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When you are born matters: the impact of date of birth on educational outcomes in England

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Abstract. This paper examines the impact of month of birth on national achievement test scores in England whilst children are in school, and on subsequent further and higher education participation. Using geographical variation in school admissions policies, we are able to split this difference into an age of starting school or length of schooling effect, and an age of sitting the test effect. We find that the month in which you are born matters for test scores at ages 7, 11, 14 and 16, with younger children performing significantly worse, on average, than their older peers. Furthermore, almost all of this difference is due to the fact that younger children sit exams up to one year earlier than older cohort members. The difference in test scores at age 16 potentially affects the number of pupils who stay on beyond compulsory schooling, with predictable labour market consequences. Indeed, we find that the impact of month of birth persists into higher education (college) decisions, with age 19/20 participation declining monotonically with month of birth. The fact that being young in your school year affects outcomes after the completion of compulsory schooling points to the need for urgent policy reform, to ensure that future cohorts of children are not adversely affected by the month of birth lottery inherent in the English education system.

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1. Introduction

Any school admissions policy involves having children in the same class or school year who are different ages. Does this difference in age have long term implications? And what should be the policy response if it does? In England, the month in which you are born affects the age at which you start school, the length of schooling you receive and the age at which you sit key tests. This paper addresses the long term consequences for education outcomes of being born in a particular month. In England, we find that being younger in your school year has significant negative effects on outcomes including national achievement tests at age 16 and higher education (college) participation at age 19/20. This negative effect is not only for the very youngest – it is incremental across the whole age distribution within a cohort. Our results show that the effects of being younger in the school year are substantial, even at later ages, but also that the effect of these differences could be remedied relatively straightforwardly and in a cost-effective way.

Of course, we are not the first paper to tackle this issue: it has been documented across many countries that children born at the end of the academic year (which runs from 1 September to 31 August in England) perform more poorly, on average, than older members of their cohort.[1] A number of factors may contribute to this: first, in a system in which exams are taken at a fixed date, some children will sit them up to a year younger than others ("absolute age" or "age of sitting the test" effect); these children may also suffer from the fact that they were "too young" when they started school ("age of starting school" effect). Moreover, the younger children may be adversely affected by the fact that they are younger than their peers ("relative age"

[1] See, for example, Alton & Massey (1998), Massey, Elliott & Ross (1996), Russell & Startup (1986), Sharp (1995), Sharp & Hutchison (1997) and Thomas (1995) for England, Elder & Lubotsky (2007) for the US, Smith (2007) for Canada, Puhani & Weber (2005) for Germany, Borg & Falzon (1995) for Malta, McEwan & Shapiro (2006) for Chile and Bedard & Dhuey (2006) for an international study.

effect). Finally, depending on the admissions system, some of the younger children may have attended school for fewer terms prior to the exam ("length of schooling" effect). However, there is relatively little reliable evidence on how each of these factors contributes to the performance shortfall of younger children in a cohort, particularly for longer-term academic outcomes.

Two recent papers have looked at this issue.[2] Fredriksson and Ockert (2005) use Swedish administrative data for the population born 1935–84 to look at the impact of school starting age on education and labour market outcomes. They find that increasing school starting age by one year increases grade point average at the age of 16 by 0.2 standard deviations. They exploit within-school variation in the age composition across cohorts to separate the impact of relative age (the age position effect) from the impact of absolute age (plus the effect of school entrance age) and find that relative age accounts for only 6 per cent of the difference in test scores at that age. However, they can only separate the effect of age at entry to school from absolute age by looking at outcomes after the end of compulsory schooling (when there is independent variation between the two). They find that starting school later has a small positive impact on earnings (although they point out that the net earnings effect over the life cycle is negative, because starting school later implies entering the labour market later as well).[3]

Black, Devereux and Salvanes (2008) identify the impact of school starting age on IQ scores, educational attainment, teenage pregnancy and earnings using Norwegian administrative data. They find that starting school younger has a significant positive effect on IQ scores at age 18 and the probability of becoming a teenage mother, but

[2] Bell & Daniels (1990), Fogelman & Gorbach (1978) and Sharp, Hutchison & Whetton (1994) for England and Cascio & Whitmore Schanzenbach (2007) and Datar (2006) for the US are some other attempts.

[3] It should be noted that there is never any variation in length of schooling in this paper.

little effect on educational attainment. In contrast to Fredriksson and Ockert (2005), Black, Devereux and Salvanes (2008) find that starting school younger has a small *positive* effect on earnings, which has disappeared by age 30. This pattern is consistent with the idea that starting school later reduces potential labour market experience at a particular age (for a given level of education), but that the importance of this year of experience is reduced as individuals age.

Our paper adds to the existing literature by exploiting a key feature of the English education system: that school admissions policies are determined by local, rather than central, education authorities.[4] This gives rise to considerable regional variation in the age at which children born on a particular day of the year start school.[5] As we are able to observe exact date of birth, this enables us to separately identify the causal impacts of age of sitting the test, and age of starting school (or length of schooling, not both[6]) for both compulsory and post-compulsory schooling outcomes – something that, to our knowledge, no other papers have been able to do.

The rest of this paper now proceeds as follows. In Section 2, we outline our modelling approaches. Section 3 provides more information about the data-sets that we use and Section 4 describes our sample. In Section 5, we document the month of birth penalty for test scores measured at ages 7, 11 and 14. Section 6 exploits geographical variation in admissions policies to show that the majority of the difference in attainment arises because children are younger when they sit the tests, rather than because they receive fewer terms of schooling prior to sitting these tests. In Section 7, we examine the persistence of month of birth penalties, by showing test

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[4] There are around 150 Local Authorities (LAs) – which are responsible for setting admissions policies – in England.

[5] There is also some variation over time within authorities.

[6] Note that there is insufficient variation in the admissions policies implemented in England for us to be able to separate the effect of age of starting school and the effect of length of schooling.

score differentials at ages 11, 14, 16 and 18, as well as higher education (college) participation decisions at age 19/20. Finally, Section 8 concludes, and considers how policy could be used to address the disadvantage faced by children who are younger in their year.

2. Modelling approach

To estimate the impact of month of birth on education outcomes, we adopt a regression discontinuity approach, running regressions of the following form:

$$y_{ist} = \alpha_s + \delta \sum_{k=1}^{11} M_{ikt} + \sum_{r=2}^{4} P_{rt} + X_{it}^{'}\beta + \lambda_t + \varepsilon_{ist} \qquad (1)$$

where M_{ikt} = 1 if individual *i* is born in month *k* (the oldest children in the year – those born in September – are our omitted category), α_s is a school fixed effect, λ_t is a set of cohort dummies and P_{rt} is a set of admissions policy dummies representing the four policies *(r=1,2,3 and 4)* we observe in the data at time *t* (described in detail in Section 3).[7] This model allows us to identify the impact of being born in a particular month, including the effects of the discontinuity on children born up to one month either side of the academic year cut-off, or indeed any month-on-month comparison as we have multiple cohorts.

In this model, we are making comparisons within schools (and therefore within admissions policy areas[8]), so as long as the observed and unobserved

[7] This is necessary because in a small number of local authorities, admission policies change across the different cohorts we consider, and these changes are not accounted for by the inclusion of school fixed effects. Note that our results are not materially different if we use school-cohort fixed effects instead.

[8] Note that local authorities (LAs) are not responsible for the admissions policies of all state schools in their areas; some are free to choose their own admissions policies. Between 2001-02 and 2006-07, LAs were responsible for admissions policies covering approximately two thirds of the state school population. Our analysis assumes that all schools follow the admissions policy set by their LA, such that we estimate something more akin to an intention-to-treat effect. The question we are asking is 'What is the impact on education outcomes of starting school in an LA that follows one admissions policy rather than any other?'. This should, theoretically, weaken the treatment effect, so our estimates are likely to provide a lower bound to the true impact of date of birth on education outcomes.

characteristics of students at the school (and the effectiveness of the school) do not vary by date of birth, we will estimate the causal impact of being born in a particular month (relative to September).[9] However, we include a variety of observed individual and local area characteristics (X'_{it}) (see Section 3.2 for details) to improve the precision of our estimates.

If we are able to ascertain that there is a significant difference between the education outcomes of children born in different months, then gaining a fuller understanding of the underlying causes of these differences becomes very important. In most countries, it is extremely difficult to separate the impacts of absolute age (age at which the child sits the test), age of starting school and length of schooling on compulsory schooling outcomes, because there is an exact linear relationship between the three:

Age at test = Age of starting school + Length of schooling

If all children in a particular cohort start school at the same time and sit tests at the same time, then it is impossible to identify these three effects separately on compulsory schooling outcomes.[10]

However, whilst it is the case that children in England all sit tests at the same time, there is geographical variation in the age at which children start school (and hence the number of terms of schooling they receive prior to the tests). If we assume that

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[9] We can check the validity of part of this assumption by testing whether the probability of being a k=1 born child compared with the probability of being a k=12 born child varies by *observed* characteristics. This could happen if parents from certain backgrounds try to ensure that their child will always be one of the oldest in the school year through conception decisions, or if children from certain backgrounds who are amongst the youngest in their year are more likely to be put into private schools (our data is for state school children only). Initial analysis of our sample suggests that there is some evidence that children who are eligible for free school meals (a proxy for low family income) are around 2.7 percentage points more likely to be born in August than children who are not eligible for free school meals. (Buckles & Hungerman (2008) find similar results for the US.) Hence, we always control for observed background characteristics (although in practice this does not make any difference to our results). Results without controlling for observable characteristics are available from the authors on request.

[10] In addition, the oldest children (in absolute terms) in each cohort will also be the oldest relative to others in their class, so the age position effect may also play a role.

unobserved geographical variables are not correlated with test scores, then we can separately identify the effects of age of sitting the test and age of starting school (or length of schooling) on education outcomes. (We must also assume that exposure to a particular policy is independent of outcome, which is just the standard conditional independence assumption.)

To separately identify these effects, we modify equation (1) by interacting our month of birth dummies, M_{ik}, with our policy dummies, P_r. Our new estimating equation is given by:

$$y_{ist} = \alpha_s + \delta_{rk} \sum_{k=1}^{12} \sum_{r=1}^{4} P_{rt} M_{ikt} + X_{it}^{'} \beta + \lambda_t + \varepsilon_{ist} \qquad (2)$$

where P_{rt}, λ_t and α_s are again admissions policy dummies, cohort dummies and school fixed effects respectively, and our omitted category is those born in September in policy area 1 (*k=1, r=1*). Given that we are comparing children across admissions policy areas (and hence across schools) to identify these effects, it becomes very important to control for all observed characteristics, X_{it}, that might affect school choice and academic outcomes (see Section 3.2 for details of the characteristics for which we are able to control).

As mentioned earlier, there is also some variation in admissions policies over time within authority. This allows us to use difference in differences methods to identify the impact of changing policies (and hence starting school earlier) amongst children in those local authorities. We use this as a robustness check on our main results.

3. Data

We use administrative data comprising a census of all children attending state (public) schools in England, which includes national achievement (Key Stage) test results at ages 7, 11, 14 and 16, plus limited background characteristics (including date of birth, home postcode and a school identifier). We also have access to data on post-compulsory schooling outcomes, including academic and vocational qualifications achieved by age 18, and higher education (college) participation decisions at ages 19 and 20 (the first two years of potential participation).

3.1 Test score outcomes: the Key Stage tests

The Key Stage tests are national achievement tests sat by all children in state schools in England: Key Stage 1 is taken at age 7, Key Stage 2 at age 11, Key Stage 3 at age 14 and Key Stage 4 (GCSEs) at age 16. Key Stage 5 covers post-compulsory education (from age 16 to age 18). We have access to results for Key Stage 1 from 1997-98 to 2006-07, for Key Stage 2 from 1994–95 to 2006–07, for Key Stage 3 from 1996–97 to 2006–07, for Key Stage 4 from 2001–02 to 2006–07 and for Key Stage 5 from 2000-01 to 2006-07. As we are not able to follow a single cohort of children throughout compulsory and post-compulsory schooling using the available data, we instead make use of two separate cohorts to piece together a complete picture. We discuss the construction of these cohorts in Section 4.

At ages 7, 11 and 14, the main subjects assessed are English, maths and science. In each case, pupils are allocated an attainment level, which can be translated into a corresponding points score (using a specified formula) ranging from 3 to 21 at Key Stage 1 (with 15 being the expected level), 15 to 33 at Key Stage 2 (with 27 being the expected level) and from 17 to 45 at Key Stage 3 (with 33 being the expected level).[11] We standardise the average of these three scores within academic year to create our main measure of attainment. We also calculate whether a pupil achieved the government's expected (target) level on the basis of their assigned score.

[11] Note that for Key Stage 2 and Key Stage 3 results, we also have access to raw test marks, which allows us to calculate a much more detailed average point score than that described above. We used these raw test scores to check the difference it makes using continuous scores. Using discrete rather than continuous measures of educational attainment makes virtually no difference to our results.

At age 16 (Key Stage 4), students tend to sit exams in up to 10 subjects (including English, maths and science). We make use of the students' capped average point score[12] (that is, the score averaged across their eight best exam results) (standardised within academic year), plus a variable indicating whether the pupil achieved at least five A*–C grades (the government's expected level).

At age 18 (Key Stage 5), students can choose between a wide range of academic and vocational qualifications. We make use of a variable indicating whether the pupil achieved a Level 3 qualification (the expected level) via an academic route – equivalent to (for example) two A-levels at grades A-E.

3.2 Background characteristics

Our data contains a limited set of individual characteristics, including date of birth, eligibility for free school meals (which can be thought of as a proxy for very low family income[13]), ethnicity, whether English is the students' first language, plus whether they have special educational needs. It also includes a school identifier.

In some of our models, it is important to control for observable characteristics that are likely to affect school (and therefore admissions policy) choice and educational attainment. To compensate partially for the lack of family background characteristics available in our data, we use the pupil's home postcode to map in detailed neighbourhood characteristics to control for any local area influences on academic outcomes. These are included alongside the available individual-level data to generate the following list of controls:

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[12] 8 points are awarded for a grade A*, 7 for an A, 6 for a B, 5 for a C, 4 for a D, 3 for an E, 2 for an F and 1 for a G.

[13] Pupils are *entitled* to free school meals if their parents receive various means-tested benefits or tax credits and have a gross household income of less than £15,575 (in 2008–09 prices). They are *eligible* for free school meals if they are both entitled and registered as such with their local authority.

- ethnicity;
- whether English is the child's first language;
- whether the child is eligible for free school meals;
- quintiles of the Index of Multiple Deprivation[14], plus quintiles of the domains comprising income, employment, and education, skills and training;
- quintiles of the Income Deprivation Affecting Children Index[15];
- age distribution of the Output Area (OA)[16] in which the child lives;
- proportion of lone parents (OA level);
- proportion of working-age population in employment (OA level);
- average social class (OA level);
- highest educational qualification of local population (OA level).

3.3 Admissions policy information

Children in England must have started school by the beginning of the term after they turn five: this is considerably earlier than in many other countries. As admissions policies are set by local (rather than central) authorities in England, however, there is considerable geographical variation in the age at which children start school. We exploit this variation to separately identify the impacts of age of sitting the test and age of starting school (or length of schooling) on academic outcomes.

Table 3.1 provides a summary of the main admissions policies that are in operation in England, together with the proportions of pupils who attend schools in areas

[14] This is a local measure of deprivation, available at Super Output Area (SOA) level (comprising approximately 1,500 households), that makes use of seven different domains: income; employment; health and disability; education, skills and training; barriers to housing and services; living environment; and crime.

[15] An additional element of the Index of Multiple Deprivation reflecting the proportion of children living in families in receipt of various means-tested benefits or tax credits.

[16] Output Areas contain approximately 150 households.

affected by these policies in our sample.[17] Table 3.2 translates these admissions policies into details of school entry date by month of birth.

It should be noted that we only observe the local authority in which students sat Key Stage 1 (age 7) for our younger cohort – and the local authority in which students sat Key Stage 2 (age 11) for our older cohort – not the local authority in which they actually started school.[18] This means that if the child has switched authorities since they started school (from one with a different admissions policy in place in the year in which they started), then the information (on age of starting school and length of schooling) that we assign to the child may be inaccurate. We have checked the importance of this potential measurement error by analysing the difference between estimates obtained by assigning admissions policy according to local authority at age 7 and/or 11 rather than local authority at age 5 using a younger cohort (for whom we observe information at all three ages). We do not find any evidence of significant differences as a result of mismeasurement of admissions policy information.[19]

4. Our sample

We use two cohorts of children for our analysis. Our first cohort includes individuals born between September 1990 and August 1993 (1,643,832 in total), who started school in academic years 1995-96, 1996-97 or 1997-98. For these individuals we observe national achievement test scores at age 7 (Key Stage 1), age 11 (Key Stage 2) and age 14 (Key Stage 3).[20] We use this cohort to explore how variation in

[17] Appendix A illustrates the accuracy of our admissions policy information (which was collected retrospectively).

[18] See Section 4 for more details of these cohorts.

[19] Results available from the authors on request.

[20] We eliminate from our analysis individuals who do not start school in the expected year and/or who do not progress through the system in the usual manner (although this is a very minor problem in England). We also restrict attention to individuals who attend state (public) schools in England.

admissions policies affects outcomes (as we would expect to see the largest length of schooling and age of starting school effects at the youngest ages).

Our second cohort comprises individuals born between September 1985 and August 1988 (1,441,137 in total), who started school in academic years 1990-91, 1991-92 or 1992-93. For these individuals we observe national achievement test scores at ages 11, 14, 16 (Key Stage 4) and 18 (Key Stage 5), as well as participation in higher education (college) at age 19 or 20. We use this cohort to examine the long-term effects of month of birth on education outcomes.

Table 4.1 summarises average outcomes for our two cohorts, while Figures 4.1 and 4.2 preview our findings in Section 5, by illustrating how education outcomes vary by gender and date of birth across academic years. The difference in outcomes between individuals born either side of the academic year cut-off (represented by the vertical lines) is similar to the gap in outcomes between August- and September-born children that we document in Section 5.[21]

These figures show that outcomes for August-born children are always lower than those for September-born children, but that this gap steadily decreases (in percentage terms) over time. It also shows that girls perform significantly better than boys, on average, at all ages, and that August-born boys have the worst absolute outcomes.

5. Month of birth penalty during compulsory schooling

This section documents the extent of the month of birth penalty in education outcomes for our younger cohort. We focus on differences in standardised average

[21] We document differences within (rather than between) cohorts in Section 5. This makes little difference to our findings (results across cohorts are available from the authors on request).

point scores and proportions of students reaching the governments' expected level in national achievement tests at ages 7, 11 and 14.

Table 5.1 (and Figures B.1 and B.2 in Appendix B) highlights a number of striking results. First, the effect of starting school (and sitting the tests) younger than your peers is a huge penalty on academic performance. For example, children born in August (at the end of academic year) score approximately half a standard deviation lower, and are 25 percentage points (over one third) less likely to reach the government's expected level, at age 7 than children born in September. Moreover, our results show that the month of birth penalty is approximately linear, such that even those born just one month later in October perform significantly worse than children born in September.

Second, there is a pronounced decline in the effect of starting school (and sitting the test) young on test scores over time, implying that the younger children catch up throughout compulsory schooling. For example, by age 14, August borns score around 0.2 standard deviations lower, and are 8 percentage points (just over 10 per cent) less likely to reach the government's expected level, than September borns. However, the month on month penalty remains significant throughout the year.

6. Decomposing the month of birth effect

In this section, we exploit variation in admissions policies by area and over time – as a result of which children born on the same day may start school at different ages, and hence receive a different amount of schooling prior to the tests – to explore the extent to which school starting age and/or length of schooling can explain these month of birth disparities.

We start by using geographical variation in admissions policies. Table 6.1 compares month of birth penalties (in terms of standardised average point scores) for children in our first cohort who start school in an area which follows a single, double or triple entry point system.

Under each system, children born in September, October, November and December start school in the September of the year in which they turn five (see Table 3.2) and hence receive the same amount of schooling.[22] As such, we would not expect to see any difference in the month of birth penalty for these children, and this presumption is borne out by the results at age 7. Moreover, this finding also provides suggestive evidence that the environment experienced by children of these ages in different areas – for example, in terms of the age composition and/or size of the class into which they enter school – is not sufficiently different to have any effect on their test scores two years later.

Children born in January and February start school in September under the single and double entry point systems, and in January under the triple entry point system, thus if age of starting school and/or length of schooling affect test scores, then we would expect to see a difference in the test scores of January and February borns who start school under the triple entry point system compared to those who start under the single or double entry point system. However, Table 6 does not highlight any consistent differences for children born in these months by admissions policy.

Children born in March and April start school in September under the single entry point system, and in January under the double and triple entry point systems, and for these individuals we do observe a difference in test scores by admissions policy. For example, April-born children who start school under a double (triple) entry point

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[22] Note, however, that children in a two or three point entry system who start school in September may spend their first few months in school with a smaller number of children in the class. (Alternatively, they may be in a regular size class but with smaller age variation between classmates.) This is probably too small a difference to have any discernible effect on test scores.

system score 0.055 (0.041) standard deviations lower at age 7 than April-born children who start school under a single entry point system. This suggests that the negative effect of receiving one less term of schooling prior to the test outweighs the potentially positive effect of starting school older for these children.

Children born in May, June, July and August start school in September under a single entry point system, in January under a double entry point system and in April under a triple entry point system. We continue to observe a significant difference between those who start school under a double vs. single entry point system, and a slightly larger difference between those who start school under a triple vs. single entry point system (as would be expected, given that these children have two extra terms of schooling rather than one), although the difference between those who start under a double vs. triple entry point system is not significant.

To check the robustness of these results, we also focus on the small number of authorities in which there was a policy change over the years in which individuals in our first cohort started school. Both Hartlepool and Manchester (two urban authorities in the North of England) changed from a triple entry point system in 1995-96 to a double entry point system in 1996-97. This means that children born in February, March, May, June, July and August all received an extra term of schooling following this policy change. Our difference in differences results for these local authorities show no significant differences between triple and double point entry, consistent with the results in Table 6.1.[23]

Moving on to compare results at age 14 (in Table 6.2), we observe no consistent differences by admissions policy, suggesting that the effects of receiving an additional term or two of schooling have only short-lived effects. This table shows

[23] Unfortunately we do not have any authorities swapping from triple point entry to single point entry for these cohorts.

that we continue to observe a significant negative relationship between test scores and age within year though.

These results suggest a simple story: the key issue is the absolute age effect, which captures the fact that (taking length of schooling into account) there is a clear and strong negative effect of sitting exams younger. Thus, for an exam system (like the one in the England) in which there is no flexibility over the time at which exams are sat, there are persistent month of birth effects and each month younger the child is, the worse they perform on average. In the next section, we examine how far these effects persist, by examining test scores at ages 16 and 18, as well as higher education (college) participation at age 19/20.

7. How long does the month of birth penalty persist?

If the month of birth penalty did not persist it would not be much of an issue. However, this section documents that it does persist through compulsory schooling and beyond. Here, we consider differences by month of birth in national achievement tests at ages 11, 14 and 16, in the proportions of young people who reach the government's expected level at ages 16 and 18, and in the proportions that go on to higher education (college) at age 19/20. Table 7.1 (and Figures C.1 and C.2 in Appendix C) illustrates these results.

These results highlight that age within year continues to have significant negative effects on education outcomes at ages 16, 18 and 19/20, suggesting permanent implications for a range of adult outcomes. Of particular interest are the effects on the tests taken at age 16 (at the end of compulsory schooling in England). These tests determine whether a child will continue into post-compulsory education and also define the first record that can potentially affect college admissions. At this point, the youngest children score 0.13 standard deviations lower than the oldest ones. This translates into a massive 5.8 percentage point higher potential drop out rate from high school for the youngest children, and a 1.5 percentage point lower college enrolment rate. Moreover, this disadvantage is not restricted to the youngest children only: the effect is approximately linear, and significant from one month onwards.

Thus starting school a year earlier than others implies underachievement when young as well as longer term impacts on attainment. The latter will have direct implications for earnings. Bur even the underachievement at younger ages may have effects that are not fully picked up by attainment, an issue which we are currently exploring.

8. Policy implications and conclusions

This paper has shown that there is a significant penalty associated with date of birth, such that the youngest children in a particular academic year perform significantly worse in national achievement tests than the oldest children. Furthermore, this penalty remains significant at age 16 – when individuals are making choices about whether to stay on beyond compulsory schooling – and also affects higher education participation decisions at age 19/20.

Our findings could be used to argue that children should start school later, given that children born on 31 August do significantly worse than those born on the 1 September, who are the same age, but start school up to one year later. But the consistency of the results amongst children of the same age who start school at different ages (in different admissions policy areas) suggests that it is not the age of starting school but the age of sitting the tests that drives these differences. Moreover, the fact that the month of birth effect is basically linear suggests that even if everyone were to start school a year later, the younger children in the class would still perform worse, on average, than the older children, simply because they are forced to sit the tests when they are younger. This effect is much more marked at early ages, when the relative age difference is large. But these differences persist, even at ages when the relative age difference should no longer have an impact.

This implies that the inflexible form of the English system – in which children are assessed at a fixed point in time (e.g. at the end of year 11, equivalent to US grade 10) – can have long term and permanent detrimental effects, if only because it leads to a substantially greater likelihood of dropping out of high school and a lower probability of college attendance.

Policy thus needs to address this issue by improving the flexibility of assessments. One simple way of doing this would be to age-normalise exam results so that students are compared to others of exactly the same age. This would ensure that students are assessed on their true ability, rather than on the luck of their month of birth draw. This is particularly important at age 16, when exam results determine who qualifies for post-compulsory education. If relatively young children on the margin are not made to drop out and are suitably supported while in school, it is clear that they will perform as well as their older counterparts with the same overall ability (as our results show that they catch up with their peers over time). An alternative way of implementing this policy (as far as exams leading to qualifications are concerned) is to have multiple examination periods and for children to sit for such exams when ready.[24]

Finally, it may be important to understand whether the high early failure rates for the younger children have other effects not reflected in the test scores we measure at older ages. We plan to explore these issues in future research.

[24] See Crawford, Dearden & Meghir (2007) for further discussion of these policy implications.

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Tables

Table 3.1 Admissions policy information

Admissions Policy	Per cent Cohort 1	Per cent Cohort 2	Description
Single entry date	43.9%	38.3%	All children start school in the September of the academic year in which they turn five.
Two entry dates	7.6%	6.4%	Children born 1 September to 29 February start school in the September of the academic year in which they turn five; children born 1 March to 31 August start school in the January of the academic year in which they turn five.
Three entry dates	16.0%	22.4%	Children start school at the beginning of the term in which they turn five, so children born 1 September to 31 December start school in September, children born 1 January to 30 April start school in January and children born 1 May to 31 August start school in April.
Other	3.0%	3.2%	Other variations on the two and three entry point systems.
Unknown	29.5%	29.8%	Schools can choose their own admissions policy, or the admissions policy in place is not known or clear

Notes:
Figures may not sum exactly due to rounding. Cohort 1 comprises children starting school in 1995-96, 1996-97and 1997-98, and Cohort 2 comprises those starting school in 1990-91, 1991-92 and 1992-93. These figures summarise the percentage of children in our sample who started school in an authority in which the admissions policy indicated was in operation (including those who joined schools that do not necessarily have to follow the admissions policy set by the local authority).

Table 3.2 Month of school entry (number of terms of schooling received in first year of full-time education)

	Single entry point	Two entry points	Three entry points
September	September (3 terms)	September (3 terms)	September (3 terms)
October	September (3 terms)	September (3 terms)	September (3 terms)
November	September (3 terms)	September (3 terms)	September (3 terms)
December	September (3 terms)	September (3 terms)	September (3 terms)
January	September (3 terms)	September (3 terms)	January (2 terms)
February	September (3 terms)	September (3 terms)	January (2 terms)
March	September (3 terms)	January (2 terms)	January (2 terms)
April	September (3 terms)	January (2 terms)	January (2 terms)
May	September (3 terms)	January (2 terms)	April (1 term)
June	September (3 terms)	January (2 terms)	April (1 term)
July	September (3 terms)	January (2 terms)	April (1 term)
August	September (3 terms)	January (2 terms)	April (1 term)

Table 4.1 Summary of education outcomes by cohort

	Cohort 1	Cohort 2
Key Stage 1 (age 7)		
Mean average point score (standard deviation)	14.8 (3.9)	
Proportion reaching expected level	0.548	
Key Stage 2 (age 11)		
Mean average point score (standard deviation)	27.2 (4.4)	26.0 (4.4)
Proportion reaching expected level	0.678	0.565
Key Stage 3 (age 14)		
Mean average point score (standard deviation)	34.2 (7.0)	33.3 (6.8)
Proportion reaching expected level	0.667	0.598
Key Stage 4 (age 16)		
Mean capped average point score (standard deviation)		4.5 (1.9)
Proportion getting 5 A*-C grades at GCSE		0.532
Key Stage 5 (age 18)		
Proportion getting a Level 3 qualification (academic route)		0.360
Higher education (college) participation (age 19/20)		
Proportion attending higher education		0.321
Total sample	*1,643,832*	*1,441,137*

Table 5.1 Month of birth penalties: mean differences in key education outcomes at ages 7, 11 and 14

	Standardised average point scores			**Proportion reaching government's expected level**		
	Age 7	**Age 11**	**Age 14**	**Age 7**	**Age 11**	**Age 14**
September base	0.302	0.178	0.114	0.676	0.743	0.711
October difference	-0.047**	-0.031**	-0.020**	-0.019**	-0.011**	-0.007**
	[0.003]	[0.003]	[0.003]	[0.002]	[0.002]	[0.001]
November	-0.098**	-0.061**	-0.038**	-0.039**	-0.021**	-0.015**
	[0.003]	[0.003]	[0.003]	[0.002]	[0.002]	[0.002]
December	-0.156**	-0.099**	-0.061**	-0.065**	-0.035**	-0.023**
	[0.004]	[0.003]	[0.003]	[0.002]	[0.002]	[0.002]
January	-0.220**	-0.131**	-0.082**	-0.092**	-0.048**	-0.031**
	[0.004]	[0.004]	[0.003]	[0.002]	[0.002]	[0.002]
February	-0.270**	-0.159**	-0.099**	-0.113**	-0.058**	-0.037**
	[0.004]	[0.003]	[0.003]	[0.002]	[0.002]	[0.002]
March	-0.323**	-0.186**	-0.114**	-0.135**	-0.067**	-0.043**
	[0.004]	[0.003]	[0.003]	[0.002]	[0.002]	[0.001]
April	-0.376**	-0.218**	-0.135**	-0.158**	-0.081**	-0.054**
	[0.004]	[0.004]	[0.003]	[0.002]	[0.002]	[0.002]
May	-0.439**	-0.250**	-0.145**	-0.184**	-0.092**	-0.056**
	[0.005]	[0.003]	[0.003]	[0.002]	[0.002]	[0.002]
June	-0.493**	-0.283**	-0.164**	-0.210**	-0.106**	-0.065**
	[0.005]	[0.004]	[0.003]	[0.002]	[0.002]	[0.002]
July	-0.538**	-0.304**	-0.177**	-0.229**	-0.115**	-0.070**
	[0.005]	[0.004]	[0.004]	[0.003]	[0.002]	[0.002]
August	-0.590**	-0.336**	-0.197**	-0.253**	-0.128**	-0.079**
	[0.005]	[0.003]	[0.003]	[0.002]	[0.002]	[0.002]

Notes:
All models include cohort dummies, admissions policies dummies, individual-level characteristics (including ethnicity, whether the child is eligible for free school meals and whether English is their first language), a series of neighbourhood characteristics (see Section 3.2 for details) and school fixed effects.
** indicates significance at the 1 per cent level; * indicates significance at the 5 per cent level. Standard errors are corrected for clustering at the local authority level.

Table 6.1 Month of birth penalty: mean differences in standardised average point scores at age 7, by admissions policy

	Standardised average point scores at age 7					
	Single entry point	**Two entry points**	**Difference to single entry point**	**Three entry points**	**Difference to single entry point**	**Difference to two entry points**
September base	0.295	0.264		0.266		
October difference	-0.047**	-0.054**	-0.006	-0.042**	0.005	0.011
	[0.004]	[0.012]	[0.012]	[0.008]	[0.009]	[0.014]
November	-0.101**	-0.102**	-0.001	-0.089**	0.012	0.013
	[0.005]	[0.011]	[0.012]	[0.007]	[0.009]	[0.013]
December	-0.154**	-0.168**	-0.014	-0.154**	0.000	0.014
	[0.005]	[0.013]	[0.014]	[0.009]	[0.010]	[0.015]
January	-0.200**	-0.237**	-0.037*	-0.243**	-0.043**	-0.006
	[0.006]	[0.014]	[0.015]	[0.008]	[0.010]	[0.016]
February	-0.259**	-0.275**	-0.016	-0.280**	-0.021	-0.005
	[0.005]	[0.016]	[0.017]	[0.009]	[0.011]	[0.018]
March	-0.307**	-0.359**	-0.052**	-0.339**	-0.032**	0.020
	[0.005]	[0.013]	[0.014]	[0.008]	[0.010]	[0.015]
April	-0.352**	-0.407**	-0.055**	-0.394**	-0.041**	0.013
	[0.005]	[0.010]	[0.012]	[0.011]	[0.012]	[0.015]
May	-0.407**	-0.457**	-0.050**	-0.486**	-0.078**	-0.029*
	[0.005]	[0.009]	[0.011]	[0.007]	[0.009]	[0.012]
June	-0.460**	-0.522**	-0.062**	-0.522**	-0.062**	0.000
	[0.005]	[0.016]	[0.017]	[0.010]	[0.011]	[0.018]
July	-0.504**	-0.555**	-0.051**	-0.569**	-0.065**	-0.014
	[0.006]	[0.010]	[0.012]	[0.012]	[0.013]	[0.015]
August	-0.557**	-0.610**	-0.053**	-0.620**	-0.063**	-0.010
	[0.005]	[0.013]	[0.014]	[0.010]	[0.012]	[0.017]

Notes:
All models include cohort dummies, admissions policy dummies, individual-level characteristics (including ethnicity, whether the child is eligible for free school meals and whether English is their first language), a series of neighbourhood characteristics (see Section 3.2 for details) and school fixed effects.
** indicates significance at the 1 per cent level; * indicates significance at the 5 per cent level. Standard errors are corrected for clustering at the local authority level.

Table 6.2 Month of birth penalty: mean differences in standardised average point scores at age 14, by admissions policy

	Standardised average point scores at age 14					
	Single entry point	**Two entry points**	**Difference to single entry point**	**Three entry points**	**Difference to single entry point**	**Difference to two entry points**
September base	0.705	0.692		0.702		
October difference	-0.006* [0.003]	-0.007 [0.005]	-0.001 [0.005]	-0.011** [0.004]	-0.004 [0.004]	-0.003 [0.006]
November	-0.017** [0.002]	-0.008 [0.005]	0.009 [0.006]	-0.011** [0.003]	0.005 [0.004]	-0.004 [0.006]
December	-0.023** [0.002]	-0.026** [0.006]	-0.003 [0.007]	-0.024** [0.004]	-0.001 [0.005]	0.002 [0.007]
January	-0.029** [0.003]	-0.037** [0.008]	-0.007 [0.008]	-0.041** [0.004]	-0.011* [0.005]	-0.004 [0.009]
February	-0.041** [0.002]	-0.034** [0.008]	0.007 [0.008]	-0.035** [0.004]	0.006 [0.005]	-0.001 [0.009]
March	-0.045** [0.002]	-0.042** [0.006]	0.003 [0.006]	-0.046** [0.004]	-0.001 [0.005]	-0.004 [0.007]
April	-0.054** [0.002]	-0.046** [0.006]	0.007 [0.006]	-0.059** [0.005]	-0.005 [0.006]	-0.012 [0.008]
May	-0.055** [0.002]	-0.056** [0.005]	-0.002 [0.005]	-0.059** [0.005]	-0.004 [0.005]	-0.002 [0.007]
June	-0.065** [0.002]	-0.067** [0.007]	-0.002 [0.007]	-0.062** [0.003]	0.003 [0.004]	0.006 [0.008]
July	-0.068** [0.002]	-0.066** [0.006]	0.003 [0.006]	-0.074** [0.005]	-0.006 [0.006]	-0.009 [0.008]
August	-0.078** [0.002]	-0.081** [0.006]	-0.003 [0.007]	-0.080** [0.005]	-0.003 [0.005]	0.000 [0.008]

Notes:
All models include cohort dummies, admissions policy dummies, individual-level characteristics (including ethnicity, whether the child is eligible for free school meals and whether English is their first language), a series of neighbourhood characteristics (see Section 3.2 for details) and school fixed effects.
** indicates significance at the 1 per cent level; * indicates significance at the 5 per cent level. Standard errors are corrected for clustering at the local authority level.

Table 7.1 Month of birth penalties: mean differences in key education outcomes at ages 11, 14, 16, 18 and 19/20

	Standardised average point scores			Proportion reaching expected level		College participation
	Age 11	Age 14	Age 16	Age 16	Age 18	Age 19/20
September	0.185	0.123	0.077	0.567	0.377	0.334
October	-0.026**	-0.016**	-0.009**	-0.004*	-0.002	-0.002
	[0.003]	[0.003]	[0.004]	[0.002]	[0.003]	[0.002]
November	-0.059**	-0.036**	-0.023**	-0.012**	-0.005	-0.005**
	[0.004]	[0.004]	[0.004]	[0.002]	[0.002]	[0.002]
December	-0.095**	-0.058**	-0.037**	-0.018**	-0.009**	-0.007**
	[0.003]	[0.003]	[0.003]	[0.002]	[0.002]	[0.002]
January	-0.138**	-0.087**	-0.055**	-0.028**	-0.013**	-0.009**
	[0.004]	[0.004]	[0.004]	[0.002]	[0.002]	[0.002]
February	-0.162**	-0.101**	-0.059**	-0.028**	-0.012**	-0.008**
	[0.004]	[0.004]	[0.004]	[0.002]	[0.002]	[0.002]
March	-0.190**	-0.118**	-0.067**	-0.030**	-0.010**	-0.008**
	[0.004]	[0.004]	[0.003]	[0.002]	[0.002]	[0.002]
April	-0.224**	-0.138**	-0.078**	-0.034**	-0.014**	-0.008**
	[0.004]	[0.004]	[0.004]	[0.002]	[0.002]	[0.002]
May	-0.257**	-0.153**	-0.082**	-0.039**	-0.013**	-0.008**
	[0.004]	[0.004]	[0.004]	[0.002]	[0.002]	[0.002]
June	-0.289**	-0.176**	-0.102**	-0.047**	-0.016**	-0.013**
	[0.004]	[0.004]	[0.004]	[0.002]	[0.002]	[0.002]
July	-0.323**	-0.195**	-0.110**	-0.052**	-0.022**	-0.015**
	[0.004]	[0.005]	[0.004]	[0.002]	[0.002]	[0.002]
August	-0.351**	-0.214**	-0.126**	-0.058**	-0.023**	-0.015**
	[0.004]	[0.004]	[0.004]	[0.002]	[0.002]	[0.002]

Notes:
All models include cohort dummies, individual-level characteristics (including ethnicity, whether the child is eligible for free school meals and whether English is their first language), a series of neighbourhood characteristics (see Section 3.2 for details) and school fixed effects.
** indicates significance at the 1 per cent level; * indicates significance at the 5 per cent level. Standard errors are corrected for clustering at the local authority level.

Figures

Figure 4.1 Proportion of students reaching governments' expected level at ages 7, 11 and 14: Cohort 1

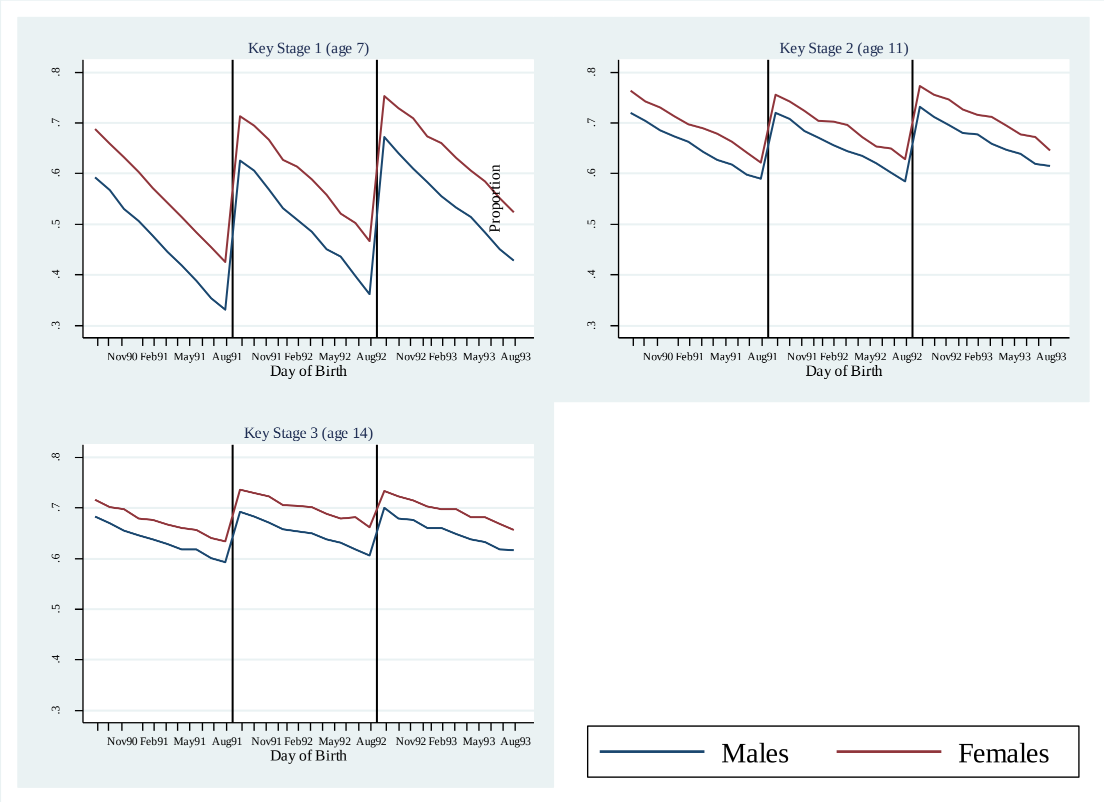
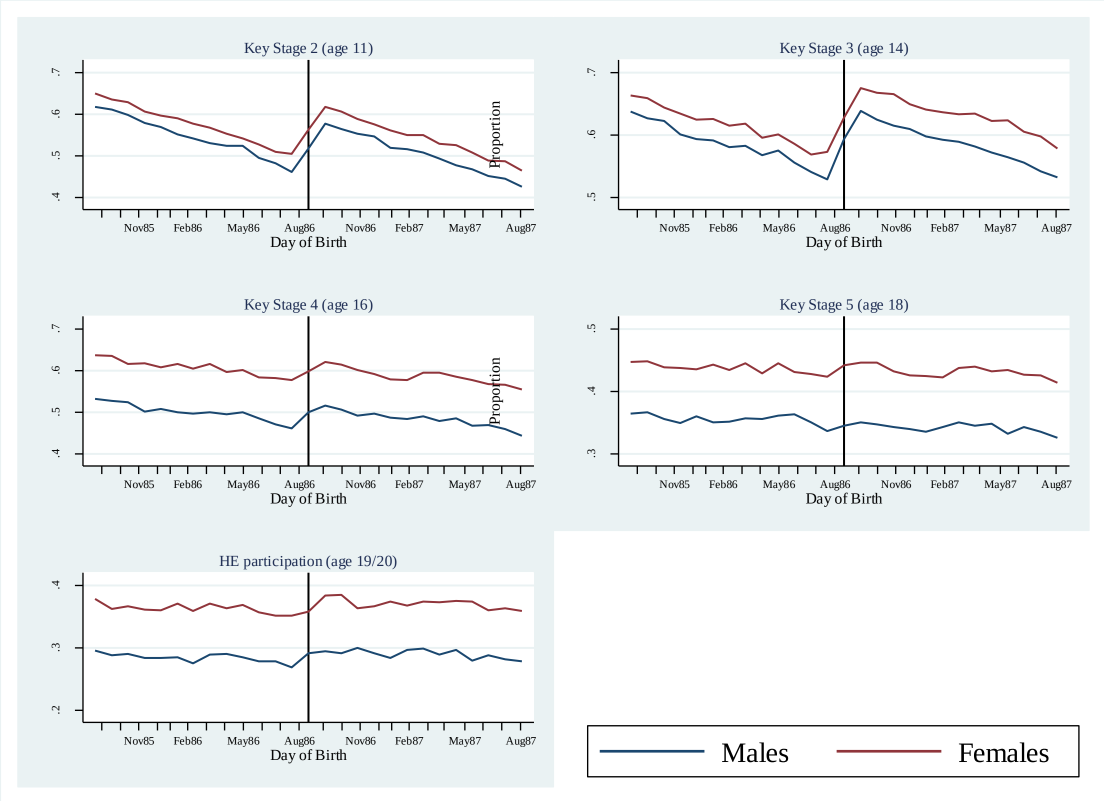
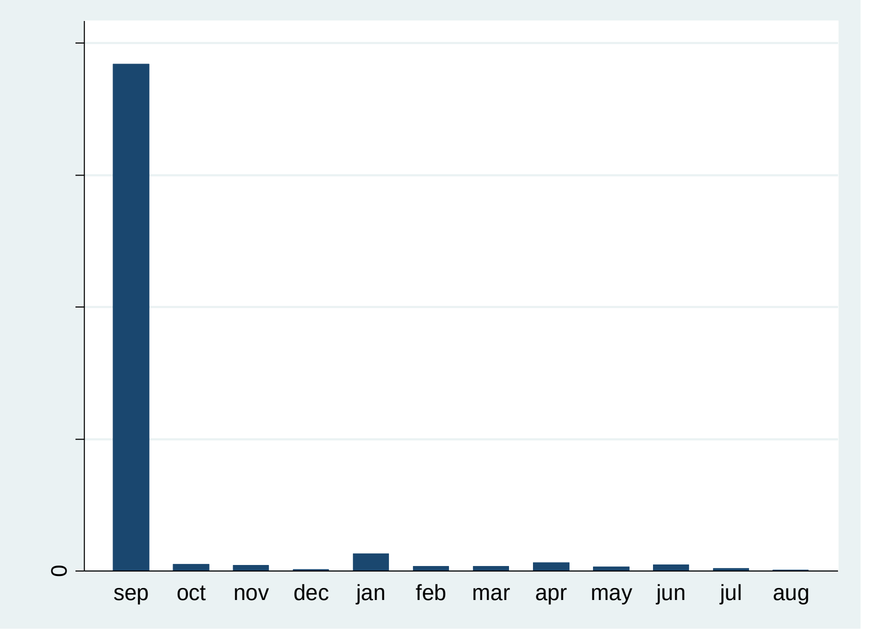


Figure 4.2 Proportion of students reaching governments' expected level at ages 11, 14, 16 and 18, plus college participation: Cohort 2



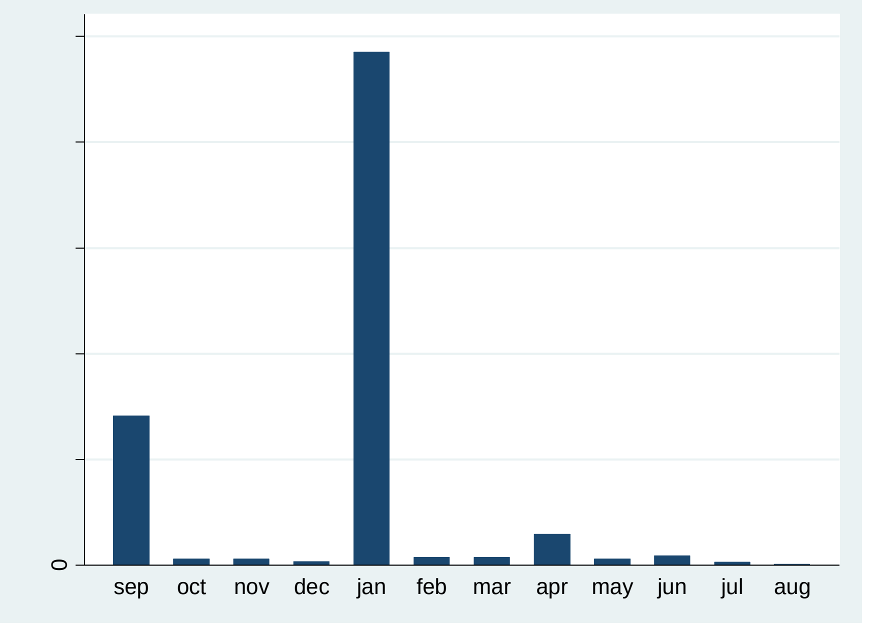
Appendix A

Figure A.1 Graph showing when children who are expected to start school in September actually start school



Notes:
This graph shows, for all children who started school in a community school in England between 2001-02 and 2006-07 in a local authority in which they were expected to start in September, when they actually started school.

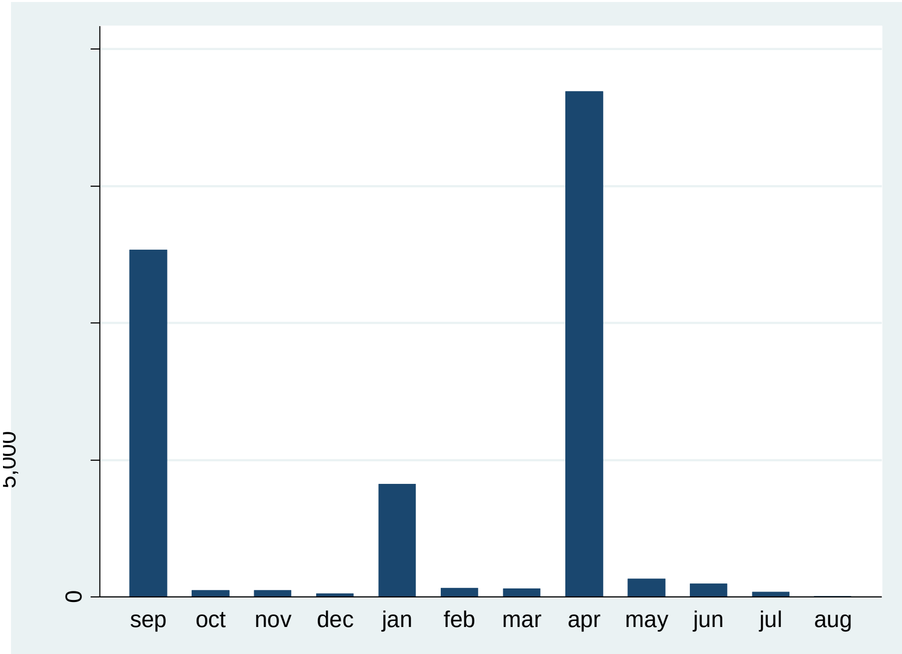
Figure A.2 Graph showing when children who are expected to start school in January actually start school



Notes:

This graph shows, for all children who started school in a community school in England between 2001-02 and 2006-07 in a local authority in which they were expected to start in January, when they actually started school.

Figure A.3. Graph showing when children who are expected to start school in April actually start school



Notes:
This graph shows, for all children who started school in a community school in England between 2001-02 and 2006-07 in a local authority in which they were expected to start in April, when they actually started school.

Appendix B

Figure B.1 Standardised average point scores at ages 7, 11 and 14, by month of birth

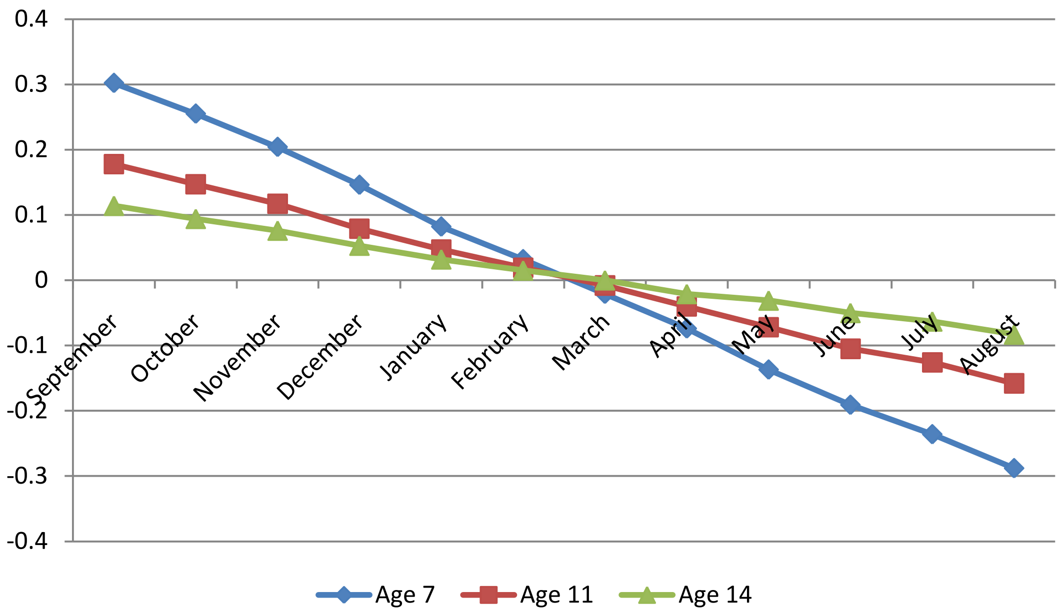
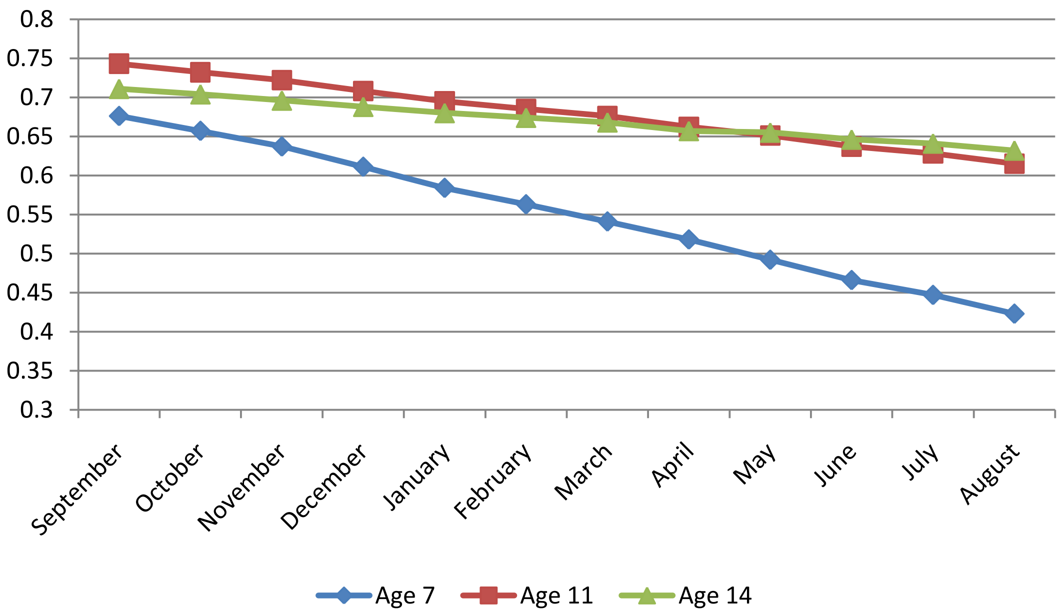


Figure B.2 Proportions of students reaching the governments' expected level at ages 7, 11 and 14, by month of birth



Appendix C

Figure C.1 Standardised average point scores at ages 11, 14 and 16, by month of birth

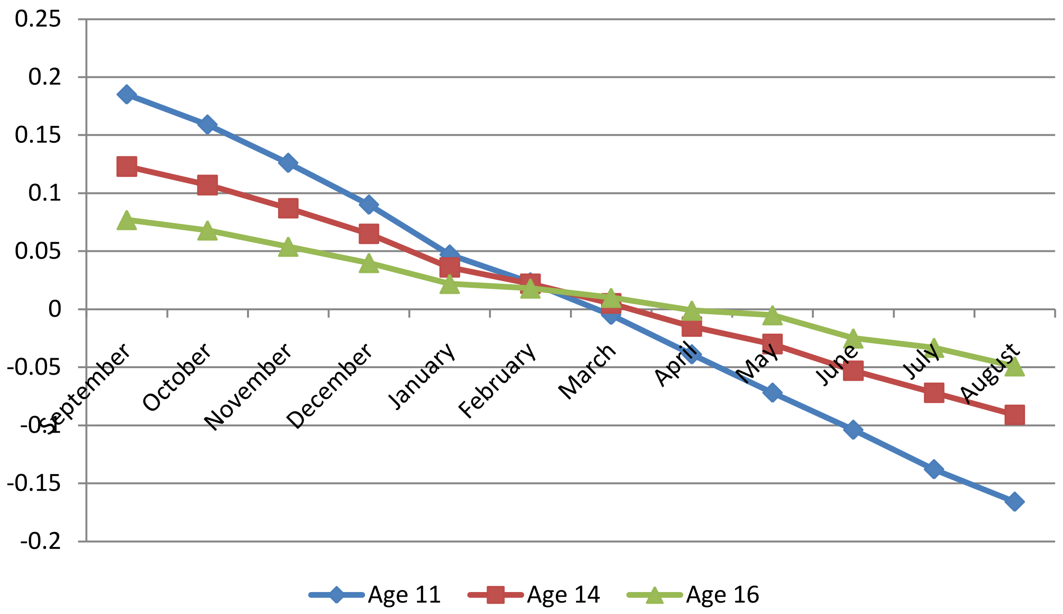


Figure C.2 Proportions reaching expected level at ages 16 and 18, and proportion starting college at age 19/20, by month of birth