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ABSTRACT

Date of Birth and Selective Schooling

We examine the effects of date of birth on state selective education using the 1944 Education Act in England and Wales as a natural experiment. We compare the probabilities of gaining selective school entry – which in our study period meant attending a grammar school – before and after the Act using a difference-in-difference approach. Before 1944, grammar school entry was achieved either noncompetitively through fee-paying or free based on a competitive 11+ exam. After 1944, all children were required to take a competitive 11+ exam and about one-third gained a grammar school place. Pre-1944 we find the children born in the middle or late in the school year (January to August) fared significantly worse in gaining a grammar school place than those born at the beginning of the school year (from September to December). Post-1944, the prospects of grammar school entry among children born in the middle of the school year (January to April) improved considerably. We argue that a greater recourse to age standardisation of 11+ test scores may well have accounted for this outcome. The youngest 'summer children' (those born at the end of the school year from May to August) remained significantly disadvantaged, however. A strong influence was the practice of streaming (or tracking) junior school children at age 7 into classes delineated by average ability.

JEL Classification: I21, I24, I28

Keywords: 1944 Education Act, date of birth, selective schooling, class streaming

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1 Introduction

Research into links between date of birth and performance in cognitive tests has a long history, stretching back to the first half of the last century. An important aspect of this work has concerned children's school and further education attainments. A recurring finding is that, on average, older pupils outperform their younger counterparts.[1] One popular focus has involved research into relationships between date of birth and the subsequent moves from primary school education to secondary school education. We examine the effects of date of birth on state selective education using the 1944 Education Act in England and Wales as a natural experiment.[2] We compare the probabilities of gaining selective school entry – which in our study period meant attending a grammar school – before and after the Act using a difference-in-difference approach.

In important respects, the pre- and post-WWII eras shared early-schooling features that would be expected to disadvantage younger children. The school year ran from September to August and the school starting age was 5. Typically in smaller primary schools, all children commenced their education at the start of the school year in which their 5th birthday took place. In this case, the average cognitive development of children born towards the end of the school year was considerably lower than those born near the start. In larger schools, children commonly experienced staggered entry; their education started in the school term during which they turned 5. Children starting in January or after Easter, were not

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[1] See, for example, Crawford et al. (2007), Crawford et al.(2013 and 2014), Robertson (2011), Black et al. (2011), Mühlenweg and Puhani (2010), Smith (2009), McEwan and Shapiro (2008), Bedard and Dhuey (2006), Puhani and Weber (2005), Glewwe and Jacoby (1995), Sharp (1995), Borg and Falzon (1995), Bell and Daniels (1990).

[2] A related body of research looks at fairness of selective schools more generally (for recent papers see, e.g., Atkinson et al., 2006; Burgess et al., 2014; Hart et al., 2016). Another strand of the literature studies whether selective schools improve academic performance (see, e.g., Clark, 2010 ; Dustmann et al. 2014; Abdulkadiroglu et al. 2012 and Guyon et al. 2012), and labour market outcomes (see, e.g., Clark and Del Bono 2014).

compensated for their missed education. Since all children by year cohort moved from primary to junior school on the same date and since all children were considered for selective schooling at the same time, predominantly at age 11, these early starting disadvantages remained influential. There was one additional school organisational feature, that had detrimental age effects on the educational progress of younger children. It was common in the larger primary schools to stream (track) classes by average ability and there was a higher incidence of younger children misplaced into the lower-ability classes (Barker Lunn, 1970).

There were two important differences between the two eras. First, competition for grammar school places greatly increased post-1944 due to the fact that an 11+ competitive entry exam was compulsorily taken by all children in their final junior school year. In contrast, the allocation of equivalent school places pre-1944 was predicated on a mix of non-competitive fee-based entry and competition for a limited supply of free grammar school places. Post-war competition was further increased by the introduction under the 1944 Education Act of free universal secondary education. This enabled a higher proportion of families financially to afford the additional years of schooling required by selective compared to nonselective schools. Second, in the post- 1944 period there was a growing awareness that younger primary school children might face school learning and performance disadvantages relative to their older contemporaries (Plowden, 1967). This recognition translated into the common practice within Local Education Authorities (LEAs) of standardising test scores in the 11+ exam with built-in age allowances. Score standardisation was especially facilitated by the comprehensive coverage of the 11+ exam.

Would these post-1944 educational changes be expected to improve or to worsen the chance of younger children attending selective school? We might expect that the net effects would be postive. *A priori*, it is difficult to make a judgement as to whether imposing comprehensive testing at age 11 improved or worsened younger children’s entry prospects.

Marginally, prospects may have worsened since in pre-war years better-off families could bypass exam entry by paying fees. However, the ability of LEAs to age-adjust test scores across all children would be expected to have significantly helped towards the goal of equalizing entry prospects by age. While we find support for significant net improvements among children born in the middle four months of the school year, the youngest, so-called summer children, remained stubbornly disadvantaged both pre- and post-1944. We argue that the practice of class streaming, common to both eras, was a major factor that lay behind the lack of improvement.

2 Background

TABLE 1 NEAR HERE

Both before and after the 1944 Education Act, state primary and secondary schools catered for the vast majority of children. As summarized in Table 1, there were important similarities between the state school systems in the two time periods.[3] Children started primary school at the age of 5. While there were many exceptions, staggered entry applied to most children. Entry took place at the start of the three school terms during which they turned 5, that is during September to December, January to April, or May to August.[4] Primary school was subdivided into infant school and junior school. At the end of infant school, each year-cohort of children entered junior school at the same time which meant that the oldest-children typically enjoyed 3 years in infant school and the youngest just 2 years and one

[3] See Hart et al. (2016) for more detailed comparisons.

[4] Exceptions typically occurred in smaller primary schools in which one entry date for all children reaching five in a school year was more common. Also, for example, younger children with working mothers were often allowed to start school at the start of the school year irrespective of date of birth.

term.[5] This length-of-schooling discrepancy persisted to the end primary education, usually at age 11, when each year cohort proceeded *en bloc* to secondary school.

In both eras it was common practice in larger junior schools to stream classes by ability. Typically, A, B, C (...up to D and E in the largest schools) classes were created in descending order of average ability. Children were streamed, principally, on the basis of either an assessment carried out in their infant school or through headmaster-approved attainment tests at the start of junior school.[6] Lower average ability-rated class streams correlated positively with the average ages of class pupils and with their average number of terms of infant-level schooling (Barker Lunn, 1970, Tables 7.2 and 7.3).[7] Since teachers tailored their lessons and material to accord with expectations of ability, streaming was thought especially to disadvantage potentially able summer children who were misallocated to the lower streams at age 7. Most class misplacements were not subsequently corrected and this tended to result in deteriorating academic performances (Barker Lunn, 1970).

Most children attending grammar schools, in both the pre- and post-war periods, sat nationally-recognised examinations at age 16 and sub-sets of these sat more advanced exams at age 17/18. Nonselective elementary schools pre-1944 and nonselective modern schools

[5] The educational disadvantages of the summer children often stretched beyond this delayed entry effect, deriving from the fact that in their first school year they attended school only during the Easter term. First, Plowden (1967) reports that in many schools there was spare classroom capacity in the first term of the school year turning into pressure on capacity by the last term that resulted in poorer quality classroom experience due to larger class sizes (see also Williams, 1964). Second, the Easter term averaged only 9 weeks of teaching before the summer holiday, being broken up by the Whitsun holiday and various pre-summer school activities, including prize days, open days and sports days.

[6] For full details of streaming selection methods based on large contemporary samples, see Jackson (1964, p.18, Table 5) and Barker Lunn (1970, p.86, Table 7.4).

[7] Campbell (2013) provides recent evidence based on the Millenium Cohort Study for England. She finds that 7 year olds in 2008 who were born in September were more than twice as likely to be placed in the highest class streams compared to their counterparts born in August.

post-1944 accounted for the great majority of state school children. With few exceptions, neither provided state-recognised national qualifications. Virtually all children attending nonselective schools finished their school education at the minimum school leaving age (14 years pre-1944, 15 years post-1944). Transfers between selective and nonselective schools were rare.

There were three major differences between the two eras. First, entry to selective schools pre-1944 was not comprehensively subject to formal testing in contrast to the universally applied post-1944 11+ exam. Prior the 1944 Education Act, it was not until 1933 that serious attempts were made to introduce testing as a means of matching student abilities with the academic demands of a grammar school education. Such entry, following the introduction of so-called special places in 1933, was based on a written entrance exam taken at primary school, at age 11 or 12. In 1933, 52% of secondary grammar school places were allocated in this way and this rose to 69% by 1938 (see Floud, 1954, Appendix 2, Table 2). So even at a late stage, about one-third of pre-war entry was non-competitive. Second, in contrast to universally free secondary school education in the post-war state system, pre-war secondary education beyond the minimum school leaving age was either fee-paying or free. About one-third of children were exempt from fees in the 1920s; by 1932 free places had risen to 48% of children, a percentage that remained more or less constant for the remainder of the decade. Obtaining free entry was predicated on a competitive 11+ exam which was open to all children, including those from higher-income families. Third, the fact that all post-war children across broad LEA districts were required to take an IQ-based exam at the same time during their last year in primary school served to facilitate the use of age standardisation of 11+ test scores.

3 Data

We base our empirical work on the British Household Panel Survey (BHPS). The BHPS is a nationally representative longitudinal survey covering approximately 5,500 households, corresponding to roughly 10,000 individuals, each year from 1991 to 2008. Specifically, we concentrate on individuals born in England and Wales during the period 1915 to 1953 who attended school during the years 1926 to 1964 and for whom the BHPS includes a rich set of information regarding gender, month and year of birth, type of school attended and parental background. Depending on the econometric specification adopted, our usable samples are composed of 2,600-2,900 individuals. Our subsequent estimates are split between those born before 1933, who attended secondary school before the 1944 Education Act came into effect, and those born in or after 1933. We also separate out a sub-sample of those born in or after 1937; their secondary school attendance was subject to the tripartite system but started after an initial and disruptive 4-year transition period. [8]

TABLE 2 NEAR HERE

Table 2 shows the percentage of pupils attending different types of secondary schools by year of birth. We note the distinction of selective schools between state grammar schools and fee-paying grammar schools. Fees were paid in respect 22% of all children attending grammar schools in our pre-1933 sample. This compared to only 5% of those born in 1933

[8] The process of establishing the new tripartite secondary school system required, especially, an expanded provision of nonselective modern schools. This involved new school building, refurbishment of former elementary schools, training and recruitment of new teachers, and setting up of administrative systems. In fact, an extension of the minimum school leaving age (from 14 to 15) under the Act was postponed from April 1945 to April 1947 because of an initial shortfall of an estimated 200,000 school places and 13,000 teachers (see Cabinet Paper, *Raising the School Leaving Age*, National Archives CAB 129/1/117). Up to 1950, about one-third of state secondary school children attended selective grammar schools falling to about one-quarter by the mid-1950s. Most grammar schools already existed in the early post-war period while new school building and reorganisation were required in respect of nonselective modern schools (Bolton, 2015).

or later. We know that significant numbers of fee-payers born before 1933 gained their secondary school places non-competitively.[9] For these, it is likely that date of birth was independent of grammar school entry. We wish to test for this but, given considerably fewer fee payers in the post-1944 era, we undertake estimation by distinguishing between non-fee payers separately and non-fee and fee payers taken together.

Among those born before 1933, there are relatively large numbers of individuals who claimed to have attended secondary modern schools rather than their pre-1944 equivalent, elementary schools. There are two likely explanations of this. First, there are individuals who started secondary school before and left school after the introduction of the 1944 Act. In many cases, their initial elementary school would have been re-named as a secondary modern school. There are others who, when covered by the BHPS questionnaire, would have named their elementary school by its subsequent classification, especially if it occupied the site of their original school. In any event, elementary and modern schools are equivalent for our purposes and make no difference to the analysis. We also have a few cases of elementary schools appearing in the later periods and, again, this is not problematic for our framework.

One of the BHPS secondary school classifications is somewhat problematic. While comprehensive schools were eventually to dominate nonselective secondary school education in England and Wales, following a significant move away from the tripartite system in 1965, their initial progress was slow. At the outset, they were regarded as an experimental alternative to tripartite schools, modelled more towards grammar school provision though catering for a much wider range of ability. There were 13 comprehensive schools in 1953 rising to 195 in 1964. They account for about 13% of pupils in our post-1944 BHPS secondary school samples while we know that at their 1964 peak in our time frame they

[9] Before 1944, able children from relatively well-off families could gain free places since competition via an 11+ exam was open to all.

actually comprised only 7% of pupils in the entire state sector (Mitchell, 1988). We also note that 3% of those born pre-1933 reported that they attended comprehensive school when, at least at the commencement of their secondary education, this was not possible. One explanation for these apparent anomalies is that some individuals who attended a modern or an elementary school that subsequently was turned into a comprehensive school – either during their school days or at a later time – may have reported the new school classification when responding to the BHPS survey. With an eye on robustness, we deal with this uncertainty by reporting results that exclude and include the reported comprehensives.

TABLE 3 NEAR HERE

We focus on the comparative probabilities of gaining selective school entry by age within given year cohorts before and after the 1944 Act Age is measured as whether born August to December (term 1), January to April (term 2), May to August (term 3). From the raw data, Table 3 shows that the selective secondary school system in the later era catered for larger percentages of all children. Also, the oldest children pre- 1944 enjoyed a much higher share of selective school places than their younger counterparts. This relative advantage appears to have been significantly eroded post- 1944.

4. Estimation

Here, we test whether or not the design and application of the 11+ exam introduced in 1944 impacted on the attainments of younger primary school children relative to their counterparts in the pre-1944 system. Children's ages are grouped into those born during September to December, January to April, and May to August. These periods correspond, respectively, to annual school terms 1, 2, and 3. We outline here a linear probability model.[10]

[10] We show the design and results for the equivalent probit model in the Appendix. The results do not differ quantitatively from the OLS estimates of the model here.

As reported in footnote 8, the first 4 years of the tripartite system, 1944 to 1947 involved a disruptive transition period involving an extensive school building and refurbishment program as well as a large scale recruitment of teachers and administrators. In what follows, therefore, we show results both including and excluding the transition years. In the former case, the first age cohort to be affected by the post-war 11+ exam would have been born in 1933. In the latter case, the first cohort would have been born in 1937.[11]

Let $S_i = 1$ if individual *i* went to a selective school and $S_i = 0$ otherwise. Then, setting the oldest year group (*term 1*) as the reference group, the difference-in-difference model of the probability of attending a selective school by age is expressed

$$(1)\ S_i = a_0(post_i) + a_1(term\ 2)_i + a_2(term\ 3)_i + a_3(post_i{*}term\ 2)_i + a_4(post_i{*}term\ 3)_i + \boldsymbol{\theta} Z_i + \lambda_t + e_i$$

where $post_i$ is a dummy taking the value 1 if the individual's birth year is 1933 (1937 omitting the transition period) or later, Z_i is a set of additional controls, and λ_t is a set of year of birth fixed effects. The controls included in Z_i are gender and separate dummy variables indicating whether mother or father has no qualifications, some qualifications or high qualifications.[12]

[11] We extended the transition period up to 7 years, considering the first post-transition cohorts as being born in 1939 or 1940, with no significant effects on our reported results.

[12] Difference-in-differences is the most appropriate method in this context as the objective is to compare the relative chances of attending grammar school among seasons of birth/terms. Estimating equation (1) provides directly the statistical test and the correct standard errors in a parsimonious way. Alternative methods -- such as regression discontinuity -- would have required splitting the sample across seasons of births – losing power as a result – and computing ad hoc tests and standard errors.

5. Findings

Table 4 shows results with respect to equation (1), the probability of going to a selective school. In the first three columns, we separate the pre- and post-1944 cohorts by those born during or after 1933 and therefore eligible to be part of the tripartite system from the outset. The second three columns, omit those born in the years 1933 to 1936 who would have started in tripartite education school during the disruptive transition period. Columns (1) and (4) present regression results that exclude both fee-paying grammar schools and comprehensive schools.[13] Columns (2) and (5) continue to omit fee-paying selective schools but include comprehensive schools. Columns (3) and (6) exclude comprehensives and include fee paying selective schools.

TABLE 4 NEAR HERE

Across all reported regressions in Table 4, the significant positive coefficients on the variable *post* in row 3 reflect that, on average, the probability of gaining a selective school place post-1944 was higher than pre-1944 for the youngest children (see Table 3). Rows 1 and 2 indicate that, relative to the oldest children in each year group pre-1944, younger children had significantly lower probabilities of attending a selective school. In general, estimates are consistent across all three regression specifications as well as in the regressions including and omitting the transition period.

The relative probabilities of attending a selective school for a large section of the post-war children are found to be significantly different. When we interact the *term 2* and *post* dummies we obtain positive and significant coefficients across all regression specifications. In fact their magnitudes are such as to completely offset the equivalent

[13] In other words, this specification excludes non-competitive fee payers and circumvents the problem of misreporting on comprehensive attendance.

negative coefficients in row 1 belonging to their pre-war counterparts. In other words, the probability of gaining a selective school place for post-war children who started school in the New Year is found to be the same as those starting in September. This is not the case, however, for the youngest cohorts who typically started school in the summer term. They display no significant differences from the negative coefficients of their pre-war counterparts in row 2.[14] We leave discussion of this result to Section 7.

6. An identification issue

Our results indicate that the Butler reform improved the secondary school prospects of those born between January and April but did not significantly alter the prospects for summer children. In order to attribute the change to the reform itself, we need to rule out the possibility of other factors that might have intervened in the period before the Butler Act. A simple way to test this is to contrast pre-trends in selective school attendance.

FIGURE 1 HERE

Figure 1 compares trends of pupils born in the three seasons in each year before the Butler Act.[15] The number of pupils attending selective schools is increasing in each year and for each season of birth. What is reassuring is that there is no evidence of any divergence in trends before the reform.

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[14] We tested the hypothesis that the coefficients for (Term 2)(Post) equalled those of (Term 3)(Post). At the 5 percent level, this is not supported by the F-tests for columns (1) and (3)-(6). The p-value for column (2) is 0.06.

[15] Figure 1 is a binned scatter plot providing a non-parametric visualisation of the relationship between "going to grammar school" and year of birth across different seasons of births. Each dot is the mean value of the variable attending grammar school associated with each year of birth. A linear fit is then estimated and plotted on top the scatter points. These graphs were obtained using -binscatter- by Michael Stepner in Stata (https://michaelstepner.com/binscatter/). Using 3-year moving averages to count the number of pupils attending grammar schools reveal the same basic trends. In both Figure 1 and its related Table 5 we restrict our sample observations to individuals born before 1930 to exclude the confounding effects of those who turned 11 at the outset of WWII.

TABLE 5 NEAR HERE

More formal placebo tests are provided in Table 5. We introduce placebo reform dummies that turn on at a given year prior to the actual reform. The interaction between these placebo dummies and the season of birth would detect differences in chances to attend grammar school before the reform. More precisely, we run the specification described by model (1) in which *post* takes the value of 1 in a year before the actual reform. For robustness purposes, separate placebo difference-in-differences are run. For instance, the first panel in Table 5 shows the coefficient on interactions between a placebo dummy turned on in 1921 (i.e., 12 years prior to the actual reform), the second panel when the placebo dummy is turned on in 1922 (i.e., 11 years prior to the actual reform), and so on. A positive and statistically significant interaction with any placebo dummy would indicate the presence of trends predating the Butler Act. The vast majority of the interaction effects are not statistically distinguishable from zero. When they are statistically significant they are so at 10% level and the estimates have opposite sign (negative instead of positive). This confirms our identification assumption.

7. Junior school class streaming and summer children

One major clue to our finding that summer children in the post-war period continued to be as disadvantaged as their pre-war counterparts relates to the common practice of streaming children in junior schools, between the ages of 7 and 11, into classes delineated by estimated ability. Streaming did not generally involve systematic allowances for age differences.[16] Given their less mature cognitive development, younger children were disproportionately represented in the lower ability streams. To the extent that this produced

[16] Less than one quarter of schools using streaming took first year pupils' ages into account (Barker Lunn 1970, pp.85/6 and Table 7.4).

feelings of failure combined with mismatched teaching provision relative to ability then the performances of potentially able younger children may have been long-term damaged.[17] Similar evidence of adverse streaming (or tracking) outcomes among younger children during early school years is not confined to our study period.[18]

By the mid-to-late 1960s, it was widely recognised that class streaming was producing longer term detrimental educational outcomes among some of the more able younger pupils who were demoralised by misplacement into lower class streams (Plowden, 1967, Baker Lunn, 1970; Chapter 10; Galton et al., 1980, p.39). It is likely that this problem could not have been fully countered simply by age-adjusting test scores at age 11, i.e. when exam-based decisions on eligibility for selective schooling were carried out. It would prove difficult to differentiate in the lower-streamed classes between demotivated able children and children whose class allocation was a good indicator of longer-term expected attainment levels. In fact, there is strong evidence that most of the class misplacement of children in their early junior school years was not subsequently corrected.[19]

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[17] Slavin (1987) highlights potential problems within classes composed of low achievers who are "…*deprived of the example and stimulation provided by high achievers, and the fact of being labeled and assigned to a low group is held to communicate low expectations for students which may be self-fulfilling*" (p.296). In other words, some able younger students may be misplaced into lower ability classes and subsequent losses of self-esteem may translate into serious long term short-falls in actual compared to potential educational outcomes.

[18] Schneeweis and Zweimüller (2014) report on Austrian children born in the 1970s to 1990s who faced the choice at 10 between attending an academic or vocational stream. Younger children within year cohorts are found to be 40% less likely to choose the academic route relative to their oldest peers.

[19] Barker Lunn's (1970) research is based on a stratified random sample of 2,000 junior schools carried out in 1963 together with 1964 cross-sectional and longitudinal cohort studies of pupils during all four years of junior school attendances. There were significantly higher chances of being allocated to the top A-stream in junior school among children (a) who had attended infant school for the maximum number of terms, and (b) who were born during the period September to December. These advantages were accentuated in schools with more than two class streams per pupil age cohort. Over their four junior school years,

Two contemporary reports looked into the effects of streaming. One study examined the 11+ performances of 1,315 children in the streamed junior schools of an English borough (Jinks, 1964). These children started school in 1956-57. The other was carried out by the Chief Inspector of Schools in the English county of Durham and involved age distributions and test scores of 7,000 junior school children (Nightingale, 1962). It analysed junior school streaming by age, head teachers systematic assessments of pupils' suitability for selective school by age, and performances in 11+ arithmetic and English tests by age. Both studies draw attention to the age-standardisation of test-score marking.

TABLE 6 NEAR HERE

We summarise several key findings in Table 6. From column (1), of children placed in junior school top A-stream classes in Jinks' case study, 39% were September-December children, 35% January-April, and only 26% May-August. Subsequent age-adjustments of 11+ test scores clearly failed fully to correct the age disadvantage of the summer children. From column (3), Jinks found that 38% from the oldest age group, 33% from the middle group, and 29% from the youngest group of children gained selective grammar school places as a result of their performances in 11+ tests,. *"As these standardised tests have a built-in system of age allowances, differences between the youngest and the oldest in the age groups should be ironed out. However, if we study the dates of birth of children entering the Grammar School we see that this is not the case"* (Jinks, 1964).

(unstandardised) class test scores in English and arithmetic revealed that 13% of children were in the wrong stream at the end of the first two years and 18% at the end of the third year. Only 36% of children found to be in the wrong first year stream were corrected (i.e. demoted or promoted), only 22% in the second year, and only 14% in the third year. On average over all years, three-quarters of children found to be in the wrong stream remained in the wrong stream. Through time it was found that able children who remained in lower streams exhibited deteriorating academic performances. his contrasted with improved performances among less able children who were misallocated but remained in high streams (see also Douglas, 1964).

In column (2), Nightingale's study shows very similar placements into junior school A-streams at junior level: 40% composed of the oldest age group, 35% the middle group and only 25% the youngest group. The 11+ exam was in two parts. Part 1 consisted of unstandardised tests in arithmetic and English. Children above a cut-off test-score were allowed to proceed to part 2 and take additional exams in these two subjects. Part 2 tests were now standardised to allow for age differences. Of all children proceeding to part 2, only 25% of summer children gained a raw score of at least 170. Of all those with marks under 170, the summer children comprised 49%.

We report in columns (4) and (5) of Table 6 our predicted probabilities of gaining grammar school places post-1944, based on our probit regressions reported in the Appendix. The columns show, respectively, estimates with and without the inclusion of the transition period. We use the samples that exclude grammar school entry with fees and comprehensive schools.[20] In line with our Table 4 findings, and in contrast to Jinks' column (3), we find no differences between the oldest and the middle-aged groups of children in gaining a selective place. However, in line with Jinks' column (3), summer children experience significantly poorer outcomes. Our findings of disadvantaged summer children are in line with the evidence of both Jinks and Nightingale.

Jinks suggests that the attitude of junior school teachers may have reinforced a strong partition in outcomes between the streams, with A-classes 'stretched' and C-classes "taught down to". Both Jinks and Nightingale emphasise the damage inflicted by psychological feelings of failure. Nightingale suggests that such reactions by the youngest children may place them beyond the help afforded by standardised age adjustment of test scores. "*Some*

[20] Comparing columns (4) and (5) the probabilities of all age groups attending selective school are found to be higher when the transition period is included. As reported in footnote 8, this is largely influenced by the higher selective/nonselective ratio of places immediately after the war while the expansion of modern schools was taking place.

head teachers set attainment tests during the first term of the Junior Course. Here, the May-August pupils were immediately disadvantaged and found themselves in "B" "C" or "D" streams alongside older pupils who had completed the Infants Course but had not innate intelligence to assimilate it. Soon, a "C" stream complex was developed, the result of which cannot be eradicated by tinkering with age allowances four years later" (Nightingale, 1962). Our results are consistent with this latter statement. So, we turn to investigate further implications of class streaming for our analysis.

8 Geographical location, gender, and class streaming

TABLE 7 NEAR HERE

Only larger primary schools could undertake class streaming given that this practice was predicated on annual pupil intakes requiring at least two classes per age cohort.[21] While age disadvantages related to cognitive development among younger school children occurred in both streamed and non-streamed school environments, the former involved the additional disadvantages resulting from a misplacement of able young pupils into lower class streams. While the BHPS does not provide direct information on class streaming, it does offer a potentially interesting indirect insight into class streaming effects. As shown in Table 7, BHPS data are broken down into different kinds of geographical locations. Two of these, villages and rural/countryside locations, are substantially more likely to be dominated by

[21] As reported by Jackson (1964), there were 23,191 primary schools in England and Wales in 1962. He assumed that a school needed to have at least 300 children between 7 and 11 to allow class streaming. There were 2892 such schools, or 12.5% of the total. In a questionnaire survey, he attempted to sample one in three of these larger schools ending up with a sample of 660 schools. Of these, 96% practised class streaming. In England in 1965, 20% of 20,789 primary schools contained over 300 pupils, with 7% over 400 pupils (Plowden, 1967, Table 14, para. 460).

small non-streamed primary schools with one class intake per year[22], compared to the more populated inner city, suburban and town locations.[23] Accordingly, we extended the regression model in (1) by adding a triple interaction term that indicated whether an individual lived in an inner city, suburb or town (which we label CST) compared to a rural/countryside area or a village.

TABLE 8 NEAR HERE

Results of the post-1944 probabilities of attending a selective school after allowing for locational differences are shown in the top section of results in Table 8.[24] For simplicity of exposition, we report only on the coefficients of the triple difference. Negative estimated coefficients indicate that the chances of attending a selective school were relatively poorer in the post-1944 era for younger children (born during the months of term 2 and term 3) who were largely brought up in a city, suburban or town location compared to rural/village locations. While these findings are in line with adverse effects of class streaming in larger primary schools, we cannot rule out other influences. For example, smaller school environments in less densely populated locations may have enjoyed better staff/student ratios as well as much less recourse to staggered school entry in the first year of school life.

TABLE 9 NEAR HERE

We can, however, provide an interesting robustness check in support of the streaming hypothesis. Boys were significantly more likely than girls to be adversely affected by class

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[22] Plowden (1967, Chapter 14) discusses rural and village primary education in the early post-war era.

[23] Substantial post-war construction of larger primary schools greatly increased the incidence of multiple class yearly intakes. For example, there was significant post-war building of large free-standing primary schools in city suburban areas (Jackson, 1964, Appendix 2).

[24] We omit the category, ‘mixed/moved around’ in Table 7 from these regressions.

streaming in junior schools. Between the ages of 7 and 10 boys were considerably less likely to be placed in A-streamed classes than girls. Evidence provided by Barker Lunn (1970) for the year 1964 is provided in Table 9 in respect of 2- streamed and 3- or 4- streamed schools.[25] In our difference-in-difference estimates reported in Table 4, we found no statistical differences between boys and girls in gaining selective school entry. These gender results are reported in the middle section of results in Table 8. Do we obtain different outcomes when we add the male/female distinction to our triple differences specification that separates more populated and less populated locations? Results of these quadruple differences are shown in the bottom section of Table 8 results. Negative effects for young male pupils in city/town/suburb areas are now accentuated, including large negative coefficients among male summer children.

9 Conclusions

Pre-1944, junior school children born after December exhibited significantly lower probabilities of attending selective grammar schools compared to children born during the first four months of the school year. Post- 1944, children born during January to April had equal probability of attending grammar school compared to the September-December cohort. In contrast, there is no evidence of improvement among post-war summer children born during May to August. They experienced significantly lower selective school selection probabilities in both pre- and post- 1944 eras. These results are robust under different models, either OLS or ordered probit, and different specifications and definitions of the sample. By

[25] Class streaming was principally based on assessments of performances English and arithmetic. Barker Lunn (1970, Chapter 7) provides evidence that girls tended to be better at English and boys better at arithmetic. Many students are not equally good in both subjects. It is suggested that the preponderance of girls in A-streams may have resulted from school assessments of ability placing more weight on English comprehension and proper usage than on arithmetic.

comparing trends in school attendance before the introduction of the 1944 reform, we can also rule out the presence of unobservable factors that could be driving the results.

The 1944 reform made salient the need to account for age differences at the time of the test. Test-score age adjustments may explain the relative improvement for children born during January to April. We conjecture that the practice of class streaming by ability, generally without taking age into account, lay behind persistent poorer selection outcomes for summer children. The contemporary literature reveals that many able younger children were misplaced into low ability classes at age 7 and that three-quarters of these were not subsequently up-graded. This resulted in a widening gap between potential and actual junior school performances, as evidenced by the classwork of misplaced able children gradually converging to their lower class norm.

In order to separate areas of high and low incidences of class streaming, we take advantage of the fact that streaming can only take place within large primary schools in the vicinity of relatively densely populated catchment areas. Hence, streaming is considerably more likely among children living in inner cities, suburban areas and towns compared to villages and rural areas. Using this distinction we show that post-war younger children displayed lower chances of gaining selective school entry in the former areas compared with the latter. That this observation is, at least in part, the result of class streaming is reinforced by the added finding that younger boys in the more populated locations fared worse than girls in gaining grammar school places. This is in line with strong contemporary evidence of the relative gender effects of class streaming.

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Table 1 Comparisons of state primary and secondary education in England and Wales before and after the 1944 Education Act

	Before 1944 Act	After 1944 Act
Statutory school starting age	5 years	5 years
Primary school terms	3 terms starting September, January, after Easter	3 terms starting September, January, after Easter
Primary school entry	At start of the school term during which reached age of 5 (many exceptions)	At start of the school term during which reached age of 5 (many exceptions)
Class streaming by average ability	Common in large primary schools (ages 7 to 11)	Common in large primary schools (ages 7 to 11) Greater prevalence due to construction of new large free-standing primary schools
Age starting secondary school	11/12 years	11/12 years
Dominant state selective secondary schools	Secondary (grammar) schools	Grammar schools
Dominant state nonselective secondary schools	(Senior departments of) elementary schools	Modern schools
Method of selection to a grammar school education	Mix of fee-paying non-competitive entry and competitive exam selection	Competitive exam selection
Age-adjusted test score at age 11	Perhaps used in some cases in relation to gaining free-grammar school places	More common due to the prevalence of standardised IQ testing of all children at age 11
Secondary school fees	Full-fees, partial-fees, and free places depending on parental circumstances	No fees
Minimum school leaving age	14 years	15 years
National school examinations	General School Certificate (age 16) and Higher School Certificate (age 17/18)	General Certificate of Education at Ordinary Level (age 16) and Advanced Level (17/18)

Table 2 Secondary school attendance by births before and after 1933 (BHPS)

Born	< 1933		≥ 1933		≥ 1937	
Secondary schools	No.	Col %	No.	Col. %	No.	Col. %
Grammar	161	17.9	553	26.5	472	26
Grammar with fees	45	5	31	1.5	26	1.4
Private	40	4.5	110	5.3	93	5.1
Elementary	288	32.1	40	1.9	16	0.9
Modern	215	23.9	937	44.9	828	45.7
Technical	33	3.7	94	4.5	77	4.2
Comprehensive	30	3.3	264	12.6	252	13.9
Other	86	9.6	58	2.8	49	2.7
Total	898	100	2087	100	1813	100

Table 3 Percentages of total BHPS samples who attended selective secondary school by season of birth

Born	< 1933	≥ 1933
	Percentage of total sample attending selective school	
Sept- Dec (term 1)	28	38
Jan-Apr (term 2)	19	37
May-Aug (term 3)	22	34

Note: This is based on the same data sample as columns (1) and (4) of Table 4. Thus, it excludes both fee-paying schools and comprehensive schools.

Table 4 Date of birth and probability of attending selective school

Born	≥ 1933			≥ 1937		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Term 2	-0.100**	-0.093*	-0.096*	-0.100**	-0.093*	-0.096*
	(0.047)	(0.047)	(0.049)	(0.047)	(0.047)	(0.049)
Term 3	-0.059**	-0.053**	-0.043*	-0.059**	-0.053**	-0.043*
	(0.025)	(0.025)	(0.022)	(0.025)	(0.025)	(0.022)
Post	0.193***	0.071***	0.147***	0.183***	0.064**	0.137***
	(0.027)	(0.025)	(0.027)	(0.027)	(0.025)	(0.028)
(Term 2) (Post)	0.092*	0.105**	0.083	0.108**	0.119**	0.098*
	(0.053)	(0.052)	(0.055)	(0.054)	(0.053)	(0.056)
(Term 3 (Post)	0.007	0.028	-0.016	0.017	0.035	-0.005
	(0.039)	(0.036)	(0.036)	(0.041)	(0.037)	(0.038)
Observations	2,615	2,909	2,691	2,358	2,640	2,429
R-squared	0.067	0.058	0.062	0.067	0.056	0.062
Gender	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth	Yes	Yes	Yes	Yes	Yes	Yes
Parental education	Yes	Yes	Yes	Yes	Yes	Yes
Comprehensive schools	No	Yes	No	No	Yes	No
Grammar with fees	No	No	Yes	No	No	Yes

Note: Every column shows estimated coefficients of separate linear probability models of going to a selective school on seasons of birth before and after the 1944 Education Act**.** *Term 2* and *Term 3* refer, respectively, to children born during January to April and during May to August (‘summer children’). *Term 1* is the reference group of oldest children born during September to December. *Post* is a dummy indicating whether the individual is born after the year 1933 (first three columns) or after 1937 (last three columns), and thus affected by the Education Act. Each regression includes year of birth fixed effects and gender. Standard errors in parenthesis are clustered at respondents’ year of birth. *** p<0.01,** p<0.05, * p<0.1.

Table 5 Placebo estimates of the probability of attending selective school prior to the reform

	(1)	(2)	(3)
(Term 2) (Post 1921)	-0.056	-0.030	-0.109
	(0.060)	(0.062)	(0.077)
(Term 3) (Post 1921)	-0.013	0.008	0.021
	(0.028)	(0.025)	(0.045)
(Term 2) (Post 1922)	-0.047	-0.023	-0.071
	(0.052)	(0.053)	(0.070)
(Term 3)(Post 1922)	-0.051	-0.031	-0.004
	(0.037)	(0.034)	(0.041)
(Term 2)(Post 1923)	-0.048	-0.024	-0.079
	(0.046)	(0.047)	(0.060)
(Term 3)(Post 1923)	-0.030	-0.009	-0.002
	(0.038)	(0.036)	(0.037)
(Term 2)(Post 1924)	-0.069	-0.041	-0.092*
	(0.045)	(0.043)	(0.053)
(Term 3)(Post 1924)	-0.033	-0.009	-0.003
	(0.035)	(0.033)	(0.034)
(Term 2)(Post 1925)	-0.043	-0.024	-0.069
	(0.045)	(0.041)	(0.050)
(Term 3)(Post 1925)	-0.039	-0.021	-0.033
	(0.033)	(0.031)	(0.039)
(Term 2)(Post 1926)	-0.059	-0.040	-0.082*
	(0.044)	(0.040)	(0.047)
(Term 3)(Post 1926)	-0.063*	-0.045	-0.055
	(0.035)	(0.034)	(0.039)
(Term 2)(Post 1927)	-0.038	-0.026	-0.058
	(0.043)	(0.039)	(0.047)
(Term 3)(Post 1927)	-0.065*	-0.048	-0.053
	(0.034)	(0.032)	(0.037)
(Term 2)(Post 1928)	-0.070	-0.055	-0.081*
	(0.048)	(0.044)	(0.047)
(Term 3)(Post 1928)	-0.070**	-0.050	-0.062*
	(0.033)	(0.031)	(0.036)
(Term 2)(Post 1929)	-0.030	-0.014	-0.038
	(0.054)	(0.051)	(0.055)
(Term 3)(Post 1929)	-0.050	-0.025	-0.053
	(0.037)	(0.036)	(0.035)
(Term 2)(Post 1930)	0.002	0.015	-0.012
	(0.055)	(0.052)	(0.054)
(Term 3)(Post 1930)	-0.046	-0.022	-0.049
	(0.036)	(0.034)	(0.035)

Table 5 continued on the next page

Table 5 continued from the last page			
(Term 2)(Post 1931)	0.042	0.056	0.039
	(0.056)	(0.054)	(0.059)
(Term 3)(Post 1931)	-0.029	-0.005	-0.025
	(0.038)	(0.036)	(0.038)
(Term 2)(Post 1932)	0.075	0.088	0.071
	(0.054)	(0.053)	(0.057)
(Term 3)(Post 1932)	-0.005	0.019	-0.010
	(0.040)	(0.038)	(0.038)
Observations	2,615	2,909	2,691
Gender	Yes	Yes	Yes
Year of birth	Yes	Yes	Yes
Parental education	Yes	Yes	Yes
Comprehensive schools	No	Yes	No
Grammar with fees	No	No	Yes

Note: Each panel shows estimates from separate regressions of going to grammar school on season of birth interacted with a placebo dummy turned on few years before the actual reform (born post 1921, post 1922, etc.). *Term 2* and *Term 3* refer, respectively, to children born during January to April and during May to August ('summer children'). *Term 1* is the reference group of oldest children born during September to December. Each regression includes year of birth fixed effects and gender. Standard errors in parenthesis are clustered at respondents' year of birth. *** p<0.01,** p<0.05, * p<0.1.

Table 6 Pupils in A-streamed classes and in selective secondary schools in the post-1944 era by season of birth

	Shares of A-stream junior school class places (% of total)		Attending selective school (% of each term)	Predictive probabilities of attending selective school (%) – Probit regressions (Table A1)	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	Jinks	**Nightingale**	**Jinks**	**≥ 1933 births**	**≥ 1937 births**
Sept. – Dec. (term 1)	39	40	38	36	34
Jan. – Apr. (term 2)	35	35	33	36	34
May – Aug. (term 3)	26	25	29	32	30
Sample sizes	536	3174	317	Using sample data reported for columns (1) and (4) in Table A1, with 2615 and 2358 respective observations.	

Table 7 Locations of individuals in the BHPS sample

Type of area mostly lived in when young	Frequency	Percent
Inner city	379	10.95
Suburban area	835	24.13
Town	829	23.96
Village	884	25.55
Rural or countryside	416	12.02
Mixed/moved around	117	3.30
Total	3,460	100.00

Table 8 Date of birth and probability of attending selective school: location and gender

Born	≥ 1933			≥ 1937		
	(1)	(2)	(3)	(4)	(5)	(6)
(Term 2) (Post) (CST)	-0.172*	-0.102	-0.227**	-0.148	-0.074	-0.200**
	(0.088)	(0.090)	(0.087)	(0.095)	(0.095)	(0.094)
(Term 3) (Post) (CST)	-0.135	-0.077	-0.177*	-0.092	-0.033	-0.134
	(0.092)	(0.090)	(0.096)	(0.092)	(0.090)	(0.097)
R-squared	0.073	0.062	0.068	0.072	0.060	0.067
Observations	2,535	2,818	2,608	2,284	2,555	2,352
(Term 2) (Post) (Male)	0.031	0.076	0.063	-0.015	0.038	0.014
	(0.098)	(0.093)	(0.102)	(0.096)	(0.091)	(0.100)
(Term 3) (Post) (Male)	-0.025	-0.003	0.009	-0.048	-0.022	-0.014
	(0.084)	(0.084)	(0.099)	(0.085)	(0.086)	(0.100)
R-squared	0.068	0.059	0.063	0.068	0.057	0.062
Observations	2,615	2,909	2,691	2,358	2,640	2,429
(Term 2) (Post) (CST) (Male)	-0.281	-0.219	-0.298	-0.218	-0.164	-0.232
	(0.218)	(0.201)	(0.241)	(0.222)	(0.202)	(0.246)
(Term 3) (Post) (CST) (Male)	-0.268	-0.185	-0.339**	-0.210	-0.142	-0.286*
	(0.165)	(0.156)	(0.163)	(0.170)	(0.161)	(0.168)
R-squared	0.076	0.066	0.072	0.077	0.064	0.072
Observations	2,535	2,818	2,608	2,284	2,555	2,352
Gender	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth	Yes	Yes	Yes	Yes	Yes	Yes
Parental education	Yes	Yes	Yes	Yes	Yes	Yes
Comprehensive schools	No	Yes	No	No	Yes	No
Grammar with fees	No	No	Yes	No	No	Yes

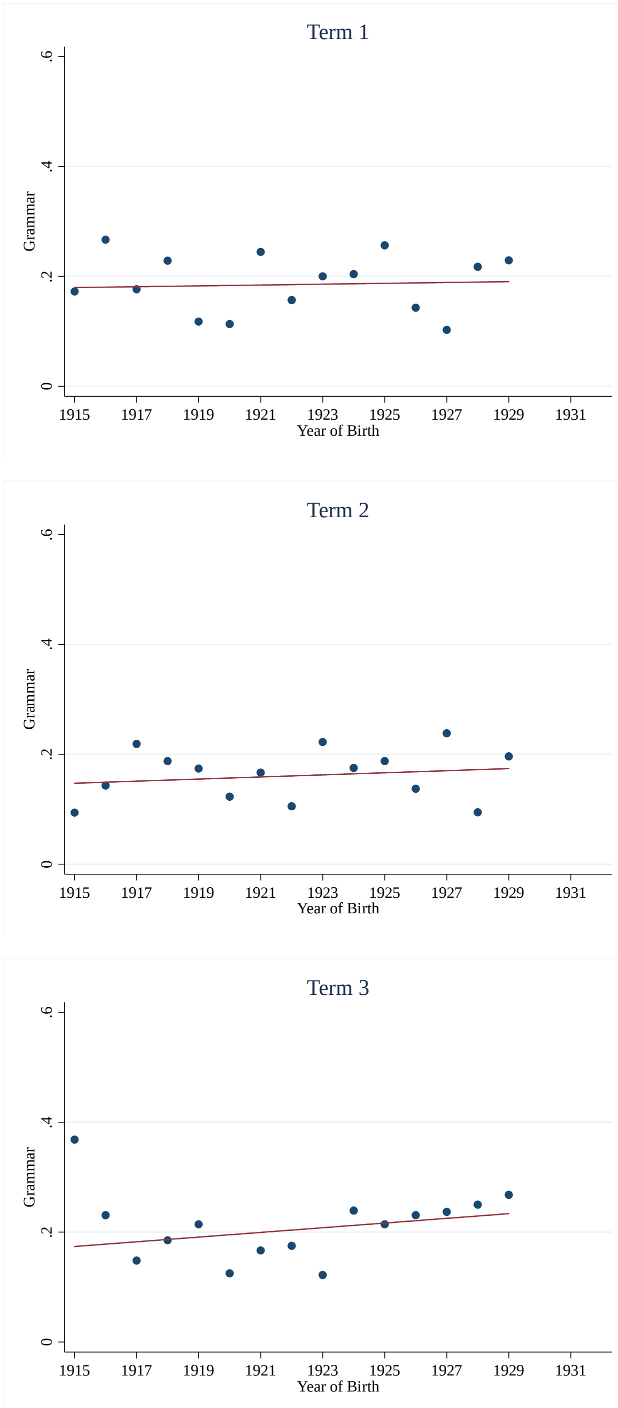
Note: Every column shows estimated coefficients of separate linear probability models of going to a selective school on seasons of birth before and after the 1944 Education Act and living in an area where class streaming would have been likely**.** *Term 2* and *Term 3* refer, respectively, to children born during January to April and during May to August ('summer children'). *Term 1* is the reference group of oldest children born during September to December. *CST* denotes inner city, town and suburban locations. The reference locations are rural areas and villages. Each regression includes all the main component of the interaction shown, year of birth fixed effects and gender. Standard errors in parenthesis are clustered at respondents' year of birth. *** p<0.01,** p<0.05, * p<0.1.

Table 9 Percentages of boys and girls in A-stream classes at start and end of junior school in 1964

	2- stream primary schools		3- or 4- stream primary schools	
	Boys	**Girls**	**Boys**	**Girls**
Age: 7+	48%	58%	30%	40%
Total children in all classes (=100%)	559	518	895	832
Age: 10+	50%	56%	32%	41%
Total children in all classes (=100%)	635	576	819	774

Source: Data extracted from Tables 7.19a and 7.19b, Barker Lunn (1970, p.395).

Figure 1 Pre-trends: Binned scatter plots and linear fits of going to grammar school prior to the 1944 Education Act by season of birth (school terms).



Note: Each plot is a binned scatter plot providing a non-parametric visualisation of the relationship between "going to grammar school" and year of birth across different seasons of births for individuals born that were born before the cut off date established by the 1944 Education Act. The linear trends are parallel across seasons of birth. *Term 1*, *Term 2* and *Term 3* refer, respectively, to children born during September to December, during January to April and during May to August.

Appendix A: Date of birth and selective schooling using a probit model

Following Puhani (2012), equations (1) can be generalized for nonlinear models in the following way:

$$(A1) \quad P(S_i = 1) = F[\, a_0(post_i) + a_1(term\ 2)_i + a_2(term\ 3)_i + a_3(post{*}term\ 2)_I$$

$$+ a_4(post{*}term\ 3)_i + e_i]$$

The conditional probability of selection, S_i is expressed as a function F of the linear index: $a_0(post_i) + a_1(term\ 2)_i + a_2(term\ 3)_i + a_3(post{*}term\ 2)_i + a_4(post{*}term\ 3)_i + \theta Z_i$. Recall that $post_i$ refers to the period after the Butler Act for term 1, 2 and 3. When F is assumed to be linear, the model reduces to be a linear probability model, given by equation (1) in the main text. The linear probability model does not constrain the predictive probabilities in the {0,1} space and therefore one may wish to use some nonlinear transformation. When F is, say, normal the model is a probit. A widely cited work by Ai and Norton (2003) shows that the coefficients of interaction terms in probit models are less intuitive than in linear models, namely, their sign and significance can be misleading. However, Puhani (2012) has shown that in empirical frameworks that include treatments and control groups (such as *Term 1, 2* and *3* in our case) and pre and post treatment (i.e., pre and post Education Act), the sign and statistical significance of these coefficients are informative. Further, treatment effects can be computed as differences between probability changes before and after Butler for born in term 2 and term 1 (see also Pinar et al., 2012).

Table A1 reports probit coefficients estimated using equation (A1). The table also shows differences in probabilities of going to grammar school between being born before and after Butler across different seasons of birth (i.e., the difference between term 2 and term 1 and the difference between term 3 and term 1 before and after the Education Act). The sign and statistical significance of each coefficient are very similar to Table 4 in the main text. However, only the magnitude of the differences in Table A1 can be compared with the coefficient in Table 4. Again, the size and significance of these are comparable to Table 4.

Table A1 Date of birth and probability of attending selective school using a probit model

Born		**≥ 1933**			**≥ 1937**	
	(1)	(2)	(3)	(4)	(5)	(6)
Probit coefficients						
Term 2	-0.313**	-0.293**	-0.284**	-0.317**	-0.295**	-0.286**
	(0.142)	(0.142)	(0.142)	(0.143)	(0.142)	(0.142)
Term 3	-0.193**	-0.135**	-0.178**	-0.191**	-0.135**	-0.177**
	(0.082)	(0.069)	(0.088)	(0.082)	(0.069)	(0.087)
Post	0.126	0.100	0.194	-0.043	-0.051	0.055
	(0.191)	(0.183)	(0.201)	(0.203)	(0.188)	(0.225)
(Term 2) (Post)	0.295*	0.260*	0.319**	0.336**	0.296*	0.356**
	(0.158)	(0.156)	(0.156)	(0.160)	(0.159)	(0.159)
(Term 3) (Post)	0.064	-0.014	0.116	0.080	0.009	0.130
	(0.116)	(0.104)	(0.114)	(0.120)	(0.109)	(0.117)
Probability differences if born before the 1944 Education Act						
Term 2	-0.099**	-0.098*	-0.081*	-0.105**	-0.101**	-0.086*
	(0.047)	(0.049)	(0.043)	(0.049)	(-0.050)	(0.046)
Term 3	-0.063**	-0.046	-0.052*	-0.065**	-0.048*	-0.055
	(0.029)	(0.025)	(0.028)	(0.030)	(0.026)	(0.030)
Probability differences if born after the 1944 Education Act						
Term 2	0.093*	0.085	0.093*	0.111**	0.101*	0.110**
	(0.053)	(0.054)	(0.049)	(0.055)	(0.056)	(0.051)
Term 3	0.018	-0.007	0.032	0.027	0.004	0.039
	(0.042)	(0.038)	(0.039)	(0.043)	(0.039)	(0.040)
Observations	2,615	2,691	2,909	2,358	2,429	2,640
Gender	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth	Yes	Yes	Yes	Yes	Yes	Yes
	Yes	Yes	Yes	Yes	Yes	Yes
Parental education						
Comprehensive schools	No	No	Yes	No	No	Yes
Grammar with fees	No	Yes	No	No	Yes	No

Note: Every column shows estimated coefficients of separate probit models of going to grammar school on date of birth before and after the 1944 Education Act. Each regression includes year of birth fixed effects and gender. Standard errors in parenthesis are clustered at respondent's year of birth. *** p<0.01 ** p<0.05* p<0.1. The table computes the differences in probability between being born in term 2 (term 3) and term 1 before and after Butler.