for the within-switchers estimate, the additional truncation from τ implies a further downward bias, while the within-stayers estimate is further upward biased. This can be seen easily (ignoring υ_{i3}) in the case when $f\left(\delta\eta_i \mid j_{i2}=1\right), f\left(\delta\eta_i \mid j_{i2}=0\right)$ have similar shapes, but are mean-shifted:

- *Within-switchers :-*

$$\begin{aligned} &E\left[\delta\eta_i \mid j_{i2}=1, \delta\eta_i \le -\tau\right] - E\left[\delta\eta_i \mid j_{i2}=0, \delta\eta_i > \tau\right] \\ &= \mu + E\left[\delta\eta_i \mid j_{i2}=0, \delta\eta_i \le -\tau-\mu\right] - E\left[\delta\eta_i \mid j_{i2}=0, \delta\eta_i > \tau\right] < 0 \end{aligned} \tag{A22}$$

- *Within-stayers :-*

$$\begin{aligned} &E\left[\delta\eta_i \mid j_{i2}=1, \delta\eta_i > -\tau\right] - E\left[\delta\eta_i \mid j_{i2}=0, \delta\eta_i \le \tau\right] = \\ &\mu + E\left[\delta\eta_i \mid j_{i2}=0, \delta\eta_i > -\tau-\mu\right] - E\left[\delta\eta_i \mid j_{i2}=0, \delta\eta_i \le \tau\right] > \mu \end{aligned} \tag{A23}$$

Where (A22) holds because $\left[\delta\eta_i \mid j_{i2}=0, \delta\eta_i \le -\tau-\mu\right] - E\left[\delta\eta_i \mid j_{i2}=0, \delta\eta_i > \tau\right] < -\mu$, and (A23) holds because the mean of a left truncated distribution can never be less than the mean in the same distribution when right truncated.

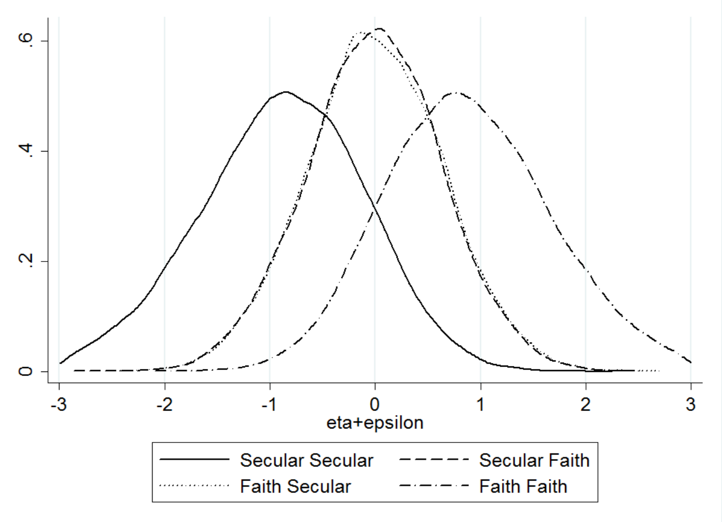
In summary, whilst the within-secondary comparison provides ambiguous biases, the direction of the biases in the within-switchers and within-stayers comparisons are unambiguous. The comparison of Faith-Secular switchers with Secular-Faith switchers is never – under any of these assumptions – more upward biased than the estimate based on comparing Faith primary with Secular primary pupils in the population, and is under the most likely scenarios either downward biased or unbiased. The comparison of Faith-Faith stayers with Secular-Secular stayers instead is always upward biased. These arguments underlie our claims that: *(i)* the stayers' comparison provides an upper bound to the effect of Faith primary schooling; and *(ii)* that by focussing on the group of switchers, we either estimate a lower bound to the causal effect of attending a Faith primary school or a close-to-unbiased estimate of this effect. The results in Section 5 suggest the latter is the most likely scenario.

Of course, if $f\left(\delta\eta_i \mid j_{i2}=1\right), f\left(\delta\eta_i \mid j_{i2}=0\right)$ have completely different shapes, it will be hard to prove anything definite about the biases induced by the within-secondary or the switchers/stayers comparisons. However, it is hard to imagine likely scenarios where this outcome could arise given the context in which we apply our analysis, and given that the evidence provided in the paper supports the assumptions and intuitions discussed here above.

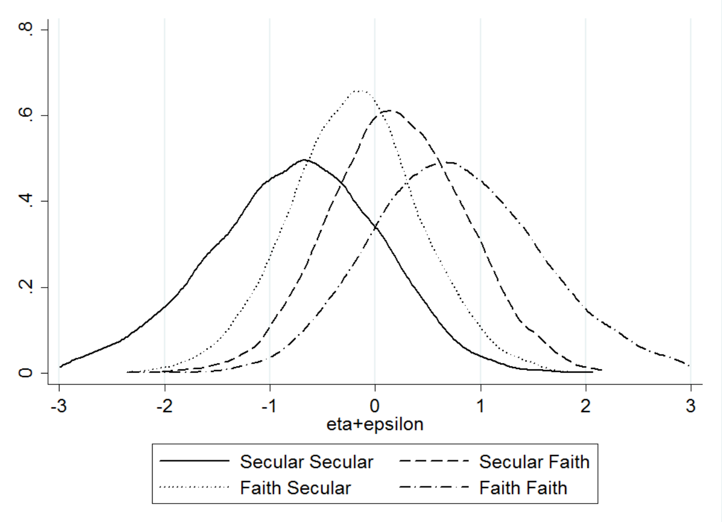
Nevertheless, we have carried out various simulations which further convince us that the conjectures described above hold under very general scenarios. In Figure A1, we report the findings from one of these exercises diagrammatically using simulations of the distributions of $\eta_i + \varepsilon_{i2}$ under some of the conditions described above. In the graphs, we assume η_i, ε_{i2}, υ_{i2} and υ_{i3} to be normally distributed, with $\text{var}(\eta_i) = 1, \text{var}(\varepsilon_{i2}) = \text{var}(\upsilon_{i2}) = \text{var}(\upsilon_{i3}) = 0.25$. The first panel illustrates the case where all selection into Faith schools is based on family/pupil background ($\delta = 1, \sigma = \tau = 0$). By construction, the difference in mean $\eta_i + \varepsilon_{i2}$ between the Faith primary and Secular primary distributions is 1.4. However, the difference in mean unobservables $\eta_i + \varepsilon_{i2}$ between the Faith-Faith and Secular-Secular stayers is larger than this, while the difference in mean unobsevables between the Faith-Secular and Secular-Faith switchers is zero. Next, in the central panel we allow for covert selection at the secondary level in favour of Faith primary pupils by setting $\tau = 0.5$. As a consequence, the distributions in Faith secondary schooling are shifted leftwards and the distributions in Secular schooling are shifted rightwards, such that the switchers' comparison becomes downward biased (mean $\eta_i + \varepsilon_{i2}$ in the Secular-Faith group is above that in the Faith-Secular group) providing a lower bound estimate, while the stayers' comparison remains upward biased. The last panel adds in selection by ε_{i2} by setting $\sigma = 1$ (plus the other assumptions as above). This shifts the stayers' distributions rightwards, because students with high ε_{i2} stay in the same sector, whereas the switchers' distributions shift leftwards. Nevertheless, the stayers' comparison remains upward biased, while the bias in the switchers' sample is small under reasonable assumptions, and always less than that in the population.

Figure A1: Diagrammatic illustration of distributions of $\eta_i + \varepsilon_{i2}$ arising from the switching process

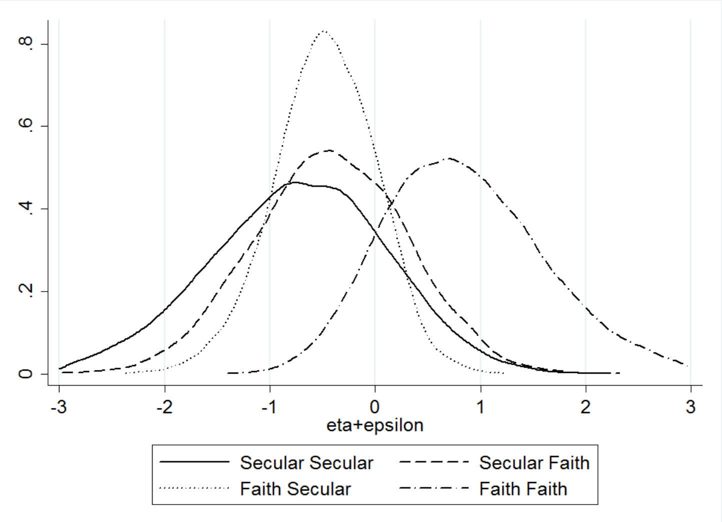
Faith and secular primary pupils in Faith and Secular secondary schools: selection based on family background/preferences only



...plus differential selection into Faith secondary schools in favour of Faith primary pupils



... plus self-selection into secondary type based on primary-type match quality



Appendix B: Calibrating selection on unobservables to selection on observables

In this appendix section we describe our implementation of the Altonji et al. (2005b) methodology. In our case, the outcome of interest is a continuous indicator (test scores), rather than a binary variable, and we want to investigate: *(a)* how much positive selection on unobservables we need in order to drive our estimates to zero; and *(b)* how robust our estimates are to the assumption of an equal amount of selection on observables and unobservables. Note that this exercise is carried out first on all pupils in the sample, and then separately for the groups of stayers and switchers.

The foundation of this analysis is a Heckman-type selection model of the form:

$$\begin{aligned} ks_{i2} &= \beta_{j2} + x'_i \gamma + g(ks_{i1}) + \xi_{i2} \\ d_{ij2} &= I(x'_i \lambda + h(ks_{i1}) + \vartheta_{i1} > 0) \end{aligned} \tag{B1}$$

In which d_{ij2} indicates Faith-primary school attendance at Key Stage 2[20]; $I(.)$ is an indicator function taking value one if its argument is above zero; and the error terms ξ_{i2} and ϑ_{i1} are jointly normally distributed with: $\mathrm{E}(\xi_{i2}) = 0$, $\mathrm{Var}(\xi_{i2}) = \sigma^2$, $\mathrm{E}(\vartheta_{i1}) = 0$, $\mathrm{Var}(\vartheta_{i1}) = 1$ and $\mathrm{Corr}(\xi_{i2}, \vartheta_{i1}) = \rho$. The parameter ρ measures the correlation between unobservables in the Faith school selection equation and in the value-added equation. Stated differently, this parameter captures the degree to which unobserved family/pupil characteristics η_i in equation (1) (in the main text) influence selection into Faith schooling. For both the objectives *(a)* and *(b)* here above, we are interested in assessing how estimates of β_{j2} change as we restrict the parameter ρ to different values.

As shown by Heckman (1979), the model in (B1) can be estimated by maximum likelihood or using a two-step method. The two-step method first estimates the probability of attending a Faith school ($d_{ij2} = 1$) parametrically using a probit model $\Pr(d_{ij2} = 1 \mid x_i, ks_{i1}) = \Phi(x'_i \lambda + h(ks_{i1}))$, and then estimates the following equation:

[20] In the empirical work we restrict attention to attendance at Faith-autonomous primary schools.

$$ks_{i2} = \beta_{j2} + x'_i \gamma + g(ks_{i1}) + \theta * MillsRatio_i + \varsigma_{i2}$$
$$MillsRatio_i = \frac{\varphi(x'_i \hat{\lambda} + \hat{h}(ks_{i1}))}{\Phi(x'_i \hat{\lambda} + \hat{h}(ks_{i1}))} \quad \text{(B2)}$$

Where ς_{i2} is a new error term, $\phi(.)$ indicates the normal density distribution, $\Phi(.)$ represents the normal cumulative distribution, $\hat{\lambda}$ and $\hat{h}(.)$ are estimated using the probit first-stage and $\theta = \sigma\rho$. The parameter ρ is only parametrically identified without exclusion restrictions on γ (i.e. non-parametric identification requires a valid instrument for selection). However, ρ can be constrained to predefined values in the estimation of system (B2) by imposing a constraint on $\theta = \sigma\rho$ once an estimate for σ is obtained (and maintained as ρ changes). We obtain this estimate of σ from unconstrained version of system (B2), where parametric identification is achieved exploiting the non-linearities in the Mills ratio. By setting ρ to different values, we can explore the sensitivity of $\hat{\beta}_{j2}$ to different assumptions about the degree of selection on unobservables into Faith schooling and find the value of ρ that is necessary to drive estimates of $\hat{\beta}_{j2}$ to zero. This allows us to answer question *(a)* spelled out above.

Additionally, Altonji et al. (2005b) discuss how to use this set-up to identify the value of ρ which implies an equal amount of selection on observables and unobservables. They argue that, if a large number of observable characteristics are available for the investigation, the extent of selection on observables provides an upper bound for the amount selection on unobservables. To see how this applies to our case, consider the latent variable $d_{ij2}* = x'_i \lambda + h(ks_{i1}) + \vartheta_{i1}$ and assume we could run the following 'thought' regression:

$$d_{ij2}* = \delta_0 + \delta_1[x_i'\hat{\gamma} + \hat{g}(ks_{i1})] + \delta_2\xi_{i2} \quad \text{(B3)}$$

In the Altonij et al. (2005b) sense, equal selection on observables and unobservables is obtained when $\delta_1 = \delta_2$. Note now that in our case, where the outcome is a continuous variable and the term ξ_{i2} does not have unit variance, we have that:

$$\delta_1 = \frac{Cov(x'_i\,\hat{\lambda} + \hat{h}(ks_{i1}), x'_{ij}\,\hat{\gamma} + \hat{g}(ks_{i1}))}{Var(x'_i\,\hat{\gamma} + \hat{g}(ks_{i1}))}$$
$$\delta_2 = \frac{Cov(\vartheta_{i1}, \xi_{i2})}{Var(\xi_{i2})} = \frac{\rho}{\sigma} \qquad \text{(B4)}$$

The constraint $\delta_1 = \delta_2$ is therefore equivalent to the constraint $\rho / \sigma = \delta_1$ or $\sigma\rho = \sigma^2 \delta_1$. Hence, in the estimation of system (B2), 'selection on observables equals selection on unobservables' implies the following constraint:

$$\theta^{ES} = \sigma^2 \frac{Cov(x'_i\,\hat{\lambda} + \hat{h}(ks_{i1}), x'_{ij}\,\hat{\gamma} + \hat{g}(ks_{i1}))}{Var(x'_i\,\hat{\gamma} + \hat{g}(ks_{i1}))} \qquad \text{(B5)}$$

Where the superscript *ES* denotes 'equal selection'. Note that we do not estimate θ^{ES} and all the parameters in equation (B1) simultaneously subject to the constraint (B5). Rather we use an iterative grid-search numerical method to estimate θ^{ES} that operates as follows:

(i) Estimate the first-stage probit model in the two-step Heckman system in (B2).

(ii) Estimate the second-stage of the Heckman selection model imposing a chosen value of $\theta = \theta_r^*$.

(iii) Calculate $\hat{\rho}_r = \hat{\sigma} \frac{Cov(x'_i\,\hat{\lambda} + \hat{h}(ks_{i1}), x'_{ij}\,\hat{\gamma} + \hat{g}(ks_{i1}))}{Var(x'_i\,\hat{\gamma} + \hat{g}(ks_{i1}))}$ from the estimated parameters in steps (i) and (ii).

(iv) Repeat (ii) and (iii) for pre-defined, incrementally different values of θ_r^*.

(v) Plot the values $\hat{\rho}_r$ thus obtained against the values of $\theta_r^* / \hat{\sigma}$. As already mentioned, an estimate of the parameter σ is obtained from the unconstrained model and assumed constant, so changing values of θ_r^* can be mapped into corresponding values for $\hat{\rho}_r$.

(vi) Find the point where $\hat{\rho}_r = \theta_r^* / \hat{\sigma}$ (i.e. the intersection of the plot in (v) with the 45-degree line) within a predefined tolerance (0.0005). This can be achieved by progressively decreasing the size of the increments in step (iv). The point where $\hat{\rho}_r = \theta_r^* / \hat{\sigma}$ is the point at which the amount of selection on observables is equal to the amount of selection on unobservables.

As discussed in Altonji et al. (2005b), the point at which selection on observables is as sizeable as selection on unobservables identifies an upper bound for the amount of selection on unobservables that one should expect provided that: *(a)* a sufficient number of controls can be included in the empirical models; *(b)* these controls can account for a substantial amount of the variation in the outcomes (as captured by the R-squared's of the models); *(c)* the set of controls is sufficiently broad to capture most of the factors that determine the outcomes. It follows that estimates of the impact of attending a Faith school where we impose an equal amount of selection on observables and unobservables provide a lower bound to what the effect of attending a religious school would be in the absence of sorting on unobservables. This helps us establishing how robust our estimates are to the 'most conservative' assumptions about sorting based on unobservables, and answers our question *(b)* set out above.

Table 1: Institutional characteristics of primary schools in England

Type	Faith	Governors (approximately)	Admissions authority	Assets owned by	Employer
Community	Secular	Parents >30%, Staff <30%, LEA 20%, Community 20%	LEA	LEA	LEA
Foundation	Mostly Secular, some C. of E.,	Parents >30%, Staff <30%, Foundation/Partnership <25%, LEA <20%, Community 10%	Governors	Foundation or Governors	Governors
Voluntary Aided	Mostly C. of E. or Catholic, some other Faith, some Secular	Foundation >50%, Parents >30%, LEA <10%, Staff <30%	Governors	Foundation	Governors
Voluntary Controlled	Mostly C. of E., some other Faith, some Secular	Parents >30%, Staff <30%, Foundation <25%, LEA <20%, Community 10%	LEA	LEA	LEA

Note: C. of E. means Church of England.

Table 2: *Ks2* (age-11) achievement by school type; descriptive statistics

Variable	Mean	Std.Dev.	Percentage of age-11 pupils
Average *ks2* score, Mathematics and English (percentiles)	50.50	26.61	100%
Faith (non-autonomous or autonomous)	53.85	26.17	29.21%
Faith-non-autonomous	52.43	26.46	9.94%
Faith-autonomous	54.58	25.99	19.27%
Secular (non-autonomous or autonomous)	49.12	26.66	70.79%
Secular-non-autonomous	49.00	26.67	68.18%
Secular-autonomous	52.17	26.78	2.61%
Autonomous (Faith or Secular)	54.30	26.03	21.88%

Note: the total number of observations is 929,955. Pupils attending or moving to schools with other religious denominations are dropped from the sample; they amount to about 0.6% of the sample (6,387) pupils. Autonomous schools include (Secular and Faith) Foundation and Voluntary Aided schools. Non-autonomous schools include Community and Voluntary Controlled schools.

Table 3: *Ks1* (age-7) achievement and pupil background by primary school type

	No controls					Postcode fixed effects					Postcode × Secondary school fixed effects				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Age-7 *ks1* points	Free meal eligible	SEN status	White	English first language	Age-7 *ks1* points	Free Meal Eligible	SEN status	White	English first language	Age-7 *ks1* points	Free meal eligible	SEN status	White	English first language
Mean	44.752	0.163	0.208	0.845	0.903	44.695	0.181	0.218	0.860	0.924	44.894	0.149	0.212	0.902	0.950
(Std.Dev.)	(10.212)	(0.369)	(0.406)	(0.361)	(0.296)	(10.198)	(0.385)	(0.413)	(0.346)	(0.265)	(10.135)	(0.356)	(0.409)	(0.297)	(0.217)
Faith-autonomous	1.657	-0.046	-0.014	0.033	0.037	1.283	-0.041	-0.013	0.024	0.034	0.231	-0.016	0.006	0.010	0.016
	(0.075)	(0.003)	(0.002)	(0.005)	(0.004)	(0.053)	(0.001)	(0.002)	(0.002)	(0.002)	(0.076)	(0.002)	(0.003)	(0.002)	(0.002)
Faith-non-autonomous	1.290	-0.080	-0.014	0.074	0.061	0.133	-0.014	0.004	0.005	0.009	-0.089	-0.007	0.010	0.007	0.006
	(0.097)	(0.003)	(0.003)	(0.006)	(0.005)	(0.078)	(0.002)	(0.003)	(0.002)	(0.002)	(0.093)	(0.002)	(0.004)	(0.003)	(0.002)
Secular-autonomous	1.209	-0.067	-0.022	0.036	0.029	0.691	-0.025	-0.024	0.012	0.005	0.311	-0.018	-0.004	0.014	-0.002
	(0.232)	(0.007)	(0.006)	(0.014)	(0.012)	(0.150)	(0.004)	(0.005)	(0.005)	(0.006)	(0.191)	(0.005)	(0.007)	(0.005)	(0.006)
Secular-non-autonomous	44.273	0.181	0.213	0.831	0.889	44.254	0.196	0.222	0.852	0.912	44.833	0.156	0.209	0.898	0.945
	(0.042)	(0.002)	(0.001)	(0.003)	(0.003)	(0.026)	(0.001)	(0.001)	(0.001)	(0.001)	(0.039)	(0.001)	(0.001)	(0.001)	(0.001)

Note: The top part of the table shows raw means and standard deviations for all schools. The bottom part shows means for Secular-non-autonomous schools, and mean differences for other school categories with respect to Secular-non-autonomous schools. Means and mean differences in the bottom part of the Table are obtained from regressions at the pupil level without controls or controlling for postcode fixed effects; standard errors clustered at the schools level. SEN means: Special Educational Needs (with and without statements). Sample size: no controls 929,955; Postcode fixed effects: 281,408; Postcode × Secondary school fixed effects: 101,199.

Table 4: School type and mean *ks2* (age-11) attainment; conditional on initial attainment, background and place of residence

	(1)	(2)	(3)	(4)	(5)
Faith-autonomous	5.582	2.338	1.686	0.962	0.817
	(0.212)	(0.158)	(0.126)	(0.148)	(0.168)
Faith-non-autonomous	3.425	0.918	0.023	-0.147	-0.222
	(0.274)	(0.199)	(0.166)	(0.174)	(0.193)
Secular-autonomous	3.168	0.925	1.118	0.973	0.671
	(0.606)	(0.453)	(0.337)	(0.371)	(0.397)
Age-7 attainment	No	Yes	Yes	Yes	Yes
Individual and school level controls	No	No	Yes	Yes	Yes
Postcode fixed effects	No	No	Yes	No	No
Postcode × Secondary school type fixed effects	No	No	No	Yes	No
Postcode × Secondary school fixed effects	No	No	No	No	Yes
R-squared	0.0073	0.6156	0.7614	0.7742	0.7821
Schools	14,821	14,821	14,020	13,357	12,089
Observations	929,955	929,955	281,408	155,085	100,199

Note: Regressions at the pupil level; standard errors clustered at the primary school level. Baseline: Secular-non-autonomous schools. Controls with descriptive statistics are listed in Appendix Table A1.

Table 5: *ks1* (age-7) achievement and pupil background by primary school type; pupils who stay or switch school types across primary and secondary phases

	(1)	(2)	(3)	(4)	(5)
	Age-7 *ks1* points	Free meal eligible	SEN status	White	English first language
Panel A: Stayers					
Faith-autonomous	2.233	-0.062	-0.034	0.031	0.037
	(0.071)	(0.002)	(0.003)	(0.003)	(0.002)
Faith-non-autonomous	-1.007	0.015	0.006	0.035	0.002
	(0.730)	(0.021)	(0.028)	(0.017)	(0.016)
Secular-autonomous	1.826	-0.039	-0.091	0.010	0.002
	(0.292)	(0.010)	(0.012)	(0.011)	(0.011)
Secular-non-autonomous	43.90	0.216	0.228	0.850	0.908
	(0.028)	(0.010)	(0.001)	(0.001)	(0.001)
Panel B: Switchers					
Faith-autonomous	-0.268	-0.019	0.014	0.008	0.026
	(0.111)	(0.003)	(0.004)	(0.004)	(0.003)
Faith-non-autonomous	-0.576	-0.011	0.015	0.011	0.016
	(0.131)	(0.004)	(0.005)	(0.004)	(0.003)
Secular-autonomous	0.171	-0.012	-0.000	0.006	0.017
	(0.264)	(0.008)	(0.011)	(0.009)	(0.009)
Secular-non-autonomous	45.07	0.166	0.213	0.856	0.917
	(0.069)	(0.002)	(0.003)	(0.003)	(0.002)

Note: Table shows means for Secular-non-autonomous schools, and mean differences for other school categories with respect to Secular-non-autonomous schools. Means and mean differences are obtained from regressions at the pupil level with postcode fixed effects; standard errors clustered at the schools level. SEN means: Special Educational Needs (with and without statements). . Panel A only includes pupils who attend the same type of schools in both periods. Panel B excludes pupils who attend the same types of school in both phases.

Table 6: School type and mean *ks2* (age-11) attainment; stayers and switchers sub-samples

	(1)	(2)
	Stayers	Switchers
Faith, autonomous	2.672	-0.106
	(0.183)	(0.225)
Faith, non-autonomous	2.036	-1.319
	(2.050)	(0.248)
Secular, autonomous	2.176	-1.293
	(0.594)	(0.513)
Age-7 attainment	Yes	Yes
Individual and school level controls	Yes	Yes
Postcode fixed effects	Yes	Yes
R-squared	0.8085	0.8528
Schools	10,535	9,956
Observations	170,931	110,481

Note: Regressions at the pupil level; standard errors clustered at the primary school level. Baseline: Secular, non-autonomous schools. Column 1 only includes pupils who attend the same type of schools in both periods. Column 2 excludes pupils who attend the same types of school in both phases. Controls with descriptive statistics are listed in Appendix Table A1.

Table 7: Sensitivity analysis with constrained correlation between selection and treatment equation

Outcome and specification	ρ=0.00	ρ=0.02	ρ=0.04	ρ=0.06	ρ=0.08	ρ=0.10	ρ=0.12	ρ=0.14	'Equal selection'
Panel A: All pupils									
Faith-autonomous	2.273	1.720	1.167	0.614	0.060	-0.287	-0.770	-1.323	*0.060*
	(*0.151*)	(0.151)	(0.151)	(0.151)	(0.151)	(0.146)	(0.151)	(0.151)	*(0.151)*
Panel B: Stayers									
Faith-autonomous	3.120	2.551	1.982	1.413	0.844	0.275	-0.294	-0.863	*-0.379*
	(0.196)	(0.196)	(0.196)	(0.196)	(0.196)	(0.196)	(0.196)	(0.196)	*(0.196)*
Panel C: Switchers									
Faith-autonomous	0.764	0.230	-0.303	-0.836	-1.369	-1.903	-2.436	-2.969	*0.254*
	(0.216)	(0.216)	(0.216)	(0.216)	(0.216)	(0.216)	(0.216)	(0.216)	*(0.216)*

Note: All specifications include 183 KS1 dummies and dummies for individual ethnicities, gender, eligibility for FSME, SEN status, English as a first language and 150 LA dummies. Regressions estimated using two-step Heckman selection models with constrained correlation between the error terms of the treatment and main equations. S.E.s corrected for clustering within Primary schools. 'Equal selection' displays estimates where ρ is constrained such that the amount of selection on observables equates the amount of selection on unobservables. Specifically, for Row 1 (All pupils), ρ = 0.080; for Row 2 (Stayers), ρ = 0.123, and for Row 3 (Switchers), ρ= 0.019 give rise to 'equal selection' on observables and unobservables.

7. Appendix Tables

Appendix Table A1: Control variables: descriptive statistics

Variable	Mean	Std.Dev.	Min,Max
Pupil Level			
Female	0.496	0.499	0,1
Native language English	0.902	0.296	0,1
Native language not available	0.022	0.148	0,1
Native language not English	0.075	0.263	0,1
Pupil eligible for free school meals (FSM)	0.163	0.369	0,1
FSM eligibility status missing	0.022	0.146	0,1
Pupil with special education needs (SEN)	0.208	0.406	0,1
SEN status missing	0.022	0.147	0,1
White ethnicity	0.845	0.361	0,1
Black Caribbean ethnicity	0.014	0.116	0,1
Black Other ethnicity	0.016	0.124	0,1
Indian ethnicity	0.019	0.136	0,1
Pakistani ethnicity	0.023	0.149	0,1
Other Asian ethnicity	0.011	0.103	0,1
Other and mixed ethnicities	0.027	0.161	0,1
Missing ethnicity	0.046	0.211	0,1
Academic Year 2001/2002	0.499	0.500	0,1
School Level			
Total number of pupils	315.8	132.5	13,1292
Pupil/teacher ratio	23.14	3.096	4.3,72.2
Fraction of pupils eligible for FSM	0.169	0.145	0,0.94
Fraction of pupils with SEN	0.197	0.095	0,0.79
Fraction of Whites in school	0.844	0.254	0,1
Fraction of Caribbean Blacks in school	0.013	0.047	0,0.79
Fraction of Other Blacks in school	0.016	0.053	0,1
Fraction of Indians in school	0.019	0.070	0,1
Fraction of Pakistani in school	0.023	0.095	0,1
Fraction of Other Asian in school	0.011	0.053	0,1
Fraction of other and mixed ethnicity in school	0.027	0.051	0,1
Fraction with missing ethnicity in school	0.047	0.167	0,1
Ratio of ethnically classified to total pupils in school	0.409	0.431	0,1

Appendix Table A2: Transition matrix between primary and secondary phase, by school type

	Future school (age 12)			
Current school (age 11)	Faith, autonomous	Faith, non-autonomous	Secular, autonomous	Secular, non-autonomous
Faith-autonomous	51.2	0.8	10.8	37.2
	(91,774)	(1,526)	(19,408)	(66,497)
Faith-non-autonomous	6.9	2.2	18.5	72.4
	(6,343)	(2,044)	(17,147)	(66,948)
Secular-autonomous	5.4	0.5	54.8	39.3
	(1,310)	(122)	(13,295)	(9,531)
Secular-non-autonomous	5.2	0.8	16.5	77.5
	(32,714)	(4,785)	(104,897)	(491,617)

Note: The table presents row percentages; total numbers in parentheses.