Socio-economic inequalities in access to Higher Education in England

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Abstract

This thesis analyses inequalities in access to Higher Education (HE) in England. In particular, it provides new evidence about this issue in three major ways.

First, it estimates the family income gradient in university participation overall, and at a group of high status institutions. It also analyses the income gradient in university applications. While there are substantial income gradients in university attendance, and attendance at high status institutions, most of these differences are driven by application decisions, particularly once we control for 'ability' at age 11. This suggests that universities do not discriminate against students from poorer backgrounds; such students are less likely to apply.

Second, it assesses the role of socio-economic status in explaining changes in university expectations across the teenage years. It analyses transitions in young people's expectations from being 'likely to apply' to being 'unlikely to apply' and vice versa, using duration modelling techniques. Young people's socio-economic background has a significant association with changes in expectations, even controlling for prior academic attainment and other potential confounding factors. This suggests more could usefully be done to maintain the educational expectations of academically able young people from less advantaged families.

Finally, it looks at the impact of aptitude tests as a screening device for entry to elite universities by looking at the effect on the proportion of successful applicants by school type (state versus private) and gender. The estimates are obtained by applying a difference in differences approach to administrative data from the University of Oxford. Although introducing the test increased the proportion of interviewees getting an offer overall, this is not the case for women. Nevertheless, the policy has no apparent effect on the overall chances of applicants being offered a place by school type or gender.

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Declaration

I hereby declare that, except where explicit attribution is made, the work presented in this thesis is entirely my own.

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Chapter 1

Investigating access to Higher Education in England

1.1 Introduction

Socioeconomic status (SES) has a strong association with application to university, attendance at university, and attendance at the most selective universities. For example, in 2011-12, only 20% of 18-19 year olds entered higher education from the bottom fifth of local areas ranked by the percentage of parents who are university graduates compared to 60% of those in the top fifth (HEFCE, 2013, Figure 19). Similarly, secondary school pupils who are eligible for Free School Meals are less than half as likely to go to university as other young people (BIS, 2012, Table 1). When the extent of inequality is so stark, the causes of this relationship are clearly a matter of academic, and public, interest.

In this thesis, I provide important new evidence about inequalities in access to Higher Education (HE) in England in three areas. First, I assess the extent of socioeconomic inequalities for a recent cohort of students, taking advantage of rich survey data from the Longitudinal Study of Young People in England to contrast the inequality associated with applying to university with the inequality associated with attending, conditional on having applied. Second, I explore young people's expectations of applying to university, taking the innovative step of using duration modelling to analyse the influence of SES on changes in young people's expectations during a critical stage of their educational careers. Third, I use new data from the University of Oxford to estimate the causal effect of a change in admissions policy, specifically the introduction of an aptitude test, on relative chances of applicants depending on their socio-economic status and gender.

However, before I proceed, this chapter provides important background for the analyses that follow. Section 1.2 further sets out the importance of studying inequality in access to HE, including explaining the economic principles that make fair access to HE desirable. Section 1.3 then assesses trends in participation in HE, in particular concentrating on evidence of changes in inequalities over time. Section 1.4 then moves on to describe the institutional context of the English Higher Education system. Finally, Section 1.5 outlines the structure of the remainder of this thesis.

This thesis is part of a wider body of research into this issue that I have undertaken. This includes an in depth study of the Longitudinal Study of Young People in England (LSYPE) and its use for analysing access to Higher Education (Anders, 2012b) and joint work with John Micklewright exploring young people's expectations of applying to university (Anders and Micklewright, 2013).

1.2 Why study access to Higher Education in England?

Reducing inequalities in participation in Higher Education is important to economists for reasons of both equity and efficiency. There are significant economic returns to Higher Education (Blundell et al., 2000, 2005; Devereux and Fan, 2011), although we should not overlook the existence of differences in returns by institution (Chevalier and Conlon, 2003; Chevalier, 2014), by subject studied (Bratti et al., 2008), by demographic characteristics (Sloane and O'Leary, 2004; Machin et al., 2009), by socio-economic status (Crawford and Vignoles, 2014), and between graduates with apparently similar characteristics (Feinstein and Vignoles, 2008; Green and Zhu, 2010). Much of the return will accrue to the individual obtaining the HE, through improved earning power after graduation. As such, access to HE that is unfairly socially graded feeds through to inequality of opportunity in the labour market, and hence economic inequalities.

Even setting aside equity concerns, there are issues of economic efficiency, not least due to a lack of perfect information among individuals choosing whether or not to attend HE (Barr, 2004, ch.14). Furthermore, inequality in access to HE imposes economic costs on the UK, as there are societal benefits from achieving fair access (Wößmann and Schültz, 2006). Human capital is a scarce resource: failure to maximise the productivity of this resource reduces economic growth (Holland et al., 2013). The UK government invests a significant amount in HE; it presumably wishes to maximise the economic gains from doing so. Furthermore, increased take-up of HE also has indirect benefits to society and government (BIS, 2013a) such as reduced crime (Lochner and Moretti, 2004), increased tax revenue (Conlon and Patrignani, 2011; Walker and Zhu, 2013) and increased public health (Grossman, 2006). All of these benefits will be maximised when the individuals who receive HE are those who stand to generate the most benefit from doing so, regardless of characteristics such as SES, ethnicity and gender.

However, there is still much that we do not know about these inequalities in England. Previous literature draws on data with important limitations. For example, while previous literature suggests that prior attainment at age 16 explains much of the SES gradient in participation (Chowdry et al., 2013), this thesis tests the robustness of this using rich survey data on, rather than administrative proxies for, SES. While many previous analyses of access to Higher Education have concentrated only on enrolment (Marcenaro-Gutierrez et al., 2007), this thesis uses data that identify whether young people apply to university in the first place.

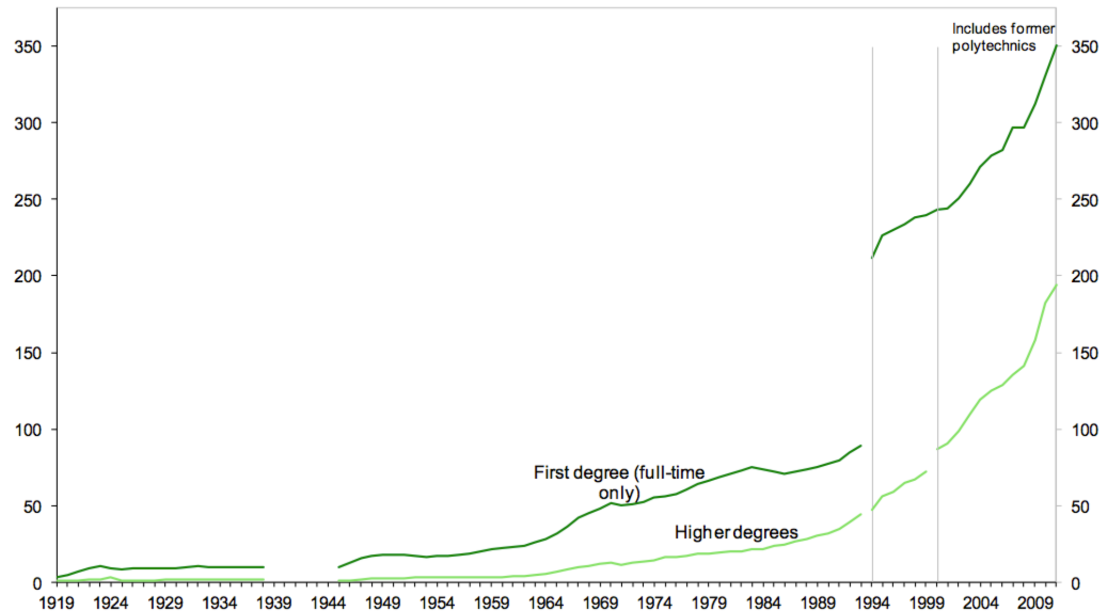
Furthermore, there have been many changes in policy over the past twenty years (see Section 1.4, below). Since these may have resulted in changes in inequality, the evidence for a recent cohort of young people presented by this thesis is important in updating work that focuses on earlier cohorts.

1.3 Trends in HE participation and inequalities

There has been a large growth in the number of individuals who obtain Higher Education in the UK (see Figure 1.1). While the focus of this thesis is England (as a result of institutional differences between the countries of the UK and limitations of the datasets used), this pattern is unlikely to be that different when we remove Wales, Scotland and Northern Ireland. Growth in the participation rate[1] follows a slightly different path, due to differential birth rates over the time period. Nevertheless, the pattern is the same (Elias and Purcell, 2004, Figure 1).

[1] The participation rate was measured for many years using the Age Participation Index (API). This reports the number of first time entrants to full-time and sandwich undergraduate courses, divided by the average of the 18 year old population and 19 year old population of Great Britain.

Figure 1.1: Number of students obtaining university degrees in the UK (thousands)



Notes: Source: Bolton (2012, p.14)

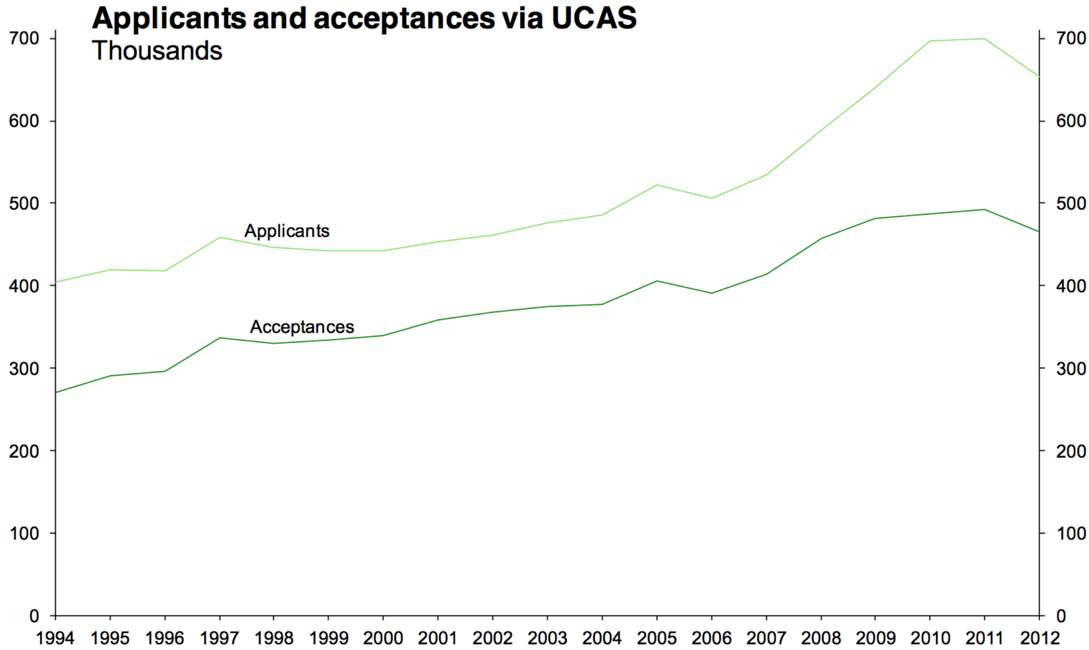
There was political support for this expansion, exemplified by the Labour Party's 2001 manifesto promise of a 50 percent HE participation rate by 2011. This goal was essentially met, although perhaps partly due to changes in measurement (Heath et al., 2013, p.238-239). The official target was the Higher Education Initial Participation Rate (HEIPR),[2] which climbed from 39% in 1999 to 49% in 2011 (Bolton, 2013b; BIS, 2013b).

Focusing on the period from 1994 onwards (and concentrating on entry, as this thesis does, rather than completion), Figure 1.2 shows continuing large increases in the number of acceptances for places at Higher Education institutions. However, it also shows the increasing number of individuals applying to university, with the gap between the two appearing to widen somewhat in very recent years. This would appear to imply increased competition for the available places.

Turning to the issue of trends in inequalities in access to HE over time, an issue is that finding comparable data on participation and a measure of SES over time is difficult. Nevertheless, a number of studies have looked at short and long term trends in differing ways. One broad finding from this work has been that this long-term expansion has, in some ways, worsened educational inequality (Galindo-Rueda et al., 2004), perhaps

[2]The HEIPR measures the participation of 17-30 year olds in HE. It is constructed by dividing the number of initial entrants to HE at each age between 17 and 30 by the total population of the relevant age (e.g. dividing the number of initial entrants aged 18 by the population of 18 year olds), then summing up each age's Initial Participation Rate to get the overall HEIPR. A change in method in 2006 boosted the HEIPR from 40% to 42%, meaning that the figures from 1999 and 2011 are not quite comparable.

Figure 1.2: Number of applicants and acceptances to UK HE institutions (thousands)



Notes: Source: Bolton (2013a, p.3)

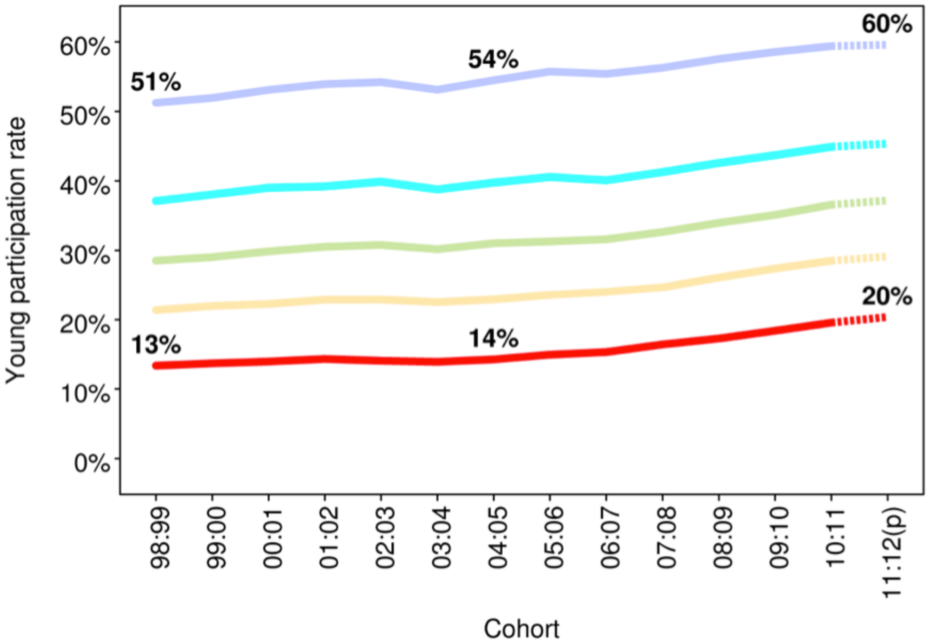
because additional places have predominantly been taken by young people from advantaged backgrounds (Machin and Vignoles, 2004; Blanden and Machin, 2004), rather than being spread throughout the SES distribution. However, the direction of travel since the mid-1990s to some extent depends upon the way one looks at the data (Bekhradnia, 2003).

Blanden and Machin (2004) and Machin and Vignoles (2004) both describe clear evidence of widening in the participation gap by parental education and parental position in the income distribution between 1981 and 1993. The gap widens in both absolute and relative terms. However, between 1993 and 1999 while the gap widens in absolute terms it narrows in relative terms. Kelly and Cook (2007), this time looking at differences by social class, also finds evidence of an upward trend in the participation gap in absolute terms between 1940 and 2000. However, contrastingly, Kelly and Cook finds that the gap has been declining in relative terms since the 1960s.

More recently, consistent data on inequality in HE participation are available using a measure of the percentage of parents in an area who have participated in HE (HEFCE Participation Of Local AReas or POLAR[3]). Figure 1.3 shows the participation rate of young people

[3]Specifically, the POLAR classification is formed by ranking Census Area Statistics wards by their young participation rates (dividing the number of young people from the wards who attended Higher Education according to records from the Higher Education Statistics Agency by the young population of the ward according to Her Majesty's Revenue and Customs) for recent cohorts, then splitting these into quintile groups (HEFCE, 2012, 2014).

Figure 1.3: Trend in young participation rate by area-level HE participation rates



Notes: Source: HEFCE (2013, Figure 10, p.13). Areas assigned quintile groups using HEFCE Participation Of Local Areas (POLAR3) data.

by quintile groups defined using this measure of area-level HE participation. Across the period from 1998/99 to 2011/12, for which these data are available, we see a slight increase in the absolute gap (two percentage points) between the most advantaged fifth and the least advantaged fifth defined in this way. However, we do see a reduction in the size of the relative gap between these groups, since the proportional size of the change for the least advantaged group is significantly larger than it is for the most advantaged group.

1.4 Institutional background

Addressing inequalities in HE participation has often been divided into issues of 'widening participation' and 'fair access'. The definitions of these terms are not used consistently in the literature or by policymakers, indeed they are sometimes used interchangeably. Often, one is seen as a constituent of the other (HEFCE, 2014). However, it is important to note that they are different concepts, which tend to be focused on different elements of the issue of access to higher education and which can lead to differing policy conclusions. Bekhradnia points out that "On the one hand, it is quite possible to widen participation without having fair access [...]. On the other hand, it is possible to concentrate on fair access in a way that detracts from a broader effort to widen participation" (Bekhradnia, 2003, p.2). For example, it may be argued that removing contextual information from university applications might in some sense be "fairer", but seems highly likely to hamper efforts to widen participation.

A common thread in descriptions of the difference between the two concepts, is that widening participation encompasses national or sector-wide changes, while fair access is about changes at the level of individual universities (and, hence, the differences in the distribution of students from less advantaged backgrounds across institutions) (Bekhradnia, 2003; OfFA). As a result of being about the policies and practices of universities, discussions of fair access have tended to focus ensuring that admissions processes are organised such that nothing other than academic ability has a bearing on university applicants chances of being offered a place.

For the purposes of this thesis, I define 'widening participation' as a broad term covering efforts through national policy to increase the proportion of young people from less advantaged backgrounds who receive Higher Education. I define 'fair access' as efforts by universities to remove barriers to attendance, including, but not limited to, ensuring that nothing other than academic ability has a bearing on applicants' chances of being offered a place. This thesis has a bearing on issues of both 'widening participation' and 'fair access', as defined here.

No English student, however well qualified at age 18, has the right to attend any given Higher Education Institution (HEI). This is unlike the situation in some European countries, such as Italy. Students choose whether to apply to university at all, and universities choose whether to accept the application an individual makes. As seen in Figure 1.2, not all individuals who apply to university are accepted. Partly for this reason, drop-out rates are relatively low, compared to other developed countries (Barr, 2004, p.332).

While the issue of measuring quality in Higher Education is certainly difficult and, to some, controversial, it is empirically true that there is variation in the labour market outcomes of individuals who have attended different English universities (Power and Whitty, 2008). While this will partly be driven by non-random selection into different institutions, work that has attempted to deal with this issue has found that otherwise similar individuals who attend higher quality HEIs do have improved labour market outcomes (Chevalier and Conlon, 2003; Hussain et al., 2009; Chevalier, 2014). While it is far from a perfect division, universities are often divided into three broad groups, in increasing order of prestige: new universities, old universities, and Russell Group universities. The first group comprises former polytechnic institutions, which were granted degree-awarding powers in 1992. All other institutions are old universities, from which the Russell Group is a self-selected group of 19 research-intensive institutions. Given the seemingly higher rates of return to an education at a Russell Group university, there has been particular attention paid to whether individuals from disadvantaged backgrounds apply to and receive a fair hearing from these prestigious institutions (Boliver, 2013).

An emerging trend in Higher Education in England, is the increasing use of additional tests as part of the admissions processes for more competitive institutions (Admissions Testing Service, 2014), such as members of the Russell Group, and more competitive courses, such as medicine (UKCAT Consortium, 2014) and law (LNAT Consortium, 2014). In the case of competitive institutions, this may be seen as a partial return to the earlier approach of Oxford and Cambridge where an entrance exam was previously used until the 1980s, in the case of Cambridge, and 1995, in the case of Oxford.

The introduction of such tests is a response to two important concerns about relying on school examination results. First, it has become more and more difficult for universities to differentiate between an increasing number of applicants to Higher Education when there is less and less to choose between highly performing applicants in terms of their grades at 'A-Level' (school exams taken at ages 17-18). For example, the proportion of exam entries being awarded the then top grade (A) increased from 16.2% in 1996 to 26.8% in 2009[4] (for Education, 2013, Table 14). Second, a growing recognition that school examination results may be 'biased' by school type, in that applicants from state schools with a given set of school grades on average outperform applicants from independent schools with the same set of school grades once they get to university (Crawford, 2014, p.55). However, there is little evidence on the implications for fair access of the trend.

In order to fund the growth in student numbers seen above, an increasing proportion of the costs of HE have been moved from taxpayers to students/graduates. This began with the introduction of up-front tuition fees of £1,000[5] in 1998 (Goodman and Kaplan, 2003), switching to a system of income contingent loans for tuition fees of up to £3,000[6]

[4]In 2010 a new top grade (A*) was introduced to help mitigate this problem.
[5]In 1998 prices.
[6]In 2005 prices.

in 2006 (Dearden et al., 2005), and, most recently, to fees of up to £9,000[7] in 2012. While each of these changes in funding policy has sparked fears of reduced participation (e.g. The Sutton Trust, 2013), particularly among those from less advantaged backgrounds, they have yet to result in any sustained reduction in participation rates (HEFCE, 2013; UCAS, 2013). Analyses suggest that while fees in isolation are likely to put individuals off university participation, such effects are offset by the impact of increasing grants and loans alongside them (Wyness, 2009; Dearden et al., 2010, 2013).

Alongside the increase in fees in 2006-07, the Office for Fair Access (OfFA) was established, charged with promoting and safeguarding fair access to English HE institutions: one of its core aims is to secure "improvements in the number and/or proportions of students in higher education from low income and other under-represented groups" (Office for Fair Access, 2014). Its role was increased as part of the further increase in tuition fees in 2012: in order to charge fees above £6,000 universities are required to agree "access agreements" with OfFA, detailing the actions they will take to promote fair access to their institution. At present, not all English universities charge fees of £9,000, but all do charge above £6,000, meaning that OfFA must have approved an access agreement for all English universities.

1.5 Thesis outline

By showing the current extent of inequality and the benefits of attending Higher Education, I have demonstrated the importance of understanding socio-economic inequalities in access to Higher Education in England. Furthermore, I have highlighted ways in which the previous literature does not address important issues, such as the relative importance of inequalities in application and inequalities in whether applicants go on to attend university. This thesis makes several important contributions to the field, proceeding as follows.

In Chapter 2 I provide important new evidence on the extent of inequalities in access to English universities using longitudinal data for a recent cohort. I estimate the association between household income and young people's chances of attending university, estimating both the unconditional income gradient and the income gradient conditional on a rich

[7] In 2012 prices.

set of background characteristics, including young people's prior academic attainment. I analyse the extent of these inequalities in application, in attendance, and in attendance conditional on application. Splitting the admissions process into these two steps and analysing them separately, as well as together, yields important new insights about the point at which inequality in access to university emerges. Also in this chapter, specifically Section 2.5, I analyse the differences in the SES gradient in access to Russell Group universities, some of the most competitive English institutions, relative to the overall SES inequalities. A version of this chapter has been published in the journal Fiscal Studies as Anders (2012a) and an earlier version was published as Anders (2012c).

The analysis in Chapter 2 splits access to HE into two main parts. First, the emergence of socio-economic inequalities in the years running up to making an application. Secondly, whether applicants' chances are affected by their social background at the point of application. The remaining two chapters concentrate on an aspect of each of these in turn.

Chapter 3 looks at the former. In order to get a better understanding of why young people from poorer backgrounds are less likely ultimately to make an application, I explore the influence of SES on changes in young people's expectations during their teenage years of applying to university. I make use of duration modelling techniques to analyse transitions in young people's expectations both from being 'likely to apply' to being 'unlikely to apply' and vice versa, since it is quite possible that the factors associated with young people raising their expectations and starting to think that they are likely to apply to university are different from the factors influencing movement in the other direction. A version of this chapter was awarded the Helen Robinson Prize for Best Paper by a Young Researcher at the WPEG[8] Conference 2014, while related work was published as Anders and Micklewright (2013).

In contrast to Chapter 3, Chapter 4 considers an issue of fair access among applicants. I address a potential implication of the increasing use of selection tests as part of the admissions processes of the most competitive English universities. Applying the quasi-experimental method of difference-in-differences to administrative data from the University of Oxford never before used for evaluation purposes, I estimate the effects of the introduction of an aptitude test as part of the admissions processes for Economics courses at this university. Specifically, I assess whether the effects differ depending on

[8]Work, Pensions and Labour Economics Study Group, Department of Economics, University of Sheffield

applicants SES (using school type as a proxy) and gender. A version of this chapter was previously published as Anders (2014).

Finally, Chapter 5 summarises and restates the main conclusions from the three substantive chapters.

Chapter 2

The link between household income, university application, and university attendance

2.1 Introduction

An intergenerationally mobile society is one in which an individual's life chances do not depend solely on their parents' socioeconomic status, but also on the individual's own abilities, efforts and luck (Blanden et al., 2005, p.4). Given the high rewards to university qualifications in the labour market, discussed in Chapter 1.2, the link between household income and university attendance will have important implications for the extent of intergenerational social mobility in society and is thus an issue of key public policy concern.

Finding the causal impact of income on education or university attendance is a difficult task. One ideally requires some sort of exogenous variation in permanent household income that is uncorrelated with other characteristics usually associated with particular income levels, such as policy changes over time, across regions or for different types of people. Very few studies have had access to such variation, with a notable exception being Acemoglu and Pischke (2001) for the US, which uses changes in the extent of income inequality over time as a source of variation.

For the UK, Blanden and Gregg (2004) compare a range of strategies to get around this problem, including controlling for sibling fixed effects and controlling for parental income post-childhood as a proxy for permanent income. However, the more typical approach (e.g. Blanden and Machin (2004); Gayle et al. (2003); Marcenaro-Gutierrez et al. (2007); Chowdry et al. (2013)) is to use a rich set of controls to try to account for the other ways in which children from richer and poorer families differ from one another in order to get as close as possible to a causal estimate of income on educational attainment or higher education (HE) participation. Due to the problem of omitted variable bias, estimates from this method cannot be treated as truly causal. I follow a similar approach in this chapter, using rich data from a recent cohort of young people, the Longitudinal Study of Young People in England (LSYPE), whose participants turned age 18 (and could therefore start university) in academic year 2008-09.

This chapter makes three important contributions to the continuing policy debate in this area. First, I demonstrate the extent of differences in university participation by household income (as opposed to other measures of socioeconomic status) for a very recent cohort of young people, using a large longitudinal dataset with income measured across multiple waves, as well as myriad other measures of socioeconomic and family background characteristics, school characteristics and rich measures of prior attainment. This is in stark contrast to much previous research, which has tended to use either much older cohorts operating under very different HE systems (e.g. Blanden and Machin, 2004) and/or where available income data have been of low quality or not present at all (e.g. Gayle et al. (2003); Marcenaro-Gutierrez et al. (2007); Chowdry et al. (2013)). This allows me to look in more detail than has hitherto been possible in the UK at the ways in which income both directly and indirectly impacts on university participation for a recent cohort of university entrants. Quantifying the extent of inequality in a meaningful way is important, giving us a better understanding of the issue than from poorly-defined comparisons such as: "a person who is well-off is seven times more likely to go to university than someone from a poor background" (Cameron, 2010).

Second, I am able to examine the relationship between household income (and other factors) and the decision to apply to university, as well as the decision to attend. This enables me to investigate whether the socioeconomic gradient in university attendance (which many other studies in the UK have found) is primarily driven by differences in the propensity of young people from different backgrounds to apply to university, or whether it is driven by factors that come after the point of application, such as accepted applicants from less advantaged backgrounds choosing not to take up their places or universities discriminating against students from poorer backgrounds. This is an important distinction from a policy viewpoint, since the appropriate response will vary depending on the stage at which one observes income gradients emerging.

Finally, building on the analysis of access to any university, I analyse the differences in participation rates by household income for a select group of 'high quality' universities in the UK known as the Russell Group, exploiting the large size of the dataset, and the fact that the young people interviewed were asked which university they attend. Alongside concerns about the overall HE participation gap, more specific concerns have been raised that young people from poorer socioeconomic backgrounds are disproportionately likely to attend less prestigious institutions (The Sutton Trust, 2008, p.7), which are likely to garner lower returns in the labour market (e.g. Chevalier and Conlon, 2003).

This chapter proceeds as follows: Section 2.2 briefly summarises the findings of previous research looking at the relationship between income (or proxies of income) and HE participation. Section 2.3 describes the data and models that I use. Section 2.4 reports the main results showing how household income affects both the probability of a young person applying to university and participation conditional on applying. Section 2.5 extends the analysis to look at whether income plays a role in determining the probability of attending a Russell Group university. Section 2.6 concludes.

2.2 Theory and previous research

Why might one expect to see a relationship between household income and university attendance? Human capital theory (Becker and Tomes, 1986) is a model of the transmission of income from parents to children under the assumption that parents maximise utility, but care for their children. Under this model, parents' income should not be related to children's outcomes unless there are credit constraints or children's human capital is included in their parent's utility function. In such cases, the model predicts a direct effect of parental income on children's outcomes (Becker and Tomes, 1986, p. 12).

In using the term credit constraints, this chapter refers primarily to its long run concept, as distinguished by Carneiro and Heckman (2002). Short run credit constraints are the more familiar constraints on financing available at a particular point in time, while long run credit constraints are ‘the inability of the child to buy the parental environment and genes that form the cognitive and non-cognitive abilities required for success’ (Carneiro and Heckman, 2002, p.705-706). Other research suggests that short-run credit constraints are not a particularly big problem (e.g. Carneiro and Heckman (2002) for the U.S. and Dearden et al. (2004) for the U.K.) and other evidence on this issue (Chowdry et al., 2012) shows that the new HE funding regime in the UK is particularly generous to students from poorer backgrounds. Blau (1999, p.263), summarising the US literature on the impact of income on educational outcomes concludes, among other points, that permanent income is more important than transitory income in explaining educational outcomes (and thus that long-run credit constraints are more important than short-run credit constraints), though still less important than other parental characteristics (such as parental education).

This chapter extends previous analysis looking at the link between household income (or proxies of income) on the probability of attending university. As discussed above, finding the causal impact of income on education or university attendance is difficult to do robustly. Acemoglu and Pischke (2001) attempt to do so by using changes in the overall income distribution over time in the US to directly address the role of household income in determining college enrolment. By examining shifts in enrolment across the income distribution during the same period they argue that the causal impact of household income may be identified. They find that a 10 percent increase in family income increases college enrolments by 1-1.4 percentage points (Acemoglu and Pischke, 2001, p.903). They also compare these causal estimates with estimates that include wider family background effects suggesting that family income, rather than other factors related to family background, explains 27 percentage points of the 36 percentage point difference in the enrolment rates of children from the top and bottom quartiles in 1992 (Acemoglu and Pischke, 2001, p.901).

Previous empirical studies from the UK have also suggested a link between household income and higher levels of university attendance. For example, Blanden and Gregg (2004) apply a variety of methods, including sibling fixed effects estimation, to a variety of UK cohort datasets and find a small impact of household income on educational outcomes, including the probability of gaining a degree. Blanden and Machin (2004) use several cohorts of data spanning different cohorts to examine the changing relationship as the proportion of the population who attend university greatly increased. They use several methods, with differing measures of participation and income inequality, to show that the expansion of participation has not been equally distributed across the population. Rather it has disproportionately resulted in increased participation rates among young people from better off families.

Gayle et al. (2003) use a single cohort of the Youth Cohort Study (YCS)[1] to model demand for HE. After controlling for prior attainment at age 16, their models suggest that ethnicity, housing tenure, region and parental education show a continued association with the probability of HE attendance. They argue that in the absence of a better alternative in the YCS, one can view housing tenure as a proxy for parental wealth, and hence that there is an association between parental wealth and university attendance. On the contrary, Marcenaro-Gutierrez et al. (2007) also use data from the YCS, but this time take advantage of multiple cohorts between 1994 and 2000 to analyse the socioeconomic gradients associated with the probability of attending university. They find no association between socioeconomic factors and the probability of attending university once they condition on academic attainment at 16 or 18 and, hence, conclude that the socioeconomic inequality in university attendance arises earlier in the education system.

Chowdry et al. (2013) use administrative data, formed by linking the National Pupil Database (NPD) and Higher Education Statistics Authority (HESA) data, to consider the association between an index of socioeconomic status and HE attendance. They find a raw gap in the probability of university attendance between the top and bottom socioeconomic quintile groups of 40.7 percentage points for boys and 44.6 percentage point for girls. They use linear probability regression models with school fixed effects to estimate the remaining socioeconomic gap controlling for other factors. The gap between the top and bottom quintile groups is significantly reduced once other individual and school controls are included, with the gap standing at 29.9 percentage points for boys and 35.8 for girls. This is reduced still further once prior attainment is controlled for, first at age 11 with the gap at 21.1 percentage points for boys and 25.6 for girls, then at age 16 with gaps of 8.7 percentage points for boys and 11.3 percentage points for girls (Chowdry et al., 2013, p.15).

These UK studies have all found sizeable gaps in participation between young people from higher and lower socioeconomic backgrounds, which are substantially reduced or

[1]Unlike the LSYPE, the YCS covers both England and Wales. Gayle et al. (2003) analyse YCS cohort 9, surveying children eligible to leave school in 1997. University participation is hence measured in autumn 2000.

even eliminated entirely once prior attainment (usually up to age 16) is accounted for. On this basis, they generally conclude that socioeconomic status affects HE participation only indirectly through its impact on academic results up to 16, but has no additional direct impact. I am able to test this finding more thoroughly in this chapter, by using a measure of permanent income rather than some proxy measure of socioeconomic status, by being able to include a variety of other background controls, including other measures of socioeconomic and family background, school characteristics and rich measures of prior attainment in the model, and also by being able to separate out the application and attendance decisions.

Very few previous studies have investigated socioeconomic gaps in university applications. Using data from the University and College Admissions Service, who broker almost all applications for undergraduate study at UK universities, the Department for Business, Innovation & Skills (2009) presented evidence of how university applications to 'Sutton 13'[2] institutions varied according to school type (which they used as a proxy for socioeconomic status). They found that, for a given level of attainment, those who applied to a 'Sutton 13' university were no more or less likely to receive an offer dependent on their school type. However, the probability of application to a 'Sutton 13' institution did vary by school type, even after conditioning on average attainment within schools. This research was carried out at school level and did not have the rich individual socio¬economic background data available in the LSYPE though. More recently, both Shiner and Noden (2014) and Boliver (2013) find that social class has an "influence in orienting candidates towards different types of university" (Shiner and Noden, 2014, p.19), even after conditioning on ethnicity, school type and academic attainment at A-Level.

Boliver (2013) also finds that young people from higher social class groups are more likely to receive an offer from a Russell Group university, conditional on having applied to one, than their peers with more disadvantaged backgrounds, after having controlled from A-Level grades. By contrast, Chowdry et al. (2013) also investigated the relationship between socioeconomic status and attendance at a group of high status institutions, covering 35% of HE participants who attend either a Russell Group institution or a university with a higher Research Assessment Exercise score than the lowest amongst the Russell

[2]The 'Sutton 13' is an alternative grouping of 'elite' universities drawn up by the Sutton Trust. It includes the following institutions: University of Birmingham, University of Bristol, University of Cambridge, Durham University, University of Edinburgh, Imperial College, London School of Economics, University of Nottingham, University of Oxford, University of St Andrews, University College London, University of Warwick and University of York.

Group. They find evidence of substantial socioeconomic differences in the likelihood of attending a high status institution, conditional on going to university, of just over 30 percentage points between males and females in the top and bottom SES quintiles. However, in line with their findings on HE participation overall, they are able to explain the vast majority of these differences by controlling for a limited set of individual characteristics, school fixed effects and rich measures of prior attainment from age 11 to age 18.

A major determinant of an individual's decision to apply to university at all or to a Russell Group institution will be their underlying 'innate' ability and factors like parental motivation and support. Clearly if these factors are correlated with family income and HE participation then estimates of the effects of income will be upward biased (Haveman and Wolfe, 1995, p.1833). To minimise the risk of this happening I follow much of the previous literature in this area by including a proxy of ability in the analysis (here measured by national achievement test scores in Maths, English and Science at age 11). This does have drawbacks. Household income and parental motivation are likely to have already impacted on the academic achievement of children at 11. As a result, models which include such controls will potentially underestimate the true impact of household income on university applications and attendance. I discuss this in more detail in the next section.

2.3 Data and models

2.3.1 Data

The Longitudinal Study of Young People in England (LSYPE) (Department for Education and National Centre for Social Research, 2011) was initially funded by the Department for Education for seven 'waves' of data, which were collected annually, beginning in Summer 2004 when cohort members were in Year 9 (aged 13-14).[3] Interviews were conducted with young people and their parents, covering information about the cohort members themselves and the households in which they grew up. This is linked with administrative data from the National Pupil Database (NPD) to provide information on the young

[3]The LSYPE has now been extended for an additional wave at age 25, funded by the Economic and Social Research Council and managed by the Centre for Longitudinal Studies at the Institute of Education. For more information see `http://www.cls.ioe.ac.uk/lsype`.

person's schooling experiences and attainment, including data from national achievement tests sat at the end of Key Stage 2 (age 10-11, the end of primary school) and Key Stage 4 (age 15-16, the end of compulsory secondary school). Having high quality data on prior attainment with low non-response is a major advantage compared to many previous studies based on survey data.

Wave 7 (currently the most recent wave) covers young people aged 19-20 and allows us to model entry to university at age 18-19 or 19-20, i.e. going from sixth form or further education college to university immediately or after a single gap year. This includes the vast majority of those who go to university. To the extent that pupils from poorer backgrounds are more likely to go to university later, however, this chapter may potentially overstate the magnitude of any income gap in participation (Bekhradnia, 2003, p.2).

Table 2.1: Percentages of Young People Achieving Key Application Milestones for the sample with variables used in determinants models

	Overall	**Female**	**Male**
University attend	39.3 (0.55)	43.0 (0.78)	35.6 (0.77)
Sample size	*7875*	*4048*	*3827*
HE attend	44.0 (0.56)	47.7 (0.79)	40.2 (0.79)
Sample size	*7875*	*4048*	*3827*
University apply	51.1 (0.56)	55.0 (0.78)	47.0 (0.81)
Sample size	*7875*	*4048*	*3827*
Uni. attend, conditional on applying	77.0 (0.60)	78.1 (0.80)	75.7 (0.91)
Sample size	*4855*	*2641*	*2214*
Russell Group attend	9.9 (0.34)	11.0 (0.49)	8.9 (0.46)
Sample size	*7864*	*4043*	*3821*
Russell Group, conditional on university	25.3 (0.70)	25.5 (0.95)	25.0 (1.04)
Sample size	*3844*	*2120*	*1724*
Russell Group, conditional on uni. apply	19.4 (0.57)	19.9 (0.78)	18.9 (0.83)
Sample size	*4855*	*2641*	*2214*

Notes: Standard errors in parentheses. Weighted using Wave 7 LSYPE Weights, which attempt to adjust for oversampling and attrition. Application, Offers, Acceptances and Attendance calculated across Wave 5, 6 and 7. Sample: Wave 7 respondents with valid income data from at least one of Waves 1-4, ethnic group, month of birth, parental education, KS3 school type.

Table 2.1 shows the percentage of individuals who reach the milestones in the university application process that I will be analysing. My sample includes individuals in Wave 7 with non-missing data on university applications from Waves 5 and 6, university attendance from Waves 6 and 7, household equivalised income, ethnic group, month of birth, parental education and KS3 school type. Measurement of university application, attendance and household income are critical to this chapter's analysis. Exclusions due to missing data on other variables occur where use of missing variable dummies would not be possible due to the small number of missing values.

I analyse university attendance, rather than HE attendance, so that I can use the sequence of questions asked about the university application process in the LSYPE. However, Table 2.1 also shows, for comparison purposes, the proportion who undertake HE. This is a broader definition than those who go to university, includes those taking HE courses at Further Education colleges, and can be more readily compared with official data. It is clear from the table that participation rates in the LSYPE appear to be higher than one would anticipate from published data. The Higher Education Initial Participation Rate (HEIPR) for ages 17-19 in 2008/09 is 32.9% and in 2009/10 is 34.1% (Department for Business, Innovation & Skills, 2011). Since the LSYPE measurement spans these two years one would expect its estimate of HE attendance to lie somewhere between these two figures. In the LSYPE it is notably larger at 44% (with a standard error of 0.56).

This is probably related to attrition in the LSYPE sample (just 62.4% of the initial sample remain by Wave 7). While I use the sampling and non-response weights provided in the data, these do not appear to be sufficient to replicate HE participation rates observed in the population. To the extent that students from poorer families are more likely to drop out of the survey, this may mean that this analysis overstates the magnitude of the income gaps in university participation.

The Russell Group refers to a group of twenty research intensive UK institutions which are often considered to be amongst the most prestigious universities in the UK.[4] Table 2.1 shows that the proportion of the whole cohort who attend a Russell Group university is just short of 10%, while the proportion of university attendees at a Russell Group university is 25%. Another comparison worth drawing here is that while 77% of those who apply to university get into one, only 19% of those who apply to university get into a Russell Group institution. The truly comparable measure is missing here, since I do not observe whether individuals apply to a Russell Group university or not.

The LSYPE measures household income at each wave between 1 and 4 (i.e. between ages 14 and 17), although the questions asked vary across the waves. An approximation to per-

[4]In March 2012 four additional institutions joined the Russell Group. However, given the timeframe of the data collection, for my purposes the Russell Group is made up of the following twenty universities: University of Birmingham, University of Bristol, University of Cambridge, Cardiff University, University of Edinburgh, University of Glasgow, Imperial College London, King's College London, University of Leeds, University of Liverpool, London School of Economics and Political Science, University of Manchester, Newcastle University, University of Nottingham, University of Oxford, Queen's University Belfast, University of Sheffield, University of Southampton, University College London and University of Warwick.

manent income is calculated by averaging across as many waves as are available for each individual (Blau, 1999, p.263). If income data is not missing at random, this could bias my estimates, but feel this is preferable to reducing the sample size. Summary statistics of household equivalised income are shown in Table 2.2. Income has been equivalised (i.e. adjusted to account for household composition) by dividing by the square root of household size at the time of each data collection point.

Table 2.2: LSYPE vs. FRS equivalised gross family income summary statistics

Characteristic	**LSYPE**	**FRS**
Mean	15,909	19,376
Standard Deviation	11,883	19,615
Minimum	226	81
Maximum	146,707	572,261
1st Percentile	2,555	3,054
10th Percentile	4,990	7,006
25th Percentile	7,780	9,617
Median	13,013	14,942
75th Percentile	20,104	23,177
90th Percentile	31,573	34,528
99th Percentile	53,568	85,242
N	8,682	9,811

Notes: **LSYPE:** Incomes adjusted to Wave 1 (2004) prices using Annual RPI. Approximation to permanent income by averaging across available income measurements between waves 1 and 4. Equivalised by dividing income measure at each time point by square root of family size at relevant time point. Weighted using LSYPE Wave 7 Respondent weights. Sample: Wave 7 respondents with valid income data from at least one of Waves 1-4. **FRS:** Income is Total Gross Household Income. Household with no children between the ages of 13 and 15 or outside England have been excluded. All incomes in 2004 prices, adjusted using annual RPI. Weighted using gross3 grossing factor.

In order to check that the income distribution generated through the above process, I derive a simple comparison measure from the Family Resources Survey (FRS) for the same years: household income is a major focus in the FRS. The comparative variables were constructed using the FRS derived family income variable. Only families with dependent children between the ages of 13 and 15, living in England (the FRS covers the whole of the UK) were included in the calculations to make the sample more comparable. Unlike the LSYPE measure, the FRS income measure is based on only one year's data, meaning one would expect greater variation in measurement.

Overall, the LSYPE appears to underestimate household incomes relative to estimates obtained from the Family Resources Survey (FRS) (see Table 2.2 for a comparison between the LSYPE and the FRS). However, to the extent that under-reporting of household income is relatively constant across the true income distribution, this should not change the relative ranking of individuals. Given that my regression models account for household income by focusing on differences in participation rates between individuals who fall into different quintiles of the income distribution, my main results should be unaffected by this under-reporting.

2.3.2 University admissions as sequential decisions

Previous research has considered differences in university participation according to various measures of socioeconomic status. However, the story is more complicated: the process of university admissions is a set of sequential decisions. Although there are in fact many nuances to this model, and many more hurdles in the process, I have chosen to simplify these into three steps: application, attendance, and attendance at a high-status university.

My decision to simplify in this way was made for reasons both of clarity and the limitations of the data. In exploring the data I discovered that very few applicants fail to receive any offers and very few of those offered a place do not accept any of them. The questions in the LSYPE then do not allow us to distinguish between those who do not attend due to failing to fulfil their conditional offers and those who choose not to attend for some other reason.

Nevertheless, assumption of even a simple sequential model like this allows me to decompose the probability of attending into the probability of applying and the probability of attending, conditional on having applied, as shown in Equation 2.1. This allows us to look at the mechanism(s) by which income may affect attendance, which has not been done in the literature before.

$$P(\text{Attend}) = P(\text{Apply}) * P(\text{Attend}|\text{Apply}) \tag{2.1}$$

Of course, this model treats these two decisions as independent. However, those students applying to university presumably do so because they feel they have some chance of receiving an offer and fulfilling any conditions required. I hope that the richness of the data and the controls used in the models (discussed in more detail below) will make this assumption plausible. A second consideration is that the inevitably smaller sample size of the conditional models means that standard errors of estimates will be larger simply for this reason. This means that comparisons between the conditional and unconditional models on the basis of changes in significance are not reliable (Gayle et al., 2000, p.63).

2.3.3 Methods and models

I begin by exploring the 'raw' relationship between household income and university admission (application, attendance and the conditional relationship). In order to do so I use the non-parametric technique of local polynomial smoothing. It allows me to assess the relationship without making any functional form assumptions. I have chosen to estimate the appropriate bandwidth using the method suggested by Silverman (1986, p.48) to fit the local polynomial.

I then move on to consider how this relationship changes once I control for other ways in which young people from richer and poorer families differ. To do so, I adopt a simple regression approach in which I account for household income by assigning individuals to quintile groups of equivalised permanent household income[5] and then control for different factors. The different model specifications I use are discussed in more detail in the next section.

I estimate regression models of university application (Apply), university attendance (Attend) and university attendance conditional on having applied (Conditional Attend). Given the binary nature of each of these decisions, I use probit regression models. This is preferable to using linear probability models, where there is no constraint on the predicted probabilities falling between 0 and 1 (Thomas, 2005, pp.445-450).

I proceed in a sequential fashion. The first model (M1) simply includes dummy variables for quintile groups of equivalised household income. This shows the 'raw' gap in HE attendance, application or conditional attendance by quintiles of income before other factors that are correlated with both income and HE decisions are accounted for, and can be thought of as the "total" effect of income on HE decisions. The following models add a series of other characteristics to the model, which are designed to account for the other ways in which young people from richer and poorer families differ from one another. These factors can be thought of as "transmission mechanisms" between family income

[5]In Anders (2012c) I used piecewise-linear parametric specifications for income, nevertheless obtaining similar results.

and university participation decisions. To the extent that they are socially graded, their inclusion will reduce the “direct” effect of household income on university participation. Their primary purpose is thus to better understand the routes through which family incomes affects education choices.

In the second model (M2) I add controls for average prior attainment in English, maths and science at Key Stage 2 in an attempt to proxy for innate ability. As outlined above, to the extent that income has already affected attainment at age 11, however, its inclusion will downward bias estimates of the direct effect of income on university participation decisions, such that the coefficient on household income now refers to its additional effect after the point at which prior attainment is measured. This is known as a ‘value-added’ model. While it is clear that there are drawbacks to such specifications (Todd and Wolpin, 2004, p.7-9) the available data do not provide the necessary information for more demanding specifications, such as the so called ‘cumulative’ specification. Such a specification would, for example allow for the possibility of correlation between attainment measures and future family inputs.

In the third model (M3) I add a variety of other observed socioeconomic factors: month of birth, ethnic group, government office region, number of siblings, number of older siblings, whether family type is lone parent or couple, and parental education. These are primarily measured at Wave 1 (age 14), but data from later waves are substituted where Wave 1 data were missing. Since most are time invariant I assume that this is not problematic. This model provides insight into the role of family income in determining university participation for a young person with otherwise identical characteristics in early secondary school.

In the fourth model (M4) I additionally account for the effects of a young person’s secondary school experience on university application and participation decisions. Again, this is likely to reduce the direct effect of household income on education choices, because a young person’s socio-economic characteristics help to determine the secondary school that they attend. The most extreme example of this will be independent schools: an individual’s household income is highly correlated with their probability of attending this school type.

When accounting for secondary school attended, I use dummy variables for school type, including whether the school is a community school, a community technology college, a foundation school, an independent school, a voluntary aided school or a voluntary controlled school. Additionally, dummy variables were included indicating whether the school is a grammar school (i.e. has a selective admissions policy) and whether it has an attached sixth form. This should allow us to identify the impact of specific school characteristics on university admissions. To test whether other observed or unobserved school characteristics were important determinants of university participation decisions, I also estimated linear probability models with school fixed effects,[6] reported in the Appendix A. These gave broadly similar results.

My fifth and subsequent models investigate the question of whether permanent income continues to play a role in determining university application and participation decisions over and above its effect on attainment at age 16. Previous research has suggested that, contingent on attainment at age 16, socioeconomic background plays very little additional role in HE participation decisions. In model five (M5) I return to simply controlling for prior attainment, this time at both Key Stage 2 and Key Stage 4, using individuals' capped GCSE point scores. I do not use Key Stage 5 results in this analysis, since they are not available to universities at the time they make their decisions. One might also be more concerned about endogeneity here than for earlier measures of attainment: individuals who have decided to go to university may put in more effort in an attempt to make sure they meet their university offer and hence obtain better grades than individuals who have decided not to go to university.

In the sixth model (M6), I once again add controls for other socioeconomic and demographic factors, so this model is comparable to M3 except that I now control for GCSE results.

For the final model (M7), I once again add school characteristics. The model is comparable with M4 except that I now control for GCSE results. As with M4, linear probability models with school fixed effects were estimated as a robustness check, and these gave broadly similar results.

I estimate the same specifications when considering participation at a Russell Group institution in Section 2.5, but there I only consider models of attendance and attendance conditional on going to university. Unlike for the analysis of attendance at any university, separate models for males and females are not estimated and reported in Appendix A,

[6] I used linear probability models when including school fixed effects due to the inconsistency of the probit estimator including fixed effects.

due to the smaller sample size in the model conditional on university attendance.

I do not observe the universities individuals have applied to. This means that I cannot be sure how much of any socioeconomic gradient in attendance at a Russell Group institution emerges because of the differing application choices of individuals across the household income distribution. An individual cannot, after all, attend a Russell Group university unless he or she applied to one or more of them. The findings from Department for Business, Innovation & Skills (2009) suggest this could well drive a socioeconomic gradient in the prestige of university attended.

2.4 Analysis of the decision process

2.4.1 Non-parametric analysis

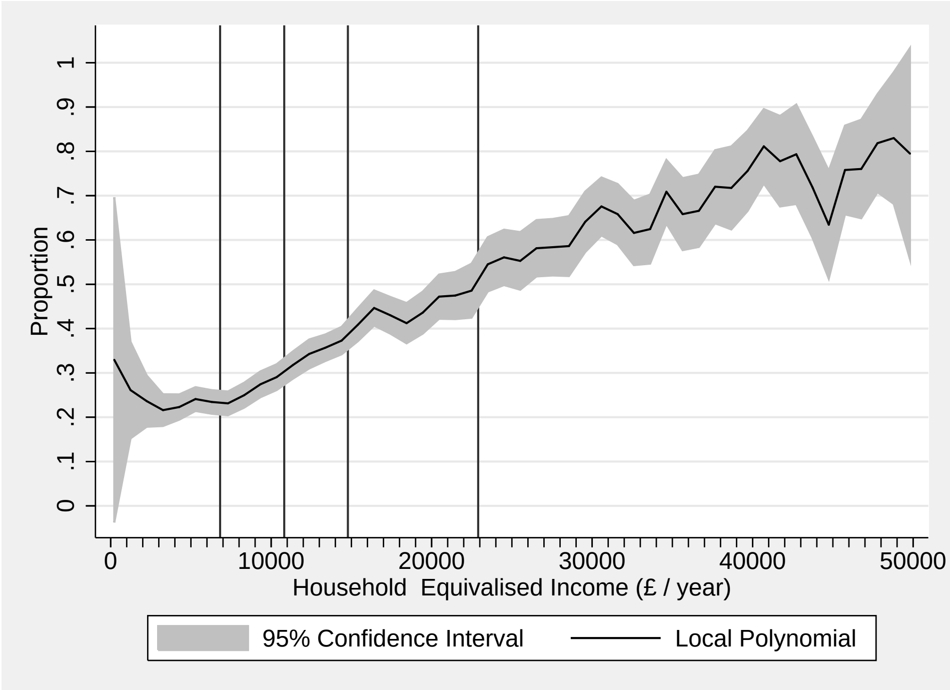
I first consider the simple unconditional university attendance model, which is comparable to much previous research in this area. Figure 2.1 presents new information on the relationship between university participation and equivalised household income in the UK. It demonstrates graphically that university participation increases with equivalised household income, roughly doubling between the 20th and 80th percentiles. For a large portion of the income distribution the relationship appears linear, however two features of the relationship seem particularly worthy of note.

First, at the bottom of the distribution (below approximately £6,000 equivalised household income, within approximately the bottom decile group) participation rates initially fall as household income rises. Further investigation suggests it is related to differences in university attendance rates by ethnic groups and measurement error of certain kinds of income amongst lone parent families[7] (see Anders (2012b) for more details). Regardless, a formal Wald test of the hypothesis of a different linear slope for the section below £6,000 fails to reject the null hypothesis of no difference at the 5% level. Furthermore, there are very few young people with household income in this bottom section, as witnessed by the large confidence intervals.

Second, the attendance rate seems to plateau at about 75%. This corresponds with an

[7]Brewer et al. (2013b) discuss the reasons for this 'tick' further, concluding that it is mainly accounted for by under-reporting of income.

Figure 2.1: University attendance at age 18-19 or 19-20 and household equivalised income



Notes: Weighted using LSYPE Wave 7 Participant Weights. Local polynomial smoothing using Epanechnikov kernel and Silverman's optimal bandwidth of 1590.738. Sample size: 7791. Vertical lines show 20th, 40th, 60th and 80th percentiles of income.

equivalised income of roughly £40,000, around the 92nd percentile of the income distribution. Such a finding is consistent with a story of credit constraints driving the relationship, at least in part, but it is also possible that preferences, participation at later ages or participation in HE rather than university may help to explain why participation is below 100% even for those from families at the very top of the income distribution. It does, however, accord with previous evidence for the US on the non-linearity of the relationship between income and children's outcomes (Mayer, 2002, pp.25-27).

Table 2.3: Probability of university application or attendance by equivalised income quintile group

Variable	Q1	Q2	Q3	Q4	Q5	Q5-Q1	N
University attend	0.23	0.26	0.34	0.45	0.66	0.43	8261
University apply	0.34	0.38	0.46	0.57	0.77	0.43	8261
Uni. attend (conditional on applying)	0.68	0.69	0.73	0.78	0.86	0.18	5073

Notes: Adjusted using LSYPE Wave 7 respondent weights, which attempt to adjust for oversampling and attrition. Sample: Wave 7 Participants with valid responses for variables used in models.

This analysis alone tells us nothing about the point in the application process at which the gap emerges. One could, for example, take from this that young people from across the income spectrum are applying to university, but those with lower household incomes do not get places. To investigate whether this is true or not, I use the additional information available in the LSYPE to look at the relationship between household income and university application, and by extension university attendance conditional on having applied.

As Figure 2.2 shows, a strikingly similar relationship holds as for the unconditional attendance model. It is interesting to note that the percentage of young people who apply to university is approximately 10 percentage points higher than the percentage who attend (shown in Figure 2.1) across the income range, implying that even young people from the richest families who have applied to university are not guaranteed to go.

Taken together, Figures 2.1 and 2.2 mean it is unsurprising that income has a much smaller association with attending university, conditional on having applied, as shown in Figure 2.3. The bulk of the raw gap arises at or before the decision to apply. Once a young person has applied to university the probability that someone in the top quintile group will attend is just 1.2 times larger than someone in the bottom quintile group. Moreover, this is before any confounding factors have been considered.

However, the extent to which this is self-selection on the basis of other characteristics cannot be identified by looking simply at this correlation. To understand the role of other characteristics in transmitting the relationship between household income and university applications and attendance, I turn now to regression modelling.

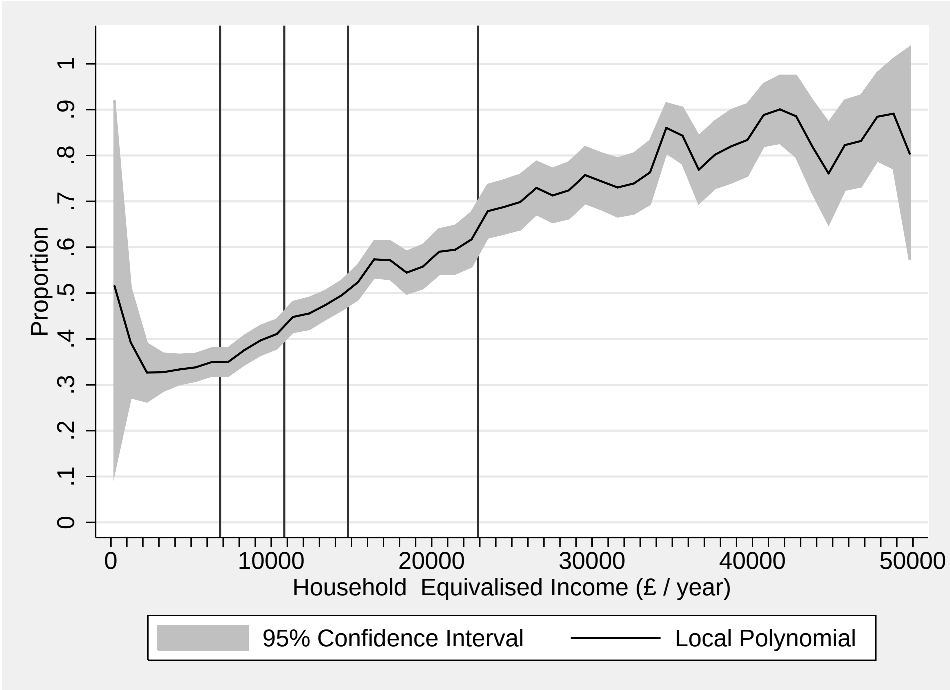
2.4.2 Regression models of university admissions

Table 2.4 presents the predicted probabilities of university application, attendance and attendance conditional on application for hypothetical individuals within each household income quintile group, whose other characteristics are held constant at sample means. Results tables reporting marginal effects of being in each quintile group (relative to the middle quintile group) at sample means, along with the marginal effects of other covariates in the models, are given in Appendix A. Also reported there are similar models estimated separately for males and females.

Considering first the attendance models,[8] the 'raw' relationship between household income and university participation shows that young people in the top quintile group are

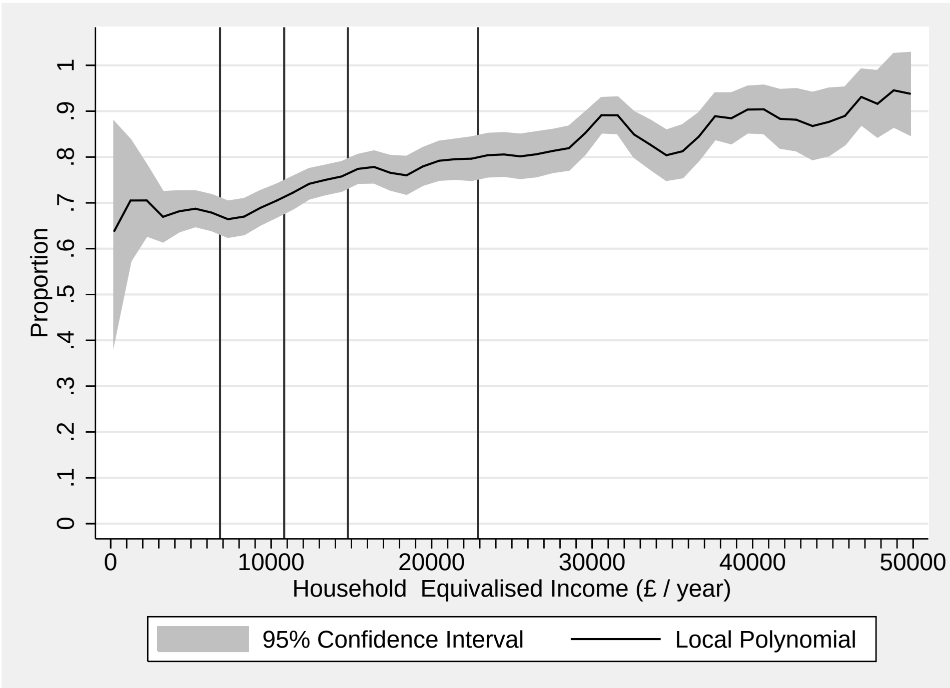
[8] See Table A.1 in Appendix A.

Figure 2.2: University application and household equivalised income



Notes: Weighted using LSYPE Wave 7 Participant Weights. Local polynomial smoothing using Epanechnikov kernel and Silverman's optimal bandwidth of 1590.738. Sample size: 7791. Vertical lines show 20th, 40th, 60th and 80th percentiles of income.

Figure 2.3: University attendance, conditional on application, and household equivalised income



Notes: Weighted using LSYPE Wave 7 Participant Weights. Local polynomial smoothing using Epanechnikov kernel and Silverman's optimal bandwidth of 2295.6094. Sample size: 4780. Vertical lines show 20th, 40th, 60th and 80th percentiles of income.

Table 2.4: Predicted probabilities by income quintile group

University attend	**M1**	**M2**	**M3**	**M4**	**M5**	**M6**	**M7**
Q1	0.23	0.33	0.34	0.35	0.41	0.38	0.38
Q2	0.27	0.31	0.33	0.35	0.36	0.36	0.37
Q3	0.34	0.36	0.37	0.38	0.37	0.38	0.38
Q4	0.45	0.40	0.40	0.40	0.38	0.39	0.39
Q5	0.66	0.55	0.49	0.46	0.44	0.44	0.43
Q5 - Q1	0.43	0.22	0.15	0.11	0.03	0.05	0.05
$P > \|F\|$	0.00	0.00	0.00	0.00	0.00	0.00	0.00
N	7,939	7,939	7,939	7,939	7,939	7,939	7,939
University application	**M1**	**M2**	**M3**	**M4**	**M5**	**M6**	**M7**
Q1	0.34	0.44	0.45	0.46	0.52	0.49	0.49
Q2	0.38	0.43	0.46	0.47	0.48	0.48	0.49
Q3	0.46	0.48	0.49	0.50	0.49	0.50	0.50
Q4	0.57	0.53	0.52	0.53	0.50	0.51	0.51
Q5	0.77	0.69	0.63	0.60	0.57	0.57	0.56
Q5 - Q1	0.43	0.25	0.18	0.14	0.05	0.08	0.07
$P > \|F\|$	0.00	0.00	0.00	0.00	0.00	0.00	0.00
N	7,939	7,939	7,939	7,939	7,939	7,939	7,939
Attending, conditional on applying	**M1**	**M2**	**M3**	**M4**	**M5**	**M6**	**M7**
Q1	0.68	0.75	0.76	0.76	0.78	0.78	0.77
Q2	0.69	0.72	0.74	0.74	0.76	0.76	0.76
Q3	0.73	0.74	0.75	0.76	0.75	0.75	0.76
Q4	0.78	0.77	0.77	0.77	0.76	0.77	0.77
Q5	0.86	0.82	0.80	0.79	0.79	0.79	0.79
Q5 - Q1	0.18	0.07	0.04	0.03	0.01	0.01	0.01
$P > \|F\|$	0.00	0.00	0.11	0.36	0.22	0.53	0.58
N	4,887	4,887	4,887	4,887	4,887	4,887	4,887
Variables	**M1**	**M2**	**M3**	**M4**	**M5**	**M6**	**M7**
Income Quintile Dummies	$\surd$	$\surd$	$\surd$	$\surd$	$\surd$	$\surd$	$\surd$
KS2 Attainment		$\surd$	$\surd$	$\surd$	$\surd$	$\surd$	$\surd$
KS4 Attainment					$\surd$	$\surd$	$\surd$
Other Socioeconomic Chars.			$\surd$	$\surd$		$\surd$	$\surd$
KS3 School Characteristics				$\surd$			$\surd$

Notes: $P > |F|$ shows p-value for test of joint significance of income group dummies in the probit regressions used to generate the predicted probabilities. Other characteristics held constant at sample means. Adjusted using LSYPE Wave 7 respondent weights. Full regression tables for these models are reported in Appendix A, Tables A.1, A.4 and A.7, respectively.

43 percentage points or just over 2.5 times more likely to attend university than those in the bottom quintile group. Comparing this with the Apply models,[9] I find that the gap between the top and bottom quintile groups is exactly the same. It comes as no surprise then that my first Conditional Attend model[10] identifies a smaller (but significant) association between household income and university participation amongst those who applied, even with no controlling factors: those in the top quintile group are 18 percentage points more likely to get into university, conditional on having applied.

These associations are much reduced once additional covariates are controlled for. The base regression model takes no account of prior attainment, which acts both as an imperfect measure of underlying ability and as a function of socioeconomic characteristics on attainment up to that point. Once Key Stage 2 attainment is included (in M2), the attendance gap between top and bottom quintile groups falls to 22 percentage points. The relatively small association between income and attendance, conditional on having applied, becomes even smaller, with the gap between top and bottom quintile groups closing to 7 percentage points.

Further drops are seen once socioeconomic characteristics are added in M3 and the marginal effects for conditional attendance become insignificant. It is interesting to examine the other significant associations in the attendance model (reported in Table A.1 of Appendix A). There are significant marginal effects for the ethnicity dummy variables, showing higher participation rates amongst non-white groups. The sibling effect dummy variables suggest a negative association between being a younger sibling and university attendance, conditional on family size. I also identify a significant estimated negative effect of 7 percentage points for lone parent family status. Some parental education variables are also significant: father having a degree relative to holding GCSE qualifications, in particular, shows a large and significant positive marginal effect comparable to moving from the bottom to the top income quintile group.

In M4, the gap in attendance between the top and bottom income quintile groups falls to 11 percentage points. Even holding school characteristics constant and for individuals with otherwise very similar socioeconomic characteristics, a significant association between household income and university attendance is still identified.[11]

[9]See Table A.4 in Appendix A.

[10]See Table A.7 in Appendix A.

[11]This is also true if I use school fixed effects instead of school type dummies to account for school characteristics. These results may be found as M8 in Table A.1 in Appendix A

For M5, I return to controlling for just prior attainment, this time at both Key Stage 2 and Key Stage 4/GCSE. In terms of university attendance, the gap between the top and bottom income quintile groups drops to 3 percentage points, although a test of joint significance of all income quintiles suggests the association is still significant. This is in contrast to much previous research which has tended to find no significant effect of socioeconomic status is generally identified once educational attainment at the age of 16 is accounted for (e.g. Marcenaro-Gutierrez et al., 2007, p.351). These results suggest that a substantial majority of the association between household income and university attendance found in previous models is channelled via educational performance earlier in the young person's school career. Nonetheless, there remains a small, but significant, association between household income and university attendance, even after controlling for prior attainment at the age of 16. I can again use the application and conditional attendance models to show that this seems to be driven by the application decision: for conditional attendance the remaining gaps are small and not statistically significant.

The further inclusion of other socioeconomic and demographic characteristics in M6 and school characteristics in M7 do not substantially alter my conclusions, with models M5, M6 and M7 producing very similar results. This suggests that, along with income, much of the effect of these other characteristics on university participation is accounted for by its effect on GCSE attainment.

There are generally very few statistically significant coefficients in the conditional attendance models. In all models in which they are included, the coefficients on prior attainment are jointly significant.[12] In M4, in addition to prior attainment the model identifies a positive significant effect on attending either an independent or grammar school and father having education to degree level. Overall, however, the picture is of very little other than prior attainment playing a role in the probability of attendance conditional on having applied, providing little evidence that universities are discriminating on any characteristics other than how qualified the young person is to attend their institution.

The application models are interesting for perhaps the opposite reason. Despite controlling for a large number of potentially confounding variables, including school fixed effects, they continue to provide estimates of a statistically significant association between household equivalised income and applying to university. Even holding very many

[12]Prior attainment is modelled using a quadratic and/or piecewise linear function. Although individual coefficients may not be significant a Wald test of joint significance always rejects the null hypothesis of no association.

other characteristics constant young people from richer backgrounds remain more likely to submit an application to go to university, although the size of the gap between the top and bottom quintile groups has reduced significantly from 43 percentage points to 7 percentage points.

The finding of small and often insignificant gradients for household income conditional on having applied is reassuring, on the assumption that otherwise similar individuals should not be advantaged or disadvantaged in the admissions process by their household income. However, a key question is left unanswered. Although individuals with different household incomes seem to stand a similar chance of getting into university, so long as they apply, do they get into similar universities? The next section provides some insight into this important issue.

2.5 Comparison between Russell Group and others

This section considers the relationship between equivalised household income and attendance at a Russell Group institution. Since they are a 'high status' group, one might expect the determinants of attending a Russell Group university to be different from the determinants of attending university in general. In particular, it is possible that although I saw only small associations between income and achieving a place at university overall (after conditioning on prior attainment up to age 16), those with high levels of income could be disproportionately attending high quality institutions. This matters for social mobility because, as noted in Chapter 1.4, university quality affects the returns that can be achieved in the labour market: if only students from richer families go to high status universities, then their advantage will be propagated.

Figure 2.4 shows the 'raw' association between household income and attendance at a Russell Group university. Since this encompasses the socioeconomic gradient in both attending university and getting a place at a Russell Group institution it is unsurprising (given my main analysis showed the existence of the former) that I see a household income gradient here too. Individuals whose household equivalised income is at the top quintile are approximately 10 percentage points more likely to attend than those at the bottom quintile.

Figure 2.5 shows the same association amongst those who go to any university. The upward slope across much of the income range shows that, amongst participants, individuals from households with higher incomes are more likely to attend a Russell Group institution.

I see this confirmed in specification M1 of Table 2.5, which presents the unconditional effect of household income quintile on attendance at a Russell Group institution, and shows that those in the top quintile group are 20 percentage points more likely to go to a Russell Group university than those in the bottom quintile group.[13] There is also the same gap amongst those who go to any university.[14]

Table 2.5: Predicted probabilities of attendance at Russell Group universities by income quintile group

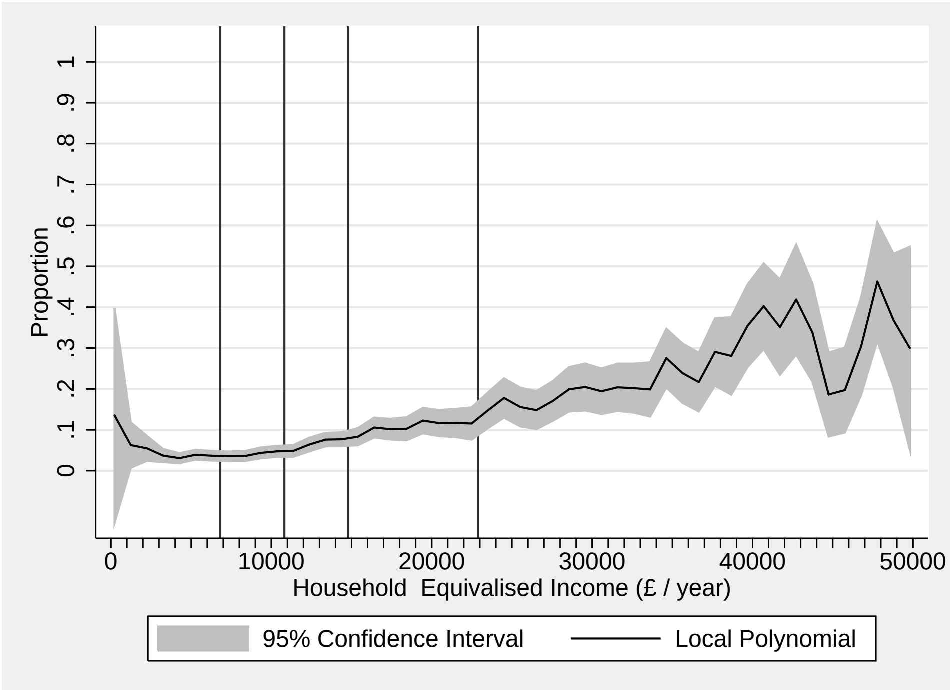
Russell Group attend	**M1**	**M2**	**M3**	**M4**	**M5**	**M6**	**M7**
Q1	0.04	0.06	0.08	0.09	0.09	0.10	0.10
Q2	0.04	0.05	0.07	0.07	0.08	0.08	0.08
Q3	0.06	0.08	0.09	0.09	0.09	0.09	0.09
Q4	0.10	0.09	0.09	0.09	0.09	0.09	0.09
Q5	0.24	0.16	0.13	0.12	0.12	0.11	0.11
Q5 - Q1	0.20	0.10	0.05	0.03	0.02	0.02	0.01
$P > \|F\|$	0.00	0.00	0.00	0.00	0.00	0.02	0.04
N	7,927	7,927	7,927	7,927	7,927	7,927	7,927
Russell Group, conditional on uni.	**M1**	**M2**	**M3**	**M4**	**M5**	**M6**	**M7**
Q1	0.16	0.21	0.24	0.25	0.24	0.25	0.25
Q2	0.15	0.18	0.20	0.21	0.21	0.22	0.22
Q3	0.19	0.21	0.23	0.24	0.23	0.24	0.24
Q4	0.23	0.23	0.24	0.24	0.23	0.24	0.24
Q5	0.36	0.32	0.29	0.28	0.28	0.27	0.27
Q5 - Q1	0.20	0.11	0.04	0.03	0.05	0.03	0.02
$P > \|F\|$	0.00	0.00	0.05	0.22	0.02	0.21	0.29
N	3,856	3,856	3,856	3,856	3,856	3,856	3,856
Variables	**M1**	**M2**	**M3**	**M4**	**M5**	**M6**	**M7**
Income Quintile Dummies	$\surd$	$\surd$	$\surd$	$\surd$	$\surd$	$\surd$	$\surd$
KS2 Attainment		$\surd$	$\surd$	$\surd$	$\surd$	$\surd$	$\surd$
KS4 Attainment					$\surd$	$\surd$	$\surd$
Other Socioeconomic Chars.			$\surd$	$\surd$		$\surd$	$\surd$
KS3 School Characteristics				$\surd$			$\surd$

Notes: $P > |F|$ shows p-value for test of joint significance of income group dummies in the probit regressions used to generate the predicted probabilities. Other characteristics held constant at sample means. Adjusted using LSYPE Wave 7 respondent weights. Full regression tables for these models are reported in Appendix A, Tables A.10, and A.11, respectively.

However, once I control for other factors this gap becomes much smaller and, in the case of going to a Russell Group university conditional on attending an university, becomes statistically insignificant in M4, M6, and M7. As one would expect, 'ability' measured

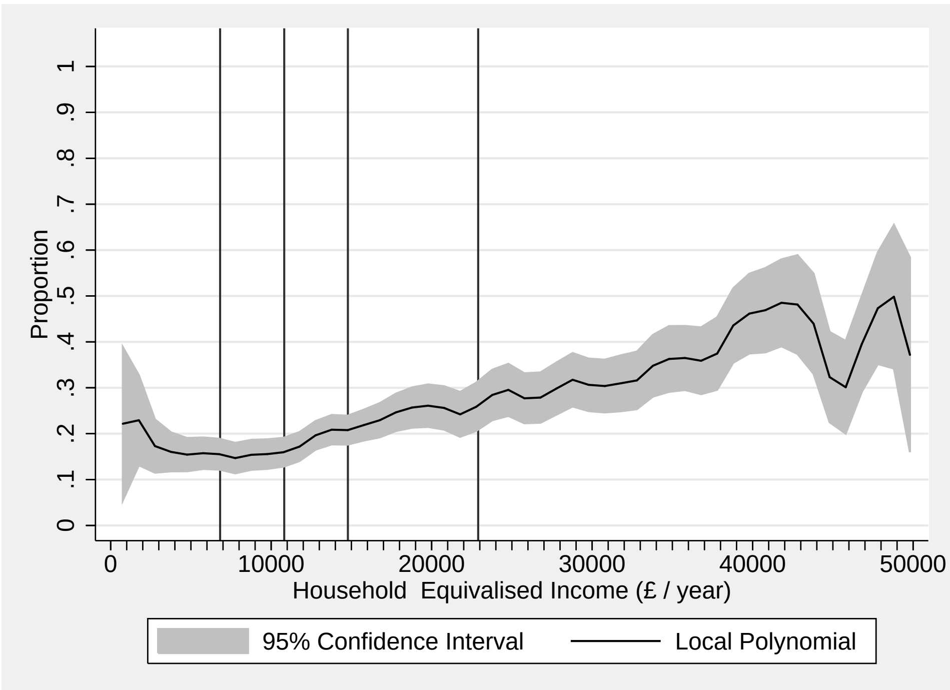
[13]For full results from this model see Table A.10 in Appendix A.
[14]For full results from this model see Table A.11 in Appendix A.

Figure 2.4: Russell Group university attendance at age 18-19 or 19-20 and household equivalised income



Notes: Weighted using LSYPE Wave 7 Participant Weights. Local polynomial smoothing using Epanechnikov kernel and Silverman's optimal bandwidth of 1577.335. Sample size: 7780. Vertical lines show 20th, 40th, 60th, and 80th percentiles of equivalised household income.

Figure 2.5: Russell Group university attendance, conditional on attending any university, and household equivalised income



Notes: Weighted using LSYPE Wave 7 Participant Weights. Local polynomial smoothing using Epanechnikov kernel and Silverman's optimal bandwidth of 2485.145. Sample size: 3771. Vertical lines show 20th, 40th, 60th and 80th percentiles of household equivalised income.

by prior attainment at age 11 (M2) explains a good proportion, but not all. However, it is when the other socioeconomic status characteristics are added in M3 that the gap narrows most strikingly. In these models, parental education to degree level is strongly positively associated with the likelihood of attending a Russell Group institution, perhaps suggesting that a parental familiarity with the university system is important in encouraging young people to apply to a Russell Group institution.

These findings suggest that the Russell Group attendance gap, conditional on attending university, is explicable by parental education and prior attainment at age 16. These results do not suggest that Russell Group universities discriminate against poorer applicants, but rather that pupils from poorer backgrounds either have lower attainment or choose not to apply for some other reason.

2.6 Conclusions

In this chapter, I have investigated the relationship between permanent household equivalised income and university applications and attendance for a recent cohort of young people in England. My research has gone beyond previous work in this area in several important respects. First, I have quantified the relationship between permanent household income and university attendance for a recent cohort of students in England. My results suggest that those in the top fifth of the income distribution are almost three times as likely to attend university as those in the bottom fifth. This relationship is reduced dramatically, but does remain statistically significant, once I control for a range of other confounding factors, including some that seem likely to lead to an underestimate of the direct effect of income on university participation decisions.

Second, by analysing the probability of application and the probability of attendance conditional on having applied separately, I demonstrate that the link is predominantly driven by the application decision. Even after controlling for prior attainment and socioeconomic background a significant application gap remains. On the contrary, I identify a relatively smaller household income gradient for attendance conditional on having applied and show that, conditional on having applied, those in the top fifth of the income distribution are approximately 1.3 times more likely to attend than those in the bottom fifth. Moreover, this difference disappears rapidly once controls for earlier educational attainment are added to the model.

Finally, I analysed attendance at Russell Group universities, a group of prestigious 'high quality' institutions. The gradient in attendance at a Russell Group university, conditional on attending any university, closes completely once prior attainment and other socio-economic characteristics have been controlled for. However, without better data on the institution choices of university applicants, it is impossible to analyse fully this Russell Group admissions process. Nonetheless, I have been able to provide more detailed evidence than has hitherto been possible on the relationship between household income and participation at high status universities in the UK.

A key finding of this chapter is that the university participation gap largely emerges at or before young people apply. This shows that narrowing the gap through policy intervention at the point of admissions will be very difficult. Such policies could only have a significant effect if they led to a change in the desire to go to university or perceptions of the university application process, in turn leading to a broader application population. Nevertheless, I analyse the implications for one such policy, introduced to an 'elite' university, in Chapter 4.

More likely to be successful are policies that intervene earlier to ensure that those from poorer backgrounds reach their potential during their academic career and hence are more likely to acquire the appropriate qualifications to apply to university. I now turn to this matter in more depth, analysing changes in young people's expectations of applying to university during their teenage years as a way of better understanding the pre-application relationship between socioeconomic status and the decision to apply to university.

Chapter 3

The influence of socio-economic status on changes to young people's expectations of applying to university

3.1 Introduction

In Chapter 2, I found a large socio-economic gradient in university application in England. Much of this inequality can be explained by differences in academic achievement that emerge long before the point at which young people apply to university (see also Chowdry et al., 2013). However, even conditioning on these earlier academic outcomes and other potential confounding factors, a socio-economic gradient in whether or not individuals make an application to university remains. This is despite the fact that a larger proportion of English 14-year-olds from disadvantaged backgrounds expect to apply to university than the overall proportion who have ultimately done so by age 21 (Anders and Micklewright, 2013, pp.42-43).

This raises the question of when and why young people from less advantaged families change their minds about making an application to university. Are their changes in expectations explicable by other factors, such as academic attainment, or does socio-economic status continue to have an influence? Given the previous evidence that much of the socio-economic gap in university attendance opens at or before the point of application, a better understanding of the dynamics of whether or not individuals expect to apply is of significant importance to the formulation of policy on reducing the socio-economic gradient in access to Higher Education.

Rather than following previous authors in using expectations data as an explanatory factor for later outcomes, in this chapter I take a step back, addressing the issue directly by analysing the influence of socio-economic status on the large number of changes in young people's expectations of applying to university between ages 14 and 17, just before young people start making applications to university. Using rich panel data from the Longitudinal Study of Young People in England (LSYPE), I take the novel approach of using duration modelling to analyse the dynamics of young people's expectations.

The research question and data used lend themselves naturally to this approach. Duration modelling allows the flexibility to make use of all available information on the timing of events (including the possibility of multiple transitions back and forth between reporting 'likely' and 'unlikely' by an individual), it can take account of changes in young people's circumstances during the period under consideration, and allows for more flexible handling of some missing outcomes data. The technique also allows separate analysis of both transitions from being 'likely to apply' to being 'unlikely to apply' and vice versa. This is important, since the factors which cause young people to raise their expectations and start thinking that they are likely to apply to university may be quite different from the causes of movement in the other direction. Despite this, duration modelling is not regularly used in such settings and, to my knowledge, has not been used before to model changes in young people's educational expectations over time.

This chapter makes an important contribution to the literature on access to Higher Education. Using the longitudinal nature of the data, I provide non-parametric estimates of changes in young people's expectations between the ages of 14 and 17, quantifying the extent of changes in expectations during this period. Making minimal assumptions, I also use this technique to examine whether young people from less advantaged backgrounds are more likely to stop, and less likely to start, thinking they are likely to apply to university than their more advantaged peers. Furthermore, taking advantage of the rich survey data and retaining the flexibility of duration modelling, I provide estimates of the continued influence of socio-economic status, after controlling for potentially confounding factors including prior academic attainment and demographic characteristics. Finally, I explore the interplay between SES and new information on academic attainment at age 16.

The chapter proceeds as follows. Section 3.2 reviews the literature on the socio-economic patterning of educational expectations and lays out a modelling strategy for identifying the influence of socio-economic status on changes in expectations. Section 3.3 describes the dataset and measures used in this chapter. Section 3.4 introduces duration modelling as applicable to these data and sets out the benefits of using it to analyse changes in expectations. Non-parametric duration modelling methods are applied in Section 3.5 to explore how young people's expectations change during their teenage years and how this is associated with socio-economic status. This initial analysis is extended through use of multiple regression models, introduced in Section 3.6 and with the results of this analysis reported in Section 3.7. Finally, Section 3.8 concludes.

3.2 Background and identification strategy

This chapter, rather than attempting to identify the effect of young people's expectations on university attendance, takes a step back. It explores the role of socio-economic status (SES) in determining the paths of young people's expectations in the first place. The importance of young people's expectations, particularly in explaining the SES gradient in academic attainment, has increasingly attracted academic interest over the past few years. This has been accompanied by policy makers emphasising the need to 'raise aspirations', particularly among high attaining, but low SES, young people.[1] Such policies, in the UK, have included the now-defunct 'Aimhigher' programme and requirements for outreach work by universities charging more than £6,000 in tuition fees in their Access Agreements with the Office For Fair Access (OFFA).

It is important to distinguish upfront between young people's expectations and their aspirations. Jerrim (2011, p.6-7) summarises the difference between the two as being that expectations "implies a realistic assessment of future outcomes, while [aspirations] reflects children's hopes and dreams". For this chapter's application, young people might hope to apply to university (an aspiration), without expecting that they will be in a realistic position to do so. Although much of the policy discourse focuses on 'raising aspirations' rather than 'raising expectations', expectations seem more likely to be informative for

[1] A DfE-funded study reflecting this concern found that most schools it surveyed indicated that "encouraging their students to apply to higher education [...] was one of their highest priorities" (Thornton et al., 2014, p.146).

the purposes of this chapter, but understanding both aspirations and expectations pose many of the same challenges.

Regardless of the interest of policymakers, studying expectations is not worthwhile if they are just an individual's whim. However, Morgan (1998) argues that "educational expectations are not 'flights of fancy' or 'vague preferences' [but rather,] because they can be explained by a reasonable theory of rational behavior, should be considered rational" (Morgan, 1998, p.157) and hence, presumably, informative. Certainly, previous work has shown a correlation between educational expectations and later outcomes. Chowdry et al. (2011) find a correlation between young people thinking it likely that they will apply to university and academic performance at age 16, even after controlling for long-run family background factors and prior attainment. Elsewhere in the world, analysis of the Longitudinal Survey of Australian Youth estimates that the "correlation between intention and entry to higher education is moderately strong (r = 0.59)" (Khoo and Ainley, 2005, p.v). Similarly, in the US, Reynolds and Pemberton (2001) report that while 29% of those who expect to complete a college degree when asked in 1979 (age 15-16) had done so by 1994 (aged 30-31), under 3% of those who did not expect to complete a college degree had done so (Reynolds and Pemberton, 2001, p.723).

Using data from the Programme of International Student Achievement (PISA) survey, Jerrim (2011) examined the socio-economic patterning of young people's expectations of completing Higher Education. He finds that that there are large differences between advantaged and disadvantaged children's expectations in most countries throughout the developed world. He finds that England is no exception to this pattern, with only a handful of OECD countries having significant differences (on either side) in the strength of the relationship. By contrast, the correlation between socio-economic advantage and expectations is significantly weaker in the US than most other OECD countries, including England.

Why do these associations between expectations and outcomes exist? One potential explanation is that young people who grow up in more deprived households "may expect less of themselves and may not fully develop their academic potential because they see little hope of ever being able to complete college or using their schooling in any effective way" (Cameron and Heckman, 1999, p.86). However, others, such as Gorard (2012), are highly critical of the jump from these plausible explanations and observed correlations between attitudes and academic outcomes to seeing the relationship as playing a truly causal role. Gorard argues that formulating policy on this basis, when evidence of causation is so weak, is misguided because of the opportunity costs and potential negative side effects of policies aimed at raising aspirations and expectations.

Given this chapter's focus on the influence of SES on the pathways of young people's expectations, expectations data are used as an outcome variable. Doing so means taking a step back from its use as an explanatory variable, as was the case in the studies above. The focus on expectations as an outcome variable means that there is no need to take a view on whether or not expectations have a causal impact on academic attainment and progression. Instead, it is enough to be convinced that young people's expectations are at least symptomatic of the underlying social processes leading from SES, prior attainment, and other background characteristics to the ultimate decision as to whether or not to apply to university.

This chapter contributes to a literature on the formation and correlates of young people's educational expectations and aspirations. Previous work has considered similar issues in differing contexts or applying differing methods. However, this is the first analysis to consider a dynamic relationship between SES and young people's expectations. Rampino and Taylor (2013) analyse young people's educational aspirations using data from the British Household Panel Study (BHPS), focusing in particular on differences by gender, using responses to questions such as "Would you like to go on to do further full-time education at a college or University after you finish school?".[2] They do not consider changes in aspirations, but do take advantage of the panel nature of the data, estimating probit models with individual-level random effects. Fumagalli (2012) also estimates binary choice models of young people's expectations of getting a place at university (with adjustment for selection effects in who is asked the question of interest) using the same dataset as that which I use. Perhaps the paper closest in aims to this chapter is Kao and Tienda (1998): using data from the US, they estimate logistic regression models of the association between young people's background characteristics and changes in educational aspirations (including an aspirations variable lagged by one time period as a covariate).

These previous studies have all found a role for socio-economic status. Kao and Tienda find that socio-economic background "exerts a strong influence on educational aspirations and is vital to their maintenance through the high school years" (Kao and Tienda,

[2]The BHPS lacks data on young people's prior academic attainment, which is available in the dataset used in this chapter, and which would be strongly expected to be relevant to educational expectations.

1998, p.370). Rampino and Taylor report that "the educational aspirations of boys are more positively affected by parental education than those of girls" (Rampino and Taylor, 2013, p.34), also noting that the effect of parental attitudes varies by gender in the same way. Fumagalli finds that young people from families with higher parental education are more responsive to new information about their academic attainment in updating their expectations of both applying to university and ultimately getting a place. In addition, she finds that, contrary to popular belief, "young people from free school meal eligible families have more positive expectations [of being accepted to university, conditional on having applied], even when grades are controlled for" (Fumagalli, 2012, p.41-42).

This chapter builds on the previous literature in two important respects. First, through use of duration modelling, this chapter analyses the dynamic relationship between SES and young people's expectations in a flexible way. Importantly, it allows for different relationships between characteristics of interest and whether young people make a transition depending on direction of the transition (i.e., 'likely to unlikely' or 'unlikely to likely'). Second, both Kao and Tienda and Rampino and Taylor focus on aspirations rather than expectations, while Fumagalli analyses formation of young people's expectations of being admitted to university, conditional on having made an application.[3] Here, the focus is on expectations of applying to university, which is distinct from any of these.

To analyse the influence of SES on the likelihood of changes in young people's expectations, one must first have some idea of the relationship between the two. Drawing on others' findings about the determinants of expectations (for example Kao and Tienda, 1998; Fumagalli, 2012; Anders and Micklewright, 2013; Rampino and Taylor, 2013) I treat the probability of transition as a function of SES and various other characteristics:

$$Pr(\Delta\text{Expectations}) = f(\text{SES}, X) \tag{3.1}$$

where X is a vector of characteristics including young people's age, academic ability, demographic characteristics, school characteristics, traumatic experiences, and local labour market conditions.

The strategy is to isolate the role of SES by controlling for elements of X. However, there

[3]As the question on likelihood of admission, conditional on application, is only asked to individuals who indicate that they are more than 'not at all likely' to apply, Fumagalli does estimate models of likelihood of applying (focusing on the probability of being at least 'not very likely' to apply) to deal with this selection problem.

are several challenges to achieving this. Several of these are discussed in Section 3.3.4 below, where the measurement of these variables in the dataset is considered. Most fundamentally, one cannot be sure that other unobserved or unobservable elements do not also appear in the function. In the absence of exogenous variation in SES (which is conceptually, let alone practically, challenging) one cannot be certain that this problem has been dealt with. However, an alternative strategy, making use of random effects (modelled either as having a normal distribution or a discrete mixing distribution), to help deal with unobserved heterogeneity is discussed and applied in Appendix B. The results obtained when I apply this method do not substantively alter the findings from this analysis in this chapter, giving me some confidence in the qualitative story from my estimates.

3.3 Data

The Longitudinal Study of Young People in England (LSYPE) is a major panel survey, funded to age 20 by the UK Department of Education. The LSYPE tracks the experiences of one cohort of young people over seven years (with one interview per year), from approximately age 14 (in 2004) to age 20 (in 2010),[4] including interviews with the young people themselves (throughout) and their parents (up to age 17). It collected a wide variety of data on participants, including details on their socio-economic background, educational attainment, and educational expectations. Only aspects of the LSYPE relevant to the research questions of this chapter are discussed here; more in depth description of the LSYPE was provided in Chapter 2.3 and is also available in Anders (2012b).

As with any longitudinal survey, the LSYPE suffers from attrition. One of the advantages of duration modelling is the option of treating missing outcome data as 'censored' (discussed further in Section 3.4). This is preferable to having to drop respondents that attrit from from the analysis, as was necessary in Chapter 2, which would mean being restricted to a complete case sample of 8,029.[5] Individuals who are not present in both Waves 1 and 2 are excluded, to ensure that at least one potential transition is observed for all

[4]Further waves following the young people as they enter the labour market are now planned, funded by the Economic and Social Research Council. For more information visit `http://www.cls.ioe.ac.uk/lsype`.

[5]This complete case sample is used (applying appropriate attrition weights) in Figure 3.1 and as a robustness check, reported in Appendix B.

individuals included the analysis. The number of participants at Wave 2 is 13,447 out of the 15,770 who initially responded at Wave 1 (i.e. an 85% response rate). However, missing data for key variables reduce the sample size in the analyses to those reported in the results tables. I weight the data for my analysis using the LSYPE-provided attrition and non-response weights for Wave 2.

This section discusses four main aspects of the data. First, the measurement of the outcome variable (young people's expectations of applying to university), including specifics of measurement in this dataset and more general challenges posed by use of expectations data as an outcome in duration modelling. Second, the sequences of expectations observed in the data. Third, the measurement of the main explanatory variable of interest (young people's SES), including construction of an index of SES from various indicators. Finally, the measurement of other characteristics that may confound the relationship between SES and changes in expectations.

3.3.1 Measurement of expectations

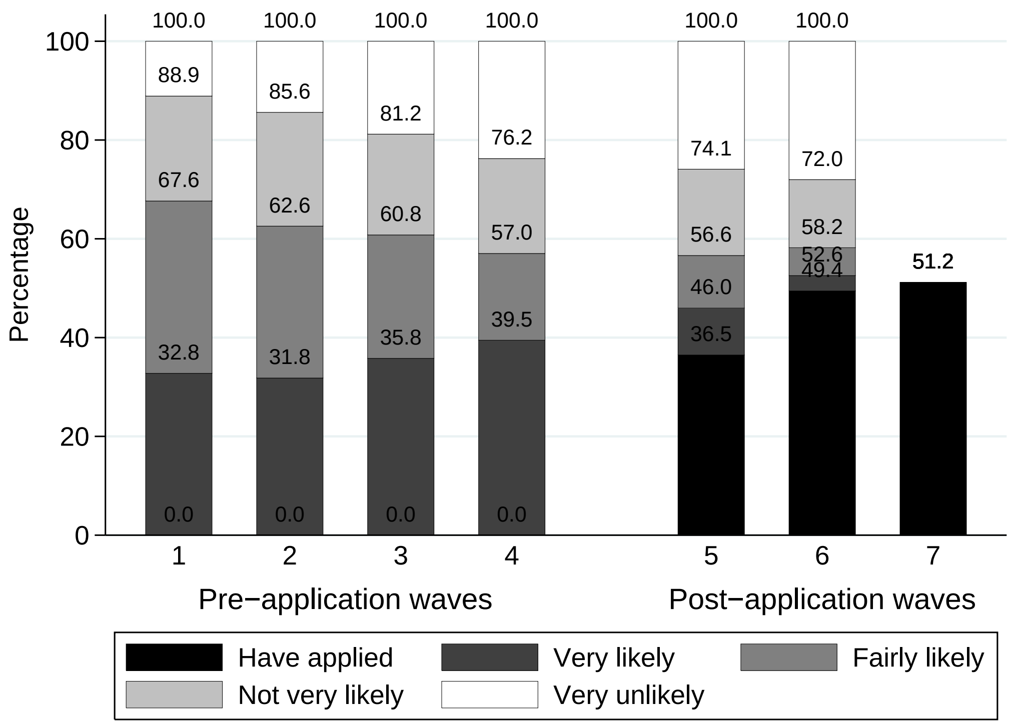
The LSYPE begins recording young people's expectations of applying to university from approximately age 14. Conveniently, given that this is the earliest point in the data, previous psychological and sociological literature has argued that this is also the age at which young people "relinquish their most preferred [occupational] choices and settle for more acceptable, available, choices" (Gutman and Akerman, 2008, p.5). Similarly, Gottfredson (2002, p.98-101) argues that by the age of 14, young people have completed 'circumscription' of their aspirations, whereby they rule out unacceptable career aspirations, and begin 'compromise' by "adjusting their aspirations to accommodate an external reality" (Gottfredson, 2002, p.100). It follows that age 14 is a natural point from which to analyse young people's expectations in a meaningful way; as such, I treat young people's periods of reporting their expectations as starting at this point at the earliest.

The LSYPE measures young people's expectations of applying to university through a single question repeated in most of the waves of the survey. Young people are asked "How likely do you think it is that you will apply to university?" and are asked to choose from the options 'very likely', 'fairly likely', 'not very likely',[6] and 'not at all likely'.

[6]In colloquial English, the expression 'not very likely' means 'fairly unlikely', rather than its more literal interpretation of anything less than 'very likely'.

To get an initial impression of the evolution of young people's expectations during this period, Figure 3.1 shows for each wave, 1 to 7, the percentages of young people who report being 'very likely', 'fairly likely', 'not very likely' and 'not at all likely' to apply to university.[7] For the purposes of this graph, only individuals with expectations data throughout the survey are included (i.e. a balanced panel or complete case sample). However, as discussed above, this restriction is relaxed after this point. From Wave 5 onwards it is necessary to include an additional category for those who have actually applied. In Wave 7, only a measure of having actually applied to university by this point is reliably available. The overall percentage who are 'likely' (or who have already applied in later waves) can be seen by following the cumulative percentage above the 'fairly likely' blocks in Figure 3.1.

Figure 3.1: Young people's expectations of university application, Wave 1 (age 13-14) to Wave 7 (age 19-20)



Notes: Sample: Wave 7 respondents with non-missing data on university expectations and university application at each wave (complete case analysis). 'Don't know' (4.4% of weighted Wave 1 respondents) treated as 'not very likely'. Wave 7 attrition and non-response weights applied. Unweighted sample size = 8,029. Data labels show cumulative percentages.

Overall, the proportion reporting that they are 'likely' to apply to university declines substantially from 68% in Wave 1 to 57% in Wave 4, at the end of the first year following

[7]Individuals may also respond that they 'don't know' whether they are likely to apply to university; however, this is not a common response (4.4% of weighted Wave 1 respondents) and I choose to classify those who report 'don't know' as being 'not very likely' to apply to university.

GCSEs. There is essentially no change in Wave 5, when actual applications begin to be included (treated, for this purpose, as 'likely' to apply, given that they are effectively 'certain' to apply), before a small rise in Wave 6 when the study members would be completing any Further Education (two years of post-compulsory education). There is no reliable question on expectations of application to university in Wave 7, only a report of whether individuals have already applied. However, individuals will continue to enter university over the subsequent few years (or even later as mature students) (UCAS, 2012). It is therefore probable that a small percentage of the sample would have responded that they were likely to expect to apply to university if they had been asked in Wave 7.

In any case, as the aim of this chapter is to understand changes in young people's expectations in the period leading up to making an application, the analysis in this chapter is deliberately curtailed at the last wave in which individuals have not yet started applying to university (Wave 4, or roughly age 17). Analysing the period in which individuals apply to university would introduce bias from non-random movement of individuals out of the sample, caused by having actually made an application. I discuss this, along with other kinds of 'right-censoring' in Section 3.4.

For the analysis in this chapter, I dichotomise the expectations variable into a distinction between young people who are 'likely' ('very likely' or 'fairly likely') or 'unlikely' ('not very likely' or 'not at all likely') to apply to university.[8] Assuming that young people are utility maximising (and that they give honest responses), they will report that they think it is likely that they will apply to university if they judge that the benefits they will derive from making an application exceed the costs they will experience as a result of doing so. They switch to thinking that it is unlikely that they will apply if their assessment of these costs and benefits changes to the point that the balance has shifted in the other direction. Many of the factors that will influence these decisions are not observed. However, I use those that are observed to assess which factors seem important in altering young people's perceptions of their potential to gain from higher education.

One problem with analysing expectations, rather than observed behaviour, is that 'talk is cheap'. This is an analysis of individual's stated preferences, rather than the revealed pref-

[8]Anders and Micklewright (2013) analyse the trends of those who report being 'very likely' to apply to university, finding that, unlike the overall proportion who report being 'likely', this in fact rises over time. This appears to be driven by a tendency for individuals' expectations to 'harden' over time, with those who report being 'fairly likely' tending towards reporting 'very likely', while those who report being 'not very likely' tend towards reporting 'not at all likely'.

erences indicated by their actions i.e. actually making an application to university. Cognitive biases, such as social desirability bias, may affect the responses. However, young people's reported expectations do seem informative as to the application behaviour observed in later waves of the LSYPE. 64% of those who say they think it is likely ('very' or 'fairly') that they will apply to university at age 14 have done so by the last point of observation (and more may do so at a later date), while only 22% of those who say they think it is unlikely have done so by the same time.

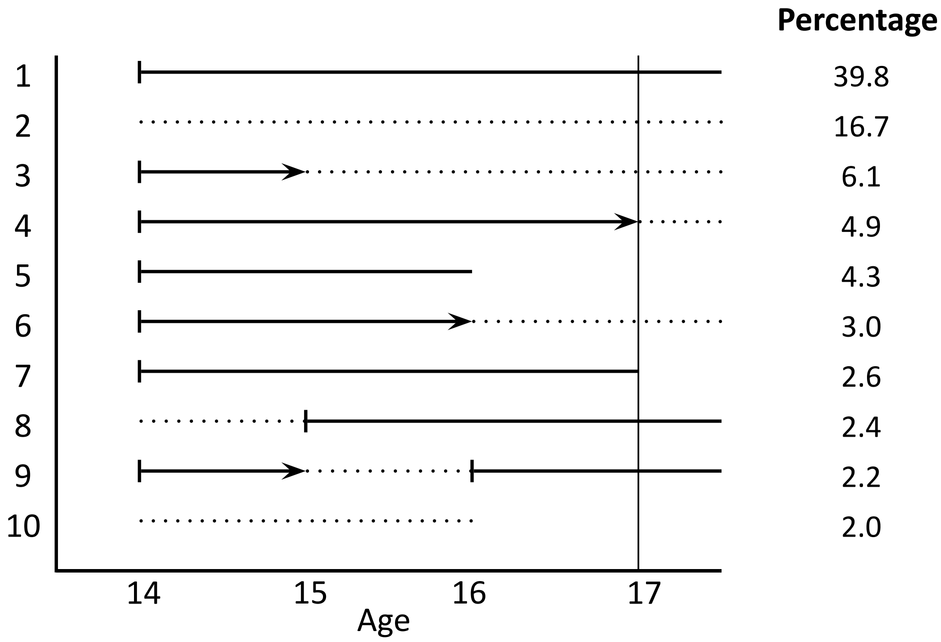
Use of a stated preference measure as an outcome variable in duration modelling in this way is innovative,[9] but raises some issues. The method is more normally employed to analyse transitions between clearly definable states, such as movement between employment and unemployment. Individuals' evaluation of their probability of applying to university will be subject to far more measurement error than transitions between such states. For example, factors such as an individual's bad mood on the day of the interview could tip them from reporting 'fairly likely' to reporting 'not very likely', if their general assessment of the costs and benefits of applying to university are finely balanced. Unlike in a standard binary regression model this does not just cause dependent variable measurement error. Since the sample for duration models depends on the reported expectation of application in the previous period, measurement error could also affect this. This will bias overall transition rates upwards, and may also affect estimated coefficients if groups are differentially affected by measurement error.

3.3.2 Sequences of expectations

To illustrate the form of data used in duration analysis, in Figure 3.2 I present the ten most common sequences of individuals' expectations between ages 14 and 17 observed in the dataset, which account for around 85% of the sample. Solid lines represent periods when the individual reports being likely to apply to university; dotted lines represent periods when individuals report being unlikely to apply to university; the absence of any line indicates missing data (including due to item non-response, unit non-response and attrition) at this time point. I have chosen to highlight the start and end of periods of being 'likely to apply': a vertical tail to the line represents the point at which the spell is observed to begin; and an arrowhead represents the point at which the spell is observed to end in a

[9]Some precedent is provided by studies of the dynamics of poverty (Bane and Ellwood, 1986, for example) where measurement of income may affect movement in or out of poverty.

Figure 3.2: Ten most common sequences of individuals' expectations from age 14 to 17 and the percentage of the total sample with each sequence



Notes: A solid line indicates that the individual reported they were 'very likely' or 'fairly likely' to apply to university at the most recent wave. A dotted line indicates that the individual reported that they were 'not very likely' or 'not at all likely' to apply to university at the most recent wave. The absence of a line indicates that there was no report from the individual at the most recent wave. An arrow tail at the start of a spell highlights that in the previous wave the negative outcome was observed. An arrow head at the end of a spell highlights that in the following wave a negative outcome was observed. The vertical line at age 17 highlights that this is the final point of observation and hence data beyond this point only provide information on whether the spell was censored (whether by no change or missing data) at this point. Calculation of frequency of spell types was weighted using LSYPE Wave 2 attrition and non-response weights. Individuals with missing data in either of Waves 1 or 2 are excluded. Percentages based on total sample size of 11,249.

transition to the person reporting that they are 'unlikely to apply' to university.[10]

After exclusions, there are a theoretical maximum of 35 possible sequences of expectations during this period, all of which are observed in the data. The most frequent sequence of expectations (40% of the sample) is for individuals to report being 'likely to apply' at every interview from age 14 to age 17. The second most frequent (17% of the sample) is reporting being 'unlikely to apply' at every interview from age 14 to age 17.

Table 3.1: Summary statistics about sequences of expectations

Group	N	Percentage	SES Index
1	4,503	40.2	0.45
2	1,857	16.6	-0.49
3	673	6.0	-0.35
4	547	4.9	-0.07
5	478	4.3	-0.23
6	342	3.1	0.04
7	279	2.5	-0.04
8	269	2.4	-0.53
9	249	2.2	0.05
10	225	2.0	-0.27
Other	1,828	15.9	-0.30
All	11,249	100	0.00

Notes: Adjusted using LSYPE-provided Wave 2 survey design, attrition and non-response weights. Individuals with missing data in either of Waves 1 or 2 are excluded.

To provide context to these records, in Table 3.1 I provide summary statistics about individuals who have the sequences of spells in Figure 3.2. I also include a category for all remaining groups, which makes up about 16% of the sample and is somewhat less advantaged than the average individual. The SES index (discussed further in Section 3.3.3) is standardised such that the sample mean is 0 and the standard deviation is 1. Individuals who always report being likely to apply to university (type 1) are, on average, half a standard deviation more advantaged than the sample as a whole. Conversely, those who always report being unlikely to apply (type 2) are roughly the same amount less advantaged than the sample as a whole.

Another important feature of the data is that, although an individual's changes in expectations seem more likely to be a continuous underlying process, I only observe their reported expectations in surveys once a year. This is, therefore, 'discrete time', as op-

[10] I could just as easily have highlighted the start and end points of periods of being 'unlikely to apply', but could not do both without loss of clarity.

posed to 'continuous time', data. This is illustrated in Figure 3.2: spells only start or end at exact ages, never somewhere in between. It follows that the models in this chapter estimate the probability of transition between these observation times, rather than at any arbitrary time point. A further limitation of discrete time data is that some transitions back and forth between the observation points are hidden, which may bias overall transition rates downwards. The issues arising from use of discrete time data in duration modelling are discussed further in Section 3.4.

3.3.3 Measurement of SES

The LSYPE includes a rich set of data on participants' characteristics. These will be important in measuring young people's socio-economic status (SES) well, in order to assess its association with changes in their expectations of applying to university. Household income, parental education, and parental occupational status are all important in measuring SES (Hauser, 1994). The rich data will also be important in controlling for other factors correlated with SES, but which seem likely to make an important contribution in their own right, such as demographic characteristics, school characteristics, local area, and prior academic attainment. I return to these in the following section (Section 3.3.4).

Household income is measured at each wave between 1 and 4. As the method used to collect information on income varies somewhat from wave to wave and previous research has suggested 'permanent' income (rather than transitory income) has a much larger effect on young people's educational outcomes (Jenkins and Schluter, 2002, p.2), I construct an approximation of the household's 'permanent' income by averaging across the four measures. I also equivalise my income measure by dividing it by the square root of household size, thus recognising the importance of family resources being stretched further in larger households. As discussed in Chapter 2, household income is underestimated to some extent in the LSYPE, relative to other social surveys where it is a major focus.

Parental education seems likely to play a role in the formation of young people's educational expectations (Ganzach, 2000), not least because young people whose parents went to university are more likely to see it as a natural next step in their education. Indeed, Table 3.3 shows that, at least based on the initial report of expectations at age 14, more of the young people who report that they are 'likely to apply' to university have at least one parent who themselves received higher education than young people who report that they are 'unlikely to apply'. Data on parental education is collected from both parents (where available) at each wave between 1 and 4 using the same questions; where both parents' education level are recorded and these differ I use the highest. Unsurprisingly, there is very little change over time, since most parents have already completed the highest educational level they will achieve by this stage of their lives.

Parents' occupational status is recorded in the LSYPE using the National Statistics Socio-Economic Classification (NS-SEC), which was designed to capture social class differences between the different occupational types (Rose and Pevalin, 2001). It is based on questions about job title, role and responsibilities asked of both parents (where available) at each wave between 1 and 4. As with parental education, where both parents' occupational status are recorded I use the highest, and, also as with parental education, there is little change in this variable over the period of analysis. I collapse the classification into four ordinal groups[11]: managerial and professional occupations; intermediate occupations; routine and manual occupations; and long-term unemployed.[12] Social class is seen by sociologists as a key element of an individual's SES, as "the experience of individuals in terms of economic security, stability and prospects will typically differ with the class positions that they hold" (Goldthorpe and McKnight, 2004). Particularly relating to the purposes of this chapter, sociological theory suggests that "young people (and their families) have, as their major educational goal, the acquisition of a level of education that will allow them to attain a class position at least as good as that of their family of origin" (Breen and Yaish, 2006, p.232). This implies that individuals from different class backgrounds will have, on average, different educational expectations.

I combine the above measures of household equivalised 'permanent' income, highest parental education, and highest parental occupational status into a single index of SES.[13] This provides a broader measure of family circumstances that any one measure would provide. I use principal components analysis with a polychoric correlation matrix (Olsson, 1979; Kolenikov and Angeles, 2009) to construct a single index, which explains roughly

[11]Some sociologists are critical of attempts to express social class in ordinal terms, most particularly in how self-employed individuals should fit into such a hierarchy (Rose et al., 2005).

[12]Individuals experiencing short-term unemployment at the time of interview are allocated a group based on their most recent job.

[13]All measures from age 14 (except income, which is averaged over available observations between age 14-17), except where not available due to item non-response at age 14, when data from later in the survey was used.

three quarters of the variation in the three individual measures.[14] I divide individuals into quintile groups on the basis of this SES index; Table 3.2 reports the family characteristics of the median individual in each quintile group, demonstrating increasing SES across all three dimensions, as would be expected.

Table 3.2: Median family characteristics by quintile group of socioeconomic status index

Quintile group	Q1	Q2	Q3	Q4	Q5
Parental Education	< A*-C GCSE	A*-C GCSE	A Level	HE < Degree	Degree
Occupational Status	Routine occupations	Routine occupations	Intermediate occupations	Higher occupations	Higher occupations
Family Income (£p.a.)	5,699	9,549	12,992	16,433	29,941
N	2,585	2,221	2,171	2,201	2,071

Notes: Adjusted using LSYPE-provided Wave 2 survey design, attrition and non-response weights. Standard errors, clustered by school, in parentheses. Family income is equivalised by dividing by the square root of household size. Sample: Wave 2 respondents with non-missing data on university expectations ('don't know' treated as 'not very likely') and university applications.

3.3.4 Measurement of other factors

The dataset also includes a rich set of participant characteristics and experiences. As discussed in Section 3.2, many of these factors are correlated with SES. However, they may also have independent effects of their own, with their exclusion resulting in omitted variable bias. It follows that it is important to be able to control well for these other factors to isolate the influence of SES. In this section I discuss the measurement and importance of academic ability, demographic characteristics (age, gender and ethnicity), school characteristics, traumatic events, and local labour market conditions.

One of the advantages of duration modelling is that it allows me to take into account different values of explanatory variables at different times. As such, in addition to describing potential explanatory factors in the dataset, I also assess their potential use as valid time-varying covariates. This requires that they are measured repeatedly and consistently throughout the LSYPE, since measurement in differing ways might result in changes that are not due to any underlying change in circumstances. Box-Steffensmeier and Jones

[14]Despite the presence of non-continuous variables, constructing my SES index using any of the following alternative methods makes no substantive difference (correlation coefficients between the indices $r > 0.98$) to my SES quintile groups: principal components analysis applied to a Pearson's correlation matrix; factor analysis treating the income, education and occupational status as continuous and using full information maximum likelihood (FIML) to deal with missing data; factor analysis treating income as continuous, and education and occupational status as ordinal, using FIML, but no weights. Given this, I am confident that my SES index is robust.

(2004, p.110-112) also highlight the importance of understanding the temporal ordering of time-varying covariates and the events it is being claimed that they are causing. Since, by their nature, time-varying covariates are not fixed, it is particularly important to assess whether, in this case, such covariates are plausibly being affected by changes in young people's expectations of applying to university. This eventuality, referred to as reverse causation, would result in endogeneity bias to the estimates (Goodliffe, 2003).

Table 3.3: Summary statistics of sample by whether young person reports being likely or unlikely to apply to university at age 14

Variable	**Mean of Unlikely**	**Mean of Likely**	**Mean of Whole Sample**	**Standard Deviation**
SES Index (Z-Score)	-0.40	0.20	0.00	1.00
	(0.02)	(0.02)	(0.02)	
Equivalised Family Permanent Income	12464.07	18029.33	16199.21	12220.12
	(209.35)	(256.24)	(208.44)	
At least one parent has Higher Education	0.06	0.25	0.19	0.39
	(0.00)	(0.01)	(0.01)	
At least one parent has 'Higher' Occ. Status	0.26	0.49	0.41	0.49
	(0.01)	(0.01)	(0.01)	
Lone Parent	0.28	0.20	0.22	0.42
	(0.01)	(0.01)	(0.00)	
Gender: Male	0.55	0.48	0.51	0.50
	(0.01)	(0.01)	(0.01)	
Ethnicity: Non-White	0.07	0.16	0.13	0.34
	(0.00)	(0.01)	(0.01)	
Age 11 Attainment Z-Score	-0.48	0.23	-0.00	0.97
	(0.02)	(0.02)	(0.02)	
Age 16 Attainment Z-Score	-0.60	0.29	-0.00	1.00
	(0.03)	(0.02)	(0.02)	
Attend Independent School	0.02	0.10	0.07	0.26
	(0.01)	(0.01)	(0.01)	
Attend Grammar School	0.01	0.05	0.04	0.19
	(0.00)	(0.01)	(0.01)	
Attend school with Sixth Form	0.52	0.56	0.55	0.50
	(0.02)	(0.02)	(0.02)	
Local Unemployment Rate (%) at Age 14	4.61	4.80	4.74	2.14
	(0.07)	(0.07)	(0.06)	
N	3686	7523	11209	

Notes: Weighted using LSYPE Wave 2 sample design and non-response weighted weights. Standard errors, clustered by school, in parentheses. Household income is equivalised by dividing by the square room of household size.

Correlation between academic ability and SES would lead to upward biased estimates of the effect of SES on young people's expectations of attending university, if it is not included in the model. Academic attainment provides an imperfect proxy for the unmeasurable individual trait of ability. A particularly important imperfection is that SES is likely to have an effect on the attainment measures available in the LSYPE. This suggests that models including attainment may underestimate the influence of SES. The LSYPE provides measures of academic attainment through linkage to selected elements of the National

Pupil Database (NPD). This provides information on the young people's academic attainment from Key Stage 2 (age 11), Key Stage 3 (age 14) and Key Stage 4 (age 16). Having high-quality, seldom-missing data on prior attainment is a major advantage compared to many surveys. Key Stage 5 data (from qualifications taken at ages 17 and 18) are now available as part of the LSYPE release. However, I do not use them as part of this analysis, since the relevant examinations are taken after the period of this analysis.

Some of the academic attainment data from ages 11 and 14 are missing where an individual was not in the state education sector and hence either did not take the relevant tests (SATS) or, if they did, the school chose not to report them. Pupils at independent schools are under no obligation to do either, although many do. A missing variable dummy is employed for Key Stage 2 scores to prevent these individuals from being excluded from my analyses. This is not an option for Key Stage 3, since the missing variable dummy would be almost perfectly collinear with an indicator of independent school attendance. Given this problem, the fact that children are unlikely to change schools immediately after taking their Key Stage 3 SATS and the low stakes nature of Key Stage 3 SATS I decide not to include it in my analysis.[15]

For Key Stage 2 (KS2), I use the average raw point score across all three subjects (Maths, English and Science[16]). KS2 SATS are relatively low stakes examinations for pupils, although they are rather higher stakes for primary schools and there is some limited use by secondary schools for tasks such as sorting pupils into ability groups. After weighting, there is a roughly normal distribution of scores ranging between approximately 0 and 100. The mean score is 65.5 and the median individual obtains a score of 67.3. I standardise this variable, creating a 'Z-score' with a mean score of zero and a standard deviation of one.

For Key Stage 4 (KS4), I use the official capped GCSE score. GCSEs (General Certificates of Secondary Education) are high stakes public examinations, taken at the end of compulsory education. They potentially have a large bearing on the individual's future education and/or employment. After weighting, the capped point score gives a range of scores from 0 to 483, with a mean of 306 and a median of 326. The capped point score is calculated from an individual's best 8 GCSEs or equivalent qualifications. This is in contrast to the uncapped score, which uses all GCSEs and equivalents taken and hence is more subject

[15] It is also worth noting that Key Stage 3 SATS were abolished in England in 2008 (BBC News).

[16] In the raw scores, Science is out of 80. I rescale it to be out of 100, ensuring it receives the same weight as Maths and English.

to manipulation by schools. Again, I standardise this so that the score has mean zero and standard deviation one. However, it should be noted that there is some potential for reverse causation in the relationship between KS4 performance and young people's educational expectations, in that individuals' beliefs about their likelihood of applying to university may affect the effort they put into these examinations.

The LSYPE collects data on young people's demographic characteristics, including their gender, age and ethnicity. While neither gender nor age are likely to be correlated with SES, they are both likely to be important in explaining changes in young people's expectations.[17] However, individuals with different ethnicities have, on average, different levels of SES (Strand, 2014). As such, failure to control for ethnicity may result in effects stemming from, for example, cultural differences between ethnicities, being incorrectly identified as SES effects. In the LSYPE, ethnicity is initially collected according to young people's self-designation, and classified into the groups White, Mixed, Indian, Pakistani, Bangladeshi, Black Caribbean, Black African and Other before the data are released.

The input of schools and teachers is important in shaping young people's educational choices. For example, Alcott (2013b) finds evidence that teacher encouragement makes it more likely that young people remain in education past the minimum leaving age. Likewise, Sanders et al. (2013) report that within-school provision of information on university increases stated likelihood of application. The LSYPE includes data on the young person's school type at time of sampling. Of particular interest, this allows me to identify those who attend academically selective 'grammar' schools (4% of the age 14 sample) and those who attend fee-paying independent schools (5% of the age 14 sample). Table 3.3 shows that a significantly larger proportion of those who think it likely that they will apply to university at age 14 than those who think it is unlikely are in one of these types of schools. It is also the case that individuals from higher SES backgrounds are more likely to be in such schools. It is not clear how much of the influence of schools is an 'independent' effect and how much reflects SES bias in the intake of different types of school. As such, in the same way as was discussed above regarding inclusion of prior attainment in a model, conditioning on school characteristics may result in an underestimate of the total influence of SES.

[17]Given the relationship between age and the passage of time in this dataset, I discuss the inclusion of age in the models further in Section 3.4.

Traumatic events within a family, such as job loss, separation or bereavement, might also be expected to have a negative influence on young people's educational expectations. Such events are to some extent random and, hence, effects would be at least partly independent of those of SES. However, there is likely to be some correlation.

The employment status of parents in the household are recorded at each wave. Drawing on previous evidence that finds an association between even short periods of worklessness and lower educational expectations (although these do not persist when additional controls are added) (Schoon et al., 2012, p.38-39), I construct a cumulative indicator of whether the young person has experienced being in a workless household by the time of each wave's interview. As I do not have data before age 14, it is not possible for this to include periods of worklessness before this point. Nevertheless, 22% of the young people's parents (after weighting) reported neither parent being in work in at least one wave. I judge that it is unlikely that young people's educational expectations affect changes in employment status in their household, and hence the risk of endogeneity bias is low. However, sociologists emphasise that an important element of social class is the increased economic security of those with higher SES (Goldthorpe and McKnight, 2004, p.6). Once again this implies that, once this factor is controlled for, my estimates of the influence of socio-economic status are likely to be understated.

I use information on the marital status of the 'main parent'[18] in a similar way as the employment indicators, constructing a cumulative indicator of whether the young person has experienced this parent going through some kind of separation (including bereavement) up to the point of each wave's interview. Unlike with the indicator for workless households, retrospective questions (asked at the first wave of the survey) about relevant events since the young person was born mean that this does cover the period before age 14. 28% of young people's main parents report having experienced such an event by the final interview with them. I define a cumulative measure on the grounds that negative consequences on a young person's attitudes from such an event are unlikely to be limited to one year. Again, I judge that there is unlikely to be problems of reverse causation with this time-varying covariate.

Local labour market conditions are important in predicting young people's decision to apply to university: other things being equal, individuals who face circumstances in which

[18]Defined as the parent most involved in the young person's education. Where there is only one parent in the household they are, by definition, the main parent.

the labour market looks less promising are more likely to remain in education longer (Reynolds and Pemberton, 2001; Fumagalli, 2012). However, on average, SES and worse local labour market conditions are likely to be negatively correlated. Unlike with the characteristics discussed above, this implies that not including this factor in the model may understate the impact of SES. To include this in my models I make use of data on the Local Authority (LA) area in which the young person's home is located is also available from the LSYPE. I use this LA identifier to link this with data on unemployment in the local labour market[19] from the Annual Population Survey (Office for National Statistics, 2004, for example). I use the unemployment rate for those aged 16-64 in the individual's LA area, with separate figures for males and females. In a small number of LAs the figures are suppressed, due to small numbers in the data. In such cases I use the Government Office Region unemployment rate (or in extremis the national unemployment rate) to avoid missing data.

3.4 Duration modelling

Duration modelling, also known as survival analysis or event history analysis, is not a common technique in educational research (Alcott, 2013a, p.50-51). However, it has several key features that make it a useful tool to address the question of changes in young people's expectations, specifically models of change i) from 'likely to apply' to 'unlikely to apply' and ii) from 'unlikely to apply' to 'likely to apply'. In this section, I introduce its key features, concepts and their importance for the application in this chapter.

Central to duration modelling is the concept of the 'spell'. A spell is an uninterrupted period of time during which a given individual remains in the same state; in this case, consistently reporting that they are 'likely to apply' to university, or conversely, consistently reporting that they are 'unlikely to apply'. Figure 3.2 shows spells as uninterrupted periods as solid lines ('likely to apply') or dotted lines ('unlikely to apply'). In some applications of duration modelling the end of a spell is permanent (or effectively permanent), such as in models of an individual's death after the onset of a disease. However, in this

[19]Since the aim is to capture the labour market conditions individuals face, it would be better to use areas designed to reflect this. Local Authorities do not necessarily reflect local labour markets well, especially in larger, rural authorities. A better alternative would be Travel To Work Areas (TTWAs). Unfortunately, information that would allow me to identify in which TTWA an individual resides is not available in the LSYPE general release.

Table 3.4: Proportion of young people saying they are likely or very likely to apply to university - always reported likely vs. current wave

Wave	Always likely	Current wave
1	0.676	0.676
2	0.552	0.626
3	0.484	0.608
4	0.429	0.570
5	0.399	0.566
6	0.384	0.582

Notes: Analysis weighted using LSYPE Wave 7 design and non-response weights. Sample: Wave 7 respondents with non-missing data on university expectations ('don't know' treated as 'not very likely') and university applications. Unweighted sample size = 8029. 'Always likely' column reports proportion of the sample who have always reported being 'very likely' or 'fairly likely' to apply to university up to and including the wave in question. 'Current wave' column reports the simple proportion of the sample who report being 'very likely' or 'fairly likely' to apply at the wave in question.

application individuals can report being 'likely to apply', then 'unlikely to apply', and then 'likely to apply' again.[20]

Since participants can move back and forth between being 'likely' and 'unlikely', the same individuals may appear in both sets of models at different time points. One can see that this is indeed the case by calculating the proportion of the sample that ever report being 'likely to apply' to university and the proportion that ever report being 'unlikely to apply'. First, considering the transition from 'likely to unlikely', 79% of the Wave 2 weighted sample (representing 9,247 out of 11,249 individuals before weighting) in the dataset report being 'likely to apply' to university (and, hence, are ever in a position to make a transition to being 'unlikely to apply') in at least one wave. In the other direction, 52% of the Wave 2 weighted sample (representing 5,330 out of 11,249 individuals before weighting) report they are 'unlikely to apply' (and, hence, are ever in a position to make a transition to being 'likely to apply') in at least one wave. In total, this sums to 131% of the sample, demonstrating the significant overlap. One can also see this is the case by looking at the sequences of expectations observed in the data in Figure 3.2: individuals of type 3 are included in the model of 'likely to unlikely' at age 15, then in the model of 'unlikely to likely' at ages 16 and 17.

To highlight the implications of using duration modelling, relative to a model of differences between the start and the end of the time period under consideration, in Table 3.4 I compare the proportion of individuals who at all points up to and including the relevant

[20]It should be noted that one reason for such sequences of transitions could be measurement error. This makes allowing for multiple spells particularly important, since ignoring spells after the first would compound the error.

wave have reported that they think it 'likely' that they will apply to university (in the left hand column), with the proportion who think it is 'likely' that they will apply at that particular point in time (in the right hand column). As also noted in Figure 3.1 earlier, the proportion who think it is 'likely' that they will apply at a given point in time falls from 68% at Wave 1 to 57% by Wave 4. However, the reduction in those who have always reported being likely to apply is much greater: from 68% at Wave 1 to 42% by Wave 4. This difference is caused by individuals who start reporting being 'likely to apply' after Wave 1 (e.g. individuals of type 8) in Figure 3.2.

The larger reductions in the proportion who have always reported being 'likely to apply' demonstrates the additional information on transitions that is picked up by using this approach. This information would be ignored if I only modelled the difference between the start and the end of the time period under consideration. In fact, as I allow for multiple transitions, the differences are even larger than suggested in this table, since the analysis in this chapter recognises that individuals can, in principle, switch back and forth as many times as there are observation periods (e.g. individuals of type 9 in Figure 3.2). Each transition from being 'likely to apply' to being 'unlikely to apply', even multiple transitions by the same individual, is captured as part of the modelling.

My multiple regression-based duration models will allow for multiple spells in a state, since this is preferable to concentrating only on the first one. However, my modelling strategy treats multiple spells as being independent from one another, making the assumption that there is no causal effect of one spell on any later spells (either of the same type i.e. 'likely to unlikely', or the converse transition i.e. 'unlikely to likely').[21]

The passage of time is, as the name suggests, fundamental to duration modelling. Models can include the length of time an individual has spent in a spell before making a transition, not throwing away this considerable amount of information as would be done in a traditional binary choice model (DesJardins, 2003; Box-Steffensmeier and Jones, 2004; Jenkins, 2004). However, as individuals in the LSYPE are all (approximately) the same age at the same point in time, where spells begin at the same point it is impossible to distinguish between age and duration effects. In the data, some spells do start at different time points, but there is not enough variation to disentangle the effects of age and duration. At

[21]However, see discussion of clustering of standard errors in Section 3.6. Furthermore, I attempt to partially relax the assumption of independence of multiple spells of the same type using random effects models, discussed in Appendix B. However, it maintains the assumption of no effect of an individual's spell of being 'likely to apply' on subsequent spells of being 'unlikely to apply'.

this stage of life, I judge that age effects are more important to educational expectations than duration in the state, and concentrate on these. Other important characteristics of individuals may also change over time and duration modelling is able to incorporate such time-varying covariates[22]

As discussed in Section 3.3, since I have discrete (as opposed to continuous) time data, I use discrete time duration modelling techniques, as the most appropriate. One potential problem with this is that, since young people are born in different months and the LSYPE interviews are staggered over several months, there will be some variation in individuals' age by month when they are give their responses. In order to reduce the possibility that this could affect results, I include individuals' month of birth and month of interview in all my regression models, attempting to standardise results as if individuals were all both born and interviewed in August each year.

A key concept in duration modelling is that of an individual being 'at risk' of making a transition, and therefore relevant to my modelling. When modelling a transition it only makes sense to consider those who are in a position to make that transition. As a minimum, this excludes those who already in the state of interest. For example, it does not make sense to consider the probability that someone who already reports being 'unlikely to apply' to university *becomes* 'unlikely to apply' to university. While it may be interesting to consider the question of whether an individual *remains* 'unlikely to apply', that is a different question (and, in fact, just the inverse of my other model: whether an individual currently reporting being 'unlikely to apply' becomes 'likely to apply'). In some applications individuals may become not at risk in other ways.

Duration modelling can also treat expectations data that are missing as 'censored', rather than dropping individuals for whom expectations are not observed (even in only one wave) from the sample. 'Censoring'[23] is where the start and/or end points of a spell is not observed in the data. It has the consequence that the true length of the spell is unknown, only that it is at least as long as the period it is observed to last.

When the start of a spell is not observed this is referred to as 'left censoring'; this can be particularly problematic, as it prevents modelling of duration dependence, since one does not know how long a spell has lasted at any given point (Iceland, 1997). However,

[22]This was discussed further in Section 3.3.4.

[23]Censoring is sometimes confused with 'truncation'. This is when the probability of inclusion of a spell is affected by its length or where spells are cut short for the same reason. I do not have to deal with truncation in my data.

as discussed in Section 3.3.1, I treat all spells as starting at age 14 and, hence, exclude the possibility of left censoring in this dataset by construction.

Not observing the end of a spell is referred to as 'right censoring'. Taking the example of models for the 'likely to unlikely' transition, this occurs where 'likely to apply' is observed in the final report for an individual, whether this is due to the end of the period under analysis (at age 17 in this case), or earlier as a result of attrition. Still concentrating on the 'likely to unlikely' transition, there is right censoring in the sequences of spells in Figure 3.2 for individuals of type 1, 8, and 9 (in the case of the final observation being still 'likely to apply'); and types 5 and 7 (resulting from attrition).

Treating individuals who attrit from the sample as right censored will only result in unbiased estimates under the assumption that this missing data censoring is 'uninformative' (Clark et al., 2003, p.236), i.e. that individuals whose outcomes are missing are just as likely to make a transition between reporting being 'likely to apply' to university and being 'unlikely to apply' (or vice versa) as the individuals that are observed. It seems unlikely that this assumption is justified. However, van den Berg et al. (2006) suggests it is likely that while informative attrition will affect the rate of transitions, it is less likely to bias the effect of covariates on those rates. As a robustness check, I also repeat my analysis including only those still participating in the survey at Wave 4 (when the response rate relative to Wave 1 has fallen to 73% (Collingwood et al., 2010, p.52)), using the LSYPE-provided attrition and non-response weights for Wave 4.[24]

All of these features are important in fitting the most appropriate model to understand changes to young people's expectations during these critical years for their education.

3.5 Nonparametric analysis of transitions

In this chapter I model the probability and timing of young people's transitions from reporting they are 1) 'likely to apply' to 'unlikely to apply' or, conversely, 2) 'unlikely to apply' to 'likely to apply'. Restricting my attention to those who are 'at risk' of making each transition, it follows that I am interested in the likelihood of the following events:

1. for the transition from 'likely to apply' to 'unlikely to apply': whether individuals, who at the previous wave said they were 'likely to apply' to university, switch to

[24] I report the results of this analysis and discuss the differences in Appendix B.

reporting that they are ‘unlikely to apply’; and

2. for the transition from ‘unlikely to likely’: whether individuals, who at the previous wave said they were ‘unlikely to apply’ to university, switch to reporting that they are ‘likely to apply’.

To begin exploring these transitions, I conduct non-parametric analysis of the probability and timings of transitions between being ‘likely’ and ‘unlikely’ to apply to university and consider the association between the probability of making a transition and young people’s SES. In order to do this I make use of Kaplan-Meier estimates of the probability that spells have not ended with a transition by a given age. To obtain Kaplan-Meier estimates one first calculates, at each time point in the data, the number of individuals that do not make a transition divided by the number that are in a position to make a transition. The estimate for each time point is the product of all of the proportions just calculated from the first time point up to the time point in question. Kaplan-Meier estimates are able to handle right-censoring in the data, since individuals who are censored are removed from the denominator, since they are no longer ‘at risk’. These estimates of ‘survival’ will be calculated both for the sample as a whole, and for sub-samples defined by SES.

In order to perform this analysis, I restrict the spells under consideration to those beginning at age 14 (the start of the dataset). By definition, this also means concentrating on an individual’s first spell at risk, ignoring any later spells either as ‘likely’ or ‘unlikely’. Below, I indicate the kinds of spells excluded as a result. Among the costs and benefits of the multiple regression-based analysis introduced in Section 3.6, this restriction will be relaxed.

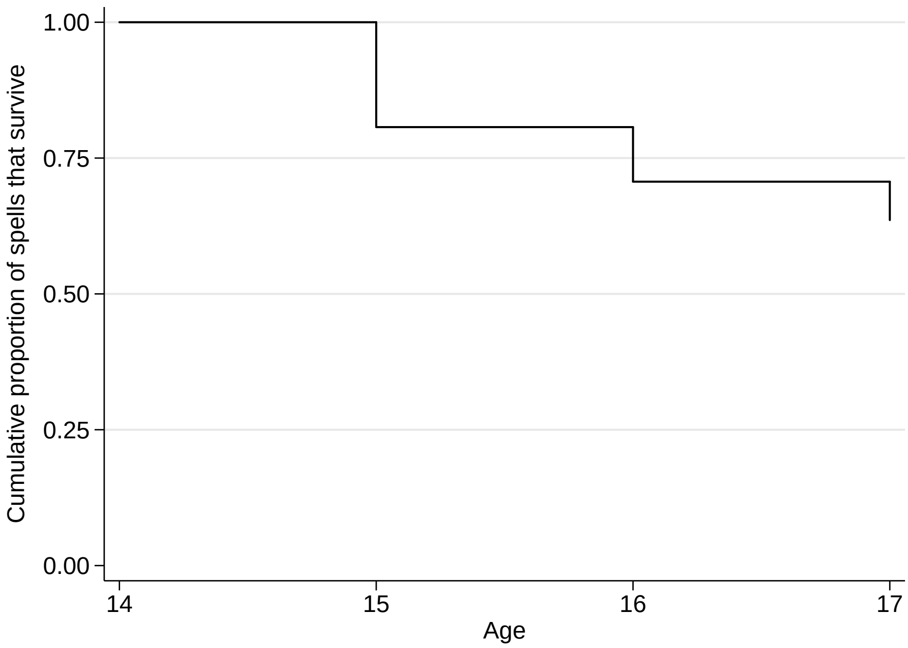
It was not possible to perform non-parametric statistical inference on the difference between estimated survival functions as part of this analysis. The relevant statistical test, the log-rank test, is “not appropriate” with sampling weights (StataCorp, 2013, p.446). Instead, I perform Cox regression-based tests, which make the proportional hazards assumption. However, I checked the robustness of this approach by performing log-rank tests of the equality of the survival curves estimated using unweighted data. In all cases the two sets of results were in agreement.

I first consider the transition from ‘likely to unlikely’, before moving on to the transition from ‘unlikely to likely’.

3.5.1 From likely to unlikely

I begin by analysing the age at which young people stop thinking they are likely to apply to university. Relating this to the sequences of expectations shown in Figure 3.2, this means including the first (or only) spell of individuals of type 1, 3, 4, 5, 6, 7 or 9 (amongst others not shown in the diagram), but not the spell that type 8 spends reporting being 'likely to apply'. Nevertheless, this includes over 70% of the individuals in the data, with much of the remainder being individuals who never report being 'likely to apply' rather than individuals who are excluded simply because of this restriction.

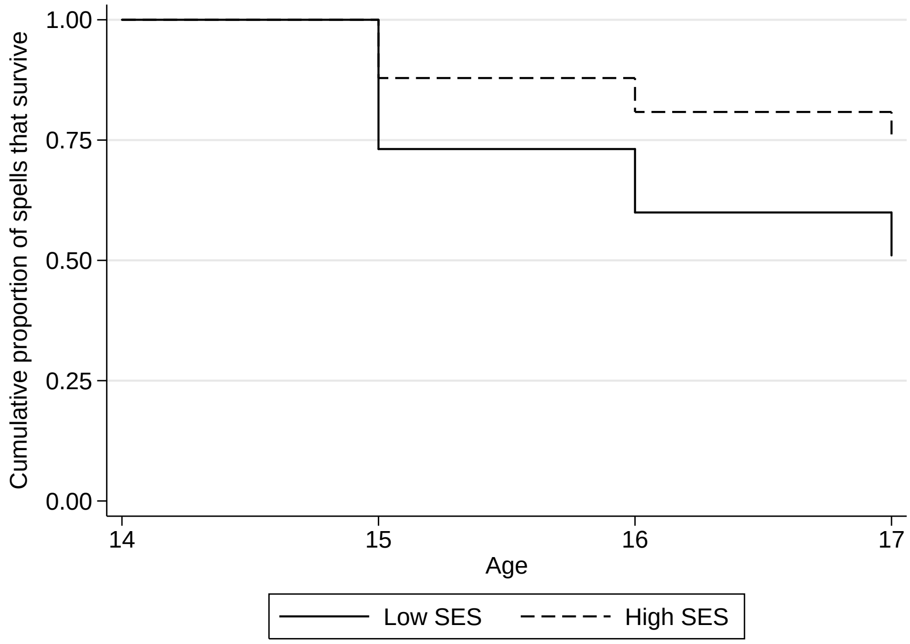
Figure 3.3: Probability that an individual who reports being 'likely to apply' at age 14 has not moved to reporting that they are 'unlikely to apply', by age



Notes: Kaplan-Meier estimated survivor function. Excludes spells beginning after age 14. Analysis weighted using Wave 2 sample design and non-response weights. Unweighted number of subjects: 6,129; weighted number of subjects: 6,009.

Figure 3.3 shows that 70% of periods of reporting being 'likely to apply' continue until at least age 16, at which point young people will be in the process of taking their GCSEs. Conversely, this means that 30% of such periods have ended with the individual switching to reporting they are 'unlikely to apply' by this age. Looking right to the end of the ages under consideration, roughly a third of the observed periods of being 'likely to apply' end by age 17. There are evidently a significant number of transitions during this stage of life. However, this sheds no light on the reasons for these changes, other than young people's age increasing.

Figure 3.4: Probability that an individual who reports being 'likely to apply' at age 14 has not moved to reporting that they are 'unlikely to apply', by age and household SES



Notes: Kaplan-Meier estimated survivor function. Excludes spells beginning after age 14. Analysis weighted using Wave 2 sample design and non-response weights. 'High SES' denotes individuals in the top two quintiles of SES, while 'low SES' refers to all other individuals. Unweighted number of subjects: 6,129; weighted number of subjects: 6,009. Cox regression-based test for equality of survivor functions rejects the null hypothesis of no difference (p<0.01)

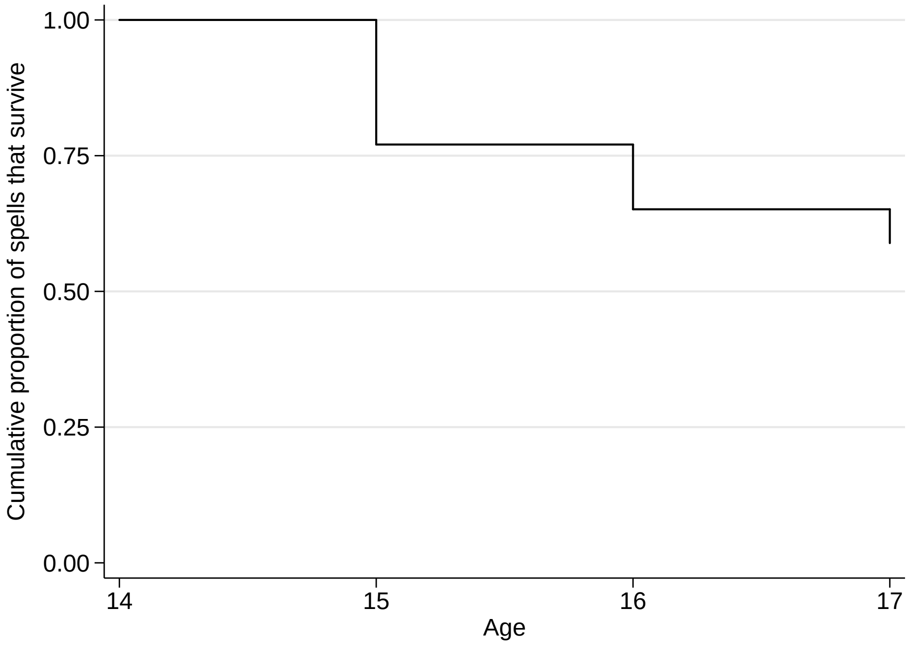
A simple way of assessing the association between the probability of transition and family background is by estimating the survivor function for different groups of SES. For ease of interpretation I dichotomise SES into 'high' (comprising the top 40% of the distribution of my SES index) and 'low' (comprising the bottom 60% of the distribution). Figure 3.4 shows that individuals from lower SES households are more likely to make a transition to reporting 'unlikely to apply' than their richer counterparts throughout the period under analysis: 40% of those from lower SES backgrounds have made a transition from 'likely to unlikely' by age 16, whereas only 20% of those from high SES backgrounds have done so. Making the assumption of proportional hazards allows me to carry out a Cox-regression based test, which rejects the null hypothesis of no difference between the two estimated survivor functions (p=0.00).

3.5.2 From unlikely to likely

It is possible that the relationship between SES and young people raising their expectations is quite different from that associated with movement in the opposite direction.

The analysis of this transition from 'unlikely to likely' includes the first (or only) spell from individuals of types 2, 8 and 10 in Figure 3.2, but not the spell that types 3, 4, 6 and 9 spend reporting being 'unlikely to apply'. This represents over 20% of the overall sample, but much of the remainder again comprises individuals who never report being 'unlikely to apply', rather than exclusions because of restricting to spells that start at age 14.

Figure 3.5: Probability that an individual who reports being 'unlikely to apply' at age 14 has not moved to reporting that they are 'likely to apply', by age

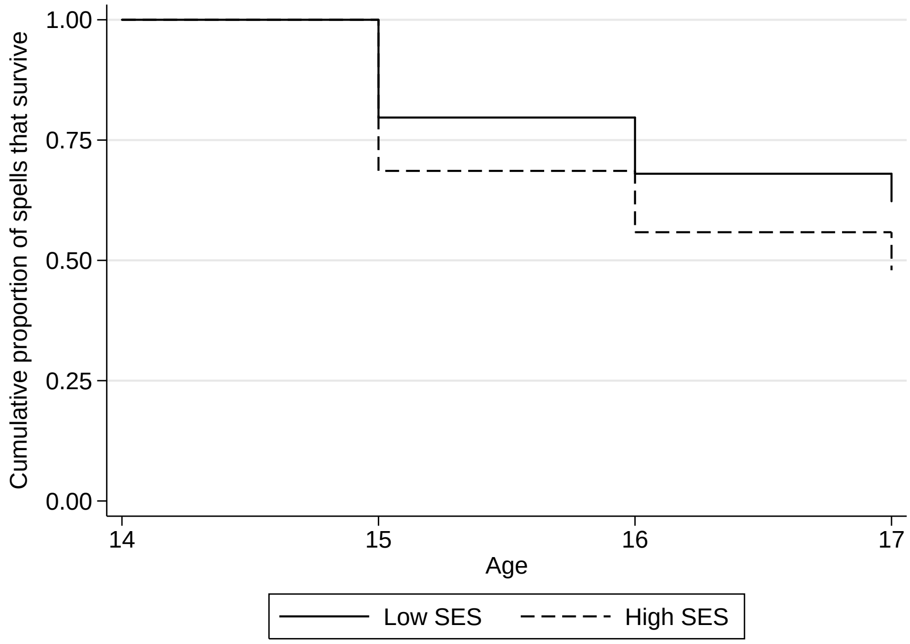


Notes: Kaplan-Meier estimated survivor function. Excludes spells beginning after age 14. Analysis weighted using Wave 2 sample design and non-response weights. Unweighted number of subjects: 2,556; weighted number of subjects: 2,946.

As with the opposite transition, Figure 3.5 shows the proportion of periods of being 'unlikely to apply' that do not end in transition to being 'likely to apply' by a given age. Almost 25% of spells end by age 15 and around a third of spells have ended in transition by the last point of observation at age 17. These are higher rates of transition than those seen for the same time points in my analysis of the transition from 'likely to unlikely' above, this despite a larger overall shift in the opposite direction. Although this initially seems counterintuitive, it is consistent because of the larger absolute numbers of young people who start out saying they are 'likely to apply' (as shown in Figure 3.1). Furthermore, it again highlights the large number of transitions between the two states.

In common with transitions from 'likely to unlikely', Figure 3.6 shows that there are clear socio-economic differences in the expected proportion of transitions from being 'unlikely to apply' to being 'likely to apply'. However, in this case those from the less advantaged

Figure 3.6: Probability that an individual who reports being 'unlikely to apply' at age 14 has not moved to reporting that they are 'likely to apply', by age and SES



Notes: Kaplan-Meier estimated survivor function. Excludes spells beginning after age 14. Analysis weighted using Wave 2 sample design and non-response weights. 'High SES' denotes individuals in the top two quintiles of SES, while 'low SES' refers to all other individuals. Unweighted number of subjects: 2,556; weighted number of subjects: 2,946. Cox regression-based test for equality of survivor functions rejects the null hypothesis of no difference (p<0.01).

groups are less likely to make a transition out of being 'unlikely' than their more advantaged peers. Again, a Cox regression-based test allows me to reject the null hypothesis of no difference between the two survivor functions (p=0.00).

Comparing Figure 3.6 with Figure 3.4 it is clear that the differences in rates of transition from being 'unlikely' to being 'likely' by SES are markedly smaller than for the transition in the opposite direction: by age 16 68% of those from lower SES backgrounds have made a transition from 'unlikely to likely', while 56% of those from more advantaged backgrounds had done so. This suggests that more of the inequality in expectations builds from less advantaged individuals having a higher probability of switching to reporting being 'unlikely', than from movements in the other direction. Nevertheless, the inequality in probability of transition from 'unlikely to likely' compounds the widening socio-economic and demographic inequality of expectations generated by the larger proportion of less advantaged individuals switching from being 'likely to unlikely' seen above.

However, the analysis so far has limitations: it cannot accommodate spells that started after age 14 (or, hence, multiple spells from one individual); and it cannot control for additional covariates. In order to relax these limitations, I now turn to multiple regression-based duration modelling techniques.

3.6 Multiple regression models

I estimate multiple regression duration models using the so-called 'easy estimation' methods detailed by Jenkins (1995). These are implemented using a standard binary dependent variable regression model applied to a dataset organised such that there is one observation for each time point that each individual is 'at risk' of making the transition of interest. I show the derivation of this method in Appendix C. The model exposition concentrates on the transition from 'likely to apply' to 'unlikely to apply' only to avoid unnecessary duplication; it is easy to see how the model is modified for the transition from 'unlikely to apply' to 'likely to apply'.

The outcome of interest, as outlined in Section 3.3, is a simple indicator of whether the individual reports being unlikely to apply to university:

$$\begin{aligned} Y_{it} &= 1 \text{ if young person } i \text{ is unlikely to apply to university at time } t \\ &= 0 \text{ if young person } i \text{ is likely to apply to university at time } t \end{aligned} \tag{3.2}$$

However, as noted above, it only makes sense to include in modelling individuals who are 'at risk' of the transition in question occurring. I define a variable d_{it}, which indicates whether an individual makes the transition at a given time point, given that the individual was at risk of making the transition (i.e. they reported being likely to apply in the previous period). d_{it} takes no value where individuals are not 'at risk' of making a transition and so these observations are not included in models. The variable is formally defined as:

$$\begin{aligned} d_{it} &= 1 \text{ if } Y_{it} = 1 \cap Y_{it-1} = 0 \\ &= 0 \text{ if } Y_{it} = 0 \cap Y_{it-1} = 0 \end{aligned} \tag{3.3}$$

A large component of changes in young people's expectations may simply be explained by the age they have reached. If I ignore this in modelling it may result in omitted variable bias, with other covariates picking up the variation that should have been explained by age alone. I include a simple function of age in my models, denoted by α. Imposing functional form restrictions here would increase the risk of not adequately accounting for the underlying probability of transition at each age, which may also affect other estimates through omitted variable bias. Particularly because I have relatively few time periods, I use a piecewise constant age function, implemented through inclusion in the model of a dummy variable for each age (except for the first, making this the base category):

$$\alpha(A_{it}) = \alpha_0 + \alpha_{16}A_{16.it} + \alpha_{17}A_{17.it} \tag{3.4}$$

In duration models it is common to model the effect of the length of time individuals have spent in their current state on the probability of transition. A relevant example of this 'duration dependence' could be that time spent believing that you are unlikely to go to university affects one's attitudes towards and, hence, performance in school work. Such lower performance then becomes self-reinforcing of the view that you are unlikely to be in a position to apply to university. The effect of the length of time spent in a state is referred to as a 'baseline hazard rate'. In some applications, parametric 'baseline hazard functions' are used to make statements about how the underlying probability of transition changes as the length of a spell increases. However, introducing a baseline hazard function to the models in this chapter has not been possible because such a large proportion of spells in the data start at the same point in time (age 14). As a result, the variables for age and time in state are highly collinear.

Since my outcome variable (d_{it}) is dichotomous, I opt to use complementary log-log regression models.[25] Using these variables and **x**, which is a vector of time-invariant and time-varying control variables (discussed further below), I estimate regression models of the form:

$$\log(-\log(1 - d_{it})) = \alpha(A_{it}) + \boldsymbol{\beta}\mathbf{x}_{it} + \varepsilon_{it} \tag{3.5}$$

This method of estimating duration models involves multiple observations per individual. As a consequence, ignoring the survey design, I would estimate standard errors clustered at the individual level. However, given that young people in the Longitudinal Study of Young People in England are clustered within schools, the estimated standard errors are calculated more conservatively, taking into account this higher level clustering.

[25]The other major alternative used in duration modelling of this type are logistic models. As a robustness check, I also estimate my models using this method. Doing so makes little difference to the results.

I begin with a baseline model (M0), only including my age function.[26] This performs a number of roles. First, it places the survivor functions from Section 3.5 into this regression framework, this time allowing for multiple spells from one individual and also for spells that begin later than age 14. Second, it allows me to inspect the raw coefficients on age, providing insights on when adjustment of expectations most often occurs. Third, it provides a baseline against which I can assess the following models, in which I include additional explanatory variables.

My first model of substantive interest (M1) attempts to capture the 'total' association between SES and the probability that individuals make a transition between being 'likely' and 'unlikely' to apply. In addition to the age dummy variables, I include dummy variables indicating which quintile group of socio-economic status (SES), measured using the index described in Section 3.3.3, an individual is in. I leave out a variable for the third (middle) quintile group, making it the baseline category.

My second model (M2) attempts to identify the 'conditional' association between SES and the probability of making a transition, controlling for demographic characteristics, school characteristics, traumatic experiences and local labour market conditions. For demographic characteristics, the model includes gender, ethnic group, number of siblings, number of older siblings, and region of residence. For school characteristics, I include indicators for fee-paying independent schools, selective 'grammar' schools, and for whether the school has a post-16 'sixth form'.[27] To capture the effect of traumatic experiences, I include time varying measures derived from experience of being in a workless household or having experienced a family separation. Finally, I include data proxying local labour market conditions faced by young people, specifically the local youth unemployment rate within an individual's Local Authority of residence. Since many of these variables are socially graded, I expect them to reduce the conditional association between coming from an advantaged family and the probability of transition, allowing us to assess the remaining 'effect' attributable to SES. However, as discussed in Section 3.3.4, the effect of SES on these variables may mean I start to underestimate the influence of SES on changes in expectations.

[26] M0 does also include the month of birth and month of interview variables to try and control for the differences in age of the panel members when interviewed.

[27] I also estimate linear probability models including school fixed effects as a robustness check. As might be anticipated, the influence of SES is somewhat reduced in these models, but they do not alter the overall narrative.

My third model (M3) contains the same variables as M2, and adds covariates to control for an individual's observable prior academic attainment. I include a standardised score of young people's performance at age 11 (Key Stage 2). Undoubtedly, young people's academic performance affects whether they stand a realistic chance of making a successful application to university and, hence, affects whether young people maintain their current expectations. As with some of the variables above, young people's attainment at age 11 is already likely to be affected by SES, meaning that results including prior attainment only show SES effects conditional on these results. This model is my preferred specification for identifying the 'conditional' effect of SES on changes in young people's expectations of applying to university.

My final two models specifically address whether young people's expectations are affected by the new information on their academic attainment provided by performance in examinations at age 16. The first of these (M4) adds a variable for an individual's performance in end of secondary school examinations at age 16 (Key Stage 4), standardised with mean zero and standard deviation one, and interacted with the age variable indicating that they will have received their results (age 17). As such, it will provide an estimate of the association between a one standard deviation increase in young people's performance at age 16 and the risk of transition from 'likely' to 'unlikely' or vice versa, conditional on family background and attainment at age 11. However, in interpreting this finding, it is important to note that individuals' performance in examinations at 16 is likely to be endogenous: young people's expectations of applying to university are likely to affect their effort at school and hence performance in the these examinations. As such, particular caution should be taken in the interpretation of this model. The results should only be used as indicative for the question of responsiveness to new information on academic attainment; results from M3 are likely to be a more reliable guide to the overall association between SES and changes in young people's expectations.

The final model (M5) builds on M4, but relaxes the implicit assumption that this new information on academic performance affects all young people in the same way. I introduce an interaction between KS4 performance and SES, which allows me to explore whether individuals are more or less likely to adjust their expectations in response to their results depending on their SES background. The same caveats apply in terms of the potential endogeneity in performance at age 16, but this still provides suggestive evidence on a potentially important driver of inequality in expectations of applying to university.

Given the complexity of interpreting interaction effects, and in the interests of parsimony, I also estimate variants of models M4 and M5, in which the dummy variables for each quintile group of SES have been replaced by a single variable of my underlying SES index, standardised so that it has mean zero and standard deviation one. This simplification comes at the cost of assuming a linear relationship between my SES index and the risk of transition. However, robustness checks[28] suggest that this does not seem to affect the overall narrative of my analysis. As such, in my discussion of the results, I focus these variants, referred to as M4C and M5C.

3.7 Results

The results tables focus on the influence of SES on changes in expectations during this period.[29] Once again, I explore the transition from 'likely to unlikely and the transition from 'unlikely to likely' separately.

I report the results of the models using hazard ratios (exponentiated coefficients from the underlying complementary log-log regression model). These are multiplicative, rather than additive; they express no difference from the baseline group when they are equal to 1 (rather than 0, as would be the case if I were discussing coefficients). As such, when I refer to a hazard ratio being statistically significant, this means that it is statistically significantly different from 1, rather than from 0.

In models focusing on the influence of SES on transitions (M1-M3), I concentrate on the hazard ratios for each quintile group of SES, relative to a baseline category of the middle (third) quintile group. These may be interpreted as the probability that an individual in the relevant SES quintile group makes a transition, conditional on being in the state at that point, divided by the probability that an individual in the middle SES quintile group makes a transition (conditional in the same way). In order to examine the overall patterns of young people's transitions as they age, I also report hazard ratios from each model associated with each age, relative to a baseline of the period between the interview at age 14 and age 15.

In models focusing on the responsiveness of young people to new information on their

[28]The full results of M4, M4C, M5 and M5C are reported in Appendix B for comparison.

[29]Regression tables reporting the full set of hazard ratios are reported in Appendix B, along with their counterparts for several variations on the models (as discussed elsewhere in the chapter).

academic attainment (M4C and M5C), I concentrate on the hazard ratio associated with change in SES and the hazard ratio associated with change in both SES and KS4 performance. The former may be interpreted as the probability that an individual makes a transition, divided by the probability than an individual with one standard deviation lower SES makes a transition (conditioned as above). The latter may be interpreted as the probability that an individual makes a transition divided by the probability than an individual with one standard deviation lower SES *and* one standard deviation lower KS4 performance makes a transition.

It is also natural to want to test whether each model adds explanatory power, relative to the one before. In many circumstances this would be done with likelihood ratio tests. However, as a result of accounting for the complex survey design of the data, these are not valid. Instead, I conduct F tests of the joint significance of all additional coefficients, relative to the previous model. As the results simply show that each model does provide additional explanatory power relative to the one before, they are only reported in Appendix B.

3.7.1 From likely to unlikely

The results for the transition from 'likely to unlikely' are reported in Table 3.5. I begin by discussing the results from the baseline model (M0), to examine the point in time at which individuals currently reporting being 'likely to apply' are most likely to change to reporting being 'unlikely to apply'. The hazard ratios reported for ages 16 and 17 are statistically significantly less than one. This suggests the individuals are most likely to make a transition between their reports at age 14 and 15, with the rate of transitions slowing after this point. This reflects the Kaplan-Meier survivor function plotted in Figure 3.3, where the largest step was the first. However, it has commonly been observed in duration modelling that one reason for such an observation is that individuals who are most likely to make a transition have already done so before later time points (Jenkins, 2004, p.81), hence the sample at risk are systematically less likely to change their report just for this reason. Controlling for factors associated with this compositional change may, therefore, reduce the apparent effect of age.

In the first model including SES (M1), I find that the estimated hazard ratios are statistically significantly different from one for each of the quintile groups of SES, with young

Table 3.5: Estimated hazard ratios of transition from reporting being likely to apply to reporting being unlikely to apply by quintiles of socioeconomic status

	M0	**M1**	**M2**	**M3**	**M4**
Age 16	0.89	0.90	0.91	0.95	0.94
	(-2.41)**	(-2.33)**	(-2.07)**	(-1.08)	(-1.22)
Age 17	0.74	0.77	0.85	0.92	0.92
	(-6.60)***	(-5.82)***	(-3.48)***	(-1.83)*	(-1.76)*
SES Q1 (Low)		1.46	1.54	1.13	1.10
		(6.33)***	(6.59)***	(1.80)*	(1.42)
SES Q2		1.40	1.31	1.17	1.16
		(5.61)***	(4.49)***	(2.53)**	(2.42)**
SES Q4		0.75	0.80	0.80	0.80
		(-4.76)***	(-3.69)***	(-3.67)***	(-3.71)***
SES Q5 (High)		0.33	0.39	0.47	0.47
		(-13.45)***	(-11.89)***	(-9.66)***	(-9.59)***
Significance of SES ($P > \|F\|$)		0.00	0.00	0.00	0.00
N	9,247	9,247	9,247	9,247	9,247
Variables	**M0**	**M1**	**M2**	**M3**	**M4**
Age	$\surd$	$\surd$	$\surd$	$\surd$	$\surd$
SES Quintile Dummies		$\surd$	$\surd$	$\surd$	$\surd$
Demographics & School			$\surd$	$\surd$	$\surd$
Prior Attainment				$\surd$	$\surd$
Age 16 Attainment					$\surd$

Notes: Reporting hazard ratios. $P > |F|$ shows p-value from joint significance test of the hypothesis that exponentiated coefficients on all SES group dummies in the underlying conditional log-log regression model are equal to 1. Adjusted using LSYPE-provided Wave 2 survey design and non-response weights. T-statistics of the null hypothesis that the hazard ratio is equal to one, based on standard errors clustered by individual's school, are reported in parentheses. Estimated risks are relative to base categories of Age 15 and SES quintile group 3.

people from less advantaged backgrounds being significantly more likely to switch from reporting being 'likely' to reporting being 'unlikely'. To take the extremes, those in the least advantaged quintile group have more than four times the hazard of making a transition than those in the most advantaged SES group. In addition, the size of the change in hazard between each quintile group tends to increase further up the SES distribution: the smallest gap in hazard is between Q1 and Q2 (only equivalent to a 5 percent reduction in the probability of transition), while the largest is between Q4 and Q5 (equivalent to more than a 50% reduction in the hazard of transition). Also worthy of note is that inclusion of SES in the model has made very little difference to the correlation between age and hazard of transition.

Given previous evidence on the young people's expectations of applying to university by SES the strong relationship is unsurprising. However, the aim in the following models is to assess what, if anything, explains these gaps, and whether the SES gradient persists once other factors have been controlled for.

Moving to the second model including SES (M2), I add various demographic and school characteristics. Several of these (notably including gender, ethnicity, and school characteristics) have large hazard ratios that are statistically significantly different from one (reported in Table B.1 of Appendix B.1). There is some reduction in the socio-economic inequalities observed in earlier models: the hazard of an individual from the least advantaged SES quintile group making a transition from 'likely to unlikely' is now estimated to be just under 4 times greater than the hazard of an individual from the most advantaged group doing so. The estimated hazard of transition for individuals in the highest SES quintile group remains dramatically different from the estimated hazard for individuals in any other quintile group: individuals have less than half the hazard of making a transition as individuals in the second most advantaged fifth of the distribution.

As anticipated, inclusion of prior academic attainment from age 11 (in M3) makes a much bigger difference to the estimated influence of SES on young people's expectations. A noticeable feature of the estimated influence of SES quintile groups is that there is now no difference in the hazard of transition between the lowest two quintile groups; conditional on other characteristics, young people in the bottom 40% of the SES distribution have approximately 15% higher hazard of making a transition from 'likely to unlikely' than individuals in the middle. By contrast, the influence of being in a higher SES group continues to be large reductions in the hazard of transition from 'likely to unlikely': young people in the top SES quintile group still have approximately 50% of the hazard of making a transition as individuals in the middle.

Furthermore, introducing prior attainment reduces estimated differences in the hazard of transition by age, which become only statistically significant at a 0.1 level. This suggests that, in the case of the transition from 'likely to unlikely', much of the apparent effects of age were driven by the reduced presence in the sample of individuals with lower prior attainment by later time points.

In summary, there continues to be a strong relationship between young people's socio-economic background and their hazard of continuing to report being 'likely to apply' to university. Individuals from the least advantaged fifth of the SES distribution still have almost 2.5 times the hazard of making a transition as individuals in the most advantaged quintile group.

Table 3.6: Estimated odds ratios of transition from reporting being likely to apply to reporting being unlikely to apply by interaction of socio-economic status and new information on attainment at age 16

	M4C	**M5C**
Age 16	0.92	0.92
	(-1.45)	(-1.45)
Age 17	1.00	1.05
	(-0.03)	(0.84)
SES Z-Score	0.68	0.69
	(-11.00)	(-10.41)
KS4 Z-Score (After results)	0.51	0.46
	(-9.51)	(-9.98)
SES * KS4		0.79
		(-3.11)
N	9,247	9,247
Variables	**M4C**	**M5C**
Age	√	√
SES Index Z-Score	√	√
Demographics & School	√	√
Prior Attainment	√	√
Age 16 Attainment	√	√
Age 16 Attainment and SES Interaction		√

Notes: Reporting hazard ratios. Adjusted using LSYPE-provided Wave 2 survey design and non-response weights. T-statistics of the null hypothesis that the hazard ratio is equal to one, based on standard errors clustered by individual's school, are reported in parentheses. Estimated risks are relative to base category of Age 15.

What explains the reduction in the size of the SES gap once prior attainment has been included? Two possibilities are that young people from less advantaged backgrounds are less likely to have achieved strong results at age 16, for whatever reason. Alternatively, it could be that their expectations are more sensitive to the results that they receive. My final models aim to shed light on this question.

I first examine whether KS4 results do have an association with changes in young people's expectations of applying to university. I report the results from M4 in Table 3.5 in order to check for unexpected changes in the main effects. Given the likely endogeneity of performance at age 16, estimates from M3 are likely to be a better guide to the 'conditional' association between SES and the hazard of transition, although there are only slight changes in practice. For parsimony and ease of interpretation, at this point I switch to use of models in which SES is measured using the index variable defined in Section 3.3.3. Comparing the results of M4 (final column of Table 3.5) and M4C (first column of Table 3.6) suggests that this simplification does not seem to have much of an effect on other variables in the model. However, the main coefficient here is on the KS4 performance variable, which unsurprisingly shows that a one standard deviation improvement in results at age 16 are associated with a having approximately a 20% reduction in the hazard of moving from reporting 'likely to apply' to reporting 'unlikely to apply'.

Results from M5C, in the second column of Table 3.6, then provides evidence on the question of differing responsiveness of young people to age 16 exam results. The estimate reported in the interaction row of Table 3.6 should be interpreted as the additional expected change in the hazard ratio associated with a one standard deviation increase in KS4 scores when the individual in question is one standard deviation further up the SES distribution. As I do find a statistically significant estimate for this interaction term, this suggests that young people's SES background does affect how likely they are to adjust their expectations downwards when faced with a similar set of KS4 results. Specifically, the hazard ratio of 0.79 shows that, in general, young people from more advantaged backgrounds are less likely to respond to poorer results by lowering their expectations of applying to university.[30]

Table 3.7: Estimated hazard ratios of transition from reporting being unlikely to apply to reporting being likely to apply by quintiles of socioeconomic status

	M0	M1	M2	M3	M4
Age 16	0.88	0.88	0.90	0.91	0.90
	(-2.28)**	(-2.30)**	(-1.80)*	(-1.72)*	(-1.86)*
Age 17	0.63	0.63	0.63	0.64	0.76
	(-7.90)***	(-8.13)***	(-7.90)***	(-7.61)***	(-4.48)***
SES Q1 (Low)		0.76	0.70	0.79	0.81
		(-3.80)***	(-4.28)***	(-2.78)***	(-2.57)**
SES Q2		0.89	0.88	0.91	0.91
		(-1.75)*	(-1.83)*	(-1.43)	(-1.38)
SES Q4		1.29	1.25	1.16	1.15
		(3.42)***	(3.05)***	(2.00)**	(1.87)*
SES Q5 (High)		1.94	1.92	1.71	1.67
		(7.76)***	(7.68)***	(6.25)***	(5.99)***
Significance of SES ($P > \|F\|$)		0.00	0.00	0.00	0.00
N	5,330	5,330	5,330	5,330	5,330
Variables	**M0**	**M1**	**M2**	**M3**	**M4**
Age	$\surd$	$\surd$	$\surd$	$\surd$	$\surd$
SES Quintile Dummies		$\surd$	$\surd$	$\surd$	$\surd$
Demographics & School			$\surd$	$\surd$	$\surd$
Prior Attainment				$\surd$	$\surd$
Age 16 Attainment					$\surd$

Notes: Reporting hazard ratios. $P > |F|$ shows p-value from joint significance test of the hypothesis that exponentiated coefficients on all SES group dummies in the underlying conditional log-log regression model are equal to 1. Adjusted using LSYPE-provided Wave 2 survey design and non-response weights. T-statistics of the null hypothesis that the hazard ratio is equal to one, based on standard errors clustered by individual's school, are reported in parentheses. Estimated risks are relative to base categories of Age 15 and SES quintile group 3.

3.7.2 From unlikely to likely

I now turn to the transition back from being ‘unlikely to apply’ to being ‘likely to apply’. I report the results in Table 3.7, concentrating again just on the association between young people’s SES quintile group and the hazard of young people raising their expectations. As remarked above, it may well be the case that the relationship explaining the likelihood of transition from ‘unlikely to likely’ is quite different from that explaining ‘likely to unlikely’; this could be in terms of different significant factors, different directions of effects and different strengths of relationships. However, this is not the case for the unconditional relationship between young people’s age and the hazard that they make a transition from ‘unlikely to likely’ (in M0): as with the opposite transition, as individuals get older they appear to become less likely to switch, albeit more dramatically by age 17.

Turning to SES (in M1), once again there is a large gradient in young people’s chances of making a transition depending on their relative advantage. In this case, young people from more advantaged backgrounds have a greater hazard of making a transition from reporting ‘unlikely’ to reporting ‘likely’. Individuals from the most advantaged quintile group of the SES index have more than 2.5 times the hazard of making a transition as their counterparts in the least advantaged fifth of the distribution. This is a large difference, although not as large as the difference between these groups in the hazard of moving from ‘likely to unlikely’, where the unconditional hazard ratio was greater than four. However, as with the inverse transition, will this apparent influence of SES be reduced when I add further covariates?

The additional covariates in M2 do nothing to reduce the association between SES and the hazard of making a transition from ‘unlikely to likely’. The hazard ratios barely change for any of the quintile groups of SES. Coefficients on some of the variables added at this point (reported in Table B.2 of Appendix B.1) suggest large and significant relationships with the hazard of transition: in particular young people who from ethnic minorities and young women are much more likely to switch to being ‘likely to apply’. However, the results suggest that these are largely independent of SES and/or cancel one another out.

On the other hand, controlling for prior attainment does more to explain the SES influence on young people’s chances of changing their minds from ‘unlikely to likely’, particularly

[30] I do also estimate separate versions of this model using dummy variables for quintiles of SES. While the results from this model suggest that a linear relationship is unlikely to provide the best fit, a joint test of the interaction terms still suggests that the overall form of the relationship reported in Table 3.6 is robust.

at the more advantaged end of the SES distribution. Nevertheless, a large SES gradient remains, with individuals in the top quintile group of the SES index having more than twice the hazard of moving from ‘unlikely’ to ‘likely’ as peers in the bottom group. The most advantaged fifth of the sample remain outliers from the rest of the distribution: their hazard of transition is almost fifty percent higher than in the quintile group just below them.

In contrast to the results for ‘likely to unlikely’, the coefficients on whether an individual attends an independent school, a grammar school, or a school with a sixth form (reported in Appendix B) are not statistically significant. However, it would appear that in the former two cases this is due to there only being a very small number of such individuals in the sample on which models of the transition from ‘unlikely to likely’ are estimated: there are very few individuals from independent or grammar schools who ever report being ‘unlikely to apply’ to university during this period.

Another noticeable difference between the two directions of transition is that, in contrast to the model of ‘likely to unlikely’, even inclusion of young people’s prior attainment in the model of ‘unlikely to likely’ does not fully explain the role of age: the coefficient on age 16 becomes only significant at the 10% level, while the coefficient on age 17 remains highly significant. One explanation for this is that, while it’s never too late to decide against making an application to university, it can get too late for individuals to start thinking that they will. If they have not been planning to apply to university, young people will not have taken key actions necessary in order to be in a position to make a competitive application. Arguably this is closer to a duration effect than an age effect, being picked up by the age variables due to the absence of duration parameters: it is less likely to be present for young people who only spend a single period reporting being ‘unlikely to apply’, for example.

In summary, as with the transition from ‘likely to unlikely’, there remains a large, statistically significant relationship between young people’s socio-economic advantage and the likelihood that they move into thinking they are ‘likely to apply’.

Again, the question arises of whether young people from less advantaged backgrounds are responding differently to new information on their academic attainment. Specifically, in this case, the hypothesis that may partially explain the growing inequality in expectations is that individuals from lower SES backgrounds are less responsive to just as promis-

Table 3.8: Estimated odds ratios of transition from reporting being unlikely to apply to reporting being likely to apply by interaction of socio-economic status and new information on attainment at age 16

	M4C	M5C
Age 16	0.88	0.88
	(-1.92)	(-1.94)
Age 17	0.75	0.73
	(-4.02)	(-4.29)
SES Z-Score	1.34	1.35
	(7.20)	(7.34)
KS4 Z-Score (After results)	1.84	2.06
	(8.32)	(8.05)
SES * KS4		1.22
		(2.32)
N	5,330	5,330

Variables	**M4C**	**M5C**
Age	√	√
SES Index Z-Score	√	√
Demographics & School	√	√
Prior Attainment	√	√
Age 16 Attainment	√	√
Age 16 Attainment and SES Interaction		√

Notes: Reporting hazard ratios. Adjusted using LSYPE-provided Wave 2 survey design and non-response weights. T-statistics of the null hypothesis that the hazard ratio is equal to one, based on standard errors clustered by individual's school, are reported in parentheses. Estimated risks are relative to base category of Age 15.

ing new information at age 16 as peers with similar prior academic attainment from more advantaged homes. As with the transition from 'likely to unlikely', I switch at this point to use of a continuous measure of SES. As such, in Table 3.8, the estimate reported in the interaction row (SES * KS4) reports the additional expected change in the risk of transition associated with a one standard deviation increase in KS4 scores when the individual in question is one standard deviation further up the SES distribution.

Indeed, the results do suggest differential sensitivity to new information on academic performance may be important in explaining the observed changes in expectations. There is a statistically significant hazard ratio of 1.29 associated with the interaction term,[31] suggesting that individuals with the same age 16 performance but with more advantaged parents are more likely to revise their expectations in light of better academic results at age 16.

3.8 Conclusions

In this chapter I have investigated how young people's expectations of applying to university change between age 14 and age 17, just before individuals start making applications. My findings confirm that this is a period when many young people do change their expectations of applying to university. They also highlight that this change is not just from being 'likely to apply' to being 'unlikely to apply', but rather runs in both directions.

While young people across the socio-economic status distribution start their adolescence with high educational expectations, those from less advantaged backgrounds are much more likely to revise their expectations downwards and much less likely to raise their expectations during this period. This relationship persists even once I control for many other factors correlated with SES and, perhaps most notably, young people's prior academic attainment. The least advantaged fifth of young people have more than twice the chances of switching from reporting being 'likely to apply' to reporting being 'unlikely to apply' as the most advantaged fifth, conditional on prior attainment. Conversely, the most advantaged fifth of young people have more than twice the chances of changing from reporting being 'unlikely to apply' to reporting being 'likely to apply' as the most

[31]As with the model from 'likely to unlikely', the results from a separate model model where I use dummy variables for quintile groups of SES suggest that a linear relationship is unlikely to provide the best fit. Nevertheless, in a model in which dummy variables are used, a joint test of the interaction terms suggests this finding is robust.

advantaged fifth, again conditional on prior attainment.

In Chapter 2 I found that much of the socio-economic gradient in access to university opened at or before the point of application. This chapter builds on this, finding that a substantial portion of this socio-economic gap in university applications opens between ages 14 and 17. A positive implication of this is that it is not too late to target policies, both to maintain and to raise educational expectations, at bright individuals from less advantaged backgrounds during this period of their lives. However, of the two, raising expectations of applying to university may be less effective than maintaining expectations and becomes increasingly difficult as individuals get older.

I also find some evidence that young people from differing SES backgrounds react differently to new information on their academic attainment at age 16. This differential is also asymmetric, helping to explain the growth in inequality of expectations: more advantaged young people are less responsive to results in lowering their expectations, but more responsive to results in raising them. After these exam results is a difficult point in time to reach young people, as many move between educational institutions or leave full time education altogether. However, it may be the case that providing fresh guidance in the light of the results is very important in ensuring young people's educational expectations are appropriate.

Chapter 4

The impact on socio-economic and gender inequalities of using an aptitude test as part of the admissions process at an elite university

4.1 Introduction

Having considered the emergence of socio-economic inequalities in the years running up to making an application in Chapter 3, this chapter now turns to take an in depth look at one aspect of the admissions process itself, specifically those at a highly selective Russell Group university. As we saw in Chapter 2, university applicants from the bottom income quintile group are almost 20 percentage points less likely to attend a Russell Group institution than those from the top income quintile group.

In particular, this chapter considers the increasing use of aptitude tests as part of the admissions processes at elite universities in England, which potentially has significant implications for fair access to these institutions. While the intention is to improve the efficiency of the process, making it easier to select individuals with a better 'aptitude' [1] for their university course, is this efficiency gain traded off against other aims of the admissions process? In particular, previous research suggests there are reasons to think ap-

[1]'Aptitude' is taken broadly as a measure of potential attainment, as against prior attainment such as measured by A Levels or GCSEs, or innate ability.

titude testing may have side effects on the proportion of applicants from different socio-economic backgrounds (Rothstein, 2002) and different genders (Tannenbaum, 2012) who get a place.

To explain this concern, let us take the example of fair access by socioeconomic status. There are at least two potential reasons that the introduction of an aptitude test could result in a smaller intake of those from lower socioeconomic backgrounds. First, the outcomes of the test could reflect skills acquired in previous education, hence skewing the distribution of those offered a place towards those who received certain kinds of schooling, or training to the test, both of which might be of concern (Stringer, 2008). Alternatively, it could reflect genuine differences in aptitude for the university's degree programme across the socioeconomic spectrum. However, there are also reasons to see the possibility of the opposite effect as a result of the introduction of an aptitude test, with more offers of places made to those from less advantaged backgrounds. If more weight is given to aptitude test results over and above school examination results then this could help overcome bias in those indicators caused by schooling rather than underlying ability. This chapter aims to identify which, if either, of these effects seems to dominate and hence understand the wider impact of using aptitude tests as a selection tool.

In 2007-2008, the University of Oxford, an elite British university, introduced an aptitude test as part of the admissions process for Economics-related subjects. The test, named the Thinking Skills Assessment, was intended to assess critical thinking and problem solving skills, seen as useful for predicting aptitude for these courses at the university.[2] I use administrative data from the University's admissions system, covering all undergraduate applications, to estimate the differential impact of the introduction of this test on applicants by their socioeconomic backgrounds and their gender. I employ a difference in differences framework: this attempts to control for any general trends in the proportion offered an interview and the proportion admitted using those seen in subjects where the aptitude test was not introduced, hence isolating the impact due to the policy change.

The chapter proceeds as follows. In Section 4.2, I survey the literature on access to elite universities, identify important details about the use of aptitude tests in university admis-

[2]In Appendix D I give further details of the Thinking Skills Assessment and reproduce a number of questions from the specimen paper.

sions, and lay out the research questions for this chapter. I then detail the admissions process at the University of Oxford in Section 4.3 and describe the data used in this work in Section 4.4. Section 4.5 describes the changes in admissions during the period analysed and identifies the particular features of the change in policy. It then lays out the empirical strategy for identifying the changes in outcomes that seem to be associated with its introduction and presents simple estimates of impact. I extend this using regression analysis, describing my models in Section 4.6 and presenting the results in Section 4.7. I consider an alternative way of looking at the results in Section 4.8 and conduct various robustness checks in Section 4.9, before concluding in Section 4.10.

4.2 Previous research and research questions

Why take an interest in the admissions processes of elite universities, and the introduction of an aptitude test in particular? I consider these questions in turn.

Given the higher wage premiums graduates from elite universities seem to command (Chevalier and Conlon, 2003), fair access to these institutions is important to future equality of opportunity. Furthermore, one cannot necessarily rely on insights about fair access to all universities to understand inequalities at elite universities; Pallais argues that "it is entirely plausible that barriers to enrollment at the most selective institutions are somewhat different than at the margin of enrollment" (Pallais and Turner, 2008, p.132) and as such the correct policy response may well be different.

The current UK government's belief is that "progress over the last few years in securing fair access to the most selective universities has been inadequate, and that much more determined action now needs to be taken" (Willetts, 2011). Previous research from both the UK and the US has highlighted concern about the equality of opportunity in access to elite Higher Education institutions. In Chapter 2 I showed that, among young English people who do attend university, those from the bottom income quintile group are almost 20 percentage points less likely to attend a Russell Group institution (a group of elite UK universities) than those from the top income quintile group. Similarly, analysis by Boliver (2013) highlighted that Russell Group applicants from state schools are less likely to receive offers of admission from Russell Group universities in comparison with their equivalently qualified peers from private schools. Such concerns also exist in the US: "Less than 11 percent of first–year students matriculating at 20 highly selective institutions were from the bottom income quartile of the income distribution" (Pallais and Turner, 2006, p.357).

Specifically regarding the University of Oxford, Bhattacharya et al. (2012) use administrative data from one undergraduate programme to estimate the expected performance of the marginal admitted candidate by sex and school type, arguing that in an academically fair process this threshold for admission would be equal between such groups. However, they estimate that the expected performance of the marginal candidate from an independent school is approximately 0.3 standard deviations higher than their state school counterpart. Similarly, the expected performance of the marginally admitted male candidate is about 0.6 standard deviations higher than their female counterpart. Contrary to much evidence this suggests that, at the margin, increasing the number of male entrants and those from independent schools would increase expected degree performance of the intake.

Aptitude testing has become a much more important issue in recent years. As more students have begun to reach the upper bound of performance in A Levels (examinations taken by most English students aiming for entry to Higher Education, usually at age 18) it has become harder for universities to differentiate between potential students at the top end of the ability distribution.[3] This has led to an increasing use of aptitude tests among elite institutions, including the BioMedical Aptitude Test and United Kingdom Clinical Aptitude Test for admission to medical courses at many universities; the Physics Aptitude Test, at the University of Oxford; and, the focus of this chapter, the Thinking Skills Assessment at the University of Oxford, the University of Cambridge and University College London (Admissions Testing Service, 2013b);. However, an important question is whether this response is a sensible course of action, especially in the light of the inequalities discussed above.

Taking aptitude as a measure of potential ability in a given field, then aptitude tests should be effective at predicting the performance of candidates once they reach university and should do so without being biased by candidates' other characteristics. Unfortunately, McDonald et al. (2001b) find little evidence that the Scholastic Aptitude Test (SAT) predicts attainment once at college in the US any better than high school record alone. These

[3]This analysis covers the period before the introduction of the new A* grade for A-Levels, which has ameliorated this problem to some extent.

findings were replicated in a pilot study in Britain (McDonald et al., 2001a), which does have a very different institutional setting, most notably nationally comparable end of school examinations. A more recent Department of Business, Innovation and Skills (BIS) report comes to similar conclusions, arguing that the SAT does not provide significantly more information on applicants' likely performance at undergraduate level, relative to a baseline of GCSE (English school examinations taken at the end of compulsory education) attainment scores (Kirkup et al., 2010, p.20).

On the question of bias in aptitude test scores, the fact that "low-income students not only are less likely to take college placement tests but also tend to have lower scores on these exams" (Pallais and Turner, 2008, p.135) suggests, on the face of it, that aptitude testing could cause more harm than good. In addition, Pallais and Turner (2008) note that the "gap [in aptitude tests between low and high income students] is particularly marked at the top of the distribution from which elite colleges and universities are likely to draw students", which means that, even if aptitude testing becomes commonplace among HE institutions of all kinds, its effects remain particularly pertinent to elite universities.

There have long been concerns about gender differences on performance in aptitude testing in the US (Linn and Hyde, 1989) and, while finding differences in scores by socio-economic status or gender does not necessarily imply bias (Zwick, 2007, p.20), McDonald et al. (2001b) do identify specific evidence of biases in the SAT, in the US, with "consistent evidence that [it] under-predicts female attainment" once they get to university and more mixed evidence on bias by ethnic groups. Similarly, Wikström and Wikström (2014) present evidence from Sweden that, on average, females perform worse than males in the SweSAT (a national university admissions test), while the opposite is true in measures based on their performance at school. Tannenbaum (2012) argues that one reason for these findings is differing gender styles in test taking, analysing in particular the SAT and differing attitudes to risk.

Although these analyses cannot necessarily be extrapolated to the Thinking Skills Assessment, no analysis that I am aware of evaluates whether its predictive power is significantly higher than a baseline of school examination results, nor whether there is evidence of bias in its assessments. The research that has been done specifically into the Thinking Skills Assessment has been restricted to simple analysis of predictive validity with no baseline. Research by Cambridge Assessment (the developers and administrators of the test) sought to examine the extent to which the TSA could predict future academic performance (Emery, 2006; Emery et al., 2006). This was conducted using data from the University of Cambridge courses in Computer Science, Economics, Engineering and Natural Science for students who took the TSA in 2003. As is standard practice in evaluating the predictive validity of selection tests, this involved calculating correlations between TSA score and subsequent academic outcomes. In particular, the research finds a correlation between higher marks in the TSA and higher marks in first year university examinations; strong similarities in the candidates that would be rejected by a low TSA cut off score and those rejected under the present selection system; and higher mean TSA scores among those gaining higher degree classification marks in the same examinations.

The authors also state that the correlations, some (but not all) of which are statistically significant, are likely to be an underestimate of the true predictive power since they do not include those who were unsuccessful in getting a place at the university. However, there are potential problems in some of the analysis done because of the data they were able to work with. Rather than having any data where the TSA was administered but not used for selection, the TSA was already in use in the selection process (Emery et al., 2006, p.13). This means that care should be taken in interpretation, especially of the distributions suggesting similarity between those who would be rejected by a TSA cut off and those rejected by the original selection methods.

With rather limited evidence on predictive validity, one should also consider the wider consequences of introducing an aptitude test. McDonald et al. (2001b, p.53) highlight the importance of this, and draw on the concept of 'consequential validity' (Messick, 1989, p.8). This refers to the wider consequences of introducing the test on other aspects of the admissions process. In this context, one might expect to see a reduced focus on the other information about a candidate that an admissions tutor has: use of aptitude testing may reduce focus on a candidate's examinations results. This might have positive consequences, given known socioeconomic gradients in attainment in such exams. However, that is only the case if the alternative provides a fairer assessment of candidates' ability.

'Consequential validity' also refers to responses to the use of aptitude testing outside the admissions process itself. For example, Wilmouth (1991) argues that students might spend increased time preparing for aptitude tests and less on their academic studies (cited in McDonald et al., 2001b, p.54). This could have a negative knock-on effect on individuals' academic attainment, both in the short term and on their attainment at university. Similarly, Geiser (2008) argues that the education system should reward individuals who work hard throughout their school careers, attaining highly as a result; aptitude testing may incentivise bright individuals to work less hard at achieving high levels of attainment, if they believe they can be successful in gaining access to higher education simply by doing well on a test supposedly designed to assess innate skills.

This chapter contributes to the literature by providing evidence on the consequences of aptitude testing for applicants to an elite British university. Given concerns about bias in scores on aptitude tests (Zwick, 2007, p.20) I pay particular attention to these issues, with the chapter's research questions as follows:

1. Does use of the TSA have an effect on the proportion of applicants called to interview, the proportion of applicants offered a place, or the proportion of interviewees offered a place?
2. Do these impacts differ for high and low socioeconomic status applicants?
3. Do these impacts differ for female and male applicants?

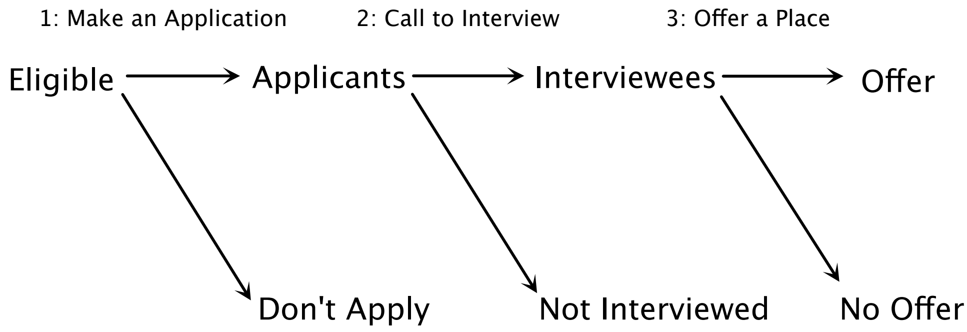
4.3 The admissions process

Unlike at some British universities, the admissions process at the University of Oxford consists of more than one stage, with a shortlist of candidates invited to interview before final admissions decisions are taken. I show the basic form of the admissions process graphically in Figure 4.1, highlighting three key decision points that make up the process. First, individuals choose whether to apply to Oxford; second, the University chooses which applicants to call to interview[4]; and third, the University chooses whether to offer interviewees a place. Since I am using administrative data from the University (which I will describe further in Section 4.4), I can analyse the latter two decision points, but not the first.

Referring back to the idea of 'consequential validity' of using an aptitude test, and the potential for wider societal effects of its introduction, an important part of the story is the impact of the introduction of the TSA on who applies to Economics courses at the

[4]Starting in 2009, the University introduced use of contextual data in selection to interview across all subjects. Qualified applicants with various combinations of 'flags' (indicating more challenging circumstances based on prior education and area-based measures) are strongly recommend for interview (University of Oxford, 2014).

Figure 4.1: Simplified model of the admissions process



University of Oxford. Unfortunately, the data available do not allow for the proportion of young people who choose to apply to be modelled since potential applicants are not observed by the university. In any case, the denominator is rather poorly defined. Do we really want to consider the proportion of *all* young people of this age who apply, or restrict attention to a subset of 'eligible' applicants? If the latter, whom should we regard as an eligible applicant? However, without addressing this matter we might be ignoring significant effects of the policy change. I return to this issue in Section 4.8.

Thus far, I have described the decision to call candidates to interview, and whether ultimately to offer them a place, as being made by 'the University'. However, to understand who actually makes the decisions it is important to understand the unusual way admissions are organised at the University of Oxford. The University is made up of more than 30 different, fairly autonomous, 'colleges'. Much undergraduate teaching occurs within these colleges, rather than at university level, although students at all colleges, on the same course, study towards the same degree examinations. It is usually one or more of the members of staff who undertake this undergraduate teaching within a college who decide which applicants to invite to interview and, subsequently, which to offer places to. For this purpose, they are referred to as 'admissions tutors'.

A college's admissions tutors' decision over whether to admit an individual is final: University departments cannot overrule college decisions. Most applications for undergraduate courses are made to colleges. However, some individuals do make open applications (which are not to any particular college); these are allocated to a college with a lower applications to places ratio and then proceed on the same basis.

It is worth noting that applicants receiving an offer do not necessarily receive that offer from the college they applied to. The aim of the reallocation process is to ensure that the number of applicants considered by a college is proportional to the number of places available there. Those who are reallocated to other colleges are usually more marginal applicants (since colleges have first refusal on those applicants who apply to them). Under 25% of successful applicants are reallocated, with it being somewhat less likely among Economics applicants. The college an individual applies to (or is allocated to if they make an open application) and the college an individual receives an offer from are both recorded in the dataset.[5]

All colleges that admit undergraduates admit Economics students.[6] However, the proportion of applicants for Economics and the proportion of offers going to Economics applicants at each college vary greatly (and do not necessarily track one another directly). For example, at the top end, one college received 6.1% of applications to Economics and hosted 8.1% of the university's Economics undergraduates. At the other extreme, one college received just under 1.5% of Economics applications, and went on to host 1% of the university's undergraduate economists.

4.4 Data

I use administrative data from the University of Oxford covering undergraduate admissions made in the years 2005 to 2010. The dataset includes information on all applications to undergraduate courses. This includes applications to Philosophy, Politics and Economics (PPE) and Economics and Management (E&M), the University of Oxford's two main undergraduate degrees in Economics and the subjects for which the aptitude test was introduced; applications to these two courses make up 11% of total applications to Oxford during this period (see Table 4.1). Throughout the chapter I refer to these two courses as Economics, for convenience (although I do explore potentially important differences at various points during the chapter).

The progress of applicants through the admissions process is recorded comprehensively

[5]I test the robustness of my results to these more marginally accepted candidates by treating these individuals as not having received an offer. In relevant models this does reduce the absolute size of differences and hence statistical significance, but does not materially alter the findings.

[6]I exclude the very small Permanent Private Halls (PPHs), some of which do not offer Economics, and a college that only accepts mature students (mature students do not have a school affiliation, so we are missing our limited measure of SES). Without exclusion these colleges would produce a missing value in proportions of applicants in certain circumstances, resulting in inconsistent sample sizes.

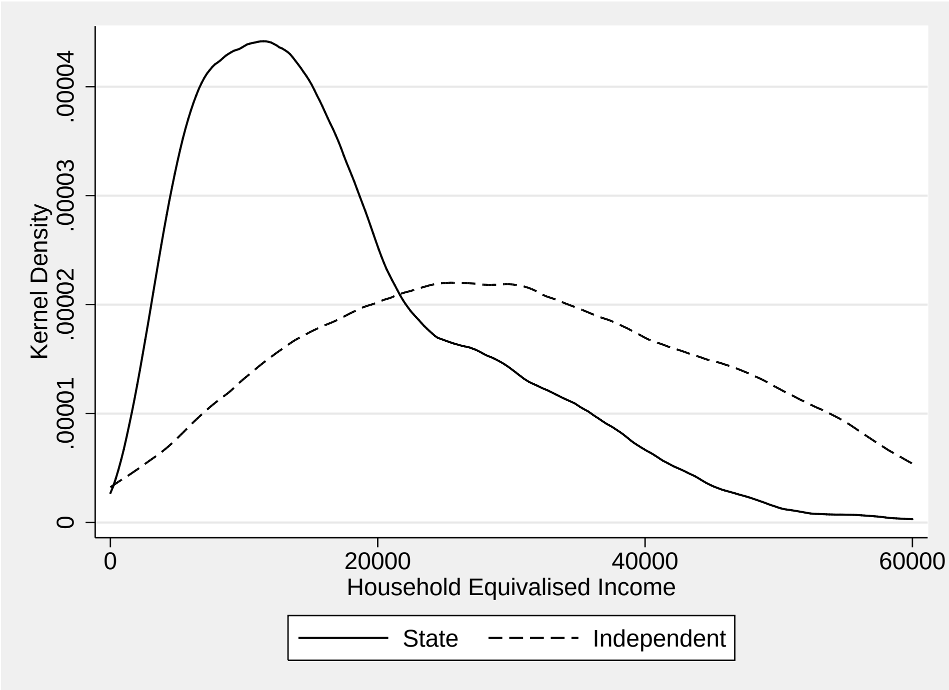
in the dataset, tracking the individuals who apply, whether they are called to interview, and ultimately whether they are offered a place at the University. Other than details on an applicant's successes or failures (discussed in Section 4.3), the available data from the process is relatively sparse: it includes their gender, school type (i.e. independent or state), school postcode (which may be linked to data on area level deprivation), and their qualifications, with which to attempt to understand the additional effects attributable to the TSA. Coming from administrative data collected as part of the admissions exercise, the dataset does not include information on the performance of successful individuals once they have been admitted.

Likewise, as its purpose is to summarise all undergraduate admissions, the dataset does not include information on aspects of the process which are course-specific. Notably, for the purposes of this chapter, this means there is no data on individuals' performance in the TSA itself. In any case, this would not, of course, be available for Economics applicants in years prior to its introduction, or for non-Economics applicants in any year. Hence, test scores would not be of use as part of a difference in differences approach to estimating the impact of the introduction of the TSA. While differences in TSA performance between different groups may be part of the explanation for the results, this is beyond the scope of this chapter.

To answer my research questions, I need a proxy for socioeconomic status. Unfortunately, the dataset includes no information on individuals' family backgrounds.[7] I use the variable indicating whether an individual applicant attended an independent school, a state school or neither of these at time of application. I use school type as a proxy for socioeconomic status in this way because of the correlation between the two: in the UK independent schools are primarily fee-paying schools, catering for those from affluent backgrounds. The remainder of the population attends state schools, where funding is provided by the government either through Local Authorities (sometimes referred to as maintained schools) or, increasingly, direct to the schools (which are known as academies). While only about 18% of those in education between the ages of 16 and 18 attend an independent school (Department for Education, 2010), 38% of applicants observed in the dataset are from independent schools.

[7]Applications to UK universities are made through the Universities and Colleges Admissions Service (UCAS). As part of this process, individuals are asked to provide information on their ethnic origin, parental education and occupational background. However, these questions are not compulsory. In any event, any responses are not provided to the institutions to which the individual has applied (except in aggregate, and at a later date). As such, they do not form part of this dataset.

Figure 4.2: Kernel density estimate of the distribution of household equivalised income among young people who apply to university, by whether the young person attends independent school



Notes: Calculations based on data from the Longitudinal Study of Young People in England. Independent school status measured at age 14. Equivalised household income measure constructed as per Chapter 2, specifically equivalised by dividing by square root of household size.

Attending an independent school does correlate with individuals' socioeconomic status. Using data from the Longitudinal Study of Young People in England (LSYPE), specifically the same income measure constructed in Chapter 2, I estimate that median household equivalised income for university applicants from state schools is about £14,800, while for those attending an independent school it is just over £31,000.[8]

However, there are drawbacks compared to other measures. First, it is a very blunt instrument, providing us with only a binary indicator of status. Second, it proxies socioeconomic status with error: as can be seen from Figure 4.2 there is large overlap in the distributions of household income in households where a teenager is at independent or state school. There will be many reasons for this; for example, in more affluent areas or where schools are selective, more young people from richer backgrounds will attend state schools. Furthermore, in the other direction, individuals from poor backgrounds may attend independent schools, for example supported by bursaries. On the other hand, use

[8]The LSYPE's measurement of school type is based on a combination of administrative and survey data from approximately age 14. It would be better to measure at age 17 or 18, since a greater proportion of the school population are in independent schools for the two post-compulsory education years leading up to university (about 17.5% vs. 7%). Unfortunately, this is not available: it would make the difference in average income less stark, but would be extremely unlikely to eliminate it.

of independent school status does have an intuitive appeal. It is both an instantly interpretable distinction and is often the basis for targets regarding fair access that universities negotiate with the UK Government's Office for Fair Access.

The data also include the post codes of the schools that individuals are currently attending (or attended the previous year in the case of applicants who apply shortly after leaving school). By linking with the Income Deprivation Affecting Children Index (IDACI) I attempt to achieve a more nuanced picture of the individual's SES from their school's neighbourhood. IDACI "is expressed as the proportion of all children aged 0-15 living in income deprived families" (McLennan et al., 2011, p.22-23). This too will proxy socioeconomic status with error: for example, some schools in deprived neighbourhoods may still attract children from affluent families. However, using another dataset, I show that school IDACI is weakly correlated with an individual's socioeconomic status (see Appendix E.1).

For the purposes of this analysis I exclude all overseas applicants; those who apply without school affiliation (primarily mature students); and those affiliated to schools where the school type is unavailable for some other reason (about 2% of UK applicants). 63,986 UK applicants for whom details about school type are observed remain in the dataset.

Academic attainment of applicants will clearly be an important factor in admissions to any university. In England, the majority of universities use applicants' performance in 'AS Levels', which are exams taken at around the age of 17, one year into post-compulsory education. In addition, most offers of places will be conditional on applicants achieving a particular set of results in 'A Levels' (these build on AS Levels and are taken two years into post-compulsory education): at the University of Oxford this is typically achieving 3 A-Levels at grade A (the maximum). However, among applicants for courses at Oxford there is very little variability among results in either of these qualifications, with most applicants achieving top grades.

As a result, applicants' performance in General Certificates of Secondary Education (GCSEs) is taken into consideration. In England, these are the predominant examinations taken at the end of compulsory education, usually while individuals are aged 16. In the dataset, I observe the number of GCSEs that applicants have passed and the number of GCSE A*s (the maximum possible grade) that they achieved. As would be expected, GCSE performance differs significantly between applicants, interviewees and those offered a place: the number of GCSE A*s an applicant holds is a good predictor of selection to interview and for an offer.[9]

Table 4.1: Summary statistics of applicants by their school type

Variable	Overall	Independent	State
Proportion getting an Interview	0.72	0.79	0.68
Proportion getting an Offer	0.26	0.30	0.23
Proportion of Interviewees getting an Offer	0.36	0.38	0.34
Proportion applying to Economics	0.11	0.12	0.10
Mean No. of GCSEs passed	10.28	9.99	10.46
Mean No. of GCSE A*s	6.15	7.01	5.63
N	63986	24470	39516

Notes: Individuals for whom school type is unknown are excluded. Standard errors suppressed as all ≈ 0.

Table 4.2: Summary statistics of applicants by their gender

Variable	Overall	Female	Male
Proportion getting an Interview	0.72	0.72	0.72
Proportion getting an Offer	0.26	0.24	0.27
Proportion of Interviewees getting an Offer	0.36	0.34	0.37
Proportion applying to Economics	0.11	0.07	0.14
Mean No. of GCSEs passed	10.28	10.29	10.28
Mean No. of GCSE A*s	6.15	6.48	5.85
N	63986	30985	33001

Notes: Individuals for whom school type is unknown are excluded. Standard errors suppressed as all ≈ 0.

Applicants from independent schools have different observable characteristics, on average. For example, Table 4.1 shows that they receive on average fewer GCSEs. While this may seem counter-intuitive, independent schools may encourage their pupils to take slightly fewer GCSEs to maximise performance on those they do take. Indeed, applicants from independent schools have more GCSEs awarded A*s (the highest grade). In addition, a larger proportion of independent school applicants apply to Economics than do state school applicants. Likewise, there are observable differences, on average, between male and female applicants (Table 4.2). Female applicants are just as likely to get an interview, but less likely to receive an offer. This is despite having a statistically significantly higher mean number of GCSEs awarded A*s than their male counterparts. They are also half as likely to apply to Economics as male applicants.

Less obviously, admissions statistics and average attainment of applicants also differ significantly by course choice. Table 4.3 shows summary statistics for the two groups, Economics and all other subjects. It shows us that Economics applicants are already less likely

[9] Using a simple linear probability model containing only the number of GCSE A*s held by a candidate as a continuous regressor, I estimate that each additional GCSE A* increases a candidate's probability of being offered a place by approximately 4.6 percentage points. The t-statistic on this coefficient is 83.3 and the overall model has an R^2 of 0.10. I get very similar results with a linear probability model of selection to interview.

Table 4.3: Summary statistics of applicants by subject group applied to

Variable	Overall	Economics	Others
Proportion getting an Interview	0.72	0.69	0.72
	(0.00)	(0.01)	(0.00)
Proportion getting an Offer	0.26	0.22	0.26
	(0.00)	(0.00)	(0.00)
Proportion of Interviewees getting an Offer	0.36	0.31	0.36
	(0.00)	(0.01)	(0.00)
Proportion from Independent school	0.38	0.44	0.38
	(0.00)	(0.01)	(0.00)
Proportion who are female	0.48	0.33	0.50
	(0.00)	(0.01)	(0.00)
Mean No. of GCSEs passed	10.28	10.26	10.29
	(0.01)	(0.02)	(0.01)
Mean No. of GCSE A*s	6.15	6.33	6.13
	(0.01)	(0.04)	(0.01)
N	63986	6904	57082

Notes: Individuals for whom school type is unknown are excluded. Standard errors in parentheses.

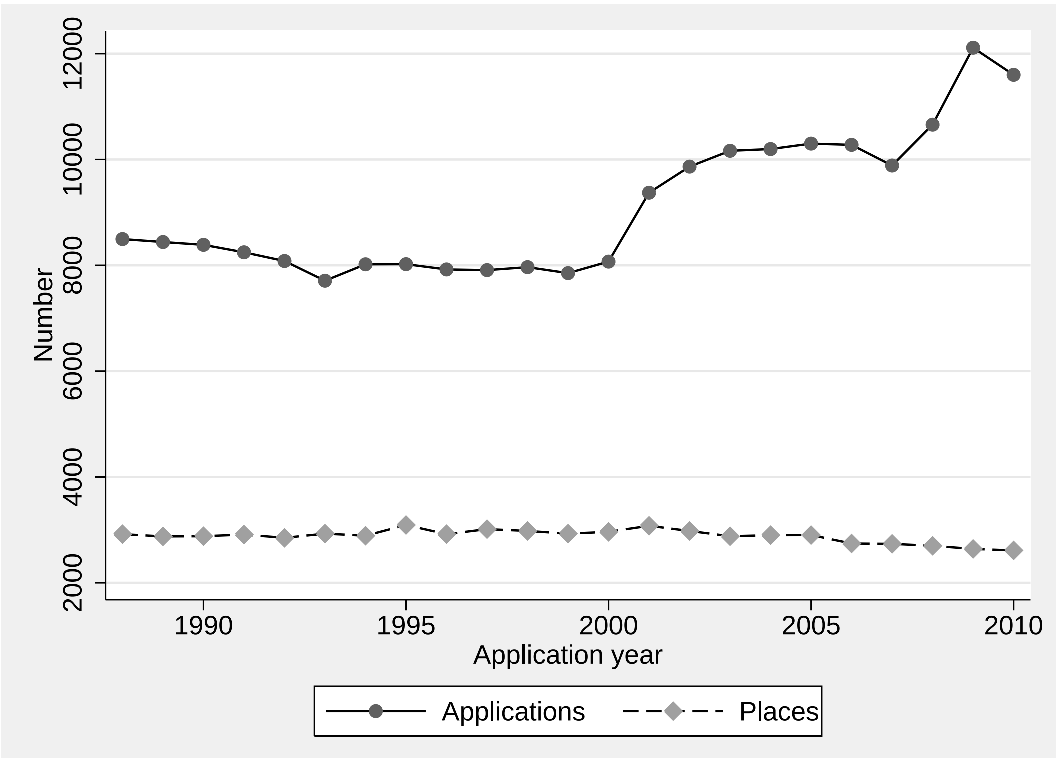
to get an interview than other subjects, and are less likely ultimately to receive an offer (these differences are statistically significant). The supply of places is effectively fixed: as the proportion getting an offer is driven by differences in demand there is no particular reason to expect the proportions to be the same across courses. In addition, there is a larger proportion of applicants from independent schools for Economics. Importantly for this work, applicants for Economics have, on average, statistically significantly fewer GCSEs but more A* grades achieved than applicants for other subjects, again on average. This suggests GCSE performance may be a particularly important predictor for Economics, relative to other subjects: I attempt to mitigate this potential problem for my estimation strategy by controlling for GCSE performance using least squares regression as part of my analysis.

Given their importance in the admissions process, it is also important to consider the differences between colleges. Within the University of Oxford, colleges have differing academic reputations. It seems plausible that this may affect the quality of applicants to, and selectivity of, individual colleges. The University-produced 'Norrington score' may capture some of this. According to the University website it "provides a way of measuring the performance of students at each college in the end of university exams" (University of Oxford, 2013). The Norrington score is based on the classifications of undergraduate degrees awarded, attaching a score of 5 to a first class degree, 3 to an upper second class degree, 2 to a lower second class degree, 1 to a third class degree and 0 to a pass. It is calculated by dividing the total college score by the total possible score the college could attain and multiplying by 100 to yield a percentage. I assign each college's Norrington score to the group of applicants in the autumn following the examinations on which the score is based. This means that it will be the most recent piece of information on college quality that applicants and interviewers will have.

4.5 Trends in admissions and introduction of the TSA

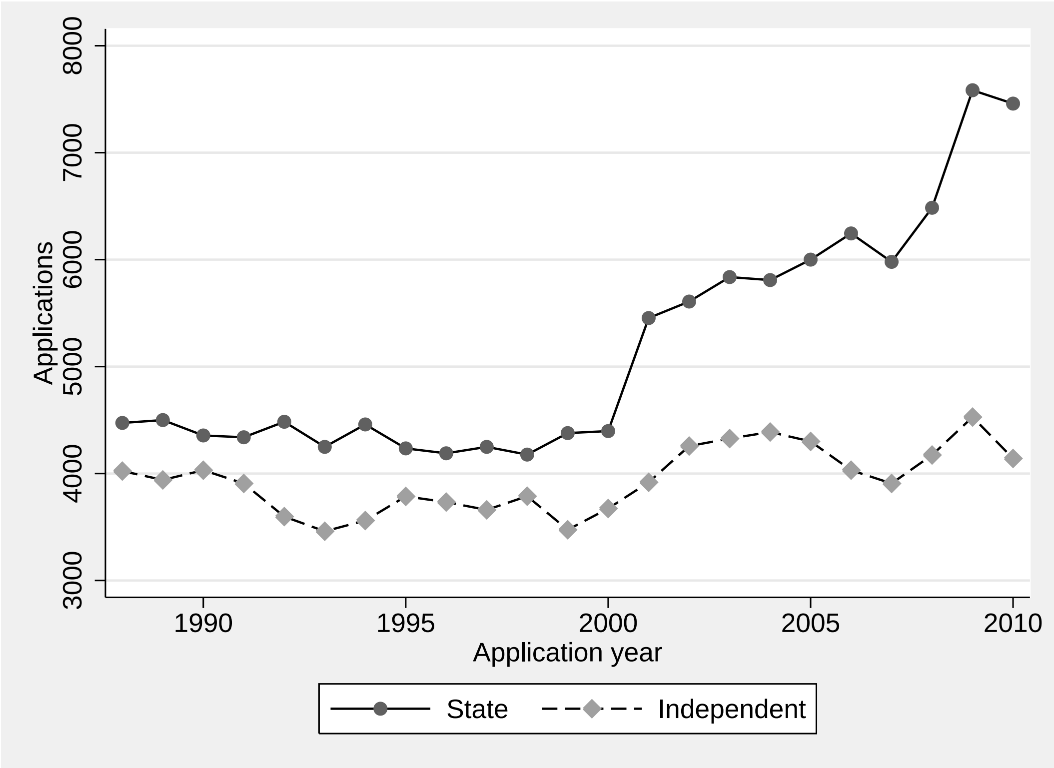
The University of Oxford has experienced a large increase in applications for all courses since the year 2000, as can be seen in Figure 4.3. After roughly 10 years of receiving approximately 8,000 applications from UK students each year, this grew rapidly by about 50% to a peak of around 12,000 in 2009, although it fell back somewhat in 2010. This has been driven particularly by a large increase in the number of applications from state school pupils during this period (see Figure 4.4), rising from under 4,500 to about 7,500. However, there has been no corresponding increase in the number of offers made to UK students, which have continued at around 3,000 and, if anything, declined slightly as more offers have gone to overseas applicants. It follows that getting a place has become considerably more competitive.

Figure 4.3: Number of applications from and offers given to UK students, by year



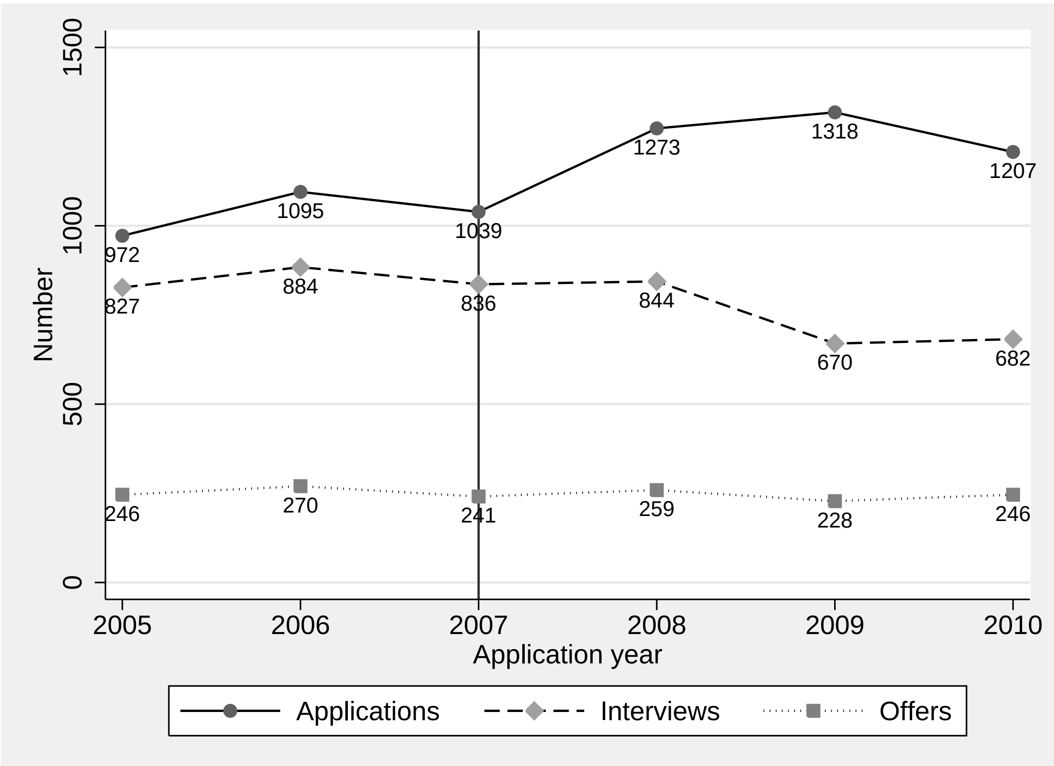
Notes: Source: Oxford University Admissions Statistics, across all subjects. Individuals for whom school type is unknown are excluded.

Figure 4.4: Number of applications from UK students, by year and school type



Notes: Source: Oxford University Admissions Statistics, across all subjects. Individuals for whom school type is unknown are excluded.

Figure 4.5: Number of applications to, interviews for and offers for Economics from UK students, by year



Notes: Sample size: 6,904. Individuals for whom school type is unknown are excluded. Vertical line indicates the year 2007, when test was administered but not used to inform decisions. In years before the line the test was not used; and in years after the test was used as part of the admissions process.

Over the shorter period for which I can observe subject-specific figures,[10] Economics is no exception to the pattern of increasing applications. Figure 4.5 shows that the number of applications has risen from 972 in 2005 to a peak of 1,318 in 2009 (with a similar slight reduction in 2010 as that seen in the overall figures, but still above that seen between 2005-2007). Again, the number of places to study Economics awarded to UK students has not risen alongside this.

Faced with this large increase in the number of applications, and the labour-intensive nature of the interview stage of the admissions process, the decision was taken to introduce a guideline for the number of interviews a college should conduct per place it had available. Figure 4.5 shows this fall in the number of interviews, from 836 in 2007 to 682 in 2010. This is a sizeable difference; with potential knock-on effects. The TSA was introduced at the same time in order to support this policy, providing admissions tutors with additional information with which to select applicants to call to interview. As such, the test was a requirement for all individuals applying to these subjects; this is unlike some institutions' use of the TSA, where it is administered only to interviewees (Admissions Testing Service, 2013a). Candidates sit the TSA at their school[11] on a date in early November, just under a month after the deadline for applications. Results are available to admissions tutors shortly afterwards, but are not released to the candidates until early the following year, importantly this is after interviews have been conducted and offers made.

The TSA was introduced in a phased approach. Applicants to Philosophy, Politics and Economics (PPE) at the University first sat the TSA in 2007. A complication in 2007 is that the test was administered to PPE applicants, but the results were not released to admissions tutors until after they had selected which applicants to call for interview. As such, it was not used to make decisions on who to call to interview, but was available to make decisions on which applicants to offer places to. This means one might expect to see some of the effects of the policy (for example due to changing behaviour by applicants), but not others (due to changing behaviour by admissions tutors in selecting candidates for interview). Applicants to Economics and Management (E&M) first sat the test in 2008. Unlike in PPE, the results of the TSA were available to admissions tutors when deciding

[10]It should be noted that this covers only about half the period of the large rise in applications to the University in general.

[11]If the school is not willing to administer the test then candidates may take it at an approved test centre, usually another school or college nearby.

which applicants to call for interview from that first year. However, in a different complication the guideline for the number of interviews per place was not introduced for TSA until 2009. These differences in implementation have the potential to distort the analysis. Since the impact of the test is our fundamental interest, I elect to exclude 2007 from the analysis. Since applicants do sit the test in 2008 and the results are available throughout the process to admissions tutors, I do not exclude it. However, the later implementation of the target number of interviews per place in E&M means there was a relatively larger number of E&M than PPE interviews in 2008: as such E&M interviews will weigh particularly heavily in that year. I am careful to discuss explore and discuss potential implications for the results in 2008.[12]

In my analysis, I exploit the fact that in the data there are two years where the aptitude test was not administered (2005 and 2006); and three years where it was administered to all Economics applicants (2008, 2009 and 2010). The policy has then continued in more recent years, but I do not have access to the data from this period. This natural experiment presents an opportunity to evaluate the effects stemming from this policy change, with no other major confounding policy changes affecting admissions having been undertaken at this time, to my knowledge.[13]

As noted above, since 2000 there have been large increases in the number of applications to the University, but no increase in the number of offers made. Estimating the impact of the TSA just by looking at characteristics before the change in policy and comparing them to the same characteristics afterwards would likely be biased downwards by the general downward trend in the proportion of applicants receiving an offer. Instead, I estimate the impact using a difference in differences (DiD) framework. This attempts to control for any general trends using the trends seen in subjects where the TSA was not introduced, hence attempting to isolate the changes in our outcome measures of interest that are due to the introduction of the TSA. The identifying assumption is that changes in the outcome variables for Economics applicants, over and above those seen among applicants to other subjects, are due to the introduction of the TSA: this requires that the trends in the treatment and control groups are the same, the so-called 'common trends'

[12]Although not reported in this chapter, I do also run models including 2007 to check for unexpected effects, and run models that estimate the effect for PPE and E&M application processes separately. These do not alter the main thrust of the findings.

[13]Undergraduate tuition fees rose from £1000 to a maximum of £3000 in the academic year 2006/7. The majority of applications for that year's entry would be made in 2005, at the very beginning of this dataset. As such, any changes in application behaviour associated with this policy change should not confound the analysis in this chapter, although they could affect pre-treatment trends.

assumption. For most of my analysis, the 'treatment' group is Economics and the 'control' group are all other subjects. The policy of interest, the introduction of the TSA, is 'off' in 2005 and 2006, and 'on' in 2008, 2009 and 2010.

Common trends are more likely if the 'control' group (other subjects) has similar observable characteristics to the Economics 'treatment' group. In Section 4.4, I discussed some of the differences between the profile of the average Economics applicant and the average applicant to other subjects, noting in particular differences in the average academic attainment between the two groups. However, the subject groups are not so different that it casts doubt on the validity of other subjects as a 'control' group. I also use a more restricted control group as a robustness check, which I discuss further in Section 4.9.

Table 4.4: Proportion of applicants who receive an offer, proportion of applicants who receive an interview, and proportion of interviewees who receive an offer, by year and subject group: difference in differences estimates

Apply $\rightarrow$ Offer	**Policy Off**	**Policy On**	**Difference**
Economics	0.250	0.193	-0.057
	(0.013)	(0.010)	(0.012)***
Others	0.284	0.241	-0.043
	(0.006)	(0.006)	(0.005)***
Difference	-0.034	-0.048	-0.014
	(0.014)***	(0.011)***	(0.013)
Apply $\rightarrow$ Interview	**Policy Off**	**Policy On**	**Difference**
Economics	0.828	0.578	-0.250
	(0.015)	(0.016)	(0.023)***
Others	0.788	0.677	-0.111
	(0.007)	(0.007)	(0.006)***
Difference	0.040	-0.099	-0.139
	(0.016)***	(0.017)***	(0.024)***
Interview $\rightarrow$ Offer	**Policy Off**	**Policy On**	**Difference**
Economics	0.302	0.334	0.032
	(0.016)	(0.012)	(0.017)*
Others	0.361	0.356	-0.004
	(0.006)	(0.006)	(0.005)
Difference	-0.059	-0.023	0.036
	(0.017)***	(0.013)*	(0.018)**

Notes: Analysis excludes individuals for whom school type is unknown. Policy Off in 2005, 2006 and 2007; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Sample sizes: Apply $\rightarrow$ Offer: 63986 Apply $\rightarrow$ Interview: 63986 Interview $\rightarrow$ Offer: 46106

Table 4.4 shows the change in the proportion of applicants getting interviews and places from before to after the policy change, for Economics and other subjects. While there is a significant reduction in the proportion of Economics applicants receiving offers, this is matched by a similar fall in the proportion getting an offer in other subjects.

By contrast, the reduction in proportion of Economics applicants getting an interview is significantly larger than that seen in other subjects, driven by the introduction of a guideline number of interviews per available place. Table 4.4 shows a simple estimate of the effect of the policy's introduction on the proportion of applicants who receive an interview: a 11.5 percentage point reduction. When coupled with no effect on the overall proportion receiving an offer, this implies that the policy must have resulted in an increase in the proportion of interviewees getting an offer. This is indeed borne out, with the proportion of Economics interviewees receiving an offer increasing, even as this statistic falls for other subjects. A simple estimate of the impact of the policy changes is a 5.4 percentage point increase in the proportion of interviewees who receive an offer.

A reduction in the proportion of applicants who are called to interview would appear to be an increase in efficiency of the admissions process. However, it could be that this is a trade-off against other aims: selecting the highest quality applicants for the course and doing so without bias from applicants' other characteristics. Testing the first of these might be possible, but would require data on candidates' performance in their final examinations, which is not available in the dataset. However, I now shed some light on the second aim.

The large reduction in the proportion of applicants called for interviews clearly allows for the possibility of relative changes in the proportion of applicants from different genders or school types. Neither do the findings so far rule out the possibility of the policy having an effect on the proportion of applicants receiving an offer and coming from a particular group, since countervailing effects could offset one another.

To consider these matters, I present versions of Table 4.4 that separate out the overall effect of the policy into separate effects by our groups of interest. For the exposition of this analysis, I concentrate on effects by school type. However, it is easy to see how this is translated to analyse differences by gender.

For these purposes, instead of using the overall proportion of applicants who get a place, I analyse two sets of proportions: one where the numerator consists of only those getting an offer (or an interview) <u>and</u> coming from an independent school; and the other where the numerator consists of only those getting an offer (or an interview) <u>and</u> coming from a state school (on the right side of the table). In both cases, the denominator remains, as for Table 4.4, all applicants (or interviewees, in the case of Offer | Interview).

To make this clearer, I define the following notation:

$A_I =$ Number of applicants from independent schools

$A_S =$ Number of applicants from state schools

$I_I =$ Number of interviewees from independent schools

$I_S =$ Number of interviewees from state schools

$O_I =$ Number of offers to individuals from independent schools

$O_S =$ Number of offers to individuals from state schools

The proportions reported in the table are as follows:

Proportion of applicants receiving an offer : Independent: $\frac{O_I}{A_I+A_S}$ State: $\frac{O_S}{A_I+A_S}$

Proportion of applicants receiving an interview : Independent: $\frac{I_I}{A_I+A_S}$ State: $\frac{I_S}{A_I+A_S}$

Proportion of interviewees receiving an offer : Independent: $\frac{I_I}{I_I+I_S}$ State: $\frac{O_S}{I_I+I_S}$

This DiD analysis is presented in Table 4.5. How do these proportions relate to the previous analysis and to one another? The proportions reported in Table 4.4 were of the form $\frac{I_I+I_S}{A_I+A_S}$ (this particular example is the proportion of applicants called to interview). The proportions separated by school type are a simple decomposition of this overall proportion, since $\frac{I_I}{A_I+A_S} + \frac{I_S}{A_I+A_S} = \frac{I_I+I_S}{A_I+A_S}$. Ensuring that the outcome variables for the independent and state school analyses have the same denominator allows easy comparison of the DiD estimates from each to see whether there are differential effects of the policy on applicants from the two school types.

In the case of the overall proportion receiving an offer, the story does not immediately seem more complex than suggested by Table 4.4. In the top panel, there is no statistically significant change in the proportion of all applicants who are successful and come from either school type as a result of the policy change.

However, looking at the middle panel, at first look there would appear to be a difference between the effects on the proportion of all applicants called to interview by school type. The difference in difference estimate of the effect on the proportion relating to state school interviewees is a reduction of 5.4 percentage points, while the relevant ef-

Table 4.5: Proportion of all applicants who receive an offer, proportion of all applicants who receive an interview, and proportion of all interviewees who receive an offer, by school type, year and subject group: difference in differences estimates

	Independent			State		
Apply → Offer	**Policy Off**	**Policy On**	**Difference**	**Policy Off**	**Policy On**	**Difference**
Economics	0.123	0.091	-0.032	0.127	0.102	-0.025
	(0.010)	(0.006)	(0.009)***	(0.010)	(0.008)	(0.008)***
Others	0.128	0.106	-0.022	0.156	0.135	-0.020
	(0.006)	(0.004)	(0.004)***	(0.006)	(0.004)	(0.004)***
Difference	-0.005	-0.015	-0.009	-0.029	-0.034	-0.005
	(0.012)	(0.007)**	(0.010)	(0.012)***	(0.009)***	(0.009)

	Independent			State		
Apply → Interview	**Policy Off**	**Policy On**	**Difference**	**Policy Off**	**Policy On**	**Difference**
Economics	0.392	0.268	-0.124	0.436	0.310	-0.126
	(0.022)	(0.015)	(0.014)***	(0.020)	(0.020)	(0.017)***
Others	0.321	0.283	-0.038	0.466	0.394	-0.072
	(0.014)	(0.010)	(0.008)***	(0.014)	(0.010)	(0.009)***
Difference	0.071	-0.015	-0.085	-0.030	-0.084	-0.054
	(0.026)***	(0.017)	(0.016)***	(0.024)	(0.022)***	(0.019)***

	Independent			State		
Interview → Offer	**Policy Off**	**Policy On**	**Difference**	**Policy Off**	**Policy On**	**Difference**
Economics	0.148	0.158	0.010	0.153	0.176	0.023
	(0.012)	(0.010)	(0.013)	(0.013)	(0.011)	(0.010)*
Others	0.163	0.156	-0.007	0.198	0.200	0.002
	(0.007)	(0.006)	(0.005)	(0.007)	(0.006)	(0.005)
Difference	-0.015	0.002	0.016	-0.044	-0.024	0.020
	(0.014)	(0.011)	(0.014)	(0.015)***	(0.012)**	(0.011)*

Notes: Outcome variables reported are (**Apply** → **Offer**) proportion of all applicants who receive an offer and come from given school type, (**Apply** → **Interview**) proportion of all applicants who receive an interview and come from given school type, and (**Interview** → **Offer**) proportion of all interviewees who receive an offer and come from given school type. Analysis excludes individuals for whom school type is unknown. Policy Off in 2005, 2006 and 2007; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Sample sizes: Apply → Offer: 63986; Apply → Interview: 63986; Interview → Offer: 46106.

fect relating to those from independent schools is a reduction of 8.5 percentage points. There are reductions in both these proportions, but the effect on the proportion of all interviewees being called to interview and coming from independent school is larger; the estimated effect is roughly 3 percentage points greater in magnitude. Nevertheless, we cannot reject the null hypothesis of no difference between these two estimates at the conventional 5% level (although we can at the 10% level).

Finally, turning to the bottom panel of Table 4.5 the proportion of interviewees who receive offers and come from state schools is estimated to increase slightly more than the proportion of all interviewees who are successful and come from independent schools (2.0 percentage points, compared with 1.6 percentage points). However, a simple t-test confirms that the estimated effects are not significantly different from one another.

Table 4.6: Proportion of all applicants who receive an offer, proportion of all applicants who receive an interview, and proportion of all interviewees who receive an offer, by gender, year and subject group: difference in differences estimates

	Female			Male		
Apply → Offer	**Policy Off**	**Policy On**	**Difference**	**Policy Off**	**Policy On**	**Difference**
Economics	0.089	0.057	-0.032	0.161	0.136	-0.025
	(0.006)	(0.005)	(0.007)***	(0.010)	(0.008)	(0.011)***
Others	0.135	0.115	-0.020	0.149	0.126	-0.023
	(0.003)	(0.003)	(0.003)***	(0.005)	(0.005)	(0.005)***
Difference	-0.047	-0.059	-0.012	0.012	0.010	-0.002
	(0.007)***	(0.006)***	(0.007)	(0.011)	(0.009)	(0.012)
	Female			Male		
Apply → Interview	**Policy Off**	**Policy On**	**Difference**	**Policy Off**	**Policy On**	**Difference**
Economics	0.269	0.167	-0.102	0.558	0.411	-0.147
	(0.012)	(0.008)	(0.013)***	(0.016)	(0.012)	(0.022)***
Others	0.391	0.342	-0.049	0.396	0.335	-0.062
	(0.008)	(0.007)	(0.008)***	(0.010)	(0.007)	(0.009)***
Difference	-0.122	-0.175	-0.053	0.162	0.076	-0.086
	(0.014)***	(0.011)***	(0.015)***	(0.019)***	(0.014)***	(0.024)***
	Female			Male		
Interview → Offer	**Policy Off**	**Policy On**	**Difference**	**Policy Off**	**Policy On**	**Difference**
Economics	0.107	0.098	-0.009	0.195	0.236	0.041
	(0.008)	(0.007)	(0.010)	(0.012)	(0.010)	(0.015)***
Others	0.172	0.170	-0.001	0.189	0.186	-0.003
	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)
Difference	-0.065	-0.072	-0.008	0.006	0.050	0.044
	(0.009)***	(0.008)***	(0.011)	(0.013)	(0.012)***	(0.016)***

Notes: Outcome variables reported are (**Apply → Offer**) proportion of all applicants who receive an offer and come from given school type, (**Apply → Interview**) proportion of all applicants who receive an interview and come from given school type, and (**Interview → Offer**) proportion of all interviewees who receive an offer and come from given school type. Analysis excludes individuals for whom school type is unknown. Policy Off in 2005, 2006 and 2007; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Sample sizes: Apply → Interview: 63986; Apply → Offer: 63986; Interview → Offer: 46106.

In Table 4.6 I report the same analysis split by gender, rather than school type. I do not find statistically significant differences in the overall effect of introducing the TSA on the proportion of applicants getting an offer by gender, although if there is any difference it is to the detriment of female applicants. However, again there are differences in the results by gender when considering the two separate stages of the admissions process. I first consider the proportion of applicants offered an interview: the proportion of all applicants offered an interview and who are female has declined by 5.5 percentage points, compared to a larger decline of 8.6 percentage points in the proportion of all applicants offered an interview and who are male. However, we cannot reject the null hypothesis of no difference between these two estimates at the conventional 5% level (although we can at the 10% level).

In any case, the difference appears to be offset at the latter stage of the admissions process. We saw above that the proportion of interviewees getting an offer increased in response to the introduction of the TSA (offsetting the falling numbers getting an interview): the results by gender suggest that this is entirely driven by the proportion of all interviewees receiving an offer and who are men (4.4 percentage point increase, compared to a very small decrease for females). This difference does appear to be statistically significant at the 5% level. Given that the aptitude test is primarily used to select candidates for interview, finding an effect at the latter stage of the admissions process may seem unexpected. However, an indirect effect of this type is possible. One explanation is that the TSA is filtering out the kind of female interviewees who previously went on to perform well at interview and hence receive an offer. I investigate such explanations further while discussing the results from the regression models in Section 4.7.

So far, these results answer my research questions in the following ways: they do not suggest an impact on the proportion of applicants offered a place, but do reflect the negative impact on the proportion of applicants called to interview caused by the introduction of a target number of interviews per place. As such, there is an offsetting increase in the proportion of interviewees offered a place. I find some limited evidence of differences in these impacts by the socioeconomic status of applicants, with the proportion of applicants getting an interview and coming from an independent school declining more than for its state school counterpart. In addition, there is evidence of differential effects on the proportion of applicants getting an interview and the proportion of interviewees getting an offer by gender. Nevertheless, these results should not overshadow the finding that in neither of these cases (differences by school type or gender) is there a statistically significant overall difference in the proportion of all applicants who receive an offer.

However, this simple analysis has limitations, which I aim to check and/or relax, as appropriate, using regression analysis below.

4.6 Regression analysis

DiD estimates may be conveniently recovered using least squares regression. In addition, regression analysis allows increased model flexibility compared to those I have used thus far. I use this flexibility to check for different effects by year and to control for college-, course- and time-varying covariates that could affect the validity of the common trends assumption.

As discussed in Section 4.3, decisions about who to admit are made by admissions tutors at each college. Given their importance, I perform regression analysis using colleges as the unit of analysis. I collapse individual applicant records into college-level averages, also maintaining separate observations by year and course group. After exclusions, the data include 29 colleges, six years and two course groups (Economics and Others). This gives 348 college, year, course group combinations forming available observations for the regression analysis. In all specifications, year variables are grouped in some way, reducing the number of observations to those shown in later results tables.

I weight the observations to take account of the average number of applicants a college receives per year across the whole period from 2005 to 2010. Colleges vary significantly in size so, as the underlying research questions are about the effects on applicants, weighting to be representative of the numbers of applicants is appropriate. Failure to do this would implicitly give each college an equal weighting, exaggerating the influence of small colleges on the overall results. The weighting strategy takes into account the fact that the observations are means, made up of observations of individuals' characteristics and progress through the admissions process.[14]

I begin by replicating the analysis in Section 4.5 above in a regression framework, using an equation of the form shown in Equation 4.1. As a result of the weighting strategy, one would not expect the point estimates to be identical to those in earlier analysis, but they

[14]This echoes the approach by Card (1992), who estimates the impact of minimum wages using observations from 51 states, weighting these by the average size of the sample for relevant workers in each state.

should be very close.

$$\begin{aligned} Y_{jt} = \alpha + \beta_p \text{Treated}_j \\ + \gamma \text{Policy On}_t \\ + \delta \text{Treated}_j * \text{Policy On}_t + \varepsilon_{jt} \end{aligned} \tag{4.1}$$

where Y_{jt} is the outcome of interest at college j in year t; Treated are dummy variables indicating the two treatment groups (both PPE and E&M); Policy On is a dummy variable set to 0 in years 2005 and 2006, and 1 in 2008, 2009 and 2010; and ε is an error term (which I discuss further below).

The coefficients on Treated (β) control for pre-existing differences between applicants to these and other subjects; the coefficient on Policy On (γ) controls for general trends in the variables relative to the base years of 2005 and 2006; and the coefficient on the interaction term between the Treated and Policy On variables (δ) allows us to recover the impact of the TSA, under the identifying assumption of common trends.

However, regression analysis makes it easy to introduce more flexibility than I have allowed for so far; I take advantage of this in various ways. First, I allow for different effects each year by replacing the Policy On dummy variables with a set of year dummies. Equation 4.2 shows the form of equation used.

$$\begin{aligned} Y_{jt} = \alpha + \beta \text{Treated}_j \\ + \gamma_8 2008_t + \gamma_9 2009_t + \gamma_{10} 2010_t \\ + \delta_8 \text{Treated}_j * 2008_t + \delta_9 \text{Treated}_j * 2009_t + \delta_{10} \text{Treated}_j * 2010_t + \varepsilon_{jt} \end{aligned} \tag{4.2}$$

where 2008, 2009 and 2010 are dummy variables indicating cohorts where the policy is on.

The interpretation for Equation 4.2 is very similar to that for Equation 4.1. The coefficient on Treated (β) still controls for pre-existing differences between applicants to Economics and other subjects; the coefficients on 2008, 2009 and 2010 (γ) control for general trends in the variables relative to the base years of 2005 and 2006; and the coefficients on the interaction terms between the Treated and year variables (δ_8, δ_9 and δ_{10}) allow us to recover the estimated impact of the TSA for each of these treatment years.

I also use regression to include additional college-, course-, and time-varying covariates. Including these covariates aims to help control for omitted college- and course-specific trends in the outcome variables that could otherwise undermine the common trends assumption. Firstly, I include measures of the average academic performance of applicants from our groups of interest (applicants from independent and state schools for school type analysis; male and female applicants for analysis by gender) to each course group at each college (using the number of GCSEs and the number of GCSE A*s held by the mean applicant from each school type). These aim to control for changes in the success of candidates from each school type that are due to observable differences in their prior academic attainment. Secondly, I include an annual measure of the performance of the college's undergraduates at the end of their degrees (using the Norrington score, discussed in Section 4.4). This aims to control for the possibility that the quality of applicants to a college is affected by its academic reputation. I use a regression equation very similar to that in Equation 4.2, except for the addition of this vector of college-level controls.

As is common in DiD analysis, various aspects of the data are problematic for classical statistical inference (Bertrand et al., 2004). However, there is a growing literature on inference in such circumstances (Brewer et al., 2013a). In particular, I adapt advice from Angrist and Pischke (2009, ch. 8) in my approach to obtaining appropriate standard errors. First, while admissions tutors are college- and subject-specific, some courses have more than one subject area. It follows that there may be cases where the same admissions tutor makes decisions in different courses. As such, I allow for clustering between courses, other than between the treatment and control groups (i.e. Economics-related subjects and others). Given that most courses do have different admissions tutors, this is a very conservative approach[15]. Second, repeated observations across several years, often likely with the same admissions tutor with persistent preferences over time, makes autocorrelation/serial correlation likely (Kennedy, 2008, p.118).

As the observations are in the form of college, year, course group combinations, this

[15] Nevertheless, one might wish to allow clustering even between Economics and other subjects. However, in doing so the number of clusters is reduced to equal the number of colleges (after the exclusions described above): this is only 29 clusters. This is short of the minimum of 42 recommended for standard clustering techniques by Angrist and Pischke (2009). The 'wild bootstrap t-procedure' (Cameron et al., 2008) is more effective at avoiding type II errors with such a small number of clusters. Performing inference even on this extremely conservative basis does not materially alter the statistical significance of my results. I implement this using the command by Bansi Malde, available from `http://www.ifs.org.uk/publications/6231`

already allows for clustering within college and course group combinations. However, it assumes independence by year. As such, I use Stata's cluster option to define clusters as the 58 college and course group combinations, allowing for serial correlation.

4.7 Results

Given this chapter's particular focus on the potential for differential effects on applicants by their socioeconomic background or gender, I take as given the picture of the reduction in proportion of applicants who are called for interview and offsetting increase in the proportion of interviewees who are offered a place.[16] I proceed immediately to analyse whether evidence exists of differential effects for applicants, beginning with school type before turning to gender.

Results are presented in tables for each stage of the admissions process, with regression models in numbered columns. In each column, the DiD estimates of policy impact are shown either by rows giving the interaction between Economics and policy on (δ) or by rows giving the interaction between Economics and treatment years (δ_8, δ_9 and δ_{10}) depending on the model. I then report the differences between the DiD estimated effects for each pair of models, with the statistical significance of the differences indicated using stars,[17] to allow us to assess whether there are differential effects. I will not discuss the "Simple" models (columns 1 and 2) in each case, since they are very similar (but for weighting) to the analysis from Tables 4.5 and 4.6 in Section 4.5.

4.7.1 School type

In the case of the proportion of applicants getting an offer, Table 4.7 shows no unexpected results when separating the successful proportion into those from independent and state schools. The only small deviation from this is that in 2008 the estimate for the proportion from independent schools is noticeably more negative than that for state

[16] I do estimate these regression models to check the robustness of the analysis in Table 4.4, but do not report the results in this chapter as they do not differ in their findings.

[17] I conduct cross-model hypothesis testing using a seemingly-unrelated regression technique, specifically the Stata suest command, as this allows weights and clustering to be taken into account. Since the models being compared contain the same regressors this has no impact on the estimated standard errors (Zellner, 1962, p.351). Stars indicate statistical significance as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.7: Proportion of all applicants getting an offer, comparing proportions who are successful and come from either independent or state schools: difference in differences estimates

Variable \ Model	Simple		Years		Controls	
	(1)	(2)	(3)	(4)	(5)	(6)
	Ind.	State	Ind.	State	Ind.	State
Constant (α)	0.129	0.155	0.129	0.155	0.135	-0.149
	(0.006)***	(0.006)***	(0.006)***	(0.006)***	(0.156)	(0.147)
Treated (β)	-0.006	-0.028	-0.006	-0.028	-0.012	-0.026
	(0.012)	(0.012)**	(0.012)	(0.012)**	(0.011)	(0.009)***
Policy On (γ)	-0.023	-0.020				
	(0.004)***	(0.004)***				
2008 (γ_8)			-0.005	-0.012	-0.006	-0.025
			(0.004)	(0.004)***	(0.010)	(0.009)***
2009 (γ_9)			-0.028	-0.028	-0.042	-0.039
			(0.004)***	(0.005)***	(0.006)***	(0.006)***
2010 (γ_{10})			-0.032	-0.019	-0.049	-0.029
			(0.006)***	(0.005)***	(0.008)***	(0.006)***
Treated*Policy On (δ)	-0.008	-0.005				
	(0.010)	(0.009)				
Treated*2008 (δ_8)			-0.026	-0.004	-0.020	-0.005
			(0.011)**	(0.012)	(0.012)*	(0.011)
Treated*2009 (δ_9)			-0.005	-0.013	-0.005	-0.012
			(0.011)	(0.010)	(0.012)	(0.010)
Treated*2010 (δ_{10})			0.005	-0.001	-0.007	0.003
			(0.013)	(0.012)	(0.013)	(0.011)
Mean No. of GCSEs (State)					-0.021	0.010
					(0.013)	(0.013)
Mean No. of GCSEs (Ind.)					-0.000	-0.026
					(0.010)	(0.010)**
Mean No. of A*s (State)					0.003	0.015
					(0.005)	(0.005)***
Mean No. of A*s (Ind.)					0.025	-0.007
					(0.005)***	(0.004)*
Norrington Score / 10					0.477	6.254
					(1.022)	(0.925)***
Differences in estimated effects by school type						
Treated*Policy On (δ)	-0.003					
Treated*2008 (δ_8)			-0.022		-0.015	
Treated*2009 (δ_9)			0.007		0.007	
Treated*2010 (δ_{10})			0.006		-0.010	
N	116		232		232	

Notes: Analysis excludes individuals for whom school type is unknown. For Simple model (columns 1 and 2), Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. For other models (columns 3, 4, 5 and 6), base category for years is pooling of observations for 2005 and 2006. 'Ind.' is a contraction of Independent. Cross-model hypothesis testing conducted using seemingly-unrelated regressions. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.8: Proportion of all applicants getting an interview, comparing proportions who are successful and come from either independent or state schools: difference in differences estimates

Variable \ Model	Simple		Years		Controls	
	(1)	(2)	(3)	(4)	(5)	(6)
	Ind.	State	Ind.	State	Ind.	State
Constant (α)	0.323	0.464	0.323	0.464	0.722	-0.210
	(0.015)***	(0.015)***	(0.015)***	(0.015)***	(0.331)**	(0.255)
Treated (β)	0.065	-0.024	0.065	-0.024	0.053	-0.018
	(0.025)***	(0.023)**	(0.025)***	(0.023)**	(0.024)**	(0.021)***
Policy On (γ)	-0.040	-0.071				
	(0.009)***	(0.009)***				
2008 (γ_8)			-0.015	-0.060	-0.023	-0.084
			(0.008)**	(0.007)***	(0.018)	(0.019)***
2009 (γ_9)			-0.041	-0.078	-0.063	-0.090
			(0.009)***	(0.010)***	(0.012)***	(0.014)***
2010 (γ_{10})			-0.060	-0.073	-0.092	-0.084
			(0.013)***	(0.012)***	(0.017)***	(0.016)***
Treated*Policy On (δ)	-0.085	-0.059				
	(0.016)***	(0.018)***				
Treated*2008 (δ_8)			-0.080	-0.015	-0.068	-0.020
			(0.017)***	(0.017)	(0.020)***	(0.020)
Treated*2009 (δ_9)			-0.101	-0.103	-0.098	-0.102
			(0.021)***	(0.023)***	(0.024)***	(0.023)***
Treated*2010 (δ_{10})			-0.076	-0.065	-0.100	-0.051
			(0.021)***	(0.025)***	(0.020)***	(0.027)*
Mean No. of GCSEs (State)					-0.019	0.030
					(0.026)	(0.025)
Mean No. of GCSEs (Ind.)					-0.010	-0.042
					(0.025)	(0.018)**
Mean No. of A*s (State)					0.001	0.021
					(0.010)	(0.010)**
Mean No. of A*s (Ind.)					0.054	-0.024
					(0.010)***	(0.010)**
Norrington Score / 10					-6.827	12.140
					(2.072)***	(1.983)***
Differences in estimated effects by school type						
Treated*Policy On (δ)	-0.026					
Treated*2008 (δ_8)			-0.066**		-0.048	
Treated*2009 (δ_9)			0.003		0.004	
Treated*2010 (δ_{10})			-0.012		-0.048	
N	116		232		232	

Notes: Analysis excludes individuals for whom school type is unknown. For Simple model (columns 1 and 2), Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. For other models (columns 3, 4, 5 and 6), base category for years is pooling of observations for 2005 and 2006. ‘Ind.’ is a contraction of Independent. Cross-model hypothesis testing conducted using seemingly-unrelated regressions. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.9: Proportion of all interviewees getting an offer, comparing proportions who are successful and come from either independent or state schools: difference in differences estimates

Variable \ Model	Simple		Years		Controls	
	(1)	(2)	(3)	(4)	(5)	(6)
	Ind.	State	Ind.	State	Ind.	State
Constant (α)	0.163	0.197	0.163	0.197	0.266	-0.169
	(0.007)***	(0.007)***	(0.007)***	(0.007)***	(0.196)**	(0.242)
Treated (β)	-0.016	-0.043	-0.016	-0.043	-0.027	-0.041
	(0.014)***	(0.014)***	(0.014)***	(0.014)***	(0.013)**	(0.012)***
Policy On (γ)	-0.007	0.002				
	(0.005)***	(0.005)***				
2008 (γ_8)			0.009	0.004	-0.001	-0.004
			(0.005)*	(0.005)***	(0.012)	(0.011)***
2009 (γ_9)			-0.014	-0.007	-0.037	-0.021
			(0.005)**	(0.006)***	(0.008)***	(0.008)**
2010 (γ_{10})			-0.015	0.011	-0.046	-0.003
			(0.007)**	(0.007)***	(0.010)***	(0.009)***
Treated*Policy On (δ)	0.016	0.018				
	(0.013)***	(0.011)*				
Treated*2008 (δ_8)			-0.018	0.009	-0.017	0.006
			(0.014)***	(0.016)	(0.014)***	(0.015)
Treated*2009 (δ_9)			0.036	0.025	0.037	0.027
			(0.018)*	(0.017)***	(0.017)**	(0.018)***
Treated*2010 (δ_{10})			0.040	0.023	0.013	0.021
			(0.019)**	(0.017)***	(0.019)***	(0.016)*
Mean No. of GCSEs (State)					-0.017	-0.005
					(0.016)	(0.017)
Mean No. of GCSEs (Ind.)					-0.010	-0.012
					(0.015)	(0.021)**
Mean No. of A*s (State)					0.009	0.014
					(0.007)	(0.009)**
Mean No. of A*s (Ind.)					0.031	-0.006
					(0.006)***	(0.007)**
Norrington Score / 10					-1.354	7.320
					(1.106)***	(1.505)***
Differences in estimated effects by school type						
Treated*Policy On (δ)	-0.002					
Treated*2008 (δ_8)			-0.027		-0.024	
Treated*2009 (δ_9)			0.010		0.011	
Treated*2010 (δ_{10})			0.017		-0.007	
N	116		232		231	

Notes: Analysis excludes individuals for whom school type is unknown. For Simple model (columns 1 and 2), Policy Off in 2005and 2006; Policy On in 2008, 2009 and 2010. For other models (columns 3, 4, 5 and 6), base category for years is pooling of observations for 2005 and 2006. 'Ind.' is a contraction of Independent. Cross-model hypothesis testing conducted using seemingly-unrelated regressions. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

schools (although still not statistically significant).[18] However, this is not maintained in subsequent years and is reduced in the model with additional controls. This suggests that the introduction of the TSA has not had a differential overall impact on the proportion of all applicants who are ultimately offered a place and come from each school type. However, this does not mean the same will be true at the intermediate stages of the process.

The additional controls in models 5 and 6 also behave as might be expected. There is a correlation between the mean number of GCSE A*s held by applicants of a given school type and the proportion of applicants who are successful and come from that same school type. We might also expect to see a negative relationship between average GCSE performance among one school type and the successful proportion from the other: to admissions tutors, applicants from different school types are substitutes and a rise in the performance of one of these groups might be expected to reduce demand for applicants from the other, other things being equal. However, if this effect exists it is too weak to be identified. The coefficients on the Norrington Score imply that a greater proportion of all applicants to colleges with higher performing existing undergraduates will be offered a place and come from state schools; there is no statistically significant effect on the proportion of all applicants who get an offer and come from an independent school. While the implications are rather difficult to interpret, its inclusion in the model aims to help to control for the possibility that individuals attempt to choose colleges strategically to improve their chances of admissions.

Table 4.8 gives a more complex picture of the proportion of applicants who are called to interview: the simple DiD estimate was that the effect of the introduction of the aptitude test was more negative on the proportion of all applicants who were called to interview and came from independent schools than it was on the state school proportion, but that this difference was not statistically significant. However, from more flexible regression analysis we see that the estimated impact varies significantly year by year. Much of the difference in the simple estimates appears to be driven by a statistically significantly difference between the impacts by school type in 2008 (δ_8).[19] However, as with the pro-

[18]Examining the results separately by PPE and E&M does not suggest this is driven by the relatively larger number of E&M interviews in that year.

[19]Examining these results separately for PPE and E&M (not reported here) suggests one of the reasons for this is that the policy setting a target number of interviews per place for E&M was not yet active. As such, the number of interviews for E&M weigh relatively larger than in other years. Focussing only on PPE, the estimate is for the same direction of difference in effects, but not statistically significant.

portion getting an offer, this difference between estimates becomes statistically insignificantly different from one another when controls are added to the model. Furthermore, by the following year this differential has vanished: in 2009 and 2010 the differences between the two estimates are in each case much smaller and not statistically significant. Considering the other controls in the model, there is also some evidence of a trade-off between candidates of different school types, with a positive effect of average GCSE performance of independent school applicants on the proportion of all applicants who get an offer and come from independent schools, but a negative effect of the same variable on the proportion from state schools. In summary, it would appear that any difference in effects may be driven by observable background characteristics, likely prior attainment, and is, at most, only short lived.

Finally, Table 4.9[20] also confirms the simple DiD estimates by failing to find strong evidence of a difference by school type in the proportion of interviewees who receive an offer. While there is (as with the proportion of applicants offered an interview) a noticeably larger difference by school type in 2008, it is not statistically significant. The inclusion of additional covariates makes a much smaller difference to the estimated effects (and the gap between them) than in modelling the proportion of applicants offered an interview: this seems likely to be down to the smaller variation in observable characteristics between those interviewed.

The results from the regression analysis add confidence to findings from Section 4.5 in two ways. The estimates show a reasonably consistent story over time (particularly given the unusual circumstances in 2008); namely, that there is no evidence of different effects on the two proportions by school type. Second, they give some confidence that the results are not driven by changes in other observable characteristics, notably the average performance of applicants from each school type, or differences in college choice.

4.7.2 Gender

I now explore the results by gender in the same way. In the case of the proportion of applicants getting an offer, Table 4.10 confirms our earlier results. In no years are the differences by gender between the estimated effects statistically significant. As with analysis

[20]The reduction in sample size in columns 5 and 6 in Table 4.9 is due to the fact that at one college in one year none of the state school applicants were invited to an interview.

Table 4.10: Proportion of all applicants getting an offer, comparing proportions who are successful and are either male or female: difference in differences estimates

Variable \ Model	Simple		Years		Controls	
	(1)	(2)	(3)	(4)	(5)	(6)
	Female	Male	Female	Male	Female	Male
Constant (α)	0.135	0.149	0.135	0.149	0.087	-0.076
	(0.003)***	(0.005)***	(0.003)***	(0.005)***	(0.098)	(0.165)
Treated (β)	-0.046	0.012	-0.046	0.012	-0.053	0.008
	(0.008)***	(0.012)	(0.008)***	(0.012)	(0.007)***	(0.009)
Policy On (γ)	-0.020	-0.022				
	(0.003)***	(0.005)***				
2008 (γ_8)			-0.007	-0.010	-0.010	-0.016
			(0.004)	(0.005)**	(0.006)*	(0.008)*
2009 (γ_9)			-0.031	-0.025	-0.039	-0.039
			(0.004)***	(0.006)***	(0.004)***	(0.006)***
2010 (γ_{10})			-0.021	-0.029	-0.029	-0.046
			(0.005)***	(0.005)***	(0.005)***	(0.007)***
Treated*Policy On (δ)	-0.013	-0.000				
	(0.008)*	(0.012)				
Treated*2008 (δ_8)			-0.029	-0.000	-0.024	-0.002
			(0.011)***	(0.015)	(0.009)***	(0.014)
Treated*2009 (δ_9)			-0.013	-0.006	-0.007	-0.012
			(0.008)	(0.012)	(0.008)	(0.011)
Treated*2010 (δ_{10})			-0.003	0.007	-0.004	0.003
			(0.009)	(0.015)	(0.009)	(0.014)
Mean No. of GCSEs (Male)					-0.008	-0.033
					(0.009)	(0.013)**
Mean No. of GCSEs (Female)					-0.007	0.006
					(0.006)	(0.008)
Mean No. of A*s (Male)					-0.003	0.019
					(0.004)	(0.005)***
Mean No. of A*s (Female)					0.020	-0.002
					(0.004)***	(0.004)
Norrington Score / 10					1.334	5.987
					(0.553)**	(1.237)***
Differences in estimated effects by gender						
Treated*Policy On (δ)	-0.013					
Treated*2008 (δ_8)			-0.028		-0.021	
Treated*2009 (δ_9)			-0.007		0.004	
Treated*2010 (δ_{10})			-0.009		-0.007	
N	116		232		230	

Notes: Analysis excludes individuals for whom school type is unknown. For Simple model (columns 1 and 2), Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. For other models (columns 3, 4, 5 and 6), base category for years is pooling of observations for 2005 and 2006. Cross-model hypothesis testing conducted using seemingly-unrelated regressions. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.11: Proportion of all applicants getting an interview, comparing proportions who are successful and are either male or female: difference in differences estimates

Variable \ Model	Simple		Years		Controls	
	(1)	(2)	(3)	(4)	(5)	(6)
	Female	Male	Female	Male	Female	Male
Constant (α)	0.392	0.396	0.392	0.396	0.149	0.353
	(0.009)***	(0.011)***	(0.009)***	(0.011)***	(0.214)	(0.261)
Treated (β)	-0.119	0.160	-0.119	0.160	-0.131	0.156
	(0.018)***	(0.022)***	(0.018)***	(0.022)***	(0.012)***	(0.019)***
Policy On (γ)	-0.050	-0.060				
	(0.008)***	(0.009)***				
2008 (γ_8)			-0.029	-0.046	-0.040	-0.051
			(0.008)***	(0.009)***	(0.012)***	(0.015)***
2009 (γ_9)			-0.061	-0.058	-0.070	-0.078
			(0.009)***	(0.011)***	(0.008)***	(0.011)***
2010 (γ_{10})			-0.059	-0.075	-0.070	-0.096
			(0.010)***	(0.010)***	(0.009)***	(0.013)***
Treated*Policy On (δ)	-0.057	-0.087				
	(0.018)***	(0.027)***				
Treated*2008 (δ_8)			-0.048	-0.047	-0.036	-0.051
			(0.019)**	(0.028)*	(0.014)**	(0.024)**
Treated*2009 (δ_9)			-0.082	-0.122	-0.070	-0.131
			(0.021)***	(0.031)***	(0.017)***	(0.030)***
Treated*2010 (δ_{10})			-0.049	-0.092	-0.046	-0.100
			(0.021)**	(0.028)***	(0.015)***	(0.027)***
Mean No. of GCSEs (Male)					0.028	-0.048
					(0.017)*	(0.025)*
Mean No. of GCSEs (Female)					-0.020	-0.001
					(0.012)*	(0.016)
Mean No. of A*s (Male)					-0.001	0.023
					(0.008)	(0.008)***
Mean No. of A*s (Female)					0.025	0.003
					(0.006)***	(0.010)
Norrington Score / 10					0.183	5.830
					(1.028)**	(1.899)***
Differences in estimated effects by gender						
Treated*Policy On (δ)	0.030					
Treated*2008 (δ_8)			-0.001		0.015	
Treated*2009 (δ_9)			0.041		0.061	
Treated*2010 (δ_{10})			0.043		0.054	
N	116		232		230	

Notes: Analysis excludes individuals for whom school type is unknown. For Simple model (columns 1 and 2), Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. For other models (columns 3, 4, 5 and 6), base category for years is pooling of observations for 2005 and 2006. Cross-model hypothesis testing conducted using seemingly-unrelated regressions. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.12: Proportion of all interviewees getting an offer, comparing proportions who are successful and are either male or female: difference in differences estimates

Variable \ Model	Simple		Years		Controls	
	(1)	(2)	(3)	(4)	(5)	(6)
	Female	Male	Female	Male	Female	Male
Constant (α)	0.172	0.188	0.172	0.188	-0.047	0.010
	(0.004)***	(0.007)***	(0.004)***	(0.007)***	(0.133)	(0.206)
Treated (β)	-0.064	0.006	-0.064	0.006	-0.068	-0.002
	(0.010)***	(0.014)***	(0.010)***	(0.014)***	(0.010)***	(0.011)***
Policy On (γ)	-0.002	-0.002				
	(0.005)***	(0.007)***				
2008 (γ_8)			0.008	0.006	-0.001	-0.003
			(0.006)***	(0.007)***	(0.008)***	(0.010)***
2009 (γ_9)			-0.017	-0.004	-0.028	-0.026
			(0.005)***	(0.008)***	(0.007)***	(0.009)***
2010 (γ_{10})			0.002	-0.007	-0.011	-0.030
			(0.006)***	(0.007)***	(0.008)***	(0.009)***
Treated*Policy On (δ)	-0.012	0.046				
	(0.011)***	(0.016)***				
Treated*2008 (δ_8)			-0.035	0.026	-0.037	0.026
			(0.015)**	(0.020)*	(0.013)***	(0.018)**
Treated*2009 (δ_9)			-0.006	0.067	-0.016	0.064
			(0.013)***	(0.019)***	(0.015)***	(0.018)***
Treated*2010 (δ_{10})			0.003	0.060	-0.011	0.051
			(0.014)**	(0.023)***	(0.015)***	(0.023)**
Mean No. of GCSEs (Male)					0.015	-0.032
					(0.012)*	(0.016)**
Mean No. of GCSEs (Female)					-0.013	-0.004
					(0.009)*	(0.019)
Mean No. of A*s (Male)					-0.001	0.022
					(0.006)	(0.007)***
Mean No. of A*s (Female)					0.015	-0.000
					(0.005)***	(0.007)
Norrington Score / 10					1.522	5.999
					(0.915)*	(1.454)***
Differences in estimated effects by gender						
Treated*Policy On (δ)	-0.058***					
Treated*2008 (δ_8)			-0.061**		-0.063***	
Treated*2009 (δ_9)			-0.074***		-0.080***	
Treated*2010 (δ_{10})			-0.057*		-0.062**	
N	116		232		230	

Notes: Analysis excludes individuals for whom school type is unknown. For Simple model (columns 1 and 2), Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. For other models (columns 3, 4, 5 and 6), base category for years is pooling of observations for 2005 and 2006. Cross-model hypothesis testing conducted using seemingly-unrelated regressions. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

by school type, the additional controls in models 5 and 6 also behave as expected. There are positive correlations between the mean number of GCSE A*s held by applicants of a particular gender and the proportion of applicants who are successful and are of that gender. Likewise, any negative effects of increased performance by one gender on admissions chances of the other are either non-existent or too weak to be identified. The coefficients on the Norrington Score imply that a greater proportion of all applicants to colleges with higher performing existing undergraduates will be offered a place; this association is noticeably stronger for the success of male than female applicants, supporting its inclusion in the model.

Turning to the proportion of applicants called to interview, Table 4.11 shows a broadly consistent story of a larger decline in the proportion of applicants being called to interview who are male than the same proportion for females. However, the differences in estimated effects are not statistically significant. Examining these results separately for PPE and E&M (not reported here) suggests that the differences are driven more by changes in E&M. This seems likely to be because E&M received more applicants per place and, as such, the target number of interviews per place resulted in larger overall changes in the proportion of applicants called to interview.[21] Nevertheless, the results for PPE are not contradictory, but rather weaker.

Finally, Table 4.12 confirms the simple DiD estimate of a difference by gender in the proportion of all interviewees who receive an offer. The models provide consistently statistically significant evidence that the increase in the proportion of all interviewees receiving an offer is more positive for males than females. Generally this is explained by the increase in the proportion of all interviewees getting an offer being concentrated among males. Once again, the addition of covariates produces coefficients that conform to the pattern seen in earlier models. As with the results by school type, the inclusion of covariates in this model makes less difference than that seen for the earlier stage of the admissions process; however, if anything, their inclusion strengthens the statistical significance of the differences between the estimates for males and females.

I noted in Section 4.5 that an effect at the point of interview like this, given that the test is primarily used to screen applicants for interview, appears odd at first glance. However, a plausible explanation is that the TSA is more likely to screen out female applicants who

[21]This is also hinted at by the smaller estimated effects in 2008, when this part of the policy had not yet been introduced for E&M.

would in the past have been offered a place once they were interviewed. Further investigation, considering combinations of gender and school type, suggests that this may be partly be due to a larger reduction in the proportion of all applicants invited to interview who were female and from an independent school. This is larger than the reduction in the proportion for the combination of female and state school. By contrast, the difference in effects between males and females from state schools in the proportion of all applicants getting an interview is much smaller. However, this only provides a potential pointer towards possible causes.

As with school type, the results from this regression analysis add confidence to findings from Section 4.5. When it comes to the proportion of interviewees who receive an offer, the regression estimates show a consistent and statistically significant set of estimates over time, with the overall increases driven by the proportion who receive an offer and are male. Furthermore, the regression models with additional controls suggest that the results are not driven by changes in other observable characteristics within the groups.

4.8 Alternative outcome measures

Proportions of applicants who are successful and come from a particular gender or school type is not the only way to think about the admissions process. In this section, I take an alternative approach, looking at each stage of the admissions process and analysing the share of the individuals that come from each of our groups of interest. Since all applicants in the dataset are classified as coming from either independent or state schools, the shares of each sum to 1. The same is the case for males and females. As such, we can restrict interest to just one of the shares in each case: I choose the share who come from a state school and the share who are female. Returning to the graphical representation of the admissions process in Figure 4.1, instead of considering the decision points themselves, I analyse the share of applicants, interviewees, and those who receive an offer who come from state schools and, separately, the share of each of these groups who are female.

Concentrating on outcome measures of this type, generally with respect to school type, is popular in the press (for example Vasagar, 2011), perhaps because a single figure is more readily comprehensible. Furthermore, while the main analysis produced estimated effects that are comparable in absolute terms, this alternative approach implicitly takes into account the size of the effects relative to the baseline proportion of successful applicants of each type. The importance of this will become apparent in the discussion of the results by gender below.

This alternative approach also allows us to consider an important additional aspect, which the main analysis was not able to address. As discussed in Section 4.3, the proportion of young people who choose to apply cannot be analysed, since potential applicants are not observed by the University. However, a related, though not identical, question is whether there is an impact on the make up of the pool of applicants i.e. the share of applicants who are female, or the share from state schools. An increase in the proportion of applicants from independent schools who do in fact apply will decrease this figure (holding state school applications constant) and vice versa. Rather than taking as a given the pool of applicants or interviewees, as the main analysis does, this approach focuses on the cumulative effect of the policy change (including changes in application behaviour) up to a given point in the admissions process. One drawback of these outcome variables is that they do not tell us about any overall changes in the number of interviews and offers.

Turning to school type first, I apply the same DiD method as for the analysis in Section 4.5 to identify the impact of the introduction of the TSA on the relative numbers of applicants from independent and state schools by comparing the change in share of applicants, interviewees and those receiving an offer between Economics and other subjects.[22] Adopting the same notation as that introduced in Section 4.5 the outcome variables are as follows:

$$\text{Share of \textbf{applicants} from state schools: } \frac{A_S}{A_I + A_S}$$
$$\text{Share of \textbf{interviewees} from state schools: } \frac{I_S}{I_I + I_S}$$
$$\text{Share of those \textbf{offered} a place from state schools: } \frac{O_S}{O_I + O_S}$$

How do these relate to the outcome variables for my main analysis? While those took the form $\frac{I_S}{A_I+A_S}$ (in the case of the proportion of all applicants called to interview and coming

[22]I do subject these figures to the same regression analysis as used above, but do not report the results as they are not substantively different as those reported.

from a state school), these alternative outcome variables concentrate on proportions within a particular stage of the admissions process. They have the same denominators as the main analysis's outcomes, but quite different numerators.

Table 4.13: Share of applicants from State schools, share of interviewees from State schools, and share of those who receive an offer from State schools, by year and subject group: simple difference in differences estimates

Applicants	**Policy Off**	**Policy On**	**Difference**
Economics	0.551	0.575	0.024
	(0.023)	(0.023)	(0.014)*
Others	0.617	0.632	0.015
	(0.016)	(0.013)	(0.008)*
Difference	-0.066	-0.057	0.009
	(0.028)***	(0.026)**	(0.016)
Interviewees	**Policy Off**	**Policy On**	**Difference**
Economics	0.527	0.536	0.009
	(0.023)	(0.026)	(0.013)
Others	0.592	0.582	-0.010
	(0.018)	(0.014)	(0.009)
Difference	-0.066	-0.046	0.020
	(0.029)**	(0.029)	(0.016)
Offered	**Policy Off**	**Policy On**	**Difference**
Economics	0.508	0.527	0.019
	(0.032)	(0.025)	(0.026)
Others	0.548	0.561	0.013
	(0.017)	(0.014)	(0.011)
Difference	-0.040	-0.034	0.006
	(0.036)	(0.029)	(0.028)

Notes: Analysis excludes individuals for whom school type is unknown. Policy Off in 2005, 2006 and 2007; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Sample sizes: Applicants: 63986 Interviewees: 46106 Attendees: 16412

Reading across the rows in the top panel of Table 4.13 reveals that the share of applicants from state schools has been rising in all subjects, Economics included. Figure 4.4 shows a large increase in the number of applications from state schools, suggesting this is the cause, rather than any decline in the number of applications from independent schools. Furthermore, the difference between Economics and other subjects (seen by reading down each column) shows that Economics applicants are more likely to be from independent schools than those to other subjects. However, the DiD estimate, in the bottom right hand cell, highlights that the increase was not statistically significantly larger in Economics when the TSA was introduced: there is no strong evidence that the introduction of the TSA affected the makeup of applicants in this way. It should be remembered that this analysis only covers the three years following the introduction of the policy; changes in behaviour by applicants are likely to take some time.

Unlike among applicants, there is only a very small rise in the proportion of Economics interviewees who come from state schools. In fact, among non-Economics subjects the proportion declines a small amount, however this is far from statistical significance. With no significant changes in the proportion of interviewees from state school among either the treatment or control groups it comes as little surprise that the DiD estimate provides no evidence of a statistically significant effect of the policy on the proportion of interviewees who come from a state school.

Finally, considering the proportion of those offered a place that come from state schools (the statistic that receives most popular attention), the story is very similar to that for interviewees. In each case, these results echo the findings from Section 4.5, suggesting that the policy does not have a large impact on the kinds of young people who make it through the admissions process.

Subjecting the analysis in this section to the same regression modelling as in Section 4.6 does not materially alter the interpretation of these findings. I also take the approach further in analysing differences by socioeconomic status in Appendix E.1, using the applicants' schools' IDACI (Income Deprivation Affecting Children and Infants Index) figure as the outcome of interest. The analysis does not seem inconsistent with the findings reported above.

Turning now to the same analysis by gender, the story seems initially similar. The DiD estimate of the effect on the share of applicants who are female is zero. However, there is change in the composition of interviewees. The share of interviewees for Economics who are female falls by 3.6 percentage points, at a time when this figure is rising (marginally) among other subjects. This results in an estimated impact of the TSA of a 4.5 percentage point reduction in the share of interviewees who are female. Furthermore, regression analysis (allowing for different effects by year and including the same covariates as in the main analysis) casts little doubt on this finding.

Why do these results seemingly differ from our findings for gender in the main analysis, where the proportion of applicants offered an interview and who are male declines more than the proportion of all applicants offered an interview and who are female? It is because the proportion for males starts at a higher baseline than for females; as such, the larger absolute decline for the male proportion has a relatively smaller effect on the gender makeup of interviewees.

Table 4.14: Share of applicants who are female, share of interviewees who are female, and share of those who receive an offer who are female, by year and subject group: simple difference in differences estimates

Applicants	**Policy Off**	**Policy On**	**Difference**
Economics	0.325	0.323	-0.002
	(0.013)	(0.008)	(0.014)
Others	0.505	0.502	-0.003
	(0.013)	(0.009)	(0.012)
Difference	-0.180	-0.179	0.000
	(0.018)***	(0.011)***	(0.018)
Interviewees	**Policy Off**	**Policy On**	**Difference**
Economics	0.326	0.289	-0.036
	(0.014)	(0.011)	(0.016)*
Others	0.497	0.505	0.009
	(0.011)	(0.009)	(0.010)
Difference	-0.171	-0.216	-0.045
	(0.018)***	(0.014)***	(0.019)***
Offered	**Policy Off**	**Policy On**	**Difference**
Economics	0.355	0.293	-0.061
	(0.019)	(0.018)	(0.027)*
Others	0.476	0.478	0.002
	(0.012)	(0.011)	(0.013)
Difference	-0.122	-0.184	-0.063
	(0.022)***	(0.021)***	(0.029)**

Notes: Analysis excludes individuals for whom school type is unknown. Policy Off in 2005, 2006 and 2007; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Sample sizes: Applicants: 63986 Interviewees: 46106 Attendees: 16412

Considering those offered a place the figures are similar: there is a fall in the female share of those offered a place to study Economics, despite the opposite trend among other subjects. This leads to an estimated negative effect of the TSA of 6.3 percentage points. However, unlike in the case of interviewees, these estimates are reduced to statistical insignificance by the inclusion of additional controls in regression analysis.

These results do not suggest that the introduction of the TSA has had a detrimental effect on the proportion of female applicants to Economics courses at the University of Oxford. However, a gap would appear to open in the share of interviewees who are female, and hence on into the share of those offered a place. The estimated effects are larger than those recovered above for changes in shares from state schools. However, in this case, regression analysis reduces rather than adds to our confidence: the statistical evidence only remains strong in the case of the share of interviewees who are female.

4.9 Robustness

The extent to which one can trust the findings from DiD analysis rests on the validity of the common trends assumption that underlies it. This cannot be tested directly, since the trend one would wish to look at is an unobserved counterfactual. However, robustness checks can provide some evidence that the assumption seems likely to hold.

The first of these I employ is a 'placebo' test. This involves estimating the 'effect' across a period when the policy was not introduced, in this case between 2005 and 2006. The treatment and control groups remain as specified for the main analysis (Economics as treatment, all other subjects as controls). Finding an effect during this period, when there was no policy to produce one, would suggest a failure of the common trends assumption was inducing the apparent impact. The results from the placebo treatment on the proportion of all applicants who get a place, all applicants who get an interview and all interviewees who get a place are shown in Table 4.15, using the same output from linear regression employed in Section 4.7. No significant effect is identified at any stage of the admissions process, which is reassuring. This continues to hold true when the proportions of applicants are analysed separately by school type or gender (not shown).

Second, I alter my control group to one which should even more closely resemble the

Table 4.15: Proportion of all applicants getting an offer, all applicants getting an interview, and all interviewees getting an offer - placebo test: difference in differences estimates

	(1) Offer	(2) Interview	(3) Inter.$\rightarrow$Offer
Constant (α)	0.292	0.805	0.362
	(0.006)***	(0.007)***	(0.007)***
Treated (β)	-0.040	0.050	-0.066
	(0.016)**	(0.016)***	(0.019)***
Policy Placebo (γ)	-0.014	-0.033	-0.003
	(0.006)**	(0.004)***	(0.007)
Treated*Policy Placebo (δ)	0.013	-0.012	0.017
	(0.016)	(0.021)	(0.018)
N	116	116	116
R^2	0.064	0.157	0.128

Notes: Analysis excludes individuals for whom school type is unknown. Policy Off in 2005; Policy On in 2006. Standard errors, clustered by college, in parentheses. Stars indicate staistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.16: Proportion of applicants getting an offer, applicants getting an interview, and interviewees getting an offer - restricted control group: difference in differences estimates

	(1) Offer	(2) Interview	(3) Inter.$\rightarrow$Offer
Constant (α)	0.245	0.667	0.368
	(0.007)***	(0.014)***	(0.010)***
Treated (β)	0.005	0.162	-0.066
	(0.016)	(0.019)***	(0.020)***
Policy On (γ)	-0.031	-0.050	-0.016
	(0.007)***	(0.012)***	(0.011)
Treated*Policy On (δ)	-0.025	-0.204	0.046
	(0.014)*	(0.025)***	(0.021)**
N	116	116	116
R^2	0.148	0.597	0.108

Notes: Analysis excludes individuals for whom school type is unknown. Policy Off in 2005, 2006 and 2007; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college, in parentheses. Stars indicate staistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.17: Proportion of all applicants getting an offer, an interview, and interviewees getting an offer - comparing applicants from schools in high and low SES areas: difference in differences estimates

Variable \ Outcome	Offer		Interview		Interview→Offer	
	(1)	(2)	(3)	(4)	(5)	(6)
	High	Low	High	Low	High	Low
Constant (α)	0.140	0.150	0.363	0.429	0.177	0.190
	(0.004)***	(0.004)***	(0.007)***	(0.007)***	(0.005)***	(0.005)***
Treated (β)	-0.015	-0.020	0.055	-0.013	-0.027	-0.034
	(0.011)	(0.011)*	(0.018)***	(0.017)	(0.013)**	(0.013)**
Policy On (γ)	-0.020	-0.023	-0.040	-0.068	-0.002	-0.004
	(0.004)***	(0.003)***	(0.005)***	(0.004)***	(0.004)	(0.004)
Treated*Policy On (δ)	-0.004	-0.010	-0.094	-0.050	0.023	0.013
	(0.011)	(0.010)	(0.018)***	(0.019)**	(0.014)	(0.014)
N	116	116	116	116	116	116
R^2	0.137	0.218	0.440	0.456	0.058	0.092

Notes: Analysis excludes individuals for whom school type is unknown. Policy Off in 2005, 2006 and 2007; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college, in parentheses. Stars indicate staistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

treatment group: applicants to Social Science courses.[23] Table 4.16 shows the results, with the interaction between Economics and Policy On (δ) being the key coefficient of interest in each model. It shows the estimated impact on the proportion of applicants getting an interview as being a reduction of 22.9 percentage points, while for the proportion of interviewees getting a place the estimate is an increase of 6.0 percentage points. These are rather larger than the estimates in the main analysis of 14.4 percentage points and 6.4 percentage points, respectively, but tell a similar story. The impact on the proportion of applicants who get a place is estimated at close to zero and statistically insignificant. Once again, there is little divergence from this picture when the proportions of applicants are analysed separately by school type or gender (not shown).

Finally, I employ an alternative proxy of socioeconomic status. Instead of attendance at an independent school, I define a binary variable set to zero when applicants attend schools in the three most deprived fifths of postcodes, according to the Index of Deprivation Affecting Children and Infants (IDACI),[24] and set to one when they attend schools in the least two deprived fifths of postcodes. This roughly replicates the proportions of independent school applicants. The polychoric correlation between an individual attend-

[23]I define Social Science courses as follows: Experimental Psychology; Geography; History and Economics (although an Economics subject this did not introduce the TSA); History and Politics; Law; Law with Law Studies in Europe; and Psychology, Philosophy and Physiology (PPP).

[24]I take an alternative approach to analysis using IDACI in Appendix E.1. This does not involve converting it to a dichotomous variable in this way, which does reduce the informative content of the variable. I also include more detail on the construction of the IDACI.

ing an independent school and attending a school in a 'high SES area' is 0.37. Looked at another way, 52% of individuals in the dataset who attend a school in a 'high SES area' are attending an independent school. By contrast, only 29% of those attending a school in a 'low SES area' are attending an independent school. I re-estimate my DiD model, with successful proportions split by this variable.

The results are shown in Table 4.17 and produce similar estimates to those from the main analysis. For example, the proportion of all applicants who are called to interview and come from a school in a high SES area is reduced by 7.9 percentage points, compared with 8.5 percentage points for independent schools. Similarly, the proportion of all applicants who are called to interview and come from a school in a low SES area is reduced by 5.0 percentage points, compared with 5.9 percentage points for state schools.

The results from these robustness checks are very encouraging, producing no significant effect from a placebo test and substantively similar results to my main analysis for the two other tests.

4.10 Conclusions

This chapter has estimated the effects of introducing an aptitude test to an elite university's admissions process using difference in differences methods and data from the University of Oxford. No evidence is found of an overall impact on the proportion of applicants who receive an offer of a place to study at the University. The policy was coupled with a policy setting a target number of interviews per place, reducing the proportion of applicants invited to interview (by 14 percentage points). Offsetting this, the proportion of interviewees receiving an interview increased (by 3.6 percentage points), driven by the reduction in the number of interviewees rather than an increase in the number of offers.

There is no clear evidence of differential effects on the proportion of all applicants offered a place by the school type individuals come from. Splitting the admissions process into its constituent parts: at first glance, there appeared to be evidence that the reduction in the proportion of applicants called to interview had a larger (negative) effect on the proportion of all applicants getting an interview who come independent school, although when examined more closely this was driven by peculiarities relating to the first year of introduction. Furthermore, there is little convincing evidence of heterogeneity by school type in the proportion of interviewees offered a place.

In the case of differences by gender, while there is no strong evidence of overall differences between the effects on the proportion of all applicants getting an offer and who come from each gender, there is some evidence of males and females being affected differently by the introduction of an aptitude test at different points of the admissions process. Males appear relatively less likely to be called for an interview, while female interviewees are subsequently less likely to be offered a place. However, the statistical evidence is weaker in the case of the former.

In concluding, it is important to consider the issue of external validity and how relevant these findings are beyond this immediate setting. Admissions procedures at the University of Oxford are relatively similar to those at the University of Cambridge, which also uses the TSA as part of its selection processes to a wider range of courses. However, these two universities together make up about 1.5% of undergraduate places available in the Higher Education sector during the period of analysis. Admissions procedures are somewhat different at other highly selective universities in England, particularly in that many applicants are offered a place without having been interviewed. Nevertheless, we should note that these other highly selective universities are increasingly using selection tests similar in nature to the TSA, especially for highly competitive courses, with the LNAT (Law National Aptitude Test) for Law and the UKCAT (UK Clinical Aptitude Test) for Medicine both stressing their focus on skills and aptitude rather than knowledge. Furthermore, undergraduates who study at these highly selective institutions and who study these highly competitive subjects are more likely to enter highly influential jobs. For example, analysis by the Social Mobility and Child Poverty Commission finds that 75% of senior judges went to the Universities of Oxford or Cambridge, while a further 20% went to a Russell Group institution (Milburn, 2013, p.32).

To return to the question posed in the title, I do not find strong evidence that introducing an aptitude test to the admissions process of an elite university will have differing effects on applicants' chances of being offered a place depending on their socioeconomic status. Furthermore, while I do find differences in the effects of introducing the test on each gender at different points of the admissions process, I do not find strong evidence that the introduction of an aptitude test affects the relative chances of admission by gender.

Chapter 5

Summary and conclusions

5.1 Summary

In this thesis I have analysed inequalities in access to Higher Education (HE) in England. I have provided important new evidence about this issue, making use of new data, researching new areas, and taking innovative approaches.

First, in Chapter 2, I estimated the household income gradient in university participation for a recent cohort of young people in England; there was previously little work on socio-economic status gradients in access to university measured using income. Those in the top fifth of the income distribution are almost three times as likely to attend university as those in the bottom fifth. This relationship persisted, albeit smaller, even once I controlled for a range of other confounding factors, including some that seem likely to lead to an underestimate of the direct effect of income on university participation decisions.

I built on this by analysing the income gradient in university applications, using the more in depth information on the university admissions process available in the LSYPE. While I found substantial income gradients in university attendance, most of this inequality emerges at or before the point of application: even after controlling for prior attainment and socioeconomic background a significant application gap remains. By contrast, the household income gradient for attendance conditional on having applied is much smaller: those in the top fifth of the income distribution are approximately 1.3 times more likely to attend than those in the bottom fifth. Moreover, this difference disappears rapidly once controls for earlier educational attainment are added to the model.

I also analysed attendance at Russell Group universities, a group of 'high status' institutions. The gradient in attendance at a Russell Group university, conditional on attending any university, closes completely once I control for prior attainment and other socio-economic characteristics. Without better data on the institution choices of university applicants, it is impossible to analyse this Russell Group admissions gradient fully. Nevertheless, this analysis provides fresh insights compared to previous work in this field.

Second, in Chapter 3, I assessed the role of socio-economic status in explaining changes in university expectations between ages 14 and 17. I analysed transitions in young people's expectations from being 'likely to apply' to being 'unlikely to apply' and vice versa. I took the innovative approach of using duration modelling techniques to analyse changes in expectations directly. My findings confirm that this is a period when a great deal of change occurs in young people's expectations. They also highlight that this change is not just from being 'likely to apply' to being 'unlikely to apply', but rather runs in both directions.

Importantly, I found that young people's socioeconomic background does have a significant association with changes in expectations: while young people across the socio-economic status distribution start their adolescence with high educational expectations, those from less advantaged backgrounds are much more likely to revise their expectations downwards and much less likely to raise their expectations during this period. This finding persisted, even once I controlled for prior academic attainment and other potential confounding factors, suggesting that a substantial portion of the socio-economic status gap in university applications opens during this period.

Furthermore, I examined how young people respond to new information on their academic attainment provided by the results of examinations taken at age 16. Unsurprisingly, these results do affect the probability of changing from reporting being 'likely' to 'unlikely' or vice versa. More interestingly, the results also suggest that the extent of this responsiveness is affected by socioeconomic status; young people from less advantaged backgrounds are more likely to respond to equivalent results at age 16 by lowering their expectations, but less likely to respond by raising their expectations. As such, these differences in response compound inequality in university expectations.

Finally, in Chapter 4, I looked in depth at one aspect of entry to an elite university. Specifically, I estimated the effect of the introduction of an aptitude test as a screening device in this context on the proportion of successful applicants by school type (state versus private) and gender. The estimates were obtained by applying a difference in differences approach to administrative data from the University of Oxford, taking advantage of the introduction of the Thinking Skills Assessment for Economics subjects, but not others.

Overall, I found no clear evidence of differential effects on the proportion of all applicants offered a place by individuals' school type. Splitting the admissions process into its constituent parts: at first glance, there appeared to be evidence that the reduction in the proportion of applicants called to interview had a larger (negative) effect on the proportion of all applicants getting an interview who come independent school, although when examined more closely this was driven by peculiarities relating to the first year of introduction. Furthermore, there is little convincing evidence of heterogeneity by school type in the proportion of interviewees offered a place.

However, while my estimates suggested that introducing the test increased the proportion of interviewees getting an offer overall, this was not found to be the case for women. There is some evidence of males and females being affected differently by the introduction of an aptitude test at different points of the admissions process. Males appear relatively less likely to be called for an interview, while female interviewees are subsequently less likely to be offered a place. Nevertheless, I do not find strong evidence that the introduction of this aptitude test to the admissions process of an elite university had differing effects on applicants' chances of being offered a place depending on their gender overall.

5.2 Main conclusions

A major theme that has emerged from the constituent chapters of this thesis is that socio-economic inequalities in access to Higher Education emerge before the point of application. They develop through socio-economic inequalities in academic attainment, for example as measured through GCSE performance at age 16, and widening inequalities in expectations of applying to university. Obviously, these two processes will be intertwined. This suggests that reducing the extent of socio-economic inequality is more likely to be achieved through policies that target young people from deprived backgrounds earlier in their educational careers. As well as concurring with much previous evidence on the emergence of socio-economic inequality in educational attainment (Cunha et al., 2006), this thesis develops the literature further by highlighting the ongoing link between inequality and educational decisions, such as the continued association between household income and application to university even once examination performance at age 16 is accounted for.

My results also suggest that universities do not discriminate against students from poorer backgrounds; rather, such students are less likely to apply, for potentially a multitude of reasons. This finding persists when we consider specifically access to a group of the most prestigious institutions (albeit with the data available, I could not estimate all relevant stages of admissions, specifically whether young people choose to make an application to such an institution). However, this should not be an excuse for universities to assume that the issue is somebody else's problem. As I showed in Chapter 4, reforms to admissions systems can make a difference to fair access, even if it only a small one. Universities should rigorously evaluate their admissions procedures to ensure that these support the aim of fair access, as defined in Chapter 1.

In addition, findings from Chapter 3 suggest that more could usefully be done to maintain the educational expectations of academically able young people from less advantaged families during their teenage years. A positive implication of this is that it is not too late to target policies, both to maintain and to raise educational expectations, at bright individuals from less advantaged backgrounds during this period of their lives. However, of the two, raising expectations of applying to university may be less effective than maintaining expectations. Furthermore, my results do suggest that expectations become increasingly fixed as young people get older, further highlighting the need to target interventions towards the start of this period.

5.3 Future research

Unsurprisingly, as well as providing answers, this thesis raises new questions. As such, the findings presented in this thesis point to new areas of research. Below, I highlight key issues raised by this thesis that future research could address in order to enhance understanding of inequalities in access to Higher Education in England.

Due to the constantly evolving policy environment, ongoing work will be needed to analyse whether the levels of and reasons for inequalities are changing in response. Most obviously, the further increases in undergraduate tuition fees for students starting in or after September 2011, along with the changes to the financial support systems available (Chowdry et al., 2012), mean that analysis of inequality for a more recent cohort will be important to understanding whether these reforms have made a difference to SES gradients in access. Use of new, but comparable, data such as the recently commissioned second cohort of the Longitudinal Study of Young People in England (LSYPE2) will hopefully make it easy for future research to analyse changes in the intervening period.

The work reported in Chapter 2 was unable to analyse all steps in the university admissions process that I would have liked to. Most particularly, not being able to identify whether individuals *applied* to Russell Group universities meant that I couldn't address the issue of income gradients in attending these institutions, conditional on having applied to at least one. Boliver (2013) has used data that were able to separate out these two issues, but which did not have the detailed prior attainment data (particularly performance at age 16) or rich family background data, such as a measure of household income, that I was able to draw on. It would be possible to bring more certainty to this issue if the necessary questions are included in future surveys of this age range (such as the LSYPE2) or if it were possible to analyse the National Pupil Database (NPD) linked to UCAS university application and admissions data, although this would still face the restriction of not having rich measures of SES.

Furthermore, because of the data currently available, my work also leaves out the important step of graduation from university and subsequent activities (most commonly entry to the labour market). There is some evidence that "pupils from independent and selective state schools, those from state schools with a low proportion of FSM-eligible pupils and those from high-value-added state schools are [...] significantly more likely to drop out, significantly less likely to complete their degree and significantly less likely to graduate with a first or a 2:1 than their counterparts in non-selective state schools, state schools with a high proportion of FSM-eligible pupils and low-value-added state schools respectively" (Crawford, 2014, p.74), once confounding factors such as prior attainment have been controlled for. This points to the conditional SES gradient in receiving Higher Education being potentially smaller than the SES gradient in attending university, but relies on administrative data. In a few years, much more can be learnt about these issues by extending my analysis to analyse socio-economic inequality in drop-out, degree classification, and early data on labour market outcomes, using the same rich family background data available in the LSYPE along with the forthcoming age 25 wave of data.

The importance of the subjects that young people choose to study, or are encouraged to study, while at school for their chances of entering HE is another area that has not received sufficient attention (although there has been some work for much older cohorts van de Werfhorst et al. (2003)). In particular, differing qualification choices at ages 14 and 16 being associated with SES may be an important part of the reason for the large gradient in access to university that I have found. Future work using the LSYPE and linked administrative data from the NPD and the Higher Education Statistics Authority would allow additional insights into this potentially important driver of inequality.

I highlighted in Chapter 4 that I could only assess how the chances of entry to the University of Oxford changed as a result of the introduction of the aptitude test. With additional data, linking these admissions data to degree examination results, it would be possible to assess whether the efficiency gained in the admissions process from introducing an aptitude test is traded off against selecting the highest quality applicants for the course (i.e. maximising their performance at the end of the course).

Finally, this thesis has concentrated exclusively on access to undergraduate higher education. However, more work is needed to analyse the extent of inequality in access to postgraduate courses, particularly in light of the increasing proportion of young people entering such courses and the additional returns to completing such courses (Lindley and Machin, 2011). The large upfront costs of many postgraduate courses suggests that young people from disadvantaged backgrounds are less likely to be able to take advantage these additional returns, and hence may be placed at a disadvantage in upper levels of the labour market.

Appendix A

Supplementary results from Chapter 2

Table A.1: Models for university attendance, reporting marginal effects at means

	M1-Probit	M2-Probit	M3-Probit	M4-Probit	M8-LP (with FE)	M5-Probit	M6-Probit	M7-Probit	M9-LP (with FE)
1st quintile of household income (Low)	-0.102	-0.033	-0.043	-0.044	-0.015	0.049	0.007	0.005	0.008
	(0.017)***	(0.019)*	(0.023)*	(0.023)*	(0.017)	(0.019)***	(0.021)	(0.021)	(0.016)
2nd quintile of household income	-0.071	-0.055	-0.047	-0.049	-0.038	-0.011	-0.018	-0.019	-0.016
	(0.019)***	(0.020)***	(0.022)**	(0.023)**	(0.016)**	(0.017)	(0.018)	(0.019)	(0.014)
4th quintile of household income	0.109	0.057	0.036	0.028	0.020	0.019	0.019	0.017	0.008
	(0.019)***	(0.020)***	(0.020)*	(0.021)	(0.016)	(0.016)	(0.017)	(0.017)	(0.015)
5th quintile of household income (High)	0.326	0.230	0.158	0.109	0.073	0.094	0.080	0.071	0.045
	(0.020)***	(0.022)***	(0.024)***	(0.026)***	(0.019)***	(0.020)***	(0.021)***	(0.022)***	(0.017)***
KS2 Score		-0.059	-0.014	0.016	-0.095	-0.033	-0.002	0.005	-0.004
		(0.030)*	(0.033)	(0.035)	(0.018)***	(0.035)	(0.037)	(0.037)	(0.018)
KS2 Score Squared		0.017	0.014	0.011	0.016	0.001	-0.001	-0.001	-0.001
		(0.002)***	(0.003)***	(0.003)***	(0.002)***	(0.003)	(0.003)	(0.003)	(0.002)
Capped GCSE Score (Low)						0.011	0.010	0.009	-0.002
						(0.004)***	(0.004)**	(0.004)**	(0.001)**
Capped GCSE Score (High)						0.057	0.055	0.056	0.050
						(0.005)***	(0.005)***	(0.005)***	(0.003)***
Capped GCSE Score (High) Squared						-0.000	-0.000	-0.000	-0.000
						(0.000)	(0.000)	(0.000)	(0.000)
Male			-0.109	-0.118	-0.086		-0.022	-0.026	-0.021
			(0.014)***	(0.013)***	(0.010)***		(0.012)*	(0.012)**	(0.010)**
Lone Parent Family			-0.070	-0.069	-0.045		-0.011	-0.011	-0.003
			(0.020)***	(0.020)***	(0.013)***		(0.017)	(0.017)	(0.012)
Mother's Education - No Quals			-0.034	-0.026	-0.007		-0.010	-0.010	0.005
			(0.023)	(0.023)	(0.017)		(0.021)	(0.021)	(0.015)
Mother's Education - Below GCSEs			-0.041	-0.039	-0.031		-0.021	-0.022	-0.013
			(0.030)	(0.030)	(0.020)		(0.026)	(0.026)	(0.018)
Mother's Education - A Levels			0.026	0.015	-0.002		0.003	0.001	-0.007
			(0.020)	(0.020)	(0.018)		(0.017)	(0.017)	(0.016)
Mother's Education - HE Below Degree			0.075	0.062	0.051		0.043	0.041	0.039
			(0.022)***	(0.023)***	(0.019)***		(0.019)**	(0.020)**	(0.017)**
Mother's Education - Degree			0.075	0.058	0.033		0.007	0.005	0.001
			(0.028)***	(0.028)**	(0.022)		(0.024)	(0.024)	(0.020)
Father's Education - No Quals			0.005	0.012	0.017		0.029	0.030	0.032
			(0.024)	(0.023)	(0.017)		(0.021)	(0.021)	(0.015)**
Father's Education - Below GCSEs			0.002	0.007	0.009		0.025	0.026	0.020
			(0.031)	(0.031)	(0.022)		(0.026)	(0.026)	(0.020)
Father's Education - A Levels			-0.006	-0.007	-0.001		-0.010	-0.011	-0.006
			(0.020)	(0.020)	(0.017)		(0.017)	(0.017)	(0.015)
Father's Education - HE Below Degree			0.083	0.082	0.070		0.061	0.059	0.053
			(0.024)***	(0.024)***	(0.020)***		(0.020)***	(0.020)***	(0.018)***
Father's Education - Degree			0.200	0.187	0.150		0.113	0.110	0.096
			(0.026)***	(0.026)***	(0.021)***		(0.023)***	(0.023)***	(0.019)***
KS3 School Type - CTC				-0.032				-0.123	
				(0.069)				(0.035)***	
KS3 School Type - Foundation				0.005				-0.020	
				(0.021)				(0.018)	
KS3 School Type - Independent				0.380				0.082	
				(0.043)***				(0.036)**	
KS3 School Type - Voluntary Aided				0.081				0.016	
				(0.023)***				(0.020)	
KS3 School Type - Voluntary Controlled				0.062				0.019	
				(0.042)				(0.034)	
Grammar School				0.197				0.073	
				(0.043)***				(0.042)*	
School has Sixth Form				0.045				0.034	
				(0.016)***				(0.014)**	
Region	No	No	Yes	Yes	No	No	Yes	Yes	No
Ethnicity	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Month of Birth	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Sibling effects	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
F Test	.	318.497	34.675	30.591	45.718	323.044	42.302	36.731	105.037
Prob > F	.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sub-sample size	7939	7939	7939	7939	7939	7939	7939	7939	7939

Notes: Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Marginal effects estimated at sample means, holding household equivalised income constant. Weighted using LSYPE Wave 7 respondent weights, which attempt to adjust for oversampling and attrition. Standard errors (adjusted for school level clustering and stratification by deprivation) reported in parentheses. Prior attainment variables are divided by 10, hence the coefficient estimates represent the expected change in probability for an additional 10 points. Base category for household equivalised income is middle (3rd) quintile group. Base category for parental education is achieving GCSEs or equivalents. Base category for family type is married or cohabiting couple. Base category for KS3 School Type is Community School. Base category for sex is female. Sample: Wave 7 participants with valid responses for all variables used in models. Marginal effect for discrete variables is the change from base category.

Table A.2: Models for university attendance, reporting marginal effects at means - Males

	M1-Probit	M2-Probit	M3-Probit	M4-Probit	M8-LP (with FE)	M5-Probit	M6-Probit	M7-Probit	M9-LP (with FE)
1st quintile of household income (Low)	-0.091	-0.019	-0.015	-0.010	0.011	0.070	0.040	0.039	0.025
	(0.023)***	(0.026)	(0.032)	(0.031)	(0.025)	(0.027)**	(0.030)	(0.030)	(0.022)
2nd quintile of household income	-0.053	-0.035	-0.024	-0.021	-0.017	0.010	0.009	0.009	-0.001
	(0.024)**	(0.026)	(0.030)	(0.031)	(0.024)	(0.023)	(0.027)	(0.027)	(0.021)
4th quintile of household income	0.093	0.048	0.029	0.023	0.023	0.020	0.022	0.020	0.012
	(0.027)***	(0.027)*	(0.029)	(0.029)	(0.024)	(0.022)	(0.024)	(0.024)	(0.021)
5th quintile of household income (High)	0.337	0.250	0.160	0.125	0.094	0.112	0.087	0.089	0.057
	(0.029)***	(0.030)***	(0.031)***	(0.033)***	(0.027)***	(0.028)***	(0.028)***	(0.028)***	(0.024)**
KS2 Score		-0.071	-0.007	0.029	-0.095	0.009	0.062	0.076	0.033
		(0.040)*	(0.044)	(0.045)	(0.030)***	(0.047)	(0.047)	(0.048)	(0.028)
KS2 Score Squared		0.018	0.012	0.009	0.016	-0.002	-0.005	-0.006	-0.004
		(0.003)***	(0.003)***	(0.004)**	(0.003)***	(0.004)	(0.004)	(0.004)*	(0.002)
Capped GCSE Score (Low)						0.015	0.012	0.012	-0.003
						(0.006)**	(0.006)*	(0.006)*	(0.001)**
Capped GCSE Score (High)						0.045	0.044	0.045	0.049
						(0.007)***	(0.007)***	(0.007)***	(0.004)***
Capped GCSE Score (High) Squared						0.000	0.000	0.000	0.000
						(0.000)	(0.000)	(0.000)	(0.000)
Lone Parent Family			-0.078	-0.078	-0.056		-0.028	-0.028	-0.024
			(0.026)***	(0.026)***	(0.020)***		(0.023)	(0.023)	(0.018)
Mother's Education - No Quals			-0.006	0.005	0.023		0.024	0.022	0.038
			(0.032)	(0.032)	(0.026)		(0.029)	(0.029)	(0.023)*
Mother's Education - Below GCSEs			-0.000	0.001	-0.005		0.046	0.044	0.023
			(0.041)	(0.041)	(0.030)		(0.036)	(0.036)	(0.028)
Mother's Education - A Levels			0.032	0.013	0.007		0.009	0.006	0.007
			(0.026)	(0.027)	(0.026)		(0.024)	(0.024)	(0.024)
Mother's Education - HE Below Degree			0.086	0.076	0.076		0.074	0.074	0.069
			(0.030)***	(0.031)**	(0.028)***		(0.026)***	(0.026)***	(0.025)***
Mother's Education - Degree			0.081	0.067	0.039		0.024	0.022	0.011
			(0.036)**	(0.037)*	(0.033)		(0.032)	(0.032)	(0.030)
Father's Education - No Quals			0.014	0.015	0.038		0.031	0.032	0.048
			(0.032)	(0.032)	(0.025)		(0.030)	(0.030)	(0.023)**
Father's Education - Below GCSEs			0.010	0.018	0.027		0.045	0.047	0.048
			(0.044)	(0.044)	(0.033)		(0.037)	(0.037)	(0.029)*
Father's Education - A Levels			0.014	0.012	0.019		0.004	0.002	0.004
			(0.027)	(0.027)	(0.024)		(0.023)	(0.022)	(0.022)
Father's Education - HE Below Degree			0.099	0.100	0.091		0.085	0.083	0.080
			(0.032)***	(0.032)***	(0.030)***		(0.028)***	(0.028)***	(0.027)***
Father's Education - Degree			0.213	0.204	0.187		0.117	0.117	0.123
			(0.035)***	(0.036)***	(0.032)***		(0.033)***	(0.033)***	(0.030)***
KS3 School Type - CTC				0.057				-0.113	
				(0.080)				(0.085)	
KS3 School Type - Foundation				0.007				-0.025	
				(0.028)				(0.023)	
KS3 School Type - Independent				0.285				-0.004	
				(0.052)***				(0.043)	
KS3 School Type - Voluntary Aided				0.085				0.003	
				(0.031)***				(0.026)	
KS3 School Type - Voluntary Controlled				0.059				0.004	
				(0.051)				(0.038)	
Grammar School				0.190				0.082	
				(0.048)***				(0.049)*	
School has Sixth Form				0.035				0.030	
				(0.020)*				(0.017)*	
Region	No	No	Yes	Yes	No	No	Yes	Yes	No
Ethnicity	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Month of Birth	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Sibling effects	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
F Test	.	162.024	16.644	16.483	19.541	137.261	21.056	18.589	45.576
Prob > F	.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sub-sample size	3848	3848	3848	3848	3848	3848	3848	3848	3848

Notes: Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Marginal effects estimated at sample means, holding household equivalised income constant. Weighted using LSYPE Wave 7 respondent weights, which attempt to adjust for oversampling and attrition. Standard errors (adjusted for school level clustering and stratification by deprivation) reported in parentheses. Prior attainment variables are divided by 10, hence the coefficient estimates represent the expected change in probability for an additional 10 points. Base category for household equivalised income is middle (3rd) quintile group. Base category for parental education is achieving GCSEs or equivalents. Base category for family type is married or cohabiting couple. Base category for KS3 School Type is Community School. Sample: Wave 7 participants with valid responses for all variables used in models. Marginal effect for discrete variables is the change from base category.

Table A.3: Models for university attendance, reporting marginal effects at means - Females

	M1-Probit	M2-Probit	M3-Probit	M4-Probit	M8-LP (with FE)	M5-Probit	M6-Probit	M7-Probit	M9-LP (with FE)
1st quintile of household income (Low)	-0.118	-0.054	-0.078	-0.082	-0.056	0.029	-0.027	-0.031	-0.019
	(0.025)***	(0.028)*	(0.036)**	(0.035)**	(0.027)**	(0.025)	(0.030)	(0.030)	(0.025)
2nd quintile of household income	-0.095	-0.083	-0.081	-0.086	-0.073	-0.034	-0.052	-0.056	-0.041
	(0.026)***	(0.028)***	(0.033)**	(0.033)***	(0.024)***	(0.023)	(0.027)*	(0.028)**	(0.022)*
4th quintile of household income	0.121	0.062	0.045	0.034	0.014	0.018	0.020	0.016	0.008
	(0.026)***	(0.027)**	(0.029)	(0.029)	(0.025)	(0.024)	(0.025)	(0.025)	(0.023)
5th quintile of household income (High)	0.313	0.202	0.144	0.084	0.042	0.076	0.067	0.047	0.023
	(0.027)***	(0.032)***	(0.035)***	(0.033)**	(0.027)	(0.028)***	(0.028)**	(0.028)*	(0.025)
KS2 Score		-0.055	-0.015	0.009	-0.090	-0.066	-0.047	-0.050	-0.045
		(0.042)	(0.046)	(0.051)	(0.027)***	(0.045)	(0.047)	(0.049)	(0.028)
KS2 Score Squared		0.017	0.014	0.012	0.016	0.003	0.002	0.003	0.003
		(0.003)***	(0.004)***	(0.004)***	(0.002)***	(0.004)	(0.004)	(0.004)	(0.002)
Capped GCSE Score (Low)						0.009	0.008	0.007	-0.002
						(0.004)**	(0.004)*	(0.004)	(0.001)
Capped GCSE Score (High)						0.066	0.065	0.067	0.057
						(0.006)***	(0.007)***	(0.007)***	(0.004)***
Capped GCSE Score (High) Squared						-0.001	-0.001	-0.001	-0.001
						(0.000)*	(0.000)*	(0.000)**	(0.000)***
Lone Parent Family			-0.065	-0.068	-0.043		0.005	0.003	0.006
			(0.027)**	(0.027)**	(0.020)**		(0.023)	(0.023)	(0.018)
Mother's Education - No Quals			-0.063	-0.058	-0.036		-0.044	-0.045	-0.029
			(0.034)*	(0.034)*	(0.026)		(0.030)	(0.031)	(0.023)
Mother's Education - Below GCSEs			-0.074	-0.075	-0.053		-0.077	-0.081	-0.048
			(0.042)*	(0.041)*	(0.031)*		(0.035)**	(0.036)**	(0.028)*
Mother's Education - A Levels			0.025	0.025	0.001		-0.004	-0.003	-0.015
			(0.029)	(0.028)	(0.026)		(0.024)	(0.024)	(0.024)
Mother's Education - HE Below Degree			0.072	0.056	0.036		0.017	0.013	0.016
			(0.032)**	(0.032)*	(0.028)		(0.027)	(0.028)	(0.025)
Mother's Education - Degree			0.079	0.062	0.032		0.003	-0.002	-0.000
			(0.043)*	(0.041)	(0.033)		(0.034)	(0.034)	(0.029)
Father's Education - No Quals			-0.002	0.011	-0.002		0.026	0.031	0.022
			(0.033)	(0.033)	(0.025)		(0.027)	(0.028)	(0.023)
Father's Education - Below GCSEs			-0.009	-0.008	-0.006		0.004	0.005	0.004
			(0.044)	(0.044)	(0.034)		(0.035)	(0.036)	(0.030)
Father's Education - A Levels			-0.031	-0.029	-0.023		-0.022	-0.023	-0.017
			(0.029)	(0.030)	(0.025)		(0.026)	(0.026)	(0.023)
Father's Education - HE Below Degree			0.057	0.051	0.040		0.032	0.031	0.017
			(0.035)*	(0.034)	(0.029)		(0.028)	(0.028)	(0.027)
Father's Education - Degree			0.170	0.153	0.098		0.093	0.089	0.062
			(0.040)***	(0.039)***	(0.031)***		(0.032)***	(0.032)***	(0.028)**
KS3 School Type - CTC				-0.108				-0.138	
				(0.134)				(0.075)*	
KS3 School Type - Foundation				0.006				-0.010	
				(0.030)				(0.029)	
KS3 School Type - Independent				0.488				0.192	
				(0.074)***				(0.056)***	
KS3 School Type - Voluntary Aided				0.069				0.019	
				(0.031)**				(0.028)	
KS3 School Type - Voluntary Controlled				0.071				0.034	
				(0.057)				(0.053)	
Grammar School				0.207				0.052	
				(0.073)***				(0.065)	
School has Sixth Form				0.054				0.038	
				(0.022)**				(0.020)*	
Region	No	No	Yes	Yes	No	No	Yes	Yes	No
Ethnicity	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Month of Birth	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Sibling effects	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
F Test	.	167.699	18.579	15.485	21.510	173.221	25.854	21.337	50.766
Prob > F	.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sub-sample size	4091	4091	4091	4091	4091	4091	4091	4091	4091

Notes: Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Marginal effects estimated at sample means, holding household equivalised income constant. Weighted using LSYPE Wave 7 respondent weights, which attempt to adjust for oversampling and attrition. Standard errors (adjusted for school level clustering and stratification by deprivation) reported in parentheses. Prior attainment variables are divided by 10, hence the coefficient estimates represent the expected change in probability for an additional 10 points. Base category for household equivalised income is middle (3rd) quintile group. Base category for parental education is achieving GCSEs or equivalents. Base category for family type is married or cohabiting couple. Base category for KS3 School Type is Community School. Sample: Wave 7 participants with valid responses for all variables used in models. Marginal effect for discrete variables is the change from base category.

Table A.4: Models for university application, reporting marginal effects at means

	M1-Probit	M2-Probit	M3-Probit	M4-Probit	M8-LP (with FE)	M5-Probit	M6-Probit	M7-Probit	M9-LP (with FE)
1st quintile of household income (Low)	-0.114	-0.044	-0.060	-0.060	-0.037	0.051	-0.009	-0.010	-0.005
	(0.020)***	(0.022)**	(0.025)**	(0.025)**	(0.019)*	(0.024)**	(0.026)	(0.026)	(0.017)
2nd quintile of household income	-0.075	-0.059	-0.048	-0.048	-0.045	-0.014	-0.023	-0.023	-0.024
	(0.021)***	(0.023)**	(0.025)*	(0.025)*	(0.018)**	(0.023)	(0.024)	(0.025)	(0.016)
4th quintile of household income	0.111	0.060	0.040	0.032	0.029	0.023	0.024	0.021	0.016
	(0.020)***	(0.021)***	(0.022)*	(0.022)	(0.017)*	(0.021)	(0.022)	(0.022)	(0.016)
5th quintile of household income (High)	0.315	0.237	0.176	0.125	0.080	0.133	0.117	0.102	0.052
	(0.020)***	(0.022)***	(0.024)***	(0.025)***	(0.019)***	(0.025)***	(0.027)***	(0.027)***	(0.017)***
KS2 Score		-0.128	-0.103	-0.090	-0.051	-0.020	0.000	-0.000	-0.018
		(0.031)***	(0.036)***	(0.037)**	(0.022)**	(0.036)	(0.041)	(0.042)	(0.022)
KS2 Score Squared		0.022	0.020	0.019	0.013	-0.002	-0.002	-0.002	0.000
		(0.002)***	(0.003)***	(0.003)***	(0.002)***	(0.003)	(0.003)	(0.003)	(0.002)
Capped GCSE Score (Low)						0.012	0.010	0.010	0.001
						(0.002)***	(0.003)***	(0.003)***	(0.001)
Capped GCSE Score (High)						0.043	0.042	0.042	0.063
						(0.006)***	(0.006)***	(0.006)***	(0.003)***
Capped GCSE Score (High) Squared						0.001	0.001	0.001	-0.001
						(0.000)***	(0.000)***	(0.000)***	(0.000)***
Male			-0.117	-0.125	-0.094		-0.033	-0.036	-0.028
			(0.016)***	(0.015)***	(0.011)***		(0.016)**	(0.016)**	(0.010)***
Lone Parent Family			-0.072	-0.071	-0.057		-0.005	-0.007	-0.012
			(0.022)***	(0.022)***	(0.015)***		(0.023)	(0.023)	(0.014)
Mother's Education - No Quals			-0.027	-0.019	-0.016		-0.002	-0.003	-0.001
			(0.025)	(0.025)	(0.019)		(0.026)	(0.026)	(0.017)
Mother's Education - Below GCSEs			-0.033	-0.031	-0.036		-0.019	-0.020	-0.017
			(0.028)	(0.029)	(0.022)		(0.030)	(0.031)	(0.020)
Mother's Education - A Levels			0.027	0.013	-0.003		-0.003	-0.008	-0.009
			(0.024)	(0.024)	(0.019)		(0.025)	(0.026)	(0.017)
Mother's Education - HE Below Degree			0.093	0.076	0.057		0.070	0.065	0.045
			(0.025)***	(0.026)***	(0.019)***		(0.026)***	(0.026)**	(0.017)***
Mother's Education - Degree			0.102	0.084	0.044		0.027	0.022	0.016
			(0.033)***	(0.034)**	(0.021)**		(0.034)	(0.034)	(0.019)
Father's Education - No Quals			-0.024	-0.017	-0.011		0.001	0.002	0.009
			(0.025)	(0.025)	(0.019)		(0.025)	(0.025)	(0.017)
Father's Education - Below GCSEs			-0.015	-0.010	-0.003		0.011	0.011	0.010
			(0.031)	(0.031)	(0.025)		(0.031)	(0.031)	(0.022)
Father's Education - A Levels			-0.008	-0.009	-0.001		-0.012	-0.015	-0.005
			(0.022)	(0.022)	(0.018)		(0.023)	(0.023)	(0.016)
Father's Education - HE Below Degree			0.077	0.076	0.066		0.064	0.063	0.050
			(0.026)***	(0.027)***	(0.021)***		(0.028)**	(0.028)**	(0.019)***
Father's Education - Degree			0.200	0.179	0.108		0.133	0.126	0.060
			(0.031)***	(0.031)***	(0.020)***		(0.032)***	(0.033)***	(0.018)***
KS3 School Type - CTC				-0.051				-0.191	
				(0.108)				(0.051)***	
KS3 School Type - Foundation				0.014				-0.022	
				(0.026)				(0.026)	
KS3 School Type - Independent				0.489				0.176	
				(0.065)***				(0.049)***	
KS3 School Type - Voluntary Aided				0.095				0.023	
				(0.026)***				(0.030)	
KS3 School Type - Voluntary Controlled				0.036				-0.009	
				(0.047)				(0.042)	
Grammar School				0.155				0.001	
				(0.049)***				(0.060)	
School has Sixth Form				0.069				0.067	
				(0.017)***				(0.017)***	
Region	No	No	Yes	Yes	No	No	Yes	Yes	No
Ethnicity	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Month of Birth	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Sibling effects	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
F Test	.	276.757	27.272	25.733	41.017	261.631	38.370	33.360	98.650
Prob > F	.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sub-sample size	7939	7939	7939	7939	7939	7939	7939	7939	7939

Notes: Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Marginal effects estimated at sample means, holding household equivalised income constant. Weighted using LSYPE Wave 7 respondent weights, which attempt to adjust for oversampling and attrition. Standard errors (adjusted for school level clustering and stratification by deprivation) reported in parentheses. Prior attainment variables are divided by 10, hence the coefficient estimates represent the expected change in probability for an additional 10 points. Base category for household equivalised income is middle (3rd) quintile group. Base category for parental education is achieving GCSEs or equivalents. Base category for family type is married or cohabiting couple. Base category for KS3 School Type is Community School. Base category for sex is female. Sample: Wave 7 participants with valid responses for all variables used in models. Marginal effect for discrete variables is the change from base category.

Table A.5: Models for university application, reporting marginal effects at means - Males

	M1-Probit	M2-Probit	M3-Probit	M4-Probit	M8-LP (with FE)	M5-Probit	M6-Probit	M7-Probit	M9-LP (with FE)
1st quintile of household income (Low)	-0.091	-0.016	-0.008	-0.004	0.010	0.087	0.054	0.052	0.032
	(0.027)***	(0.030)	(0.034)	(0.034)	(0.028)	(0.033)***	(0.036)	(0.036)	(0.026)
2nd quintile of household income	-0.044	-0.026	-0.000	0.006	-0.007	0.027	0.031	0.033	0.006
	(0.028)	(0.029)	(0.033)	(0.034)	(0.026)	(0.029)	(0.033)	(0.033)	(0.024)
4th quintile of household income	0.109	0.067	0.050	0.044	0.042	0.041	0.046	0.044	0.027
	(0.028)***	(0.030)**	(0.032)	(0.032)	(0.026)	(0.029)	(0.032)	(0.032)	(0.023)
5th quintile of household income (High)	0.336	0.268	0.195	0.149	0.101	0.152	0.124	0.118	0.063
	(0.028)***	(0.031)***	(0.035)***	(0.034)***	(0.027)***	(0.032)***	(0.035)***	(0.035)***	(0.025)**
KS2 Score		-0.134	-0.083	-0.056	-0.035	0.027	0.073	0.086	0.041
		(0.044)***	(0.049)*	(0.050)	(0.033)	(0.051)	(0.057)	(0.057)	(0.032)
KS2 Score Squared		0.022	0.018	0.015	0.011	-0.005	-0.008	-0.009	-0.005
		(0.004)***	(0.004)***	(0.004)***	(0.003)***	(0.004)	(0.005)*	(0.005)*	(0.003)*
Capped GCSE Score (Low)						0.013	0.011	0.010	0.000
						(0.003)***	(0.003)***	(0.003)***	(0.002)
Capped GCSE Score (High)						0.034	0.038	0.038	0.061
						(0.008)***	(0.009)***	(0.009)***	(0.004)***
Capped GCSE Score (High) Squared						0.002	0.002	0.002	-0.001
						(0.000)***	(0.000)***	(0.001)***	(0.000)***
Lone Parent Family			-0.101	-0.101	-0.079		-0.044	-0.044	-0.047
			(0.030)***	(0.031)***	(0.023)***		(0.031)	(0.031)	(0.021)**
Mother's Education - No Quals			0.004	0.014	0.002		0.048	0.044	0.024
			(0.036)	(0.036)	(0.028)		(0.037)	(0.037)	(0.026)
Mother's Education - Below GCSEs			0.021	0.023	-0.005		0.068	0.067	0.027
			(0.041)	(0.041)	(0.034)		(0.042)	(0.042)	(0.031)
Mother's Education - A Levels			0.058	0.034	0.012		0.030	0.023	0.013
			(0.031)*	(0.031)	(0.027)		(0.035)	(0.035)	(0.025)
Mother's Education - HE Below Degree			0.098	0.080	0.075		0.093	0.086	0.068
			(0.035)***	(0.035)**	(0.029)***		(0.035)***	(0.035)**	(0.026)***
Mother's Education - Degree			0.116	0.097	0.054		0.052	0.046	0.030
			(0.041)***	(0.042)**	(0.031)*		(0.044)	(0.044)	(0.028)
Father's Education - No Quals			-0.033	-0.032	-0.006		-0.014	-0.013	0.009
			(0.034)	(0.034)	(0.028)		(0.036)	(0.036)	(0.026)
Father's Education - Below GCSEs			-0.013	-0.003	0.001		0.024	0.027	0.022
			(0.045)	(0.045)	(0.036)		(0.044)	(0.044)	(0.033)
Father's Education - A Levels			-0.014	-0.018	0.018		-0.027	-0.031	0.002
			(0.030)	(0.030)	(0.026)		(0.030)	(0.030)	(0.023)
Father's Education - HE Below Degree			0.101	0.102	0.092		0.104	0.102	0.080
			(0.037)***	(0.038)***	(0.032)***		(0.039)***	(0.040)**	(0.029)***
Father's Education - Degree			0.233	0.215	0.164		0.147	0.143	0.104
			(0.041)***	(0.041)***	(0.030)***		(0.043)***	(0.043)***	(0.027)***
KS3 School Type - CTC				0.090				-0.160	
				(0.134)				(0.138)	
KS3 School Type - Foundation				-0.010				-0.059	
				(0.033)				(0.030)**	
KS3 School Type - Independent				0.426				0.085	
				(0.082)***				(0.060)	
KS3 School Type - Voluntary Aided				0.100				0.002	
				(0.037)***				(0.038)	
KS3 School Type - Voluntary Controlled				-0.003				-0.075	
				(0.058)				(0.050)	
Grammar School				0.190				0.050	
				(0.050)***				(0.061)	
School has Sixth Form				0.078				0.084	
				(0.024)***				(0.023)***	
Region	No	No	Yes	Yes	No	No	Yes	Yes	No
Ethnicity	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Month of Birth	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Sibling effects	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
F Test	.	125.976	14.171	14.824	18.068	124.877	19.550	16.965	41.087
Prob > F	.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sub-sample size	3848	3848	3848	3848	3848	3848	3848	3848	3848

Notes: Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Marginal effects estimated at sample means, holding household equivalised income constant. Weighted using LSYPE Wave 7 respondent weights, which attempt to adjust for oversampling and attrition. Standard errors (adjusted for school level clustering and stratification by deprivation) reported in parentheses. Prior attainment variables are divided by 10, hence the coefficient estimates represent the expected change in probability for an additional 10 points. Base category for household equivalised income is middle (3rd) quintile group. Base category for parental education is achieving GCSEs or equivalents. Base category for family type is married or cohabiting couple. Base category for KS3 School Type is Community School. Sample: Wave 7 participants with valid responses for all variables used in models. Marginal effect for discrete variables is the change from base category.

Table A.6: Models for university application, reporting marginal effects at means - Females

	M1-Probit	M2-Probit	M3-Probit	M4-Probit	M8-LP (with FE)	M5-Probit	M6-Probit	M7-Probit	M9-LP (with FE)
1st quintile of household income (Low)	-0.145	-0.080	-0.114	-0.113	-0.091	0.011	-0.073	-0.072	-0.043
	(0.028)***	(0.030)***	(0.036)***	(0.035)***	(0.029)***	(0.033)	(0.039)*	(0.039)*	(0.026)
2nd quintile of household income	-0.113	-0.101	-0.098	-0.101	-0.091	-0.058	-0.086	-0.089	-0.058
	(0.028)***	(0.030)***	(0.032)***	(0.032)***	(0.026)***	(0.032)*	(0.035)**	(0.035)**	(0.023)**
4th quintile of household income	0.107	0.047	0.030	0.019	0.005	0.001	0.004	-0.002	-0.003
	(0.027)***	(0.028)	(0.030)	(0.030)	(0.025)	(0.031)	(0.032)	(0.032)	(0.023)
5th quintile of household income (High)	0.290	0.199	0.168	0.106	0.052	0.110	0.109	0.084	0.035
	(0.027)***	(0.030)***	(0.037)***	(0.036)***	(0.027)*	(0.036)***	(0.039)***	(0.039)**	(0.025)
KS2 Score		-0.137	-0.119	-0.120	-0.059	-0.070	-0.058	-0.071	-0.084
		(0.042)***	(0.048)**	(0.049)**	(0.032)*	(0.052)	(0.057)	(0.057)	(0.033)**
KS2 Score Squared		0.023	0.022	0.022	0.014	0.003	0.002	0.004	0.006
		(0.003)***	(0.004)***	(0.004)***	(0.003)***	(0.004)	(0.005)	(0.005)	(0.003)**
Capped GCSE Score (Low)						0.011	0.010	0.009	0.000
						(0.003)***	(0.004)***	(0.004)**	(0.002)
Capped GCSE Score (High)						0.052	0.050	0.049	0.071
						(0.008)***	(0.008)***	(0.008)***	(0.004)***
Capped GCSE Score (High) Squared						0.001	0.001	0.001	-0.001
						(0.000)**	(0.000)**	(0.000)**	(0.000)***
Lone Parent Family			-0.049	-0.052	-0.045		0.029	0.024	0.005
			(0.029)*	(0.028)*	(0.022)**		(0.031)	(0.031)	(0.020)
Mother's Education - No Quals			-0.055	-0.049	-0.020		-0.049	-0.047	-0.012
			(0.035)	(0.034)	(0.028)		(0.036)	(0.036)	(0.024)
Mother's Education - Below GCSEs			-0.080	-0.082	-0.061		-0.101	-0.104	-0.058
			(0.038)**	(0.038)**	(0.033)*		(0.042)**	(0.042)**	(0.030)**
Mother's Education - A Levels			0.004	0.001	-0.006		-0.034	-0.036	-0.024
			(0.033)	(0.033)	(0.027)		(0.036)	(0.036)	(0.025)
Mother's Education - HE Below Degree			0.093	0.078	0.057		0.051	0.048	0.037
			(0.034)***	(0.034)**	(0.028)**		(0.037)	(0.037)	(0.025)
Mother's Education - Degree			0.098	0.088	0.044		0.021	0.019	0.018
			(0.048)**	(0.048)*	(0.030)		(0.050)	(0.051)	(0.026)
Father's Education - No Quals			-0.013	-0.002	-0.020		0.016	0.018	0.010
			(0.035)	(0.035)	(0.028)		(0.035)	(0.035)	(0.025)
Father's Education - Below GCSEs			-0.020	-0.018	-0.018		-0.004	-0.003	-0.003
			(0.044)	(0.044)	(0.036)		(0.045)	(0.046)	(0.032)
Father's Education - A Levels			-0.005	-0.002	-0.026		0.006	0.003	-0.017
			(0.033)	(0.033)	(0.027)		(0.037)	(0.037)	(0.025)
Father's Education - HE Below Degree			0.047	0.041	0.043		0.021	0.020	0.023
			(0.038)	(0.038)	(0.029)		(0.041)	(0.041)	(0.027)
Father's Education - Degree			0.152	0.127	0.046		0.103	0.092	0.020
			(0.044)***	(0.045)***	(0.030)		(0.047)**	(0.048)*	(0.026)
KS3 School Type - CTC				-0.157				-0.233	
				(0.198)				(0.157)	
KS3 School Type - Foundation				0.040				0.018	
				(0.034)				(0.039)	
KS3 School Type - Independent				0.593				0.339	
				(0.081)***				(0.065)***	
KS3 School Type - Voluntary Aided				0.082				0.031	
				(0.035)**				(0.041)	
KS3 School Type - Voluntary Controlled				0.089				0.072	
				(0.061)				(0.061)	
Grammar School				0.124				-0.092	
				(0.111)				(0.132)	
School has Sixth Form				0.067				0.062	
				(0.023)***				(0.026)**	
Region	No	No	Yes	Yes	No	No	Yes	Yes	No
Ethnicity	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Month of Birth	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Sibling effects	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
F Test	.	141.563	15.185	14.806	21.796	135.105	20.761	19.032	52.397
Prob > F	.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sub-sample size	4091	4091	4091	4091	4091	4091	4091	4091	4091

Notes: Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Marginal effects estimated at sample means, holding household equivalised income constant. Weighted using LSYPE Wave 7 respondent weights, which attempt to adjust for oversampling and attrition. Standard errors (adjusted for school level clustering and stratification by deprivation) reported in parentheses. Prior attainment variables are divided by 10, hence the coefficient estimates represent the expected change in probability for an additional 10 points. Base category for household equivalised income is middle (3rd) quintile group. Base category for parental education is achieving GCSEs or equivalents. Base category for family type is married or cohabiting couple. Base category for KS3 School Type is Community School. Sample: Wave 7 participants with valid responses for all variables used in models. Marginal effect for discrete variables is the change from base category.

Table A.7: Models for university attendance, conditional on having applied, reporting marginal effects at means

	M1-Probit	M2-Probit	M3-Probit	M4-Probit	M8-LP (with FE)	M5-Probit	M6-Probit	M7-Probit	M9-LP (with FE)
1st quintile of household income (Low)	-0.052	0.006	0.007	0.004	0.015	0.052	0.032	0.029	0.026
	(0.027)*	(0.025)	(0.027)	(0.027)	(0.029)	(0.033)	(0.038)	(0.038)	(0.028)
2nd quintile of household income	-0.042	-0.020	-0.015	-0.016	0.007	0.010	0.005	0.004	0.016
	(0.027)	(0.027)	(0.027)	(0.026)	(0.025)	(0.034)	(0.035)	(0.035)	(0.024)
4th quintile of household income	0.049	0.024	0.020	0.017	0.007	0.015	0.019	0.018	0.008
	(0.022)**	(0.022)	(0.022)	(0.022)	(0.022)	(0.030)	(0.030)	(0.030)	(0.021)
5th quintile of household income (High)	0.124	0.080	0.052	0.037	0.024	0.062	0.050	0.047	0.022
	(0.022)***	(0.022)***	(0.023)**	(0.023)	(0.022)	(0.032)*	(0.032)	(0.032)	(0.021)
KS2 Score		0.029	0.050	0.070	0.144	-0.026	-0.003	0.010	-0.018
		(0.035)	(0.035)	(0.035)**	(0.039)***	(0.062)	(0.062)	(0.062)	(0.043)
KS2 Score Squared		0.004	0.003	0.001	-0.005	0.002	0.000	-0.001	0.001
		(0.003)	(0.003)	(0.003)	(0.003)*	(0.005)	(0.005)	(0.005)	(0.003)
Capped GCSE Score (Low)						0.018	0.016	0.015	0.016
						(0.008)**	(0.008)**	(0.008)*	(0.004)***
Capped GCSE Score (High)						0.049	0.048	0.049	0.054
						(0.008)***	(0.008)***	(0.008)***	(0.006)***
Capped GCSE Score (High) Squared						-0.000	-0.000	-0.000	-0.001
						(0.000)	(0.000)	(0.000)	(0.000)***
Male			-0.044	-0.049	-0.038		-0.012	-0.019	-0.003
			(0.014)***	(0.014)***	(0.015)**		(0.020)	(0.020)	(0.015)
Lone Parent Family			-0.031	-0.028	-0.031		-0.007	-0.005	-0.007
			(0.021)	(0.021)	(0.021)		(0.029)	(0.029)	(0.020)
Mother's Education - No Quals			-0.025	-0.021	-0.017		-0.033	-0.031	-0.014
			(0.026)	(0.026)	(0.029)		(0.037)	(0.037)	(0.027)
Mother's Education - Below GCSEs			-0.023	-0.021	-0.024		-0.012	-0.013	-0.006
			(0.034)	(0.033)	(0.037)		(0.047)	(0.047)	(0.036)
Mother's Education - A Levels			0.017	0.012	0.006		0.019	0.015	0.006
			(0.022)	(0.021)	(0.023)		(0.029)	(0.029)	(0.022)
Mother's Education - HE Below Degree			0.021	0.015	0.017		0.014	0.012	0.011
			(0.023)	(0.023)	(0.022)		(0.033)	(0.033)	(0.022)
Mother's Education - Degree			0.020	0.012	0.006		0.000	-0.003	0.001
			(0.025)	(0.025)	(0.023)		(0.037)	(0.037)	(0.023)
Father's Education - No Quals			0.027	0.029	0.036		0.046	0.046	0.041
			(0.024)	(0.024)	(0.028)		(0.034)	(0.034)	(0.027)
Father's Education - Below GCSEs			0.024	0.024	0.024		0.024	0.024	0.018
			(0.033)	(0.033)	(0.038)		(0.046)	(0.046)	(0.037)
Father's Education - A Levels			-0.011	-0.013	-0.002		-0.026	-0.027	-0.006
			(0.022)	(0.022)	(0.024)		(0.031)	(0.031)	(0.023)
Father's Education - HE Below Degree			0.043	0.042	0.034		0.069	0.066	0.036
			(0.026)*	(0.025)*	(0.025)		(0.035)*	(0.035)*	(0.024)
Father's Education - Degree			0.102	0.098	0.090		0.097	0.096	0.066
			(0.027)***	(0.027)***	(0.023)***		(0.038)**	(0.038)**	(0.023)***
KS3 School Type - CTC				-0.014				-0.085	
				(0.090)				(0.129)	
KS3 School Type - Foundation				-0.017				-0.034	
				(0.019)				(0.028)	
KS3 School Type - Independent				0.120				0.028	
				(0.040)***				(0.059)	
KS3 School Type - Voluntary Aided				0.025				0.009	
				(0.021)				(0.027)	
KS3 School Type - Voluntary Controlled				0.059				0.049	
				(0.045)				(0.062)	
Grammar School				0.145				0.145	
				(0.040)***				(0.059)**	
School has Sixth Form				-0.003				-0.005	
				(0.016)				(0.022)	
Region	No	No	Yes	Yes	No	No	Yes	Yes	No
Ethnicity	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Month of Birth	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Sibling effects	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
F Test	.	75.598	7.946	6.939	8.011	87.876	13.256	11.353	15.817
Prob > F	.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sub-sample size	4887	4887	4887	4887	4887	4887	4887	4887	4887

Notes: Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Marginal effects estimated at sample means, holding household equivalised income constant. Weighted using LSYPE Wave 7 respondent weights, which attempt to adjust for oversampling and attrition. Standard errors (adjusted for school level clustering and stratification by deprivation) reported in parentheses. Prior attainment variables are divided by 10, hence the coefficient estimates represent the expected change in probability for an additional 10 points. Base category for household equivalised income is middle (3rd) quintile group. Base category for parental education is achieving GCSEs or equivalents. Base category for family type is married or cohabiting couple. Base category for KS3 School Type is Community School. Base category for sex is female. Sample: Wave 7 participants with valid responses for all variables used in models. Marginal effect for discrete variables is the change from base category.

Table A.8: Models for university attendance, conditional on having applied, reporting marginal effects at means - Male

	M1-Probit	M2-Probit	M3-Probit	M4-Probit	M8-LP (with FE)	M5-Probit	M6-Probit	M7-Probit	M9-LP (with FE)
1st quintile of household income (Low)	-0.081	-0.002	-0.003	0.001	0.010	0.057	0.044	0.047	0.041
	(0.043)*	(0.041)	(0.043)	(0.042)	(0.046)	(0.053)	(0.061)	(0.060)	(0.045)
2nd quintile of household income	-0.059	-0.024	-0.018	-0.014	0.027	0.013	0.016	0.017	0.033
	(0.040)	(0.039)	(0.038)	(0.037)	(0.043)	(0.050)	(0.052)	(0.052)	(0.040)
4th quintile of household income	0.026	-0.001	-0.001	0.001	0.018	-0.004	0.007	0.009	0.017
	(0.035)	(0.036)	(0.035)	(0.035)	(0.036)	(0.046)	(0.045)	(0.045)	(0.034)
5th quintile of household income (High)	0.126	0.083	0.061	0.056	0.052	0.072	0.066	0.078	0.041
	(0.033)***	(0.035)**	(0.037)	(0.038)	(0.037)	(0.050)	(0.049)	(0.049)	(0.035)
KS2 Score		0.041	0.082	0.111	0.203	0.038	0.099	0.121	0.045
		(0.055)	(0.056)	(0.056)**	(0.064)***	(0.089)	(0.091)	(0.092)	(0.071)
KS2 Score Squared		0.004	0.002	-0.001	-0.008	-0.001	-0.006	-0.008	-0.003
		(0.004)	(0.004)	(0.004)	(0.005)*	(0.007)	(0.007)	(0.007)	(0.005)
Capped GCSE Score (Low)						0.026	0.023	0.023	0.023
						(0.014)*	(0.014)*	(0.013)*	(0.007)***
Capped GCSE Score (High)						0.033	0.035	0.037	0.051
						(0.013)**	(0.013)***	(0.013)***	(0.010)***
Capped GCSE Score (High) Squared						0.000	0.000	0.000	-0.001
						(0.001)	(0.001)	(0.001)	(0.000)***
Lone Parent Family			-0.027	-0.024	-0.047		-0.009	-0.010	-0.032
			(0.033)	(0.032)	(0.037)		(0.044)	(0.043)	(0.034)
Mother's Education - No Quals			-0.007	-0.004	-0.016		-0.012	-0.015	-0.004
			(0.040)	(0.040)	(0.047)		(0.052)	(0.053)	(0.044)
Mother's Education - Below GCSEs			-0.006	-0.004	0.013		0.056	0.051	0.067
			(0.051)	(0.050)	(0.059)		(0.068)	(0.068)	(0.057)
Mother's Education - A Levels			-0.005	-0.013	0.013		0.002	-0.003	0.039
			(0.032)	(0.032)	(0.039)		(0.044)	(0.043)	(0.038)
Mother's Education - HE Below Degree			0.048	0.043	0.062		0.063	0.065	0.077
			(0.037)	(0.037)	(0.038)		(0.049)	(0.049)	(0.036)**
Mother's Education - Degree			0.025	0.020	0.021		0.015	0.012	0.028
			(0.039)	(0.039)	(0.039)		(0.055)	(0.055)	(0.039)
Father's Education - No Quals			0.064	0.063	0.089		0.095	0.097	0.100
			(0.039)	(0.039)	(0.045)**		(0.054)*	(0.054)*	(0.044)**
Father's Education - Below GCSEs			0.065	0.070	0.037		0.090	0.095	0.056
			(0.055)	(0.054)	(0.064)		(0.072)	(0.072)	(0.060)
Father's Education - A Levels			0.041	0.037	0.018		0.030	0.028	-0.002
			(0.033)	(0.033)	(0.039)		(0.045)	(0.045)	(0.038)
Father's Education - HE Below Degree			0.068	0.069	0.052		0.107	0.104	0.058
			(0.039)*	(0.038)*	(0.041)		(0.051)**	(0.052)**	(0.039)
Father's Education - Degree			0.127	0.126	0.092		0.117	0.120	0.069
			(0.040)***	(0.039)***	(0.039)**		(0.056)**	(0.055)**	(0.039)*
KS3 School Type - CTC				-0.014				-0.109	
				(0.084)				(0.143)	
KS3 School Type - Foundation				0.011				-0.009	
				(0.030)				(0.042)	
KS3 School Type - Independent				0.067				-0.060	
				(0.062)				(0.085)	
KS3 School Type - Voluntary Aided				0.041				0.011	
				(0.030)				(0.042)	
KS3 School Type - Voluntary Controlled				0.107				0.086	
				(0.063)*				(0.083)	
Grammar School				0.139				0.149	
				(0.050)***				(0.073)**	
School has Sixth Form				-0.021				-0.024	
				(0.024)				(0.032)	
Region	No	No	Yes	Yes	No	No	Yes	Yes	No
Ethnicity	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Month of Birth	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Sibling effects	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
F Test	.	43.081	3.995	4.088	3.852	38.867	6.164	5.628	8.051
Prob > F	.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sub-sample size	2218	2218	2218	2218	2218	2218	2218	2218	2218

Notes: Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Marginal effects estimated at sample means, holding household equivalised income constant. Weighted using LSYPE Wave 7 respondent weights, which attempt to adjust for oversampling and attrition. Standard errors (adjusted for school level clustering and stratification by deprivation) reported in parentheses. Prior attainment variables are divided by 10, hence the coefficient estimates represent the expected change in probability for an additional 10 points. Base category for household equivalised income is middle (3rd) quintile group. Base category for parental education is achieving GCSEs or equivalents. Base category for family type is married or cohabiting couple. Base category for KS3 School Type is Community School. Sample: Wave 7 participants with valid responses for all variables used in models. Marginal effect for discrete variables is the change from base category.

Table A.9: Models for university attendance, conditional on having applied, reporting marginal effects at means - Female

	M1-Probit	M2-Probit	M3-Probit	M4-Probit	M8-LP (with FE)	M5-Probit	M6-Probit	M7-Probit	M9-LP (with FE)
1st quintile of household income (Low)	-0.030	0.014	0.017	0.010	0.019	0.050	0.036	0.028	0.025
	(0.034)	(0.032)	(0.033)	(0.033)	(0.039)	(0.041)	(0.049)	(0.049)	(0.037)
2nd quintile of household income	-0.028	-0.016	-0.006	-0.009	0.001	0.008	0.004	-0.000	0.012
	(0.036)	(0.034)	(0.031)	(0.031)	(0.035)	(0.045)	(0.046)	(0.046)	(0.034)
4th quintile of household income	0.067	0.043	0.036	0.029	0.022	0.030	0.035	0.029	0.018
	(0.029)**	(0.029)	(0.027)	(0.027)	(0.031)	(0.041)	(0.041)	(0.040)	(0.030)
5th quintile of household income (High)	0.122	0.078	0.050	0.027	0.011	0.053	0.047	0.031	0.009
	(0.027)***	(0.029)***	(0.030)*	(0.029)	(0.031)	(0.041)	(0.045)	(0.043)	(0.030)
KS2 Score		0.026	0.041	0.057	0.106	-0.048	-0.050	-0.041	-0.056
		(0.044)	(0.045)	(0.046)	(0.057)*	(0.079)	(0.080)	(0.081)	(0.062)
KS2 Score Squared		0.004	0.003	0.001	-0.003	0.002	0.003	0.002	0.003
		(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)	(0.005)
Capped GCSE Score (Low)						0.011	0.012	0.010	0.011
						(0.010)	(0.009)	(0.009)	(0.006)*
Capped GCSE Score (High)						0.059	0.059	0.060	0.059
						(0.011)***	(0.011)***	(0.011)***	(0.008)***
Capped GCSE Score (High) Squared						-0.001	-0.001	-0.001	-0.001
						(0.000)	(0.000)*	(0.000)**	(0.000)***
Lone Parent Family			-0.043	-0.042	-0.039		-0.015	-0.015	-0.015
			(0.026)*	(0.025)	(0.028)		(0.038)	(0.038)	(0.026)
Mother's Education - No Quals			-0.041	-0.039	-0.031		-0.058	-0.057	-0.036
			(0.034)	(0.034)	(0.041)		(0.051)	(0.051)	(0.039)
Mother's Education - Below GCSEs			-0.020	-0.021	-0.011		-0.048	-0.049	-0.023
			(0.043)	(0.043)	(0.053)		(0.064)	(0.064)	(0.052)
Mother's Education - A Levels			0.035	0.035	0.019		0.032	0.032	0.001
			(0.030)	(0.029)	(0.032)		(0.042)	(0.042)	(0.030)
Mother's Education - HE Below Degree			0.010	0.004	-0.002		-0.016	-0.019	-0.019
			(0.029)	(0.029)	(0.031)		(0.044)	(0.044)	(0.030)
Mother's Education - Degree			0.021	0.011	-0.004		-0.004	-0.010	-0.017
			(0.037)	(0.035)	(0.035)		(0.053)	(0.051)	(0.034)
Father's Education - No Quals			-0.002	0.005	0.010		0.003	0.007	0.022
			(0.033)	(0.033)	(0.040)		(0.049)	(0.048)	(0.039)
Father's Education - Below GCSEs			-0.009	-0.008	0.007		-0.029	-0.027	-0.002
			(0.041)	(0.041)	(0.055)		(0.062)	(0.061)	(0.052)
Father's Education - A Levels			-0.053	-0.050	-0.023		-0.074	-0.072	-0.020
			(0.031)*	(0.031)	(0.033)		(0.047)	(0.047)	(0.032)
Father's Education - HE Below Degree			0.022	0.017	-0.010		0.037	0.033	-0.005
			(0.034)	(0.033)	(0.036)		(0.049)	(0.048)	(0.035)
Father's Education - Degree			0.081	0.074	0.070		0.075	0.072	0.050
			(0.036)**	(0.036)**	(0.033)**		(0.054)	(0.053)	(0.032)
KS3 School Type - CTC				-0.007				-0.051	
				(0.115)				(0.153)	
KS3 School Type - Foundation				-0.034				-0.047	
				(0.024)				(0.038)	
KS3 School Type - Independent				0.165				0.119	
				(0.050)***				(0.074)	
KS3 School Type - Voluntary Aided				0.013				0.005	
				(0.025)				(0.036)	
KS3 School Type - Voluntary Controlled				0.020				0.016	
				(0.048)				(0.075)	
Grammar School				0.150				0.141	
				(0.056)***				(0.081)*	
School has Sixth Form				0.005				0.003	
				(0.020)				(0.030)	
Region	No	No	Yes	Yes	No	No	Yes	Yes	No
Ethnicity	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Month of Birth	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Sibling effects	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
F Test	.	36.353	4.343	3.771	3.947	50.524	7.808	6.498	7.875
Prob > F	.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sub-sample size	2669	2669	2669	2669	2669	2669	2669	2669	2669

Notes: Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Marginal effects estimated at sample means, holding household equivalised income constant. Weighted using LSYPE Wave 7 respondent weights, which attempt to adjust for oversampling and attrition. Standard errors (adjusted for school level clustering and stratification by deprivation) reported in parentheses. Prior attainment variables are divided by 10, hence the coefficient estimates represent the expected change in probability for an additional 10 points. Base category for household equivalised income is middle (3rd) quintile group. Base category for parental education is achieving GCSEs or equivalents. Base category for family type is married or cohabiting couple. Base category for KS3 School Type is Community School. Sample: Wave 7 participants with valid responses for all variables used in models. Marginal effect for discrete variables is the change from base category.

A.1 Models for access to Russell Group universities

Table A.10: Models for Russell Group attendance, reporting marginal effects at means

	M1-Probit	M2-Probit	M3-Probit	M4-Probit	M8-LP (with FE)	M5-Probit	M6-Probit	M7-Probit	M9-LP (with FE)
1st quintile of household income (Low)	-0.102	-0.033	-0.043	-0.044	-0.015	0.049	0.007	0.005	0.008
	(0.017)***	(0.019)*	(0.023)*	(0.023)*	(0.017)	(0.019)***	(0.021)	(0.021)	(0.016)
2nd quintile of household income	-0.071	-0.055	-0.047	-0.049	-0.038	-0.011	-0.018	-0.019	-0.016
	(0.019)***	(0.020)***	(0.022)**	(0.023)**	(0.016)**	(0.017)	(0.018)	(0.019)	(0.014)
4th quintile of household income	0.109	0.057	0.036	0.028	0.020	0.019	0.019	0.017	0.008
	(0.019)***	(0.020)***	(0.020)*	(0.021)	(0.016)	(0.016)	(0.017)	(0.017)	(0.015)
5th quintile of household income (High)	0.326	0.230	0.158	0.109	0.073	0.094	0.080	0.071	0.045
	(0.020)***	(0.022)***	(0.024)***	(0.026)***	(0.019)***	(0.020)***	(0.021)***	(0.022)***	(0.017)***
KS2 Score		-0.059	-0.014	0.016	-0.095	-0.033	-0.002	0.005	-0.004
		(0.030)*	(0.033)	(0.035)	(0.018)***	(0.035)	(0.037)	(0.037)	(0.018)
KS2 Score Squared		0.017	0.014	0.011	0.016	0.001	-0.001	-0.001	-0.001
		(0.002)***	(0.003)***	(0.003)***	(0.002)***	(0.003)	(0.003)	(0.003)	(0.002)
Capped GCSE Score (Low)						0.011	0.010	0.009	-0.002
						(0.004)***	(0.004)**	(0.004)**	(0.001)**
Capped GCSE Score (High)						0.057	0.055	0.056	0.050
						(0.005)***	(0.005)***	(0.005)***	(0.003)***
Capped GCSE Score (High) Squared						-0.000	-0.000	-0.000	-0.000
						(0.000)	(0.000)	(0.000)	(0.000)
Male			-0.109	-0.118	-0.086		-0.022	-0.026	-0.021
			(0.014)***	(0.013)***	(0.010)***		(0.012)*	(0.012)**	(0.010)**
Lone Parent Family			-0.070	-0.069	-0.045		-0.011	-0.011	-0.003
			(0.020)***	(0.020)***	(0.013)***		(0.017)	(0.017)	(0.012)
Mother’s Education - No Quals			-0.034	-0.026	-0.007		-0.010	-0.010	0.005
			(0.023)	(0.023)	(0.017)		(0.021)	(0.021)	(0.015)
Mother’s Education - Below GCSEs			-0.041	-0.039	-0.031		-0.021	-0.022	-0.013
			(0.030)	(0.030)	(0.020)		(0.026)	(0.026)	(0.018)
Mother’s Education - A Levels			0.026	0.015	-0.002		0.003	0.001	-0.007
			(0.020)	(0.020)	(0.018)		(0.017)	(0.017)	(0.016)
Mother’s Education - HE Below Degree			0.075	0.062	0.051		0.043	0.041	0.039
			(0.022)***	(0.023)***	(0.019)***		(0.019)**	(0.020)**	(0.017)**
Mother’s Education - Degree			0.075	0.058	0.033		0.007	0.005	0.001
			(0.028)***	(0.028)**	(0.022)		(0.024)	(0.024)	(0.020)
Father’s Education - No Quals			0.005	0.012	0.017		0.029	0.030	0.032
			(0.024)	(0.023)	(0.017)		(0.021)	(0.021)	(0.015)**
Father’s Education - Below GCSEs			0.002	0.007	0.009		0.025	0.026	0.020
			(0.031)	(0.031)	(0.022)		(0.026)	(0.026)	(0.020)
Father’s Education - A Levels			-0.006	-0.007	-0.001		-0.010	-0.011	-0.006
			(0.020)	(0.020)	(0.017)		(0.017)	(0.017)	(0.015)
Father’s Education - HE Below Degree			0.083	0.082	0.070		0.061	0.059	0.053
			(0.024)***	(0.024)***	(0.020)***		(0.020)***	(0.020)***	(0.018)***
Father’s Education - Degree			0.200	0.187	0.150		0.113	0.110	0.096
			(0.026)***	(0.026)***	(0.021)***		(0.023)***	(0.023)***	(0.019)***
KS3 School Type - CTC				-0.032				-0.123	
				(0.069)				(0.035)***	
KS3 School Type - Foundation				0.005				-0.020	
				(0.021)				(0.018)	
KS3 School Type - Independent				0.380				0.082	
				(0.043)***				(0.036)**	
KS3 School Type - Voluntary Aided				0.081				0.016	
				(0.023)***				(0.020)	
KS3 School Type - Voluntary Controlled				0.062				0.019	
				(0.042)				(0.034)	
Grammar School				0.197				0.073	
				(0.043)***				(0.042)*	
School has Sixth Form				0.045				0.034	
				(0.016)***				(0.014)**	
Region	No	No	Yes	Yes	No	No	Yes	Yes	No
Ethnicity	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Month of Birth	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Sibling effects	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
F Test	.	318.497	34.675	30.591	45.718	323.044	42.302	36.731	105.037
Prob > F	.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sub-sample size	7939	7939	7939	7939	7939	7939	7939	7939	7939

Notes: Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Marginal effects estimated at sample means, holding household equivalised income constant. Weighted using LSYPE Wave 7 respondent weights, which attempt to adjust for oversampling and attrition. Standard errors (adjusted for school level clustering and stratification by deprivation) reported in parentheses. Prior attainment variables are divided by 10, hence the coefficient estimates represent the expected change in probability for an additional 10 points. Base category for household equivalised income is middle (3rd) quintile group. Base category for parental education is achieving GCSEs or equivalents. Base category for family type is married or cohabiting couple. Base category for KS3 School Type is Community School. Base category for sex is female. Sample: Wave 7 participants with valid responses for all variables used in models. Marginal effect for discrete variables is the change from base category.

Table A.11: Models for Russell Group attendance, conditional on university attendance, reporting marginal effects at means

	M1-Probit	M2-Probit	M3-Probit	M4-Probit	M8-LP (with FE)	M5-Probit	M6-Probit	M7-Probit	M9-LP (with FE)
1st quintile of household income (Low)	-0.052	0.006	0.007	0.004	0.015	0.052	0.032	0.029	0.026
	(0.027)*	(0.025)	(0.027)	(0.027)	(0.029)	(0.033)	(0.038)	(0.038)	(0.028)
2nd quintile of household income	-0.042	-0.020	-0.015	-0.016	0.007	0.010	0.005	0.004	0.016
	(0.027)	(0.027)	(0.027)	(0.026)	(0.025)	(0.034)	(0.035)	(0.035)	(0.024)
4th quintile of household income	0.049	0.024	0.020	0.017	0.007	0.015	0.019	0.018	0.008
	(0.022)**	(0.022)	(0.022)	(0.022)	(0.022)	(0.030)	(0.030)	(0.030)	(0.021)
5th quintile of household income (High)	0.124	0.080	0.052	0.037	0.024	0.062	0.050	0.047	0.022
	(0.022)***	(0.022)***	(0.023)**	(0.023)	(0.022)	(0.032)*	(0.032)	(0.032)	(0.021)
KS2 Score		0.029	0.050	0.070	0.144	-0.026	-0.003	0.010	-0.018
		(0.035)	(0.035)	(0.035)**	(0.039)***	(0.062)	(0.062)	(0.062)	(0.043)
KS2 Score Squared		0.004	0.003	0.001	-0.005	0.002	0.000	-0.001	0.001
		(0.003)	(0.003)	(0.003)	(0.003)*	(0.005)	(0.005)	(0.005)	(0.003)
Capped GCSE Score (Low)						0.018	0.016	0.015	0.016
						(0.008)**	(0.008)**	(0.008)*	(0.004)***
Capped GCSE Score (High)						0.049	0.048	0.049	0.054
						(0.008)***	(0.008)***	(0.008)***	(0.006)***
Capped GCSE Score (High) Squared						-0.000	-0.000	-0.000	-0.001
						(0.000)	(0.000)	(0.000)	(0.000)***
Male			-0.044	-0.049	-0.038		-0.012	-0.019	-0.003
			(0.014)***	(0.014)***	(0.015)**		(0.020)	(0.020)	(0.015)
Lone Parent Family			-0.031	-0.028	-0.031		-0.007	-0.005	-0.007
			(0.021)	(0.021)	(0.021)		(0.029)	(0.029)	(0.020)
Mother's Education - No Quals			-0.025	-0.021	-0.017		-0.033	-0.031	-0.014
			(0.026)	(0.026)	(0.029)		(0.037)	(0.037)	(0.027)
Mother's Education - Below GCSEs			-0.023	-0.021	-0.024		-0.012	-0.013	-0.006
			(0.034)	(0.033)	(0.037)		(0.047)	(0.047)	(0.036)
Mother's Education - A Levels			0.017	0.012	0.006		0.019	0.015	0.006
			(0.022)	(0.021)	(0.023)		(0.029)	(0.029)	(0.022)
Mother's Education - HE Below Degree			0.021	0.015	0.017		0.014	0.012	0.011
			(0.023)	(0.023)	(0.022)		(0.033)	(0.033)	(0.022)
Mother's Education - Degree			0.020	0.012	0.006		0.000	-0.003	0.001
			(0.025)	(0.025)	(0.023)		(0.037)	(0.037)	(0.023)
Father's Education - No Quals			0.027	0.029	0.036		0.046	0.046	0.041
			(0.024)	(0.024)	(0.028)		(0.034)	(0.034)	(0.027)
Father's Education - Below GCSEs			0.024	0.024	0.024		0.024	0.024	0.018
			(0.033)	(0.033)	(0.038)		(0.046)	(0.046)	(0.037)
Father's Education - A Levels			-0.011	-0.013	-0.002		-0.026	-0.027	-0.006
			(0.022)	(0.022)	(0.024)		(0.031)	(0.031)	(0.023)
Father's Education - HE Below Degree			0.043	0.042	0.034		0.069	0.066	0.036
			(0.026)*	(0.025)*	(0.025)		(0.035)*	(0.035)*	(0.024)
Father's Education - Degree			0.102	0.098	0.090		0.097	0.096	0.066
			(0.027)***	(0.027)***	(0.023)***		(0.038)**	(0.038)**	(0.023)***
KS3 School Type - CTC				-0.014				-0.085	
				(0.090)				(0.129)	
KS3 School Type - Foundation				-0.017				-0.034	
				(0.019)				(0.028)	
KS3 School Type - Independent				0.120				0.028	
				(0.040)***				(0.059)	
KS3 School Type - Voluntary Aided				0.025				0.009	
				(0.021)				(0.027)	
KS3 School Type - Voluntary Controlled				0.059				0.049	
				(0.045)				(0.062)	
Grammar School				0.145				0.145	
				(0.040)***				(0.059)**	
School has Sixth Form				-0.003				-0.005	
				(0.016)				(0.022)	
Region	No	No	Yes	Yes	No	No	Yes	Yes	No
Ethnicity	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Month of Birth	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Sibling effects	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes
F Test	.	75.598	7.946	6.939	8.011	87.876	13.256	11.353	15.817
Prob > F	.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sub-sample size	4887	4887	4887	4887	4887	4887	4887	4887	4887

Notes: Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Marginal effects estimated at sample means, holding household equivalised income constant. Weighted using LSYPE Wave 7 respondent weights, which attempt to adjust for oversampling and attrition. Standard errors (adjusted for school level clustering and stratification by deprivation) reported in parentheses. Prior attainment variables are divided by 10, hence the coefficient estimates represent the expected change in probability for an additional 10 points. Base category for household equivalised income is middle (3rd) quintile group. Base category for parental education is achieving GCSEs or equivalents. Base category for family type is married or cohabiting couple. Base category for KS3 School Type is Community School. Base category for sex is female. Sample: Wave 7 participants with valid responses for all variables used in models. Marginal effect for discrete variables is the change from base category.

Appendix B

Multiple regression models for Chapter 3 - full regression tables and supplementary models

B.1 Full regression tables

Table B.1: Estimated effects on risk of transition from reporting being ‘likely to apply’ to university to reporting being ‘unlikely to apply’ to university: hazard ratios

	M0	M1	M2	M3	M4	M4C	M5	M5C
Age 16	0.89	0.90	0.91	0.95	0.94	0.95	0.95	0.95
	(0.04)**	(0.04)**	(0.04)**	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Age 17	0.74	0.77	0.85	0.92	0.92	0.92	0.97	0.97
	(0.03)***	(0.03)***	(0.04)***	(0.04)*	(0.05)*	(0.05)*	(0.05)	(0.05)
SES Q1 (Low)		1.46	1.54	1.13	1.10		1.14	
		(0.09)***	(0.10)***	(0.08)*	(0.07)		(0.08)**	
SES Q2		1.40	1.31	1.17	1.16		1.16	
		(0.08)***	(0.08)***	(0.07)**	(0.07)**		(0.07)**	
SES Q4		0.75	0.80	0.80	0.80		0.80	
		(0.05)***	(0.05)***	(0.05)***	(0.05)***		(0.05)***	
SES Q5 (High)		0.33	0.39	0.47	0.47		0.49	
		(0.03)***	(0.03)***	(0.04)***	(0.04)***		(0.04)***	
SES Z-Score						0.71		0.72
						(0.02)***		(0.02)***
Male			1.49	1.53	1.49	1.50	1.50	1.51
			(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***
Ethnicity: Mixed			0.63	0.63	0.62	0.62	0.61	0.60
			(0.06)***	(0.07)***	(0.07)***	(0.06)***	(0.07)***	(0.06)***
Ethnicity: Indian			0.19	0.17	0.17	0.16	0.17	0.16
			(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***
Ethnicity: Pakistani			0.27	0.23	0.23	0.22	0.23	0.22
			(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***
Ethnicity: Bangladeshi			0.26	0.27	0.27	0.25	0.27	0.25
			(0.05)***	(0.05)***	(0.05)***	(0.04)***	(0.05)***	(0.04)***
Ethnicity: Black Caribbean			0.37	0.27	0.26	0.27	0.26	0.27
			(0.05)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***
Ethnicity: Black African			0.20	0.17	0.17	0.17	0.17	0.17
			(0.04)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***
Ethnicity: Other			0.27	0.24	0.25	0.24	0.24	0.23
			(0.05)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***
Attended Independent School			0.30	0.27	0.29	0.29	0.29	0.30
			(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***
Attended Grammar School			0.23	0.37	0.38	0.37	0.39	0.38
			(0.05)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***
Attended School with Sixth Form			0.84	0.86	0.86	0.87	0.86	0.86
			(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***
Experienced workless household			0.97	0.88	0.83	0.76	0.85	0.78
			(0.06)	(0.06)**	(0.05)***	(0.05)***	(0.05)***	(0.05)***
Ever experienced family separation			0.96	0.95	0.94	0.94	0.94	0.95
			(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Local Youth Unemployment Rate / 10			0.96	0.95	0.95	0.95	0.96	0.95
			(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
KS2 Z-Score				0.56	0.60	0.60	0.60	0.61
				(0.01)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***
KS4 Z-Score (After results)					0.67	0.67	0.50	0.60
					(0.03)***	(0.03)***	(0.06)***	(0.03)***
KS4 Z-Score * SES Q1							1.62	
							(0.25)***	
KS4 Z-Score * SES Q2							1.47	
							(0.22)**	
KS4 Z-Score * SES Q4							1.19	
							(0.18)	
KS4 Z-Score * SES Q5							0.96	
							(0.21)	
KS4 Z-Score * SES Z-Score								0.79
								(0.05)***
Geographical			√	√	√	√	√	√
Number and order of siblings			√	√	√	√	√	√
Months of birth and interview	√	√	√	√	√	√	√	√
F test of difference from previous model	.	113.10	25.82	248.18	63.78	101.97	3.77	16.26
p-value of above test statistic	.	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Number of individuals	9,247	9,247	9,247	9,247	9,247	9,247	9,247	9,247

Notes: Reporting hazard ratios. Standard errors (clustered by individual’s school) in parentheses. Weighted using Wave 2 survey design and non-response weights. Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Estimated risks are relative to the following base categories: Age 15, SES quintile group 3, attended a non-selective state school, white, and female. Tests of model fit are relative to the model one column to the left, with the following exceptions: M4C is relative to M3, M5 is relative to M4, and M5C is relative to M4C.

Table B.2: Estimated effects on risk of transition from reporting being ‘unlikely to apply’ to university to reporting being ‘likely to apply’ to university: hazard ratios

	M0	M1	M2	M3	M4	M4C	M5	M5C
Age 16	0.88	0.88	0.90	0.91	0.90	0.90	0.90	0.90
	(0.05)**	(0.05)**	(0.05)*	(0.05)*	(0.05)*	(0.05)*	(0.05)*	(0.05)*
Age 17	0.63	0.63	0.63	0.64	0.76	0.76	0.75	0.74
	(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***
SES Q1 (Low)		0.76	0.70	0.79	0.81		0.79	
		(0.06)***	(0.06)***	(0.07)***	(0.07)**		(0.07)***	
SES Q2		0.89	0.88	0.91	0.91		0.91	
		(0.06)*	(0.06)*	(0.06)	(0.06)		(0.06)	
SES Q4		1.29	1.25	1.16	1.15		1.13	
		(0.10)***	(0.09)***	(0.08)**	(0.08)*		(0.08)*	
SES Q5 (High)		1.94	1.92	1.71	1.67		1.68	
		(0.17)***	(0.16)***	(0.15)***	(0.14)***		(0.14)***	
SES Z-Score						1.28		1.29
						(0.04)***		(0.04)***
Male			0.60	0.60	0.62	0.62	0.62	0.62
			(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***
Ethnicity: Mixed			1.50	1.55	1.56	1.54	1.58	1.55
			(0.19)***	(0.20)***	(0.20)***	(0.20)***	(0.20)***	(0.20)***
Ethnicity: Indian			2.85	3.33	3.23	3.27	3.24	3.27
			(0.48)***	(0.51)***	(0.50)***	(0.51)***	(0.51)***	(0.50)***
Ethnicity: Pakistani			3.62	4.27	4.17	4.35	4.18	4.31
			(0.44)***	(0.58)***	(0.55)***	(0.58)***	(0.55)***	(0.57)***
Ethnicity: Bangladeshi			4.69	5.26	4.92	5.16	4.96	5.17
			(0.61)***	(0.70)***	(0.65)***	(0.67)***	(0.66)***	(0.67)***
Ethnicity: Black Caribbean			2.77	3.21	3.15	3.08	3.20	3.10
			(0.43)***	(0.47)***	(0.45)***	(0.45)***	(0.46)***	(0.45)***
Ethnicity: Black African			4.87	6.40	6.08	6.15	6.11	6.11
			(1.01)***	(1.35)***	(1.27)***	(1.35)***	(1.28)***	(1.33)***
Ethnicity: Other			3.15	3.56	3.53	3.64	3.53	3.62
			(0.49)***	(0.62)***	(0.59)***	(0.59)***	(0.60)***	(0.59)***
Attended Independent School			1.29	1.37	1.33	1.34	1.32	1.32
			(0.39)	(0.36)	(0.33)	(0.34)	(0.33)	(0.33)
Attended Grammar School			1.77	1.05	0.99	0.96	0.96	0.94
			(0.34)***	(0.22)	(0.20)	(0.20)	(0.20)	(0.19)
Attended School with Sixth Form			1.07	1.04	1.04	1.03	1.04	1.03
			(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Experienced workless household			0.99	1.03	1.07	1.09	1.07	1.08
			(0.07)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)
Ever experienced family separation			1.09	1.09	1.09	1.09	1.10	1.10
			(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Local Youth Unemployment Rate / 10			1.06	1.03	1.04	1.03	1.04	1.03
			(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)
KS2 Z-Score				1.55	1.45	1.45	1.45	1.45
				(0.05)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***
KS4 Z-Score (After results)					1.73	1.74	1.90	1.88
					(0.11)***	(0.12)***	(0.31)***	(0.14)***
KS4 Z-Score * SES Q1							0.80	
							(0.14)	
KS4 Z-Score * SES Q2							0.91	
							(0.19)	
KS4 Z-Score * SES Q4							1.43	
							(0.36)	
KS4 Z-Score * SES Q5							0.67	
							(0.16)*	
KS4 Z-Score * SES Z-Score								1.18
								(0.09)**
Geographical			√	√	√	√	√	√
Number and order of siblings			√	√	√	√	√	√
Months of birth and interview	√	√	√	√	√	√	√	√
F test of difference from previous model	.	34.70	14.62	110.58	69.98	68.80	2.50	4.54
p-value of above test statistic	.	0.00	0.00	0.00	0.00	0.00	0.04	0.03
Number of individuals	5,330	5,330	5,330	5,330	5,330	5,330	5,330	5,330

Notes: Reporting hazard ratios. Standard errors (clustered by individual’s school) in parentheses. Weighted using Wave 2 survey design and non-response weights. Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Estimated risks are relative to the following base categories: Age 15, SES quintile group 3, attended a non-selective state school, white, and female. Tests of model fit are relative to the model one column to the left, with the following exceptions: M4C is relative to M3, M5 is relative to M4, and M5C is relative to M4C.

B.2 Weighting data using final wave attrition weights

One of the advantages of duration modelling is that we can treat missing outcome data at 'censored', rather than having top drop the respondent from our analysis. However, doing so will only result in unbiased estimates under the assumption that missing data censoring is 'uninformative' (Clark et al., 2003, p.236). In this appendix, I repeat my analysis, restricting the sample only to those still participating in the survey at Wave 4 (when the response rate relative to Wave 1 has fallen to 73% (Collingwood et al., 2010, p.52)), and weighting the analysis the LSYPE-provided attrition and non-response weights for Wave 4.

In other respects, the regression setup remains the same as for the analysis in the main body of the paper. I report the results from these analyses in Tables B.3 and B.4. Reassuringly, I do not find any qualitative differences from the results presented in Chapter 3.

B.3 Multiple regression models accounting for unobserved heterogeneity

Unobserved heterogeneity is a problem in many statistical analyses. However, it has the potential to cause particular bias in the case of duration analysis, including "downward bias in the time effects [and, as a result,] spurious effects of time-varying covariates" (Vermunt, 2001, p.1). These are caused by changes in the composition of the sample we are analysing at each time point: individuals who are still at risk at later time points are less likely to switch to reporting being 'unlikely to apply' partly because the most likely to switch have already done so. Obviously, some of the characteristics in the model will control for observable changes in composition, but not all of such changes will be observable. In addition, attempting to account for unobserved heterogeneity also helps to account for the shared covariance of using multiple spells from the same individual (Steele, 2005, p.16-19).

Many duration models attempt to control for unobserved heterogeneity between individuals.[1] A popular method to account for unobserved heterogeneity is by introducing

[1] These are often referred to as 'frailty' models, since, in epidemiological applications, the unobserved

Table B.3: Estimated effects on risk of transition from reporting being ‘likely to apply’ to university to reporting being ‘unlikely to apply’ to university: hazard ratios (Wave 4 weights applied, excludes individuals not in sample at age 17)

	M0	M1	M2	M3	M4	M4C	M5	M5C
Age 16	0.90	0.90	0.91	0.96	0.96	0.96	0.96	0.96
	(0.04)**	(0.04)**	(0.04)*	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Age 17	0.80	0.84	0.93	1.01	1.02	1.03	1.07	1.08
	(0.04)***	(0.04)***	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)
SES Q1 (Low)		1.51	1.57	1.15	1.12		1.14	
		(0.09)***	(0.11)***	(0.08)**	(0.08)*		(0.08)*	
SES Q2		1.43	1.32	1.17	1.15		1.15	
		(0.09)***	(0.08)***	(0.07)**	(0.07)**		(0.07)**	
SES Q4		0.74	0.79	0.79	0.79		0.78	
		(0.05)***	(0.05)***	(0.05)***	(0.05)***		(0.05)***	
SES Q5 (High)		0.33	0.39	0.47	0.47		0.48	
		(0.03)***	(0.03)***	(0.04)***	(0.04)***		(0.04)***	
SES Z-Score						0.72		0.72
						(0.02)***		(0.02)***
Male			1.46	1.49	1.47	1.47	1.47	1.48
			(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***
Ethnicity: Mixed			0.65	0.63	0.61	0.62	0.60	0.59
			(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***
Ethnicity: Indian			0.18	0.16	0.17	0.16	0.16	0.16
			(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***
Ethnicity: Pakistani			0.26	0.21	0.22	0.20	0.22	0.20
			(0.03)***	(0.03)***	(0.03)***	(0.02)***	(0.03)***	(0.02)***
Ethnicity: Bangladeshi			0.26	0.26	0.27	0.25	0.27	0.25
			(0.05)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***
Ethnicity: Black Caribbean			0.37	0.28	0.27	0.27	0.26	0.27
			(0.05)***	(0.05)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***
Ethnicity: Black African			0.17	0.15	0.15	0.15	0.14	0.15
			(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***
Ethnicity: Other			0.27	0.24	0.24	0.24	0.24	0.23
			(0.05)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***
Attended Independent School			0.27	0.24	0.26	0.27	0.27	0.28
			(0.06)***	(0.06)***	(0.06)***	(0.06)***	(0.06)***	(0.06)***
Attended Grammar School			0.24	0.39	0.39	0.39	0.40	0.40
			(0.05)***	(0.08)***	(0.08)***	(0.08)***	(0.08)***	(0.08)***
Attended School with Sixth Form			0.84	0.85	0.85	0.85	0.85	0.85
			(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***
Experienced workless household			1.02	0.95	0.89	0.82	0.91	0.85
			(0.06)	(0.06)	(0.06)*	(0.06)***	(0.06)	(0.06)**
Ever experienced family separation			0.98	0.97	0.93	0.93	0.95	0.95
			(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Local Youth Unemployment Rate / 10			0.95	0.93	0.94	0.93	0.94	0.93
			(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
KS2 Z-Score				0.56	0.60	0.61	0.61	0.61
				(0.01)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***
KS4 Z-Score (After results)					0.61	0.62	0.44	0.56
					(0.03)***	(0.03)***	(0.05)***	(0.03)***
KS4 Z-Score * SES Q1							1.56	
							(0.22)***	
KS4 Z-Score * SES Q2							1.60	
							(0.23)***	
KS4 Z-Score * SES Q4							1.27	
							(0.19)	
KS4 Z-Score * SES Q5							1.06	
							(0.22)	
KS4 Z-Score * SES Z-Score								0.82
								(0.05)***
Geographical			√	√	√	√	√	√
Number and order of siblings			√	√	√	√	√	√
Months of birth and interview	√	√	√	√	√	√	√	√
F test of difference from previous model	.	118.90	25.78	258.97	90.29	110.35	3.86	11.16
p-value of above test statistic	.	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Number of individuals	8,616	8,616	8,616	8,616	8,616	8,616	8,616	8,616

Notes: Reporting hazard ratios. Standard errors (clustered by individual’s school) in parentheses. Weighted using Wave 4 survey design and non-response weights. Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Estimated risks are relative to the following base categories: Age 15, SES quintile group 3, attended a non-selective state school, white, and female. Tests of model fit are relative to the model one column to the left, with the following exceptions: M4C is relative to M3, M5 is relative to M4, and M5C is relative to M4C.

Table B.4: Estimated effects on risk of transition from reporting being ‘unlikely to apply’ to university to reporting being ‘likely to apply’ to university: hazard ratios (Wave 4 weights applied, excludes individuals not in sample at age 17)

	M0	M1	M2	M3	M4	M4C	M5	M5C
Age 16	0.88	0.88	0.90	0.91	0.90	0.90	0.90	0.90
	(0.05)**	(0.05)**	(0.05)*	(0.05)	(0.05)*	(0.05)*	(0.05)*	(0.05)*
Age 17	0.69	0.68	0.68	0.69	0.81	0.81	0.79	0.79
	(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***
SES Q1 (Low)		0.77	0.70	0.81	0.82		0.80	
		(0.06)***	(0.06)***	(0.07)***	(0.07)**		(0.07)***	
SES Q2		0.90	0.89	0.92	0.93		0.92	
		(0.06)	(0.06)	(0.06)	(0.06)		(0.06)	
SES Q4		1.34	1.28	1.19	1.18		1.17	
		(0.10)***	(0.10)***	(0.09)**	(0.09)**		(0.09)**	
SES Q5 (High)		1.98	1.97	1.73	1.69		1.71	
		(0.17)***	(0.17)***	(0.15)***	(0.15)***		(0.15)***	
SES Z-Score						1.29		1.29
						(0.04)***		(0.05)***
Male			0.60	0.60	0.61	0.62	0.61	0.62
			(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***
Ethnicity: Mixed			1.50	1.57	1.58	1.56	1.60	1.57
			(0.22)***	(0.22)***	(0.22)***	(0.22)***	(0.22)***	(0.22)***
Ethnicity: Indian			2.73	3.26	3.17	3.20	3.17	3.19
			(0.47)***	(0.52)***	(0.52)***	(0.52)***	(0.51)***	(0.51)***
Ethnicity: Pakistani			3.66	4.24	4.15	4.33	4.15	4.29
			(0.49)***	(0.63)***	(0.60)***	(0.62)***	(0.60)***	(0.62)***
Ethnicity: Bangladeshi			5.02	5.71	5.31	5.58	5.33	5.58
			(0.69)***	(0.79)***	(0.72)***	(0.75)***	(0.72)***	(0.75)***
Ethnicity: Black Caribbean			2.72	3.12	3.08	3.01	3.14	3.03
			(0.46)***	(0.51)***	(0.49)***	(0.48)***	(0.50)***	(0.48)***
Ethnicity: Black African			5.83	8.68	7.90	8.07	7.99	8.10
			(1.20)***	(1.72)***	(1.57)***	(1.68)***	(1.61)***	(1.69)***
Ethnicity: Other			3.32	3.73	3.72	3.86	3.69	3.80
			(0.57)***	(0.71)***	(0.67)***	(0.68)***	(0.67)***	(0.68)***
Attended Independent School			1.32	1.41	1.35	1.36	1.34	1.34
			(0.39)	(0.35)	(0.33)	(0.34)	(0.33)	(0.33)
Attended Grammar School			1.75	1.00	0.95	0.92	0.92	0.90
			(0.36)***	(0.22)	(0.21)	(0.20)	(0.20)	(0.20)
Attended School with Sixth Form			1.08	1.05	1.04	1.04	1.05	1.04
			(0.06)	(0.06)	(0.06)	(0.05)	(0.06)	(0.05)
Experienced workless household			1.04	1.09	1.13	1.15	1.13	1.14
			(0.08)	(0.08)	(0.09)	(0.08)*	(0.09)	(0.08)*
Ever experienced family separation			1.13	1.14	1.14	1.14	1.15	1.14
			(0.09)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Local Youth Unemployment Rate / 10			1.08	1.05	1.05	1.05	1.05	1.05
			(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
KS2 Z-Score				1.58	1.49	1.48	1.48	1.48
				(0.05)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***
KS4 Z-Score (After results)					1.64	1.65	1.74	1.78
					(0.11)***	(0.11)***	(0.31)***	(0.14)***
KS4 Z-Score * SES Q1							0.84	
							(0.16)	
KS4 Z-Score * SES Q2							0.96	
							(0.22)	
KS4 Z-Score * SES Q4							1.52	
							(0.40)	
KS4 Z-Score * SES Q5							0.70	
							(0.18)	
KS4 Z-Score * SES Z-Score								1.17
								(0.09)**
Geographical			√	√	√	√	√	√
Number and order of siblings			√	√	√	√	√	√
Months of birth and interview	√	√	√	√	√	√	√	√
F test of difference from previous model	.	33.73	13.10	114.97	53.38	60.11	2.44	4.08
p-value of above test statistic	.	0.00	0.00	0.00	0.00	0.00	0.05	0.04
Number of individuals	4,864	4,864	4,864	4,864	4,864	4,864	4,864	4,864

Notes: Reporting hazard ratios. Standard errors (clustered by individual’s school) in parentheses. Weighted using Wave 4 survey design and non-response weights. Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Estimated risks are relative to the following base categories: Age 15, SES quintile group 3, attended a non-selective state school, white, and female. Tests of model fit are relative to the model one column to the left, with the following exceptions: M4C is relative to M3, M5 is relative to M4, and M5C is relative to M4C.

an individual-level random effect (Wooldridge, 2002, ch.10). These still allow inclusion of individual-level (i.e. non-time-varying) covariates and are relatively efficient, which is important when there are only a small number of observations for each individual. However, it makes the assumption that the individual-level random effect is not correlated with the included explanatory variables, which is almost certainly not strictly justified.

The alternative that does not make this assumption (nor any assumption about the distribution of the unobserved heterogeneity) is estimation of individual-level fixed effects. However, this approach would prevent me from being able to estimate the effect of any time-invariant covariates, which are matters of interest for this paper. Furthermore, it is unlikely that the individual-level fixed effect would be well estimated with so few observations per person in many cases: this can cause its own problems (Vermunt, 2001, p.11-12). As such, despite its assumptions not being fully met, I use random effects modelling. This is preferable to simply assuming unobserved heterogeneity is not an issue.

One must also make an assumption about the distribution of the individual-level random effects, with popular distributions including the Gamma distribution (Meyer, 1990), a normal distribution with mean zero (Jenkins, 2004, ch. 8.2), or non-parametric discrete mixing distribution (latent class analysis) (Heckman and Singer, 1984). For the models reported in this section, I assume a normal distribution for the random effects. However, I have also estimated models with a discrete mixing distribution; these models have two mass points, with Gateaux derivatives used to test the whether additional mass points would provide a better fit. This alternative assumption makes little difference to the estimated association between SES and probability of transition.

I estimate regression models of the form:

$$\text{log}(-\text{log}(1-d_{it})) = \alpha(age) + \beta\mathbf{x}_{it} + \nu_i \tag{B.1}$$

where ν is an individual-level error term, which is assumed to be normally-distributed:

$$\nu \sim N(0, \sigma_\nu^2) \tag{B.2}$$

propensity of an individual to fall sick could be thought of as their frailty.

and uncorrelated with the explanatory variables:

$$Cov(\nu_i, \mathbf{x}_{it}) = Cov(\varepsilon_{it}, \mathbf{x}_{it}) = 0 \tag{B.3}$$

I estimate models including the same variables as in the main body of the paper (other than the addition of a random effect). I estimate these models using adaptive quadrature with 8 integration points, making use of the software GLLAMM (Rabe-Hesketh and Skrondal, 2006). This allows me to include individual-level random effects, while still with accounting for the complex survey design of the data (most notably the sampling and attrition weighting scheme, and the clustering of standard errors at the school-level).

B.3.1 Regression tables

The results of these models are reported in regression tables similar to those in Appendix B.1. Models for M0 are not reported, as these would not reliably converge. This would seem to be due to an over-reliance on the random effects to explain differences between individuals in this model with very few explanatory variables.

In addition to what is reported for models without random effects, the tables also show the estimated variance of the random effect and the results of a likelihood ratio test of the difference between the model and the counterpart model with no random effect. In each case, the model that accounts for unobserved heterogeneity does provide additional explanatory power.

The models for transition from 'likely to unlikely' are reported in Table B.5, while the models for 'unlikely to likely' are reported in Table B.6. This analysis provide broadly similar evidence on the association between SES and probability of transition as models in the main body of the thesis. However, there is a somewhat different pattern of association between age and probability of transition after accounting for unobserved heterogeneity between individuals.

Table B.5: Estimated effects on risk of transition from reporting being ‘likely’ to apply to university to reporting being ‘unlikely’ to apply to university: hazard ratios

	M1	M2	M3	M4	M4C	M5	M5C
Age 16	1.24	1.21	1.24	1.23	1.24	1.23	1.23
	(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***
Age 17	1.10	1.16	1.22	1.27	1.28	1.34	1.36
	(0.06)*	(0.07)**	(0.08)***	(0.08)***	(0.08)***	(0.09)***	(0.09)***
SES Q1 (Low)	1.77	1.81	1.20	1.17		1.19	
	(0.16)***	(0.17)***	(0.11)**	(0.10)*		(0.10)*	
SES Q2	1.66	1.46	1.25	1.23		1.22	
	(0.15)***	(0.12)***	(0.10)***	(0.10)***		(0.10)**	
SES Q4	0.67	0.73	0.77	0.77		0.77	
	(0.06)***	(0.06)***	(0.06)***	(0.06)***		(0.06)***	
SES Q5 (High)	0.22	0.30	0.41	0.41		0.43	
	(0.02)***	(0.03)***	(0.04)***	(0.04)***		(0.04)***	
SES Z-Score					0.65		0.66
					(0.03)***		(0.03)***
Male		1.75	1.76	1.71	1.72	1.71	1.72
		(0.10)***	(0.10)***	(0.10)***	(0.10)***	(0.10)***	(0.10)***
Ethnicity: Mixed		0.51	0.52	0.51	0.51	0.51	0.50
		(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***
Ethnicity: Indian		0.11	0.10	0.10	0.09	0.10	0.10
		(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***
Ethnicity: Pakistani		0.17	0.13	0.13	0.12	0.13	0.12
		(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***
Ethnicity: Bangladeshi		0.16	0.17	0.17	0.15	0.17	0.15
		(0.03)***	(0.04)***	(0.04)***	(0.03)***	(0.04)***	(0.03)***
Ethnicity: Black Caribbean		0.26	0.17	0.17	0.17	0.17	0.17
		(0.05)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***
Ethnicity: Black African		0.12	0.10	0.10	0.09	0.10	0.09
		(0.03)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***
Ethnicity: Other		0.18	0.16	0.16	0.15	0.16	0.15
		(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***
Attended Independent School		0.22	0.20	0.23	0.23	0.23	0.24
		(0.06)***	(0.06)***	(0.06)***	(0.06)***	(0.06)***	(0.07)***
Attended Grammar School		0.15	0.33	0.34	0.34	0.35	0.35
		(0.03)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***
Attended School with Sixth Form		0.81	0.83	0.84	0.84	0.83	0.84
		(0.05)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***
Experienced workless household		0.96	0.87	0.83	0.74	0.84	0.76
		(0.08)	(0.07)	(0.07)**	(0.06)***	(0.07)**	(0.06)***
Ever experienced family separation		0.99	0.99	0.96	0.97	0.97	0.98
		(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Local Youth Unemployment Rate / 10		0.97	0.96	0.97	0.96	0.97	0.96
		(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
KS2 Z-Score			0.44	0.47	0.48	0.48	0.48
			(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***
KS4 Z-Score (After results)				0.59	0.59	0.42	0.53
				(0.04)***	(0.04)***	(0.06)***	(0.04)***
KS4 Z-Score * SES Q1						1.72	
						(0.32)***	
KS4 Z-Score * SES Q2						1.65	
						(0.30)***	
KS4 Z-Score * SES Q4						1.22	
						(0.23)	
KS4 Z-Score * SES Q5						0.98	
						(0.24)	
KS4 Z-Score * SES Z-Score							0.78
							(0.06)***
Geographical		√	√	√	√	√	√
Number and order of siblings		√	√	√	√	√	√
Months of birth and interview	√	√	√	√	√	√	√
χ^2 test of difference from previous model	.	667.42	397.97	63.32	183.09	13.76	12.53
p-value of above test statistic	.	0.00	0.00	0.00	0.00	0.01	0.00
Variance of Random Effect	2.19	1.64	1.33	1.31	1.34	1.29	1.31
LR test of diff. from non-RE model (χ^2)	385.48	271.36	231.84	241.99	253.91	232.91	241.32
p-value of above test statistic	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Number of individuals	9,247	9,247	9,247	9,247	9,247	9,247	9,247

Notes: Reporting hazard ratios. Standard errors (clustered by individual’s school) in parentheses. Weighted using Wave 2 survey design and non-response weights. Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Estimated risks are relative to the following base categories: Age 15, SES quintile group 3, attended a non-selective state school, white, and female. Tests of model fit are relative to the model one column to the left, with the following exceptions: M4C is relative to M3, M5 is relative to M4, and M5C is relative to M4C.

Table B.6: Estimated effects on risk of transition from reporting being ‘unlikely’ to apply to university to reporting being ‘likely’ to apply to university: hazard ratios

	M1	M2	M3	M4	M4C	M5	M5C
Age 16	1.04	1.06	1.04	1.03	1.03	1.03	1.03
	(0.07)	(0.07)	(0.07)	(0.06)	(0.07)	(0.06)	(0.06)
Age 17	0.74	0.73	0.73	0.89	0.89	0.88	0.87
	(0.05)***	(0.05)***	(0.05)***	(0.07)	(0.07)	(0.07)*	(0.07)*
SES Q1 (Low)	0.70	0.64	0.76	0.78		0.75	
	(0.07)***	(0.07)***	(0.08)***	(0.08)**		(0.08)***	
SES Q2	0.84	0.84	0.89	0.89		0.88	
	(0.07)**	(0.07)**	(0.07)	(0.07)		(0.07)	
SES Q4	1.37	1.34	1.20	1.18		1.17	
	(0.14)***	(0.13)***	(0.11)*	(0.11)*		(0.11)*	
SES Q5 (High)	2.54	2.42	2.01	1.95		1.96	
	(0.31)***	(0.28)***	(0.23)***	(0.22)***		(0.22)***	
SES Z-Score					1.36		1.37
					(0.06)***		(0.06)***
Male		0.52	0.53	0.54	0.54	0.54	0.54
		(0.03)***	(0.03)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***
Ethnicity: Mixed		1.80	1.81	1.80	1.78	1.82	1.80
		(0.32)***	(0.31)***	(0.30)***	(0.30)***	(0.30)***	(0.30)***
Ethnicity: Indian		4.19	4.85	4.76	4.85	4.77	4.81
		(0.93)***	(0.96)***	(0.96)***	(0.98)***	(0.96)***	(0.96)***
Ethnicity: Pakistani		5.55	6.85	6.56	6.93	6.58	6.84
		(0.97)***	(1.22)***	(1.15)***	(1.23)***	(1.15)***	(1.21)***
Ethnicity: Bangladeshi		7.76	8.25	7.75	8.14	7.85	8.16
		(1.46)***	(1.50)***	(1.40)***	(1.47)***	(1.41)***	(1.46)***
Ethnicity: Black Caribbean		3.87	4.51	4.35	4.21	4.42	4.24
		(0.82)***	(0.96)***	(0.90)***	(0.87)***	(0.91)***	(0.87)***
Ethnicity: Black African		7.92	10.20	9.62	9.92	9.75	9.84
		(2.37)***	(2.88)***	(2.69)***	(2.83)***	(2.75)***	(2.80)***
Ethnicity: Other		4.35	5.02	5.03	5.18	5.04	5.11
		(0.96)***	(1.21)***	(1.17)***	(1.17)***	(1.18)***	(1.17)***
Attended Independent School		1.38	1.54	1.48	1.51	1.46	1.49
		(0.61)	(0.63)	(0.57)	(0.59)	(0.56)	(0.58)
Attended Grammar School		2.06	1.08	1.03	0.98	1.00	0.97
		(0.61)**	(0.32)	(0.30)	(0.29)	(0.29)	(0.29)
Attended School with Sixth Form		1.09	1.06	1.05	1.05	1.05	1.05
		(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Experienced workless household		0.98	1.04	1.09	1.12	1.08	1.11
		(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Ever experienced family separation		1.09	1.09	1.09	1.09	1.10	1.10
		(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
Local Youth Unemployment Rate / 10		1.05	1.03	1.03	1.03	1.03	1.03
		(0.07)	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)
KS2 Z-Score			1.71	1.59	1.58	1.58	1.58
			(0.07)***	(0.06)***	(0.06)***	(0.06)***	(0.06)***
KS4 Z-Score (After results)				1.78	1.78	1.97	1.96
				(0.13)***	(0.13)***	(0.35)***	(0.17)***
KS4 Z-Score * SES Q1						0.79	
						(0.16)	
KS4 Z-Score * SES Q2						0.90	
						(0.21)	
KS4 Z-Score * SES Q4						1.53	
						(0.46)	
KS4 Z-Score * SES Q5						0.63	
						(0.17)*	
KS4 Z-Score * SES Z-Score							1.20
							(0.10)**
Geographical		√	√	√	√	√	√
Number and order of siblings		√	√	√	√	√	√
Months of birth and interview	√	√	√	√	√	√	√
χ^2 test of difference from previous model	.	334.66	210.08	61.99	123.07	9.78	4.56
p-value of above test statistic	.	0.00	0.00	0.00	0.00	0.04	0.03
Variance of Random Effect	1.51	1.28	1.04	0.99	0.99	1.00	0.99
LR test of diff. from non-RE model (χ^2)	178.19	144.84	111.63	101.51	100.05	101.96	100.23
p-value of above test statistic	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Number of individuals	5,330	5,330	5,330	5,330	5,330	5,330	5,330

Notes: Reporting hazard ratios. Standard errors (clustered by individual's school) in parentheses. Weighted using Wave 2 survey design and non-response weights. Stars indicate statistical significance: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Estimated risks are relative to the following base categories: Age 15, SES quintile group 3, attended a non-selective state school, white, and female. Tests of model fit are relative to the model one column to the left, with the following exceptions: M4C is relative to M3, M5 is relative to M4, and M5C is relative to M4C.

Appendix C

Duration modelling likelihood

Using the estimation methods detailed by Jenkins (1995) and, earlier, by Allison (1982), makes estimation of the duration models in this paper significantly easier. The method makes use of the fact that we can rewrite the likelihood function for our duration models in the same form as that for a binary dependent variable regression model. This also requires that we reorganise the dataset so that there is one observation for each period each individual is at risk of making the transition of interest. In this appendix, I walk through the steps that lead to this 'easy estimation' method.[1]

I start by setting up the duration model. I index participants as i and spell time as t. Each spell includes an indicator defining whether a transition has occurred by the point of observation. I call this indicator δ_i and define it thus:

$$\begin{aligned} \delta_i &= 1 \text{ if the spell ends with transition} \\ &= 0 \text{ otherwise} \end{aligned} \tag{C.1}$$

The cumulative distribution function is the probability that transition has occurred by time t:

$$F_{it} = Pr(T_i < t) \tag{C.2}$$

where $t \in \{1, 2, 3, ...\}$

[1] This exposition owes much to Allison (1982), Jenkins (1995) and Box-Steffensmeier and Jones (2004, p.71-72).

The converse of this is the survival function i.e. the probability that the transition has not occurred by time t:

$$S_{it} = 1 - F_{it} = Pr(T_i \geq t) \tag{C.3}$$

The probability density function is the probability that transition occurs at time t:

$$f_{it} = Pr(T_i = t) \tag{C.4}$$

Using the above, we also want to know the probability that transition occurs at time t, given that it has not occurred up to that point. This is known as the hazard rate, and can be written using the probability density and survival functions (i.e. equations C.4 and C.3) by simple application of the law of conditional probability:

$$\begin{aligned} h_{it} &= Pr(T_i = t | T_i \geq t) \\ &= \frac{f_{it}}{S_{it}} \end{aligned} \tag{C.5}$$

Since the hazard rate is of interest, we now also define our probability density and survival functions in terms of it. First, the probability density function. It is the probability that the transition occurred at time T (h_{iT}), but did not occur ($1 - h_{it}$) in any of the earlier time periods ($t = 1, 2, ..., T - 1$):

$$\begin{aligned} Pr(T_i = t) = f_{iT_i} &= h_{iT_i} \prod_{t=1}^{T_i - 1} (1 - h_{it}) \\ &= \frac{h_{iT_i}}{1 - h_{iT_i}} \prod_{t=1}^{T_i} (1 - h_{it}) \end{aligned} \tag{C.6}$$

As it will be useful in writing the likelihood function in an easily estimable form, I also multiply through by $(1 - h_{iT_i})$ (by increasing the upper limit of the product to T_i from $T_i - 1$) and also divide through by it (as can be easily seen on the left).

Likewise, the survival function is just the stream of probabilities that the event did not occur in any time periods up to and including T:

$$Pr(T_i \geq t) = S_{iT_i} = \prod_{t=1}^{T_i} (1 - h_{it}) \tag{C.7}$$

Finally, before we can write down the likelihood function, we need to make some basic assumptions about the distribution of our data. Specifically, assume that our observations are independent and that the outcome takes a Bernoulli distribution:

$$g_{\theta_1,\theta_2,\ldots,\theta_n}(\delta) = [\,\text{independence}\,] = \prod_{i=1}^{n} f_{\theta_i}(\delta)$$
$$g_{\theta_1,\theta_2,\ldots,\theta_n}(\delta) = [\,\text{Bernoulli}\,] = \prod_{i=1}^{n} \theta_i^{\delta_i}(1-\theta_i)^{1-\delta_i}$$

where θ_n is the observed outcome for each observation n. In our particular case, therefore, θ is whether or not the transition of interest occurs. We defined the probability of this event above.

As such, we're now ready to write down likelihood function. This is simply a matter of filling in our events of interest, i.e. the probability that the transition occurs (given that it hasn't before) and the probability that the event doesn't occur instead of the θ placeholder.

$$L = \prod_{i=1}^{n} \left[Pr(T_i = t)\right]^{\delta_i} \left[Pr(T_i \geq t)\right]^{1-\delta_i} \tag{C.8}$$
$$= \prod_{i=1}^{n} \left[f_{iT_i}\right]^{\delta_i} \left[S_{iT_i}\right]^{1-\delta_i} \tag{C.9}$$

Substituting in from our definitions of the hazard rate above and then rearranging we can get:

$$L = \prod_{i=1}^{n} \left(\left[\frac{h_{iT_i}}{1-h_{iT_i}} \prod_{t=1}^{T_i}(1-h_{it}) \right]^{\delta_i} \left[\prod_{t=1}^{T_i}(1-h_{it}) \right]^{1-\delta_i} \right) \tag{C.10}$$
$$= \prod_{i=1}^{n} \left(\left[\frac{h_{iT_i}}{1-h_{iT_i}} \right]^{\delta_i} \left[\prod_{t=1}^{T_i}(1-h_{it}) \right]^{\delta_i} \left[\prod_{t=1}^{T_i}(1-h_{it}) \right]^{1-\delta_i} \right)$$
$$= \prod_{i=1}^{n} \left(\left[\frac{h_{iT_i}}{1-h_{iT_i}} \right]^{\delta_i} \left[\prod_{t=1}^{T_i}(1-h_{it}) \right]^{1} \right)$$
$$= \prod_{i=1}^{n} \left(\left[\frac{h_{iT_i}}{1-h_{iT_i}} \right]^{\delta_i} \left[\prod_{t=1}^{T_i}(1-h_{it}) \right] \right) \tag{C.11}$$

By inspecting Equation C.10, we can see that spells that end in transition ($\delta_n = 1$) contribute to the left part of the likelihood function, while spells that do not end in transition ($\delta_n = 0$) contribute to the right part. This follows from the inclusion of δ and $1 - \delta$ as powers in the respective parts of the function. Although this is no longer obvious after rearrangement to Equation C.11, it is a useful way to think about the contribution each individual makes, especially when it comes to the differences when applied to a rearranged dataset below.

Taking logarithms, we find that the corresponding log-likelihood function is:

$$l = \sum_{i=1}^{n} \delta_i \log \left[\frac{h_{iT_i}}{(1 - h_{iT_i})} \right] + \sum_{i=1}^{n} \sum_{t=1}^{T_i} \log(1 - h_{it}) \tag{C.12}$$

While we could just proceed using this likelihood function, estimation would require specialist programming and maximisation would be computationally intensive. Instead, with a little work, we can rewrite this function as something more familiar. To do this, we define a new variable d_{it}:

$$\begin{aligned} d_{it} &= 1 \text{ if } \delta_i = 1 \cap t_i = T_i \\ &= 0 \text{ otherwise} \end{aligned} \tag{C.13}$$

This is the same d_{it} as defined in the main body of the paper. One can see that it differs from δ_i in that it exists for all values of t, but is only equal to 1 for the final observation of a spell. Recalling our observation about Equation C.10, that each individual makes at most one contribution to the part of the likelihood function relevant the occurrence of the transition. When we reorganise our dataset, with one observation for each time period an individual is in a position to make a transition, we still only want the period in which the individual does make the transition to contribute to that part of the likelihood. d_{it} provides for this. As such, we can rewrite and rearrange the log-likelihood function thus:

$$l = \sum_{i=1}^{n} \sum_{t=1}^{T_i} d_{it}. \log \left[\frac{h_{it}}{1 - h_{it}} \right] + \sum_{i=1}^{n} \sum_{t=1}^{T_i} \log(1 - h_{it}) \tag{C.14}$$

$$= \sum_{i=1}^{n} \sum_{t=1}^{T_i} d_{it}. \log(h_{it}) + \sum_{i=1}^{n} \sum_{t=1}^{T_i} (1 - d_{it}) \log(1 - h_{it})$$

$$= \sum_{i=1}^{n} \sum_{t=1}^{T_i} [d_{it}. \log(h_{it}) + (1 - d_{it}) \log(1 - h_{it})] \tag{C.15}$$

This is identical to the log-likelihood function for a binary regression, apart from the additional summation across multiple time periods. It follows that we can simply use a binary regression model, such as logistic regression or complementary log-log regression, applied to a dataset reorganised so that we observe all values of d_{it} (rather than just one observation per spell, with a single indicator δ_i) to carry out our estimation.

Appendix D

Example questions from the Thinking Skills Assessment

The Thinking Skills Assessment (TSA) used for admissions to the University of Oxford is made up of two sections. Firstly, a ninety minute, fifty question, multiple choice section to assess problem solving and critical thinking skills. Second, a thirty minute writing task, in which individuals may choose from four possible tasks.

The following questions from the first section are reproduced from the freely available specimen test on the Admissions Testing Service website (Admissions Testing Service, 2014), but are copyright of the University of Cambridge Local Examinations Syndicate (UCLES) 2007.

1. Every motorist pays the same amount for road tax, regardless of how much they use the roads: someone who covers as little as 1 000 miles pays the same as someone who covers 20 000. This is unfair. Road tax should be scrapped and the money raised by an increase in the tax on car fuel. Making this change would ensure that those who use the roads more would pay more. This would not only be a fairer system, but could also bring in more revenue. Which of the following best illustrates the principle underlying the argument above?

A People should receive free medical treatment only if they cannot afford to pay for it.

B People who travel to work every day by train should pay a lower fare than those who travel only occasionally.

C People who earn more than double the average wage should be made to pay much higher charges for dental treatment.

D Television channels should be paid for by subscription so that only those people who watch them should be made to pay.

E Telephone charges should be higher for business customers than for domestic customers because they are using the system only to make money.

2. Every year in Britain there are nearly 25 000 car fires, yet it is estimated that only five per cent of motorists travel with a fire extinguisher in their car. If more motorists could be encouraged to carry fire extinguishers then the number of car fires could be considerably reduced. Which of the following is the best statement of the flaw in the argument above?

A It ignores the fact that millions of motorists never experience a car fire.

B It assumes that carrying a fire extinguisher will enable fires to be put out.

C It implies that the occurrence of car fires is related to the lack of an extinguisher.

D It overlooks the possibility that fires might not be put out with an extinguisher.

E It ignores the fact that there are different extinguishers for different kinds of fires.

3. School examination results in England this year reinforce the trend in improving pass rates. There is, however, no other evidence of improvements in school leavers' abilities - such as the data coming from employers or universities. One can reasonably conclude, therefore, that teachers are simply succeeding in coaching their pupils better for examinations than in previous years. Which one of the following is an underlying assumption of the above argument?

A School examination results are a reliable indicator of pupils' abilities.

B The level of difficulty of examinations has not been falling.

C Employers' expectations of school leavers are unrealistic.

D Teachers in previous years did not attempt to coach pupils for examinations.

E Abilities of school pupils vary from year to year.

Appendix E

Supplementary results for Chapter 4

In this appendix, I build on the initial analysis from Section 4.5 and in particular Table 4.4. I report estimates of the effect of introducing the TSA on the overall proportion of applicants offered a place, proportion of applicants called to interview, and proportion of interviewees offered a place, reporting the results in Tables E.1, E.2 and E.3, respectively. In each column of these tables, the DiD estimates of policy impact are shown either by rows giving the interaction between Economics and policy on (δ) or by rows giving the interaction between Economics and treatment years (δ_8, δ_9 and δ_{10}), depending on the model. I will not discuss Model 1 in each case, since they are so similar to the analysis from Table 4.4 in Section 4.5.

Table E.1 shows that in none of the years when the policy is on is a statistically significant interaction term between the year of application and being in the treatment group identified. This confirms the earlier analysis that the introduction of the aptitude test does not seem to affect the proportion of applicants who are offered places. Adding in college-level variables, including the average GCSE performance of applicants to the college and a measure of college performance in undergraduate degrees, also has little estimated effect on our outcomes of interest. This model also shows an unsurprising relationship between the average number of GCSE A*s held by applicants to a college and the proportion of those applicants who get a place. In addition, the R^2 of the model increases significantly.

According to the simple difference in difference model the proportion of applicants who get an interview has a negative and statistically significant relationship with the introduction of the TSA. Once again, this seems to be confirmed by Table E.2's model allowing

Table E.1: Proportion of all applicants getting an offer: difference in differences estimates

	(1) Simple	(2) Years	(3) Controls
Constant (α)	0.284	0.284	-0.013
	(0.006)***	(0.006)***	(0.209)
Treated (β)	-0.034	-0.034	-0.038
	(0.016)**	(0.016)**	(0.012)***
Policy On (γ)	-0.043		
	(0.005)***		
2008 (γ_8)		-0.017	-0.031
		(0.005)***	(0.011)***
2009 (γ_9)		-0.057	-0.080
		(0.006)***	(0.006)***
2010 (γ_{10})		-0.051	-0.078
		(0.006)***	(0.008)***
Treated*Policy On (δ)	-0.013		
	(0.014)		
Treated*2008 (δ_8)		-0.029	-0.026
		(0.018)	(0.016)
Treated*2009 (δ_9)		-0.018	-0.017
		(0.012)	(0.012)
Treated*2010 (δ_{10})		0.004	-0.003
		(0.016)	(0.015)
Mean No. of GCSEs (State)			-0.011
			(0.015)
Mean No. of GCSEs (Ind.)			-0.026
			(0.015)*
Mean No. of A*s (State)			0.018
			(0.008)**
Mean No. of A*s (Ind.)			0.018
			(0.006)***
Norrington Score / 10			6.731
			(1.408)***
N	116	232	232
R^2	0.271	0.269	0.565

Notes: Analysis excludes individuals for whom school type is unknown. For Simple model (1), Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. For other models, base category for years is pooling of observations for 2005 and 2006. Standard errors in parentheses. Stars indicate staistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E.2: Proportion of all applicants getting an interview: difference in differences estimates

	(1) Simple	(2) Years	(3) Controls
Constant (α)	0.788	0.788	0.512
	(0.007)***	(0.007)***	(0.352)
Treated (β)	0.041	0.041	0.035
	(0.015)***	(0.015)***	(0.016)**
Policy On (γ)	-0.111		
	(0.005)***		
2008 (γ_8)		-0.075	-0.107
		(0.007)***	(0.018)***
2009 (γ_9)		-0.119	-0.153
		(0.007)***	(0.010)***
2010 (γ_{10})		-0.133	-0.175
		(0.007)***	(0.013)***
Treated*Policy On (δ)	-0.144		
	(0.023)***		
Treated*2008 (δ_8)		-0.095	-0.088
		(0.022)***	(0.022)***
Treated*2009 (δ_9)		-0.204	-0.199
		(0.029)***	(0.027)***
Treated*2010 (δ_{10})		-0.141	-0.151
		(0.028)***	(0.028)***
Mean No. of GCSEs (State)			0.011
			(0.026)
Mean No. of GCSEs (Ind.)			-0.052
			(0.030)*
Mean No. of A*s (State)			0.022
			(0.011)**
Mean No. of A*s (Ind.)			0.029
			(0.010)***
Norrington Score / 10			5.313
			(1.989)***
N	116	232	232
R^2	0.721	0.613	0.715

Notes: Analysis excludes individuals for whom school type is unknown. For Simple model (1), Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. For other models, base category for years is pooling of observations for 2005 and 2006. Standard errors in parentheses. Stars indicate staistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

different effects by year: all coefficients on the interaction between the treatment group and years when the policy is on are negative and significant. However, it is important to note that these vary significantly from year to year: this suggests a more complex picture than our single estimate suggested.

Table E.3: Proportion of all interviewees getting an offer: difference in differences estimates

	(1) Simple	(2) Years	(3) Controls
Constant (α)	0.360	0.360	0.097
	(0.007)***	(0.007)***	(0.251)
Treated (β)	-0.058	-0.058	-0.068
	(0.019)***	(0.019)***	(0.014)***
Policy On (γ)	-0.005		
	(0.005)		
2008 (γ_8)		0.014	-0.005
		(0.006)**	(0.009)
2009 (γ_9)		-0.021	-0.058
		(0.007)***	(0.008)***
2010 (γ_{10})		-0.004	-0.048
		(0.007)	(0.010)***
Treated*Policy On (δ)	0.034		
	(0.018)*		
Treated*2008 (δ_8)		-0.009	-0.011
		(0.023)	(0.022)
Treated*2009 (δ_9)		0.061	0.064
		(0.021)***	(0.020)***
Treated*2010 (δ_{10})		0.063	0.034
		(0.023)***	(0.022)
Mean No. of GCSEs (State)			-0.022
			(0.015)
Mean No. of GCSEs (Ind.)			-0.022
			(0.025)
Mean No. of A*s (State)			0.022
			(0.011)**
Mean No. of A*s (Ind.)			0.025
			(0.007)***
Norrington Score / 10			5.965
			(1.609)***
N	116	232	231
R^2	0.126	0.101	0.361

Notes: Analysis excludes individuals for whom school type is unknown. For Simple model (1), Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. For other models, base category for years is pooling of observations for 2005 and 2006. Standard errors in parentheses. Stars indicate staistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E.3 confirms the picture of an increase in the proportion of interviewees who receive an offer, offsetting the declining numbers who get an interview at all. One additional feature is notable: in the first year with the policy on (2008) we cannot reject the null hypothesis of no impact.

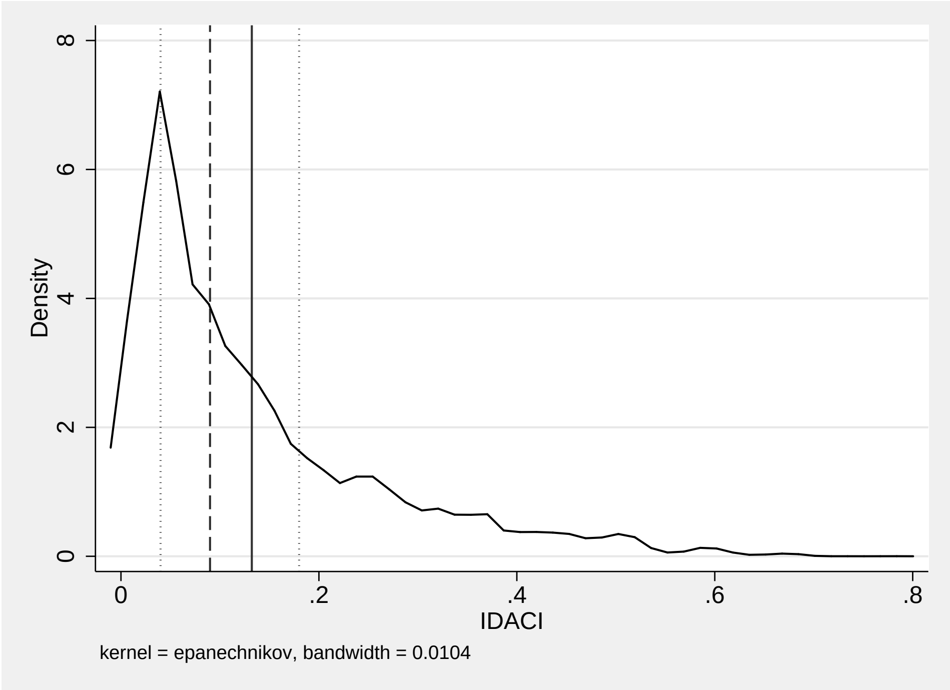
E.1 Estimated effects of the introduction of an aptitude test on an area-level deprivation index

Using the same approach to analysing stages of the admissions process as that used in Section 4.8, I also consider the effect of introducing the TSA on another proxy for applicants' SES. I use the average area deprivation level of applicants' schools, measured using the Income Deprivation Affecting Children Index (IDACI) that I described in Section 4.4.

The IDACI is constructed as the percentage of all children aged 0-15 living in income deprived families (McLennan et al., 2011, p.22-23) within a Lower Layer Super Output Area (geographical districts covering the UK containing between 400 and 1,200 households (Office of National Statistics, 2014)). This is reported to the nearest whole percent. Nevertheless, it gives more potential discrimination than the simple independent/state split used in my main analysis. Figure E.1 shows the graph of a kernel density estimate of the school IDACI of individuals in the dataset. It shows that the distribution is highly skewed, with applicants to the University of Oxford highly concentrated in schools in low-deprivation areas. This is also reflected in the difference between the mean (13%) and the median (9%). Unfortunately, school IDACI is missing in more cases (11.1%) than school type (2.2%): 11.4% of applicants at independent schools, 6.5% of applicants at state schools, and 83.4% of applicants with some other or missing school type have no school IDACI recorded.

While it would be better to use the IDACI for the young person's area of residence (rather than that of their school), this was not available for reasons of confidentiality. However, analysis using the Longitudinal Study of Young People in England (following a cohort of roughly similar age to those in the administrative data) shows that the IDACI score of a young person's school's area is correlated with their own socioeconomic status. I report the results in Table E.4. The correlation between the IDACI score for the area where a young person lives is positively correlated with the IDACI score of the area where their school is situated (Pearson's correlation coefficient = 0.46). More fundamentally, the IDACI score of the area where a young person's school is situated is weakly negatively correlated (since one is a measure of disadvantage and the other a measure of advantage) with their household income (Pearson's correlation coefficient = -0.21).

Figure E.1: Kernel density distribution of IDACI score



Notes: Solid vertical line indicates mean, dashed vertical line indicates median, and dotted vertical lines indicate upper and lower quartiles. Excludes individuals for whom school IDACI was not recorded.

Table E.4: Average characteristics of Longitudinal Study of Young People in England cohort members by IDACI quintile group of their school's area

Characteristic	IDACI quintile group of school's area				
	5th (Advantaged)	4th	3rd	2nd	1st (Disadvantaged)
IDACI score of young person's home area (%)	15	18	23	28	39
Household Income (£)	22,579	21,355	18,017	17,158	14,233
Mother has a degree (%)	30	26	22	20	14
Father has higher managerial or professional occupation (%)	43	39	31	29	20
Family in financial difficulties (%)	6	6	7	9	11
Family living in socially rented housing (%)	15	18	22	29	41
Young person attends independent school (%)	6	5	0	5	0

Notes: Data from the Longitudinal Study of Young People in England (LSYPE). Average characteristics for LSYPE cohort members who attend schools in each of five quintiles groups defined by the IDACI score of the school's area. Characteristics are measured at Wave 1 of the LSYPE, at age 14 years, except in case of income, which is averaged over measurements are ages 14, 15 and 16. Income is in 2003–2004 prices. Calculations courtesy of Claire Crawford of the Institute for Fiscal Studies/University of Warwick.

Using a continuous outcome variable also allows analysis of changes to different parts of the distribution of applicants' schools' area deprivation, not just changes to the mean. Although the method I use is not quantile regression (Koenker and Bassett, 1978; Parente and Santos Silva, 2013), it shares some of the same intuition. As in earlier sections of the paper I use college-level least squares regression, but rather than only using as observations the mean deprivation level of applicants (or interviewees, or those offered a place), I also use models with observations constructed as the lower quartiles (Q25), medians or upper quartiles (Q75) of the school IDACI for a given college, course, year combination.

Such changes are matters of interest since a shift in the mean deprivation level alone could result from a number of different changes in the underlying distribution of applicants, interviewees or those offered a place. To illustrate this, let us consider two notional shifts in the deprivation distribution of interviewees which could have identical effects on the mean deprivation of applicants. We might see an effect that only shifts the lower quartile of the deprivation distribution of interviewees and has no impact on the median or the upper quartile. This would suggest that the policy change is filtering out some of the applicants from most advantaged schools, but these are being replaced by applicants only slightly above them on the deprivation distribution. The effect is not having a broader impact further up the distribution. Alternatively, we might see an effect that shifts the lower quartile of the distribution of interviewees somewhat less than our first change, but also shifts the median interviewee's deprivation level. This would imply a somewhat broader effect, with those at the bottom of the deprivation distribution being replaced by applicants significantly further down (albeit without much effect on those attending schools in the most deprived areas).

I report the results from regression models similar to those from Section 4.6, with the coefficient on the interaction between the policy on and treatment group (δ) recovering the DiD estimate, for each stage of the admissions process in Tables E.5, E.6 and E.7. The estimates of the policy are in units of the IDACI. For example, an estimate of 1 implies an estimated 1 percentage point increase in the mean, median or quartile deprivation of applicants, interviewees or those offered a place. As such, their magnitudes are not comparable with estimates in Section 4.8. As with the main analysis, I include controls for the average GCSE performance by state and independent school applicants, interviewees or attendees and college Norrington score.

Table E.5: School IDACI of applicants - changes at the mean, lower quartile, median and upper quartile of colleges' distributions: difference in differences estimates

	(1) Mean	(2) Q25	(3) Median	(4) Q75
Constant (α)	5.997	7.754	6.175	-25.776
	(9.765)	(5.248)	(10.976)	(28.401)
Treated (β)	-0.332	-0.085	-0.220	0.324
	(0.355)	(0.229)	(0.407)	(0.729)
Policy On (γ)	0.679	0.397	0.567	0.581
	(0.381)*	(0.191)**	(0.482)	(1.064)
Treated*Policy On (δ)	0.333	0.131	0.260	0.048
	(0.422)	(0.227)	(0.445)	(0.933)
Mean No. of GCSEs (State)	0.583	-0.638	-0.733	5.009
	(0.966)	(0.364)*	(1.228)	(2.722)*
Mean No. of GCSEs (Ind.)	-0.076	0.481	0.930	-0.625
	(0.616)	(0.366)	(0.588)	(1.676)
Mean No. of A*s (State)	-0.348	0.344	0.511	-2.649
	(0.222)	(0.138)**	(0.326)	(0.952)***
Mean No. of A*s (Ind.)	-0.874	-0.684	-1.383	-1.001
	(0.285)***	(0.192)***	(0.294)***	(0.503)*
Norrington Score / 10	138.105	11.166	108.495	285.789
	(59.907)**	(33.449)	(66.867)	(158.560)*
N	162	162	162	162
R^2	0.177	0.217	0.195	0.243

Notes: Analysis excludes individuals for whom school IDACI is unknown. Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate staistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We see from Table E.5, in common with the analysis in Section 4.8, no statistically significant estimated effect on the mean IDACI of applicants' schools. If anything, the results estimate an increase in the mean area deprivation level of applicants' schools equivalent to 3 additional children in the average area living in income deprivation per 1000 children. Examining different points of the distribution adds little additional information, since all the estimates are statistically insignificant and show no obvious pattern.

Turning to those called to interview, the results for the mean again concord with those we might expect from the earlier analysis by school type. Table E.6 shows no statistically significant difference in the mean IDACI, although the estimate is again positive. Estimates for different points of the distribution are again statistically insignificant from one another or zero, but show some suggestion that the effect is larger in the areas with higher income deprivation (although none are as large as the estimate at the mean).

Finally, considering changes in the mean school-level IDACI of those who get an offer (Table E.7) shows somewhat larger absolute estimates than analysis of the interviewees. However, it is worth noting than, unlike at earlier stages and in the analysis of the propor-

Table E.6: School IDACI of interviewees - changes at the mean, lower quartile, median and upper quartile of colleges' distributions: difference in differences estimates

	(1)	(2)	(3)	(4)
	Mean	Q25	Median	Q75
Constant (α)	0.359	6.164	4.558	-25.371
	(11.886)	(5.506)	(13.058)	(30.838)
Treated (β)	-0.249	-0.155	0.072	0.766
	(0.386)	(0.288)	(0.475)	(0.778)
Policy On (γ)	0.260	0.410	0.392	0.288
	(0.503)	(0.281)	(0.632)	(1.247)
Treated*Policy On (δ)	0.532	0.005	0.174	0.375
	(0.431)	(0.319)	(0.421)	(0.927)
Mean No. of GCSEs (State)	0.077	-0.774	-1.492	3.110
	(1.253)	(0.445)*	(1.409)	(3.241)
Mean No. of GCSEs (Ind.)	0.985	0.854	1.878	0.657
	(0.677)	(0.370)**	(0.722)**	(1.496)
Mean No. of A*s (State)	-0.225	0.140	0.437	-1.796
	(0.287)	(0.136)	(0.317)	(1.101)
Mean No. of A*s (Ind.)	-0.748	-0.487	-1.282	-1.152
	(0.269)***	(0.233)**	(0.307)***	(0.733)
Norrington Score / 10	120.216	-3.341	102.596	331.748
	(68.653)*	(34.824)	(63.018)	(161.009)**
N	162	162	162	162
R^2	0.096	0.148	0.193	0.160

Notes: Analysis excludes individuals for whom school IDACI is unknown. Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate staistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

tions from state school, the estimates are negative. None of the estimates are statistically significant, so we can have little confidence in this finding, especially as it is inconsistent with most of the analysis.

E.2 Within state school variation

While the above analysis includes all applicants, I now restrict my attention to changes in the distribution of the school-level IDACI just within state school applicants. There is more than one reason for doing this. First, the vast majority of the population attend state schools and the average socioeconomic status of young people attending these schools varies significantly. As such, it would be possible for there to be large changes in the socioeconomic status of applicants, interviewees and those offered a place without observing any changes in variables relating to school type. This analysis assesses whether this is indeed the case.

The second reason is that we might be more concerned about the relevance of the school-

Table E.7: School IDACI of applicants offered a place - changes at the mean, lower quartile, median and upper quartile of colleges' distributions: difference in differences estimates

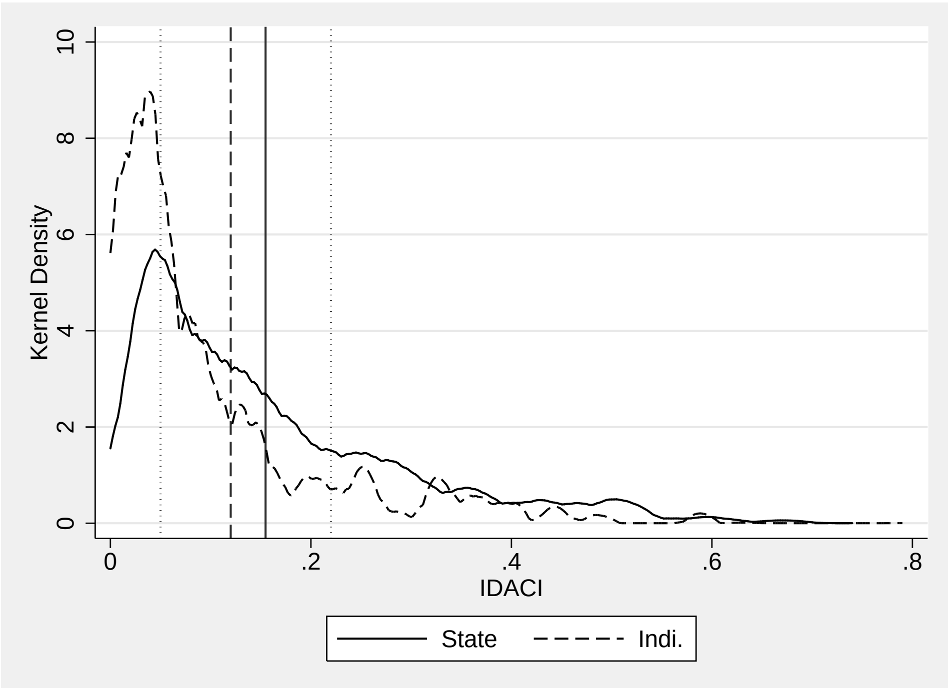
	(1) Mean	(2) Q25	(3) Median	(4) Q75
Constant (α)	9.266	7.591	0.264	22.195
	(11.152)	(4.368)*	(13.478)	(21.765)
Treated (β)	0.597	0.286	0.914	2.290
	(0.807)	(0.425)	(0.778)	(1.627)
Policy On (γ)	0.224	0.438	0.197	0.746
	(0.549)	(0.234)*	(0.669)	(1.148)
Treated*Policy On (δ)	-0.493	-0.304	-0.943	-1.466
	(0.890)	(0.403)	(0.844)	(1.919)
Mean No. of GCSEs (State)	-0.196	-0.223	0.353	-0.756
	(1.206)	(0.404)	(1.210)	(2.484)
Mean No. of GCSEs (Ind.)	0.921	0.080	0.491	1.249
	(0.516)*	(0.303)	(0.642)	(1.278)
Mean No. of A*s (State)	0.428	-0.210	0.459	0.303
	(0.467)	(0.191)	(0.474)	(0.890)
Mean No. of A*s (Ind.)	-0.627	-0.179	-1.011	-0.866
	(0.460)	(0.238)	(0.414)**	(0.927)
Norrington Score / 10	-35.798	7.594	54.127	-85.952
	(61.934)	(38.353)	(74.826)	(113.906)
N	114	114	114	114
R^2	0.051	0.061	0.085	0.046

Notes: Analysis excludes individuals for whom school IDACI is unknown. Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate staistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

area IDACI in the case of independent schools: young people who go to such schools often travel further to attend, particularly as they are far more likely to offer boarding provision. As such, excluding individuals from independent schools may give a more reliable idea about changes in individual-level socioeconomic status using school-level data.

The mean school-level IDACI of applicants from state schools (15%) is higher than that from independent schools (10%). We see the same when considering the median applicant in each case, with IDACI of 12% for the median state school applicant and of 5% for the median independent school applicant. The overall difference in the two distributions is shown by plots of the kernel density of the IDACI for independent and state school applicants in Figure E.2.

Figure E.2: Kernel density distribution of IDACI by school type



Notes: Solid vertical line indicates mean, dashed vertical line indicates median, and dotted vertical lines indicate upper and lower quartiles for state school applicants. Excludes individuals for whom school IDACI was not recorded.

The design of the results tables is the same as those earlier in this section. I report the analyses for each stage of the admissions process in Tables E.8, E.9 and E.10. Since we are only considering those from state school, I only control for the average GCSE performance of state school applicants and college's Norrington score, not the mean performance of independent school applicants.

When it comes to state school applicants, the results for the mean again concord with findings from the analysis in Section 4.8. We see from Table E.8 very little estimated

Table E.8: School IDACI of state school applicants - changes at the mean, lower quartile, median and upper quartile of colleges' distributions: difference in differences estimates

	(1) Mean	(2) Q25	(3) Median	(4) Q75
Constant (α)	21.910	6.471	17.106	42.621
	(16.160)	(8.822)	(16.748)	(32.004)
Treated (β)	-0.251	0.318	-0.157	0.025
	(0.458)	(0.299)	(0.506)	(0.996)
Policy On (γ)	0.132	-0.088	0.033	0.637
	(0.588)	(0.351)	(0.713)	(1.266)
Treated*Policy On (δ)	0.156	0.032	0.328	-0.016
	(0.686)	(0.342)	(0.692)	(1.380)
Mean No. of GCSEs (State)	-0.105	-0.005	-0.424	-0.989
	(1.502)	(0.869)	(1.640)	(3.087)
Mean No. of A*s (State)	0.332	0.320	0.385	0.202
	(0.770)	(0.163)*	(0.560)	(1.300)
Norrington Score / 10	-106.586	-43.790	-49.398	-175.889
	(105.165)	(39.161)	(82.454)	(180.250)
N	162	162	162	162
R^2	0.065	0.043	0.042	0.063

Notes: Analysis excludes individuals for whom school IDACI is unknown. Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate staistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E.9: School IDACI of state school interviewees - changes at the mean, lower quartile, median and upper quartile of colleges' distributions: difference in differences estimates

	(1) Mean	(2) Q25	(3) Median	(4) Q75
Constant (α)	18.853	5.060	11.968	23.917
	(16.708)	(12.625)	(23.914)	(26.112)
Treated (β)	-0.208	0.318	0.154	0.843
	(0.474)	(0.376)	(0.629)	(1.113)
Policy On (γ)	-0.606	-0.447	-0.566	-0.413
	(0.827)	(0.570)	(1.168)	(1.226)
Treated*Policy On (δ)	0.088	0.104	-0.243	-0.883
	(0.781)	(0.506)	(0.846)	(1.788)
Mean No. of GCSEs (State)	-0.128	0.317	0.207	-0.012
	(1.503)	(1.131)	(2.249)	(2.217)
Mean No. of A*s (State)	0.621	0.407	0.561	0.093
	(0.551)	(0.168)**	(0.496)	(1.089)
Norrington Score / 10	-88.883	-81.945	-88.896	-47.000
	(104.392)	(39.732)**	(84.930)	(222.590)
N	162	162	162	162
R^2	0.038	0.057	0.021	0.008

Notes: Analysis excludes individuals for whom school IDACI is unknown. Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate staistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E.10: School IDACI of state school applicants offered a place - changes at the mean, lower quartile, median and upper quartile of colleges' distributions: difference in differences estimates

	(1) Mean	(2) Q25	(3) Median	(4) Q75
Constant (α)	40.251	13.050	22.562	47.482
	(16.971)**	(7.928)	(15.437)	(32.317)
Treated (β)	0.507	0.672	0.286	2.469
	(0.929)	(0.502)	(1.040)	(1.886)
Policy On (γ)	0.305	0.103	-0.171	0.329
	(0.852)	(0.425)	(0.768)	(1.539)
Treated*Policy On (δ)	-0.908	-0.692	-1.104	-2.362
	(1.077)	(0.662)	(1.027)	(2.226)
Mean No. of GCSEs (State)	-1.666	-0.422	-0.589	-0.990
	(1.715)	(0.814)	(1.582)	(3.146)
Mean No. of A*s (State)	0.751	0.040	0.074	0.753
	(0.518)	(0.291)	(0.536)	(1.048)
Norrington Score / 10	-199.970	-58.965	-87.129	-326.103
	(105.383)*	(64.206)	(92.494)	(185.568)*
N	116	116	116	116
R^2	0.063	0.050	0.043	0.057

Notes: Analysis excludes individuals for whom school IDACI is unknown. Policy Off in 2005 and 2006; Policy On in 2008, 2009 and 2010. Standard errors, clustered by college-subject group combination, in parentheses. Stars indicate staistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

effect on the mean area deprivation level of applicants' schools, although the estimate is positive. Likewise with Table E.9 for the mean school-level IDACI among interviewees. In neither case does analysing the quantiles provide any obvious addition to the narrative: in all cases the difference in differences estimates are not statistically significant from either zero or each other.

Finally, I consider the changes in the school-level IDACI of those state school applicants who get an offer (Table E.10). As with the analysis of all those offered a place, the change in mean IDACI of those from state schools offered a place is estimated to be negative. However, this time the estimate is rather larger, but still far from statistical significance.

E.3 Discussion

Analysis considering changes at different quantiles is more difficult to interpret a single estimate of changes in means. However, its results have the potential to provide more information on the nature of the impact.

In this analysis, while the point estimates at different quantiles do vary from one another and from the estimated changes in means, these differences are never statistically significant from zero or each other. Nevertheless, that we see some variation is suggestive of differing impacts across the deprivation distribution. Furthermore, there is little sign of a consistent pattern towards one end of the distribution or the other.

Nevertheless, the point estimates we see tend to back up the story of very little socioeconomic change resulting from the introduction of the TSA, as seen in the main analysis.

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