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Early, late or never? When does parental education impact child outcomes?*

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Abstract
We study the intergenerational effects of parents' education on their children's educational outcomes. The endogeneity of parental education is addressed by exploiting the exogenous shift in education levels induced by the 1972 Raising of the School Leaving Age (RoSLA) from age 15 to 16 in England and Wales. Using data from the Avon Longitudinal Study of Parents and Children – a rich cohort dataset of children born in the early 1990s in Avon, England – allows us to examine the timing of impacts throughout the child's life, from pre-school assessments through the school years to the final exams at the end of the compulsory schooling period. We also determine whether there are differential effects for literacy and numeracy. We find that increasing parental education has a positive causal effect on children's outcomes that is evident at age 4 and continues to be visible up to and including the high stakes exams taken at age 16. Children of parents affected by the reform gain results approximately 0.1 standard deviations higher than those whose parents were not impacted. The effect is focused on the lower educated parents where we would expect there to be more of an impact: children of these parents gaining results approximately 0.2 standard deviations higher. The effects appear to be broadly equal across numeracy and literacy test scores.

Keywords: Intergenerational mobility, schooling, child development

JEL Classification: I20, J62, J24

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1 Introduction

It is a consistent finding across numerous countries that individuals with higher levels of schooling have children who also attain higher levels of schooling. There are two main sources of this intergenerational correlation and distinguishing between them is of considerable importance. The first explanation of the intergenerational link is a selection story – characteristics that lead parents to select into higher levels of education may also impact their abilities in child-raising or be related to other genetic and environmental factors shared with their children that will lead the children to also achieve higher levels of education. The second explanation is a causal story – as a result of attaining more education, the parents with high levels of schooling provide a better childhood experience and home environment and consequently their children do better in school. The design of policy to improve intergenerational mobility, which is arguably the top social policy goal of the current UK government, differs according to the extent of causation in the link between education levels in successive generations of a family. As the UK looks to raise the Participation Age (full-time education or employment with a vocational apprenticeship) to the age of 18 by 2015, examining the intergenerational effects on mobility of raising educational participation among the lower achieving tail is timely. The empirical challenge is to differentiate between these two mechanisms and identify whether there is a causal effect of parental education on child outcomes or whether the intergenerational correlation is purely an artefact of selection.

There have been a number of recent studies using a range of techniques to isolate the causal effects of parental education (see Holmund *et al.* 2011, for a reconciliation study for the main techniques used). Oreopoulos *et al.* (2006), Black *et al.* (2005), Chevalier (2004), Chevalier *et al*. (2005), Maurin and McNally (2008) and Carneiro *et al*. (2008) all use instrumental variables techniques with a variety of instruments and with quite diverse results. Few studies go on to assess the age at which the intergenerational education transmission emerges and the relative scale of effects across literacy and numeracy. Here, we use a rich cohort dataset – the Avon Longitudinal Study of Parents and Children (ALSPAC) – and exploit the fact that a proportion of the parents in the data were impacted by the most recent raising of the minimum school leaving age (RoSLA) in England which occurred in 1972. This policy change provides an exogenous increase in education for a cohort of the ALSPAC parents, focused on lower achieving tail of educational attainment. The high frequency longitudinal nature of the data allows us to also examine the timing of impacts throughout the child's life, from early development indicators (18-30 months) and pre-school assessments through various assessments during the school years to the final exams at the end of the compulsory schooling period. Moreover, the richness of the data also allows us to look separately at results in literacy and those in maths.

Our results suggest that increasing parental education has a positive causal effect on children's outcomes that is evident at age 4 and continues to be visible up to and including the high stakes exams taken at age 16. Children of parents affected by the reform gain results approximately 0.1 standard deviations higher than those whose parents were not impacted. Focusing on the lower educated parents where we would expect there to be more of an impact, the effect is larger: children of affected parents gaining results approximately 0.2 standard deviations higher. There are no marked differences in the extent of elevated performance between literacy and numeracy scores.

The paper proceeds as follows: in the next section we review the recent literature on the causal effect of parents' education on child outcomes, before section 3 outlines our empirical strategy. Section 4 describes the ALSPAC data, section 5 presents the results before section 6 discusses the findings and concludes.

2 Previous Literature

The majority of the recent literature on the intergenerational transmission of education can be categorised into three approaches to identifying the causal effect: (a) twin studies, (b) adoption studies, (c) instrumental variables.

(a) Twin studies

The foundation of the twin approach is that by comparing the education outcomes of identical twin sisters, the effect of the mother's education on the child's education can be inferred net of any genetic influences. Behrman and Rosenzweig (2002) first applied this approach using US data and found that the effect of father's education is more important than that of the mother's. This finding has been replicated in twin studies (using both identical (monozygotic, MZ) and non-identical (dizygotic, DZ) twins) in Scandinavian countries (see Holmlund *et al.* (2011) for Sweden, and Pronzato (2010) for Norway). However, Antonovics and Goldberg (2005) show the sensitivity of Behrman and Rosenzweig's conclusion to data coding and sample inclusion criteria, concluding themselves that there is not a dramatic difference in the importance of maternal and paternal schooling. There are, however, problems with the twin study methodology. Firstly it requires that twins are identical bar their difference in education which is assumed to be unrelated to any unobserved differences between the twins. This seems a very strong assumption as it appears highly unlikely that twins choose different levels of education for purely random reasons – there must be some reason why one twin gets a different level of education to the other and whatever leads to the difference cannot be assumed to be irrelevant for other later outcomes. Beyond the above concerns, only one parent's unobservables (the one with a twin) can be controlled using in this strategy. Moreover, even if we can control for the observable characteristics of the spouse there may remain bias in the coefficient on twin parents' schooling resulting from assortative mating on unobservables. Overall the twin methodology has serious problems and it is not clear how reliable resulting estimates can be.

(b) Adoption studies.

Compared with twin studies as a methodology, adoption studies reduce the bias in the causal estimates by eliminating the genetic link between *both* parents and the child – whereas twin studies can difference out genetic factors for just one parent. The adoption estimates capture the non-genetic effect of parental education but will remain (upwardly) biased since they also contain the effect of parental nurturing skills which differ between parents and are likely to be (positively) correlated with but not driven by education level. The adoption strategy is exploited by *inter alia* Sacerdote (2002, 2007) and Plug (2004). Examining the outcomes of Korean adoptees in the US, Sacerdote (2007) finds that an additional year of maternal education for the adopting mother increases the adoptee's years of schooling by approximately 0.1 years and increases the probability of the adoptee having a 4-year college degree by 2 percentage points. Plug, using US data, finds that genetic factors account for approximately 50 percent of the mother's education effect, and 30 percent of the father's – echoing the twin study findings that father's education is more important causally for children's outcomes. In fact, when both parents' education is included in the model, only the effect of father's education is significant, suggesting that the mother's education effect is wholly accounted for by genetic and assortative mating factors. Holmlund *et al.* (2011) also examine estimates using adoptees in Sweden and in contrast find equally important effects for mothers and fathers though in each case including spouse's education sees the coefficients halve in size and become insignificant. As acknowledged by authors using this strategy, the correlation between parents' and children's educational outcomes can still be because of non-genetic factors that are shared by both the parents and the children, with the transmission via parenting style, ethos and values and the result that both parents and children select levels of education on these unobservables. In addition, the sample sizes typically available even in registry datasets are small and the placement of adoptee children may not be random.

(c) Instrumental Variable studies.

Arguably the most clear cut strategy for isolating the true causal effect of parental education on child education is instrumental variables. In this case the biases from both the genetic and environmental transmission factors that confound OLS estimates are removed, since the variation in parental education is orthogonal to unobservables. The majority of IV strategies rely, as we do, on changes in compulsory schooling requirements which induce certain cohorts of low educated young people to increase their schooling relative to the previous cohorts. These changes are involuntary increases in schooling for a group who are likely to be drawn from those with lower prior educational attainment and a less positive attitude toward education. Other IV strategies which focus on unanticipated variations in opportunities for continuing education to the graduate level, such as Caneiro *et al.* (2008), are likely to be drawing inference from a very different part of the educational attainment distribution and there is no *a priori* reason why the effects should be similar across these groups. The 'local average treatment effect' identified at the low education part of the distribution is likely to be more important in policy terms where policy makers are concerned with low intergenerational mobility or low income in the second generation.

Oreopoulos *et al.* (2006) exploit changes in compulsory schooling requirements across US states over time to identify the causal effect of parents' education on children's probability of repeating a school grade or dropping out of high school – each are reduced by 2 to 4 percentage points for an additional year of education for either parent. Black *et al.* (2005) similarly exploit a two-year increase in the compulsory schooling required by law in Norway, introduced at different times across different regions during the 1960s and early 1970s. There are however few causal effects identified, suggesting that selection explains most of the cross-sectional correlation. The exception is for mothers and their sons, where a year increase in schooling for low educated women increases their son's subsequent schooling by one tenth of a year. On their full sample, Holmlund *et al.* (2011) find results of a similar magnitude for Swedish data, again exploiting a compulsory school leaving age reform, though they find that the coefficient on father's education is also significant and almost as large. Restricting the sample to just the lower educated parents where the reform should impact the most, the coefficients are incongruously smaller and only the mother's is significant and only when the partner's education is excluded from the regression.

Within the UK, a number of studies have exploited both the 1947 (to age 15) and the 1972 (to age 16) RoSLA to identify the intergenerational transmission of education. The combination of the NCDS 1958 birth cohort study and the 1947 RoSLA has been exploited by two studies looking at child cognitive and non-cognitive development indicators, as opposed to educational qualifications. Sabates and Duckworth (2010) estimate the impact of increasing mothers' schooling on children's *relative* rank within cohort along four dimensions of development: two cognitive, two behavioural. They find that amongst mothers who only attain the compulsory years of education, increasing schooling by one year positively impacted on the mathematics attainment of their children. There were no significant impacts on reading or on behavioural outcomes, though it is difficult to identify effects in the small estimation sample of only 467 children available around the education discontinuity. Silles (2010) examines the impact of fathers' as well as mothers' education on child's percentile rank in cognitive and non-cognitive outcomes at ages 7, 11 and 16. Despite large correlations between parental education and child cognitive development in the OLS estimates, the large standard errors on the IV estimates make them too imprecise to identify any significant effects. One problem here is that identification in this context relies on comparing successive cohorts of parents, only one of which was affected by the schooling reform. When the children are from a cohort study and born at almost the same time, this can lead to the treatment effect becoming confounded with the age of the parent at the child's birth, which may exert an independent effect on child outcomes. This may cause a problem for studies using the NCDS, for example, where all children were born in a single week of 1958.

The 1972 RoSLA that we exploit has also been utilised to identify causal effects of parental education by Chevalier (2004) and Chevalier *et al.* (2005). The former uses the Family Resources Survey, and finds that the causal impact of an additional year of parental schooling on the probability of the child remaining in school post-16 is roughly equal at 8 percentage points for either parent, though significant only at the 10% level. Chevalier *et al.* use the UK Labour Force Survey to examine the impact of parental education and income on the probability of a child remaining in school post-16 and also on the probability of attaining five or more GCSEs graded A to C (a standard measure of educational achievement in the UK). Despite large effects of parental education on the children's educational outcomes in the OLS, when instrumenting both education and parental permanent income, the parental education effects become non-significant. Both of these studies are limited by the child outcome variables available in the respective datasets.

The US study most similar to our own is that by Carneiro *et al.* (2008) using data from the children born to women in the National Longitudinal Survey of Youth (NLSY). The authors are able to look at outcomes at different stages of the children's upbringing and consider both reading/literacy and math results at these ages. The identification strategy relies on differences in the availability and costs of higher education and therefore the education margin examined is quite different to the one that we study. Moreover, only maternal education is known thus the estimated education effects will combine the direct effect plus any impact via assortative mating. Carneiro *et al.* find that for children of white mothers, an additional year of maternal education increases child reading and math test scores at age 7-8 by 0.075 and 0.1 standard deviations respectively. By age 12-14 the effects are smaller and not significant. A year increase in maternal education also causally reduces the probability of grade repetition by just under 3 percentage points at each age, tallying with the finding of Oreopoulos *et al.* For children of black mothers the results are similar, though the maths and reading impacts remain significant at age 12-14 and are stronger. Maurin and McNally (2008) also examine the higher education margin, exploiting the French student uprising of 1968 to instrument for higher education access. The student protests disrupted the education system to such an extent that the usual examination procedures were curtailed during 1968; in particular the *baccalauréat*, which if passed guarantees a place in university, was assessed using just oral examinations on a single day rather than the usual series of oral and written examinations. As a result there was a 30% increase for this cohort in the number of people attaining the qualifications to access university. Exploiting this exogenous increase in higher education, Maurin and McNally find that increased paternal education significantly reduces the probability of a child being held back a grade.

In addition, there are a small number of papers that pursue alternative identification strategies. Ermisch and Francesconi (2001) provide a theoretical model of investment by parents in the education of their children and propose conditions under which the cross-sectional associations between parents' and children's schooling can be interpreted causally. Amongst poorer parents where the authors' model suggests the education effects are causal, the estimates suggest a strong influence of both parents' education. It is clear from reviewing the recent literature that there is not a consensus regarding the *causal* effect of parents' education on the education of their children – even amongst studies employing the same identification strategy. Holmlund *et al.* (2011) suggests that the underlying causal parameter identified by each differing method is the same, with differences in estimates owing to country and time specific factors, which needs to be borne in mind when considering the wider applicability of our findings here. There are also unresolved issues over the timing of any causal effects within the upbringing of the child and also the areas affected – is any causal effect felt early on in life or is it only apparent at later school years? Moreover, is the effect universal across all subjects or specific to certain educational domains?

3 Data

As alluded to above, our data comes from the Avon Longitudinal Study of Parents and Children (ALSPAC), which is a cohort dataset comprising children who were expected to be born between 1st April 1991 and 31st December 1992 in the Avon area, a former administrative area in the South West of England which includes the city of Bristol. All mothers in Avon with children due during this period were invited to join the study, resulting in 13,971 live children at 12 months, from 13,801 mothers. Additionally, eligible children who were found in the national pupil census data but who were not in the core ALSPAC sample were invited to join the study. In total we have a potential maximum of 19,966 children who would represent a full census of children born in the study area in the applicable window.[1] The data from the study includes information from survey questionnaires completed by the mothers, the mothers' partner and the study children at various points during the children's lives – from pre-birth through to teenage years. Further to the main questionnaires there were several "clinics" at different ages, during which children completed various types of tests and questionnaires. Data from administrative sources has also been linked in, including Annual School Censuses, at school and pupil level giving test results for all Key Stages and Entry Assessment.

The Key Stages in the English schooling system are formal examinations, externally set and marked, which are taken by children in all state schools at ages 7 (Key Stage 1), 11 (KS2), 14 (KS3) and 16[2] (KS4). The KS4 assessments include GCSE exams and also vocational equivalent qualifications (Appendix Table A1 shows how academic qualifications correspond to the National Vocational Qualifications equivalence scale). These data can be explored in different forms, specifically we use KS4 points (the sum of all GCSE-equivalent points for all age 16 qualifications), the total points for

[1] Triplets and quadruplets are excluded from the data since the external data is unavailable for these children due to confidentiality concerns

[2] The ages for the KS assessments listed here refer to the age of the child at the end of the school year in which these tests are taken. Some of the younger students in the school cohort will be 6, 10, 13 and 15 when the KS tests are taken but will soon after turn 7, 11, 14 and 16 respectively.

GCSEs only so that vocational qualifications are excluded, the total points for traditional academic GCSEs[3], the score for maths GCSE and the score for English language GCSE (as a measure of literacy).

At the earlier ages we also look assessments of English and maths separately and a combined overall test score. For KS1 (age 7) the assessments are reading, writing and maths. We combine the reading and writing scores into an overall "literacy" measure. We also have information on the child's school entry assessment scores: these measures are teacher-assessed in the child's first term of Reception class (normally age 4 years), generally in late October/early November so the child has been in school for only one or two months. These assessments were not compulsory nationally at the time the ALSPAC children were entering school, however the same system was used in about 80% of schools in the Avon area at that time. We create an Entry Assessment total score by combining results for reading, writing, language and maths; we also look at maths and literacy scores separately.

We also have a number of outcomes that are not measures of formal education. One such outcome is a measure of IQ. This is taken from the Focus 8+ Clinic, to which all ALSPAC study children were invited at around 8 years of age. The children were measured using the Wechsler Intelligence Scale for Children, specifically the WISC-III ^{UK}, which was the most up-to-date at the time of the clinic. We use the total score, a sum of 10 subscales[4] (split into verbal and performance categories) which are age-adjusted and also just performance IQ, which is thought to capture the more innate "fluid intelligence" dimension of IQ. Finally we have mother-reported measures of child development in several areas from the early child-focused questionnaires. We use gross and fine motor skills scores which are averages of scores taken from questionnaires when the child is aged 18 and 30 months, scaled between 0 and 100. We do not adjust the scores for age but we do include age when measured in regressions as controls when using these dependent variables.

For the parents the education data is more restricted. There is no information regarding the age parents left full-time education or indeed an IQ type test but there are qualifications achieved. We construct three different (0,1) qualification indicators capturing whether the parent has any qualifications[5], has any O-levels (the exam preceding GCSE and taken at age 16) and has any A-levels (the exams usually sat at age 18 that determine access to university). Unfortunately there was no information on the number of each type of qualification or grades, hence our focus on the impact of the RoSLA on the broad level of qualification attainment of the parents. The increase in education experienced as a

[3] Included GCSEs: maths, English language, English literature, geography, history, French, German, Italian, Russian, Spanish, single/double award science, biology, chemistry, physics.

[4] The verbal subscales are: information, similarities, arithmetic, vocabulary and comprehension. The performance subscales are picture completion, coding, picture arrangement, block design and object assembly.

[5] Any qualifications includes CSE, vocational and skill qualifications, apprenticeships, intermediate, full and final City & Guilds, State Enrolled Nurse, State Registered Nurse, teaching qualifications, degrees, O- and A-levels.

result of the RoSLA will be felt in terms of both years in education and in qualifications attained at the end of the extra year in education resulting from the reform. Thus we do not directly observe the full extent of the RoSLA on parental education within the ALSPAC study but only in the domain of qualifications. We show the size of the change in terms of the proportion of the population that stayed on for the extra year using other data sources.[6]

Table 1 contains summary statistics for the parents in our main estimation sample (±3 years around the RoSLA). These are the characteristics of the parents who are either treated or untreated, that is they are born within the ±3 years around 1st September 1957, and so are considered to be comparable with respect to the treatment effect. We see from Table 1 that the fathers are on average slightly more educated than the mothers with fewer having no or low qualifications and more having A-levels and above. More of the mothers have teaching or nursing qualifications as we may predict. The lower part of the table shows that the ALSPAC fathers in the treatment zone are slightly older than the mothers which is in line with what we might expect, but the difference can only be small given that by definition of inclusion in the treatment zone these parents must be born within ±3 years of RoSLA.

Table 2 contains summary statistics for the children of parents in the ±3 years sample, both the overall measures and broken down by the education level of the parents. The final column ("full sample") shows that just over half of the sample are in the cohort that took their GCSEs in 2008.[7] Moving from parental education group 1, which represents children of parents with no or only low qualifications between them, to education group 4 which represents children of parents who both have a degree or equivalent, there is a clear parental education gradient in child outcomes. For example, moving from children of the lowest educated parents to children the highest increases average Key Stage 4 (age 16 examinations) score from 350.65 to 502.35 which is equivalent to an additional three GCSEs at the top grade (A). Similar gradients exist for each of the education measures and IQ. Fine motor skills averaged at age 18 and 30 months, which is a developmental indicator that is correlated with later educational outcomes, exhibits a small gradient, however gross motor skills recorded at the same points and which is not strongly related to later outcomes is almost constant across parental education groups.

[6] Appendix B contains further information about the dataset and the construction of the variables used.

[7] These children started school in September 1996 and were born therefore between September 1991 and August 1992. ALSPAC children born before September 1991 are in the school year before this and those born after August 1992 are in the school year after this.

4 Empirical Strategy

In England and Wales, compulsory schooling laws apply nationwide and govern the mandatory age by which children must start school and the minimum age[8] at which individuals are no longer required to be in full-time education. The most recent change to the minimum school leaving age came into effect from 1st September 1972[9] and required individuals to remain in school until the end of the academic year in which they turn 16 – a one year increase from the previous requirement. The law change therefore affected all individuals turning 15 on or after 1st September 1972. The educational impact of the law change was substantial: Figure 1 shows the mean age of leaving full-time education for men and women for the 10 cohorts immediately before and after RoSLA, using data from the UK Labour Force Survey. While there is a general upward trend both before and after the RoSLA, there is a discrete jump in the average years of schooling by just under one third of a year for both men and women as a result of RoSLA (implying that just under one-third of the cohort were bound by the reform). Moreover, as Figure 2 illustrates, the proportion that left school without any qualifications dropped sharply while the proportion leaving with one or more level 1 (below O-Level) or level 2 (O-Level) qualifications increased. There is no impact on A-level qualifications (which are level 3 qualifications) or higher, which suggests that the impact of RoSLA was limited to the lower end of the education distribution, with no ripple effect further up (see Chevalier *et al.* 2004, for further evidence of this). Table 3 quantifies the pattern illustrated in the Figures and shows the pattern by gender, comparing the 3-year trends pre- and post- policy in mean years of schooling, the proportion who left school by age 15 and the proportion of each cohort holding various levels of qualifications, with the change in these measures induced by RoSLA. Clearly at the national level there is a significant, discontinuous education impact at the point of RoSLA with particular impact on those leaving school at age 16 or younger.[10]

A number of studies have exploited this exogenous increase in education to estimate the causal impact of education on *inter alia* wages (Grenet, 2012 *forthcoming*; Harmon and Walker, 1995), employment (Dickson and Smith, 2011), health (Clark and Royer, 2010; Silles, 2009) and crime (Machin *et al.*, 2011). The estimated impacts of the RoSLA are substantial for wages, employment and crime, though there is mixed evidence regarding any effect on health. In each case the estimates are interpreted as 'local average treatment effects'[11] as the policy impact is limited – as illustrated above – to the lower

[8] The minimum school leaving age refers to the age that the individual will be at the end of that academic year, hence some who leave at the minimum age when that is 16 (15) will actually still only be 15 (14) on their final day in school.

[9] The Raising of the School Leaving Age Order (Statutory Instrument no. 444) was passed in March 1972, activating the clause of the Education Act 1944 which provided for the raising of the school leaving age to 16 when it was deemed possible to do so.

[10] Calculations using the Quarterly Labour Force Survey, pooled from 1993q1 to 2010q2.

[11] See Angrist and Imbens (1994).

part of the education distribution: there was no impact of the reform on educational attainment further up. Similarly, we are interested in investigating the causal effect of parental education on child outcomes amongst parents with low levels of education – a group whose children are most at risk of poor economic outcomes. Therefore though the estimated effects may be different to the average treatment effect, the LATE we estimate is arguably the most important for policy.

The availability of information on the date of birth and the qualifications of **both** parents provides an additional dimension to the "treatment" of study children, which is determined by whether none, one or both of their parents were impacted by the RoSLA. In order to capture the treatment effect as tightly as possible, we restrict our focus to parents' born in short windows around the date of the policy change. In choosing the size of the window there is a trade off between comparing parents born just before and just after the reform (which reduces any bias introduced to the treatment effect estimate when moving further away from the time of the policy change), and increasing the sample size (which improves precision of the estimates). We consider windows of ± 1 year, 3 years or 6 years around the policy change and all of our results are robust to the choice of sample window. To be included in the sample, a child must have at least one parent who was born within the sample window. If a parent is born outside of the sample window then that parent is ineligible to be considered as either treated or not (in which case a dummy is included for born before or after the sample window). If they are born within the sample window and before September 1957 they are untreated and if they are in the window and born on/after 1 September 1957 they are treated. Therefore each child's treatment status is either 0, 1 or 2 and the estimated coefficient on the treatment variable captures the intention to treat impact of increasing the education of either parent via the RoSLA.[12] If both parents are born outside of the treatment window then that child is excluded from the sample. Single parents are included in the data, a dummy variable is included to pick up the effect of the other parents' information being missing, moreover if the current partner is not the same as when the child was born we exclude these parents (fathers) from the treatment.

Table 4 illustrates the treatment matrix for the main estimation sample: ±3 years around the RoSLA policy change. Horizontally along the top of the table, the fathers of ALSPAC children are partitioned according to when they were born, while the mothers are partitioned down the left side of the table. The numbers on the right of each cell indicate the number of children in this category, and for the categories that comprise our estimation sample (highlighted cells) the number on the left gives the value of the treatment variable for children in this category. As outlined above, any parent born

[12] This specification implicitly imposes that the effect of the mother being "treated" by RoSLA is the same as the effect of the father being "treated" and that there is no additional interaction effect if both parents are treated. This is supported by the fact that the impact of RoSLA on the male and female education distributions was similar, moreover if we replace the RoSLA variable in the models for child outcomes with dummies for each parents' highest level of education, the coefficients for maternal and paternal education are almost the same. Details available from the authors on request.

outside of the window ±3 years either side of 1st September 1957 (for the main sample, ± 1 year either side or ± 6 years either side for the robustness check samples) is not included in the treatment variable, and if both parents fall outside of the window then the child is excluded from the sample. Thus only children in the highlighted cells are included in our main estimation sample. There are 262 ALSPAC children for whom both their mother and father were born more than 3 years before RoSLA and so these children are not in our main estimation sample. There are however, 274 children whose father was born more than 3 year before RoSLA but whose mother was born in the 3 years pre-RoSLA and so these children are included as part of the pre-RoSLA comparison group. There are 505 children for whom both parents were born in the first 3 years post-RoSLA and so these children are "doubly" treated and have a treatment variable value of 2. In total, for the main estimation sample there are 4967 children who have one or more parents within the treatment window. Of these 1477 are untreated, 2985 have one parent treated and as noted 505 have both parents treated.[13] Some 4,046 observations have no data on mothers or fathers date of birth, these are almost all drawn from the additional supplement sample identified at age 4 on entry into school. However, there are also a sizeable number of cases where the father's date of birth was not recorded with enough accuracy to isolate definitively treatment status. These are disproportionately associated with younger mothers outside our treatment window.

We proceed by initially estimating the reduced form impact of the RoSLA on both parents' qualifications (equation (1) below) to illustrate the first stage effect which is a pre-requisite for there being a causal effect on child outcomes via the parents' education. For the dependent variable Q_j we consider three different (0,1) qualification indicators: has any qualifications, has any O-levels and has any A-levels, and in all cases the subscript *j* refers to the parent. The indicator $RoSLA_j$ is a dummy variable for being born on or after 1st September 1957 and the vector $\boldsymbol{X}_{1j}$ contains either a linear or a quadratic term in the month of birth of the parent. Equation (1) is estimated using a linear probability model.

(1) $$Q_j = \boldsymbol{X}'_{1j}\beta_1 + \gamma_1 RoSLA_j + \varepsilon_j$$

We then estimate the reduced form effect of RoSLA treatment on the child outcomes, S_i, (see equation (2) below), controlling flexibly for the age of the mother at the child's birth and for when the father was born in addition to including controls in X_{2i} for child demographic characteristics: gender, age in months, and school cohort.[14] The subscript *i* refers to the child, though the variables themselves in some cases are characteristics of child *i*'s parents. The particular outcomes that we examine are

[13] In the +/-3 year window only 10.1% of the sample children are "double" treated and in the +/-1 year window this falls to 2.6%. This supports (0,1,2) treatment specification that excludes an interaction effect if both parents are treated – it could not be identified in the +/-1 year window which is our most important robustness sample.

[14] We also include dummies to capture if a parent records foreign qualifications and so is ineligible for treatment on this account and for parental date of birth information being missing.

various education outcomes from national tests at ages 7, 11, 14 and 16 along with school entry assessment (age 4), very early measures of development (18 and 30 months old) and IQ measured at age 8:

(2) $$S_i = \boldsymbol{X}'_{2i}\beta_2 + \gamma_2 RoSLA\_TREATMENT_i + AgeMother_i * \boldsymbol{M}'_{\boldsymbol{i}}\delta + \boldsymbol{F}'_i\varphi + u_i$$

Note that the indicator $RoSLA\_TREATMENT_i$ in equation (2) is different to the $RoSLA_j$ dummy in equation (1) as it can take values of 0,1 or 2 depending on when each of the child's parents were born. There may be physiological reasons why younger child bearing may impact child development and outcomes or there may be an impact of lower life-experience amongst younger mothers that affects their parenting skills and may carry through to child educational outcomes. Therefore it is important in our specification to control especially for the age of the mother at the child's birth in order to prevent the treatment effect being confounded with the effect of bearing the child at a younger age. We control also for the age of the father at the child's birth for similar reasons[15]. The vector $\boldsymbol{M}_i$ contains three dummy variables indicating whether the mother was born before the sample treatment window, during the window or after the window. Therefore this specification allows a different slope for the age of mother at the time of the child's birth ($\text{AgeMother}_{,i}$) for mothers born pre-, during and post- the sample treatment window as $\text{AgeMother}_{,i}$ is interacted with $\boldsymbol{M}_i$. Allowing a different quadratic shape of the mothers' age effect for pre-, during and post-window does not alter the results and so in the interest of parsimony only the linear age splines are used. For fathers the $\boldsymbol{F}_i$ vector contains a dummy for whether the father was born before the sample window or after the sample window. It is not interacted with the age of the father at the time of the child's birth and so the age of the father effect is captured more coarsely with just these dummies[16]. Unlike in other cohort studies, the children in ALSPAC are born in a window that spans two calendar years, which means that they are placed into three different school years[17]. This is an important feature of the data as it means that the results are not being driven by cohort specific idiosyncratic factors, nor is the treatment effect confounded with the effect of parents' age at the child's birth.

Clearly the younger parents in the sample will be treated and the oldest parents in the sample will be untreated, however there is a range of ages where it is the case that the parent may have been treated

[15] As alluded to above, we use a less flexible functional form to control for the effect of father's age at the child's birth since the impact of father's age should be less important that than of mother's age as the physiological effects are likely to be much smaller and if the mother is the primary care giver then any impact of being a younger father on child outcomes is likely to be lower order than is the case for the mother, hence in the interest of parsimony we use only dummies for father being born before or after the treatment window. All results are robust to using an equally flexible functional form for fathers' age as we do for mothers'.

[16] Results are robust to using the same specification for father's age as for mother's see previous footnote.

[17] In the English school system, children are assigned to a school year according to date of birth with a school cohort being all children born between 1st September in year *t* and 31st August in year *t+1*. The ALSPAC children were born between January 1991 and January 1993 and so are in three different school years: the cohorts starting school in September 1995, September 1996 and September 1997.

or may not. Figure 3 illustrates this: the youngest parent in the data who is ***untreated*** was born in August 1957 (the last month of birth for which the individuals faced a minimum school leaving age of 15) and had their child in April 1991 and so was 33 years and 8 months old at the time of the child's birth. The oldest parent in the data who is ***treated*** was born in September 1957 (the first month of birth for which the RoSLA is in effect) and had their child in December 1992 and so was 35 years and 3 months old when the child was born. Therefore any parent who is older than 35 years and 3 months is definitely ***untreated***, while any parent younger than 33 years and 8 months is definitely ***treated*** – however, the treatment status of any parent in between these ages may not be inferred from their age. Table 5 shows that depending on the sample used there are approximately 800-900 mothers who fall in this age range at the time of the child's birth, with about 100 fewer fathers in this range – as we would expect since fathers are on average slightly older than mothers. Amongst the parents in this age range who may be treated or untreated, just over half (52%) of the mothers are treated as are around 60% of the fathers. Therefore there is a 19 month range of ages that identify the treatment effect separately from the effect of parents' age at child's birth and a fairly even split between treated and non-treated within this age range. Figure 4 illustrates the density of parents' age at child's birth for mothers and fathers separately, with the vertical lines delineating the areas in which the parent is definitely treated, definitely untreated and the ambiguous 19-month range in between.

We later consider a narrower definition of the treatment, focusing in on the part of the education distribution where the treatment is actually working i.e. the lower educated parents. For these regressions we redefine the treatment variable such that only those parents with less than A-level qualifications are considered at risk of treatment. As before, to be included in the sample a child must have at least one parent born within the treatment window and now the additional stipulation is that this parent must also have less than A-level qualifications. We include a control to capture the A-level qualifications of the other parent if the other parent is higher educated.[18] The treatment variable can still take the values of 0, 1 or 2 depending on the education level and date of birth of both parents, for example it will be 2 if both parents are born after the RoSLA and within the treatment window and neither has A-level qualifications.

There may be a concern that those parents born either side of the policy change are different in observable and unobservable ways which would confound the estimated treatment effects. This should not be the case given the exogenous nature of the policy change with respect to the observable and unobservable characteristics of the parents. However, to consider this possibility Table 6 contains balancing tests of the difference between the treated and non-treated parents in terms of their fertility

[18] The focus in this specification is the impact of RoSLA on the outcomes of children via its impact at the low education margin for parents. Rather than dropping any child who has one parent with A-levels or higher, we instead take out any high educated parent effect with a control. The treatment variable compares the outcomes for children with one low educated parent affected by the RoSLA with the outcomes for comparable children whose low educated parent was not affected, given the education of the other parent.

and demographic characteristics. These are shown for both the main estimation sample – the ±3 year window around RoSLA – and the ±1 year window sample, bearing in mind that there is a great deal of overlap in the age of parents at the birth of the ALSPAC child for the treated and untreated parents in the ±1 year window. With respect to mother's age at first birth, the birth order of the ALSPAC child and the completed fertility of the ALSPAC child's parents, in the ±1 year sample there is only a statistically significant difference between the treated and untreated in the completed fertility of the ALSPAC mothers. Moreover, despite statistical significance the magnitude of the difference is small. For the larger ±3 year sample, there are small differences in the fertility characteristics of the post-treatment parents however they are small and consistent with the parents in this part of the sample necessarily being slightly younger on average than the pre-treatment sample. However, it is clear that the differences in means are small and it is also the case that for the broader sample as well as the ±1 year sample, the median birth-order (2) and number of siblings (1) is identical for the treated and untreated parents.

Geruso *et al.* (2011) pool data from a large number of Labour Force Surveys along with live birth records, abortion records and the 1971 longitudinal study and exploiting the same 1972 RoSLA in England and Wales that we do, examine the causal effect of education on fertility. Their findings indicate that the only effect of RoSLA was to reduce the incidence of teenage fertility amongst 16 and 17 year olds, with no impact at age 18 and 19 or older. Overall the authors cannot reject that the additional education had no effect on post-teen fertility and no impact on completed fertility. The effects are estimated for narrow confidence intervals strongly suggesting no effect on overall fertility. Teenage pregnancy is a very small proportion of all pregnancies, and age 16 and 17 pregnancies only a part of teen fertility, hence this evidence suggests that for the cohorts of women that we are looking at, the additional education induced by RoSLA would have a minimal impact on the composition of our sample via a timing of fertility effect or a change to completed fertility. Thus we would not expect to find any effect on fertility among mothers in our sample who are in their early to mid-thirties at the time of the child's birth.

The lower section of Table 6 compares the pre- and post-treatment parents with respect to their own parents' education (i.e. the ALSPAC children's grand-parents), measured in terms of whether they hold O-levels or not. For the ALSPAC mothers in our ±1 year sample there is no difference at all in their parents' education between the treated and untreated and for fathers any differences are small and not statistically significant. In the wider ±3 year sample, differences are all small and non-significant, with the exception of the fathers of treated ALSPAC fathers being slightly more likely to hold O-levels than the fathers of untreated ALSPAC fathers. Thus overall the educational backgrounds of the treated and untreated ALSPAC parents seem to be well balanced.

The final part of Table 6 compares the IQ of the children of untreated versus treated parents. In the ±1 year sample, there are no significant differences in IQ for the children of ALSPAC mothers nor for children of ALSPAC fathers. In the wider ±3 year sample, again there is no difference with respect to mothers though there is a small difference for children born to fathers who were treated by the RoSLA reform – but it is a reduction in IQ. Therefore if anything, this would produce a small downward bias in the effects of increased parental education on child outcomes. Moreover, looking at "performance IQ" which is thought to capture the more innate element of IQ ("fluid intelligence"), there are no differences between children of treated and untreated parents whether we look in the ±1 year sample or the ±3 year sample. Overall this provides some evidence to suggest that there is not an underlying difference in innate "ability" between the children of treated and untreated parents.

In summary, the results of these balancing tests give us confidence that there is not selection into treatment on observable characteristics of the parents nor are the children of treated parents different in their more innate characteristics than the children of the untreated parents. Therefore given the nature of the exogenous policy change, there is no reason to suspect that there is selection on unobservables either, hence the treatment variable should be an unbiased estimate of the intention-to-treat impact of RoSLA on child outcomes.

5 Results

The impact of RoSLA on parents' education

To illustrate that the national impact of the RoSLA on the education distribution is mirrored amongst the ALSPAC parents, Table 7 shows the results for the reduced form equation (1) estimates, using each of our three measures of educational attainment for the parents. The upper panel (a) refers to the full sample, whereas the lower panel refers to the sample when we restrict to only including parents who have less than A-level qualifications in the definition of the treatment variable.

Column (1) of panel (a) shows that the impact of RoSLA is to significantly increase the proportion of individuals with any qualifications by 4.4 percentage points, and the proportion with O-levels by 6.5 percentage points, both significant at the 1% level. This is in line with the national picture for men, though the ALSPAC women appear not to have increased qualification levels as much as the national average. These estimates are robust to the inclusion of a higher order polynomial in parents' date-of-birth (in months), the impact coefficients altering slightly to 3.9 percentage points for any qualifications, 6.0 percentage points for O-levels (as shown in columns (4) and (5) respectively).[19] As with the national picture, there is no impact on the proportion holding A-level qualifications – whether we use a linear (column (3)) or a quadratic (column (6)) trend in parent's date-of-birth. This

[19] Table 7 reports the impact of RoSLA on the pooled sample of parents. Estimated separately for mothers and fathers the individual RoSLA coefficients are not statistically different to each other and mirror the pooled sample both in terms of size and significance. For example, panel (a) column (1) and (2) results for women: 0.052 (any qualifications), 0.056 (O-levels); for men: 0.036 (any), 0.076 (O-levels). The full Table 7 by sex is available from the authors on request.

is important as later we will narrow the focus to the impact on parents with less than A-level qualifications on the basis that this is where the main impact of RoSLA is felt.

Imposing the restriction, panel (b), the impact of RoSLA is increased as we would expect to be the case. Now the increase in any qualification holding is 11.1 percentage points and 11.6 pp for holding O-levels, each significant at the 1% level. Allowing a quadratic in parents month of birth very slightly changes these impacts to 10.9 pp and 12.1 pp respectively. Thus the impact of RoSLA on parental qualifications is sizeable, especially when we focus on the part of the education distribution where the effect is most keenly felt. Moreover, given that the effect on qualifications amongst the ALSPAC parents closely mirrors the national impact on qualifications, we can surmise that a similar proportion of the parents were bound by the reform – around one-third receiving an extra year of education.

The impact of RoSLA on children's education

The results from estimating the impact of RoSLA on children's age 16 outcomes are displayed in Table 8 for the three different windows around the policy change: ±1 year, ±3 years and ±6 years. The KS4 outcomes are graded on the same equivalence scale which ranges from the lowest grade G which is worth 16 points, through increments of 6 points per grade to the highest grade A* being worth 58 points. The mean and standard deviation of each outcome variable are displayed in the table to give a sense of scale, as is the treatment effect as a proportion of a standard deviation, to allow comparison across later measures, such as the other KS outcomes.

Comparison of the RoSLA treatment coefficient in columns (1), (4) and (7) for KS4 total score, those in columns (2), (5) and (8) for GCSE total score and those in (3), (6) and (9) for the more academic GCSE qualifications, show the consistency of the point estimates for the treatment impact across the three sample windows. For each parent affected by RoSLA, the child's KS4 total score is raised by approximately 20 points – which is the equivalent of just over three GCSEs grades.[20] Including only GCSEs – therefore excluding the vocational equivalent qualifications – the treatment impact is approximately 14 points, just over two GCSE grades, and this remains the case when we focus just on the traditional academic subject GCSEs. Unlike for KS4 total and GCSE total, the GCSE academic total does not show a significant impact of RoSLA in the narrowest ±1 year sample window but the point estimate is similar to the other windows. The impacts as a proportion of the outcome variable standard deviation are shown in the final row of Table 8. For KS4 total score the impact is consistent across sample windows at around 14% of a standard deviation, while for GCSE total it is 10%, with the proportional impact on academic GCSEs between these two numbers.

[20]An alternative specification estimating the RoSLA effect separately for each parent whilst controlling for the education of the other parent results in insignificantly different RoSLA effects for mothers and fathers – results available from the authors on request.

The impact of mother's age at the time of the child's birth is significant for the mothers born in the sample window and also mothers born after the sample window, with the age impact higher for the younger mothers in almost all cases. We find, as we would expect, that amongst younger parents the slope of the age effect is steeper. Summarising the broad pattern, for mothers who are within the treatment area an additional year of age at child's birth increases these age 16 outcomes by approximately one GCSE grade (6 points) on average, whereas for the younger mothers the additional year increases these outcomes by closer to one and a half grades. There are few mothers who were born before the respective sample windows who have partners in the window hence the pre-window mother age effects are not significant.[21]

The impact of the RoSLA treatment on the high stakes age 16 examinations is clear with the RoSLA raising attainment by around 0.1 of a standard deviation across the alternative exam metrics; now we turn to looking at earlier assessments. Table 9 contains the estimates of the RoSLA treatment impact on early development indicators: performance IQ, school entry assessment and the Key Stage scores at ages 7, 11 and 14 (KS1, 2 and 3 respectively). We focus on the ±3 years window here as being representative of the alternative lengths of window considered. Gross motor skills is not strongly correlated with later educational outcomes and it is therefore not surprising that column (2) of Table 9 shows that there is no impact of RoSLA treatment found for this outcome. Fine motor skills however are predictive of later outcomes but there is no significant impact of RoSLA treatment on this outcome though the point estimate is positive (column (1)). "Performance IQ" which is a component of the Wechsler Intelligence Scale for Children IQ measure that we use, is thought to capture the more innate element of IQ and as such this should not be malleable to the intervention of increased parental education. As is clear in Table 2, this IQ measure is graded by parental education, however we see in Table 9 that there is no impact of RoSLA treatment on this measure. This is what we would expect as parental education does not influence innate child intelligence and supports the evidence from the balancing tests, showing that there is not an underlying difference between the more innate performance IQ of the children of parents either side of the treatment line.

Columns (4) to (7) of Table 9 show that it is in the education measures assessed within school that the RoSLA treatment impact starts to become significant. These assessments are marked according to their own non-comparable scales, so the treatment impact is converted to the proportion of a standard deviation of the outcome variable in the last row of the table. The entry assessment is carried out when children are aged 4 or just turned 5 and have been in school for just one or two months. There is a significant impact of the RoSLA treatment on this measure, approximately 8% of a standard deviation higher results for each parent affected by RoSLA. The size of impact is almost the same for the KS2 and KS3 scores, both statistically significant, with only KS1 scores exhibiting a greater

[21] All of these results are robust to the inclusion of separate quadratic (rather than linear) age splines for pre-, during and post-treatment window.

magnitude of effect at approximately 15% of a standard deviation. Unlike Carneiro *et al.* (2008) we find no fading of the education result as children age.

Table 10 considers separately results for English/literacy, columns (1) to (5), and maths, columns (6) to (10). For English/literacy, the impact of parents' RoSLA treatment is evident at school entry and is stable between 5% and 10% of a standard deviation through assessments taken at different times during the school years – albeit the KS2 effect is only significant at the 10% level and at KS3 the impact is not statistically significant. Similarly, the impact on maths scores is seen at school entry and continues to be significant in each assessment up to the end of the compulsory schooling period. There is a slight suggestion that the impact is increasing in magnitude over time for maths – from 7% of a standard deviation at school entry to 15% at GCSE. Likewise there is some evidence of larger effect on maths than literacy at older ages, but the effect sizes are narrowly insignificantly different.

The impact of RoSLA on the education of children with lower educated parents

We now restrict our focus to children of parents with lower levels of education – the parents most likely to be impacted by the RoSLA. As outlined above, to do this we redefine the treatment variable such that only those who attain less than A-levels are considered "at risk" of RoSLA treatment – as before, provided they are born within ±3 years around 1st September 1957. Therefore, if both of a child's parents have A-level education or higher that child is excluded from the sample, where one parent is considered at risk of treatment but the other is not and has A-levels or higher, this is controlled for in the regression.

Table 11 considers the age 16 outcomes – KS4 total, GCSE total and GCSE academic subjects' total for this reduced sample. We know from the lower panel of Table 7 that the impact of RoSLA on qualification holding amongst parents in this sample was approximately double the impact for the larger sample and we see a similar increase in impact in the reduced form estimates on child outcomes. The RoSLA treatment effect is now ranges from 15 to 24% of a standard deviation, with the largest effect on the broader KS4 scores which include vocational qualifications – in both GCSE based cases the points score increase is equivalent to a one grade increase in three GCSE exams. In each column we see the strong, positive effect of one parent having A-levels equivalent or higher education on child outcomes, mother's having these levels in particular associated with higher child outcome scores. Appendix Tables A2 and A3 shows the effect of RoSLA treatment on the results at different points in the children's education progress, and separate picture for maths and literacy, after selecting on parents in the treatment window without A-levels. As with the full sample, there are no significant impacts on fine or gross motor skills, nor on performance IQ, however each of the assessments from school entry onwards do show significant positive effects. As with the main results shown in Table 11, compared with the full sample the magnitude of these effects is around one-and-a-half times greater.

6 Discussion and Conclusions

The causal impact of parental education on children has potentially important policy implications for intergenerational mobility, especially among lower educated parents. Yet the available evidence from twin, adoptee and policy change studies is inconclusive. Using high frequency, high quality data from the Avon area of the UK we explore the impact of the 1972 Raising of the School Leaving Age on parents' qualifications and child educational outcomes throughout childhood. In summary, the findings suggest that the RoSLA treatment of parents impacts on child outcomes from school age upwards and that the effect does not massively increase between the entry assessment and the exams taken at the end of the compulsory schooling period. There is some variation in impact size over the years – with Key Stage 1 (age 7) results something of an outlier – but overall the impact seems to be significant at the start of school and then steady, without any strong increase in impact size exhibited. While there is some variation in this pattern when results are separated by English/literacy and maths – with larger maths effects in adolescence – these estimates whilst statistically different from zero are for the most part not statistically different to each other. As we might expect, there is no impact of RoSLA treatment on children's gross motor skills nor on their "performance IQ" which is a more innate measure of ability. This and balancing tests on grandparents' education give confidence that there is no selection effects across our treatment and control groups and the effects are causal with the treated post-reform years seeing gains equal to 0.1 standard deviation in test scores and qualifications achieved. The 'complier' group who identify our results are those who have both lower levels of education *and* who are having children in their early to mid-30s. Child bearing in this group is quite common, especially for men but also for women: for example, figures from the British Household Panel Survey suggest that amongst the cohort of women born between 1952 and 1962, who have O-levels or lower qualifications, 36.4% have a child between the ages of 30 and 38[22] i.e. the range of ages for our main estimation sample. Thus it is not the case that low educated women complete their fertility before their 30s.

The policy implications of these results are important with the UK currently planning for a Raising of the Participation Age (that is in full-time education or a job with an apprenticeship) to age 18 by 2015, as they suggest a positive impact on the educational attainment of the next generation results from increasing the schooling of individuals who wish to leave school at the first opportunity. These (future) parents who have low tastes for education or binding credit constraints identify the parental education effect, hence it is a 'local average treatment effect'. However, from a policy point of view this is an extremely important LATE as this group of individuals are most at risk of failing to achieve their own potential and a similar risk applies to the children that they go on to have. This is in line

[22] Author's own calculations using BHPS data pooled waves 1-18 (1991 to 2008).

with previous findings, for example Carneiro *et al.* (2008) find effects of a similar magnitude in the US. The *mechanisms* through which parental education causally affects children's outcomes – the "why" question – remains a very important question for future research to answer, with implications for the design of education and family-related policy.

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Table 1: Summary Statistics for parents in treatment zone, main estimation sample window: parent must be born in the +/- 3 years around 1st September 1957

	Mothers		**Fathers**	
Total number	3550		2815	
Education	N	Proportion	N	proportion
Education info missing	552	0.155	125	0.044
Education info non-missing	2998	0.845	2690	0.956

	Proportion		Proportion	
Highest education level:	…of educ non-missing	…of total	…of educ non-missing	…of total
No qualifications	0.100	0.084	0.095	0.091
Less than O-Level (CSE, Intermediate C&G)	0.118	0.100	0.072	0.069
O-Level or equivalent (Final C&G, apprenticeship)	0.263	0.222	0.250	0.239
A-Level or equivalent (SEN, Full C&G)	0.175	0.148	0.259	0.248
SRN or Teaching Qualification	0.100	0.085	0.017	0.016
Degree	0.244	0.206	0.307	0.293
Total	1.000	0.845	1.000	0.956

Age at ALSPAC child's birth	N	Mean	Std. Dev	Min	Max
Mother	3550	33.31	1.72	30	38
Father	2815	33.50	1.72	30	38

Table 2: Summary Statistics for children main estimation sample: window +/- 3 years around 1st September 1957

	Parental education group								Full Sample		
	1 (least educated)		**2**		**3**		**4** (most educated)				
	mean	**N**	**mean**	**N**	**mean**	**N**	**mean**	**N**	**mean**	**sd**	**N**
Child/Young Person is female (dummy)	0.49	1000	0.46	742	0.48	1314	0.48	1322	0.48	0.50	4967
Child/Young Person's age within sch. Year	6.30	1000	6.17	742	6.15	1314	6.37	1322	6.28	3.71	4967
Key Stage 4 Score	350.65	880	402.11	673	451.49	1071	502.35	988	423.76	146.29	4094
GCSE Total Score	257.72	889	319.41	673	379.37	1078	444.80	989	347.42	143.20	4116
GCSE Total Score (academic subjects)	177.08	835	210.05	659	254.23	1057	300.68	981	235.87	98.07	3985
Fine Motor Skills (18 and 30 months)	80.54	897	82.20	707	83.58	1242	83.58	1271	82.64	9.42	4209
Gross Motor Skills (18 and 30 months)	84.04	896	84.45	707	84.01	1242	82.53	1271	83.59	10.91	4208
IQ, aged 8	96.45	486	101.23	464	107.64	898	114.20	962	106.66	16.62	2877
Performance IQ, aged 8	93.13	488	97.03	466	101.92	903	107.15	961	101.22	17.13	2886
Entry Assessment Score	19.84	741	21.01	539	21.89	794	22.68	641	21.15	3.29	3123
Key Stage 1 Score	8.09	878	9.31	635	10.47	963	11.67	839	9.68	3.74	3791
Key Stage 2 Score	78.82	888	84.20	682	88.27	1120	92.32	1074	85.73	12.13	4253
Key Stage 3 Score	98.33	833	107.66	634	114.38	959	123.56	778	109.81	20.13	3639
Entry Assessment Literacy Score	4.77	741	5.04	539	5.22	794	5.38	641	5.06	0.79	3122
Key Stage 1 Literacy Score	2.62	877	3.00	635	3.41	962	3.81	839	3.14	1.30	3789
Key Stage 2 English Score	25.62	883	27.42	679	28.90	1112	30.33	1067	28.01	4.76	4224
Key Stage 3 English Score	40.68	748	46.06	610	51.86	929	59.70	767	49.06	17.02	3442
English Language GCSE	37.30	812	40.24	651	43.90	1051	48.12	977	42.43	9.10	3931
Entry Assessment Maths Score	4.99	740	5.31	539	5.64	794	5.90	641	5.40	1.11	3120
Key Stage 1 Maths Score	2.85	877	3.31	633	3.65	963	4.05	839	3.40	1.36	3786
Key Stage 2 Maths Score	25.55	883	27.61	676	28.92	1116	30.37	1065	28.05	4.87	4217
Key Stage 3 Maths Score	75.20	796	81.88	624	87.13	939	96.08	767	84.52	21.95	3536
Maths GCSE	34.11	819	38.84	647	43.16	1028	47.81	904	40.84	11.00	3837
GCSE cohort 2007	0.21	1000	0.20	742	0.20	1314	0.16	1322	0.19	0.39	4967
GCSE cohort 2008	0.55	1000	0.58	742	0.50	1314	0.44	1322	0.51	0.50	4967
GCSE cohort 2009	0.12	1000	0.13	742	0.12	1314	0.14	1322	0.13	0.34	4967
GCSE cohort miss	0.11	1000	0.09	742	0.17	1314	0.26	1322	0.17	0.38	4967

Note: the four parental education groups are defined as follows: each parent given score ranging from 0 = no qualifications or below GCSE qualifications, 1= GCSEs, 2 = A Levels, 3 = Degree. The parental education group is the combined parents score: group 1 = 0 or 1, 2 = 2, 3 = 3 to 4 and group 4 = 5 to 6.

Table 3: The impact of RoSLA on educational attainment – comparison of trends pre-policy and post-policy with the impact at the time of the policy

		All individuals			**Those leaving aged 16 or younger**		
		1953/4 to 1955/6 Δ3 years pre-policy	**1956/7 to 1957/8 Δ at policy**	**1958/9 to 1960/1 Δ3 years post-policy**	**1953/4 to 1955/6 Δ3 years pre-policy**	**1956/7 to 1957/8 Δ at policy**	**1958/9 to 1960/1 Δ3 years post-policy**
Mean age left full time education (years)	Men	0.029	**0.287**	0.001	–	–	–
	Women	0.014	**0.272**	0.040	–	–	–
Proportion left school by 15	Men	-0.023	**-0.203**	0.005	–	–	–
	Women	-0.017	**-0.250**	0.010	–	–	–
Proportion with…..No quals	Men	-0.013	**-0.063**	-0.009	-0.028	**-0.129**	-0.034
	Women	-0.009	**-0.109**	-0.004	-0.008	**-0.178**	-0.003
…..NVQ Level 1 quals	Men	0.002	**0.037**	0.011	0.006	**0.072**	0.012
	Women	0.010	**0.066**	-0.001	0.013	**0.103**	0.000
…..NVQ Level 2 quals	Men	0.008	**0.029**	0.012	0.024	**0.064**	0.016
	Women	-0.003	**0.057**	0.005	0.008	**0.089**	0.013
…..NVQ Level 3 quals	Men	0.002	-0.004	-0.008	0.001	0.002	0.000
	Women	0.000	0.004	-0.004	-0.006	0.000	-0.002
…..NVQ Level 4 quals	Men	0.002	-0.009	0.001	0.001	-0.011	0.006
	Women	0.005	-0.015	0.009	-0.002	-0.014	-0.004
…..NVQ Level 5 quals	Men	-0.002	0.010	-0.007	-0.003	0.002	0.001
	Women	-0.003	-0.003	-0.006	-0.004	0.000	-0.003

Notes: Calculations using the Quarterly Labour Force Survey pooled from 1993q1 to 2010q2.

Table 4: Treatment Matrix for the +/-3 year sample

+/- 3 year window

		Fathers					
		Pre-Sample	**Pre-RoSLA**	**Post-RoSLA**	**Post-Sample**	**Missing**	**Total**
Mothers	**Pre-Sample**	262	**0** 46	**1** 23	31	261	**623**
	Pre-RoSLA	**0** 274	**0** 241	**1** 116	**0** 91	**0** 450	**1172**
	Post-RoSLA	**1** 287	**1** 351	**2** 505	**1** 298	**1** 891	**2332**
	Post-Sample	314	**0** 369	**1** 1011	4519	5514	**11727**
	Missing	21	**0** 6	**1** 8	31	4046	**4112**
	Total	**1158**	**1013**	**1663**	**4970**	**11162**	**19966**

Treatment	0	1	2	**Total**
N	1477	2985	505	4967

Table 5: The breakdown of treated versus untreated for parents whose age at child's birth does not map directly into treatment status

	+/- 1 year window			+/- 3 year window or +/- 6 year window		
	Untreated	Treated	Total	Untreated	Treated	Total
Mothers	375	399	774	451	492	943
	48.45%	51.55%	100.00%	47.83%	52.17%	100.00%
Fathers	294	414	708	339	515	854
	41.53%	58.47%	100.00%	39.70%	60.30%	100.00%

Note: The 19 month range in parents' age at child's birth within which parents may be treated (born on or after 1st September 1957) or untreated (born prior to 1st September 1957) is not fully captured by the restriction that parents are born within +/- 1 year of September 1957. However, all parents whose age at child's birth places them in the 19 month range are born with +/- 3 years of September 1957, hence by definition they are all born within +/- 6 years of September 1957 and so the numbers of these samples are the same.

Table 6: Balancing tests of characteristics of treated versus non-treated parents

		+/-1 year window			+/-3 year window		
		Untreated	Treated	Diff.	Untreated	Treated	Diff.
age at first birth	Mother	30.60	30.14	-0.46	31.04	29.25	-1.80***
		310	369		747	1512	
birth order	Mother	2.11	2.06	-0.05	2.15	2.06	-0.09*
		384	457		899	1856	
	Father	1.96	1.96	0.00	1.98	1.84	-0.14***
		397	491		967	1545	
# siblings	Mother	1.45	1.57	0.12*	1.45	1.55	0.10**
		249	290		557	1192	
	Father	1.48	1.48	0.00	1.47	1.50	0.03
		259	329		608	1060	
(grand) mother's	Mother	0.18	0.18	-0.01	0.17	0.19	0.02
education		488	600		1172	2332	
	Father	0.18	0.19	0.01	0.18	0.20	0.02
		413	526		1013	1663	
(grand) father's	Mother	0.18	0.18	0.00	0.16	0.18	0.02
education		488	600		1172	2332	
	Father	0.18	0.21	0.03	0.18	0.21	0.03*
		413	526		1013	1663	
child IQ	Mother	107.91	108.02	0.12	108.19	106.79	-1.40
		272	338		651	1296	
	Father	108.32	106.21	-2.11	108.64	106.39	-2.25**
		275	348		679	1093	
Child	Mother	102.04	102.43	0.38	102.04	101.12	-0.92
“Performance		273	339		656	1298	
IQ”							
	Father	102.96	101.08	-1.89	102.88	101.34	-1.54
		275	348		682	1094	

Table 7: Impact of RoSLA on parents' qualifications, main estimation sample: parents born in +/- 3year window around 1st September 1957

Panel (a) Full sample

	(1) Any qualifications	(2) O levels	(3) A Levels	(4) Any qualifications	(5) O levels	(6) A Levels
RoSLA dummy	0.044***	0.065***	0.028	0.039**	0.060**	0.009
	(0.016)	(0.024)	(0.027)	(0.016)	(0.025)	(0.027)
Parent's DOB in months	0.000	-0.001*	-0.002***	0.017*	0.002	0.031*
	(0.000)	(0.001)	(0.001)	(0.010)	(0.015)	(0.017)
Parent's DOB in months squared				-0.002	-0.001	-0.006***
				(0.001)	(0.002)	(0.002)
Constant	0.866***	0.734***	0.506***	0.844***	0.716***	0.432***
	(0.010)	(0.016)	(0.018)	(0.017)	(0.026)	(0.029)
R-sq	0.007	0.001	0.004	0.008	0.002	0.005
Obs	5512	5512	5512	5512	5512	5512

Panel (b) Sample restricted to parents with less than A-level qualifications

	(1) Any qualifications	(2) O levels	(3) Any qualifications	(4) O levels
RoSLA dummy	0.111***	0.116***	0.109***	0.121***
	(0.033)	(0.041)	(0.034)	(0.042)
Parent's DOB in months	0.001	-0.001	0.001	-0.002
	(0.001)	(0.001)	(0.002)	(0.003)
Parent's DOB in months squared			-0.000	0.000
			(0.000)	(0.000)
Constant	0.680***	0.459***	0.672***	0.477***
	(0.021)	(0.026)	(0.034)	(0.042)
R-sq	0.025	0.008	0.025	0.008
Obs	2479	2479	2479	2479

* p<0.10, ** p<0.05, *** p<0.01

Table 8: The impact of RoSLA on child Key Stage 4 (age 16) outcomes, three windows around the policy change

	+/- 1 year window			+/- 3 year window			+/- 6 year window		
	(1) KS4 Score	(2) GCSE Total Score	(3) GCSE Academic Total	(4) KS4 Score	(5) GCSE Total Score	(6) GCSE Academic Total	(7) KS4 Score	(8) GCSE Total Score	(9) GCSE Academic Total
RoSLA Treatment	19.740***	13.090**	7.164	21.728***	13.617***	13.995***	18.155***	14.372***	10.881***
	(6.916)	(6.591)	(4.731)	(5.055)	(4.848)	(3.451)	(3.883)	(3.698)	(2.654)
Mother's Age at child's birth: pre-window	5.116	2.087	4.527	8.393	-0.383	-2.585	-21.955	-0.547	-8.381
	(10.480)	(12.823)	(9.122)	(12.073)	(13.262)	(9.980)	(36.130)	(39.319)	(33.104)
Mother's Age at child's birth: in window				11.008***	7.002***	8.407***	5.755***	5.901***	5.097***
				(2.071)	(1.988)	(1.375)	(0.909)	(0.861)	(0.606)
Mother's Age at child's birth: post-window	10.443***	10.730***	7.320***	10.536***	11.570***	9.157***	7.462***	10.351***	7.066***
	(2.552)	(2.256)	(1.503)	(1.684)	(1.598)	(1.091)	(1.886)	(1.714)	(1.148)
R squared	0.081	0.100	0.057	0.090	0.099	0.076	0.081	0.102	0.075
Observations	1523	1531	1481	4094	4116	3985	7570	7621	7378
Outcome Mean	430.90	358.02	243.09	423.76	347.42	235.87	416.82	337.31	227.91
Outcome SD	145.65	141.86	97.05	146.29	143.20	98.07	147.87	144.32	97.41
Treatment as a % of SD	**13.55**	**9.23**	**7.38**	**14.85**	**9.51**	**14.27**	**12.28**	**9.96**	**11.17**

* p<0.10, ** p<0.05, *** p<0.01

Additional controls included: child gender, month of birth, GCSE cohort, parent foreign qualifications, missing education info dummy, dummies for father born pre- or post-sample window.

Table 9: The impact of RoSLA on child outcomes throughout childhood, +/-3 year sample window

	(1) Fine Motor Skills (18 and 30 months)	(2) Gross Motor Skills (18 and 30 months)	(3) Performance IQ, aged 8	(4) Entry Assessment Score	(5) Key Stage 1 Score	(6) Key Stage 2 Score	(7) Key Stage 3 Score
RoSLA Treatment	0.294	-0.125	0.077	0.262**	0.561***	0.853**	1.834**
	(0.324)	(0.386)	(0.698)	(0.129)	(0.135)	(0.403)	(0.738)
Mother's Age at child's birth: pre-window	0.598	1.300*	1.583	-0.090	0.055	-0.449	0.444
	(0.758)	(0.780)	(2.161)	(0.488)	(0.455)	(1.110)	(1.981)
Mother's Age at child's birth: in window	0.068	0.069	0.573*	0.141***	0.229***	0.543***	0.960***
	(0.134)	(0.178)	(0.293)	(0.053)	(0.053)	(0.169)	(0.309)
Mother's Age at child's birth: post-window	-0.070	-0.337***	0.849***	0.230***	0.216***	1.033***	1.339***
	(0.111)	(0.128)	(0.242)	(0.041)	(0.044)	(0.145)	(0.239)
R squared	0.034	0.017	0.025	0.095	0.102	0.077	0.086
Observations	4209	4208	2886	3123	3791	4253	3639
Outcome Mean	82.64	83.59	101.22	21.15	9.68	85.73	109.81
Outcome SD	9.42	10.91	17.13	3.29	3.74	12.13	20.13
Treatment as % of SD	**3.12**	**-1.14**	**0.45**	**7.98**	**14.98**	**7.03**	**9.11**

* p<0.10, ** p<0.05, *** p<0.01

Additional controls included: child gender, month of birth, GCSE cohort, parent foreign qualifications, missing education info dummy, dummies for father born pre- or post-sample window. Child age in months is included as an additional covariate for the early development scores.

Table 10: The impact of RoSLA on child English/literacy and Maths outcomes throughout childhood, +/-3 year sample window

	(1) Entry Assessment Literacy	(2) KS1 Literacy Score	(3) KS2 English Score	(4) KS3 English Score	(5) English Language GCSE	(6) Entry Assessment Maths	(7) KS1 Maths Score	(8) KS2 Maths Score	(9) KS3 Maths Score	(10) Maths GCSE
RoSLA Treatment	0.057*	0.171***	0.274*	0.940	0.870***	0.078*	0.212***	0.374**	2.473***	1.688***
	(0.031)	(0.047)	(0.159)	(0.644)	(0.311)	(0.045)	(0.049)	(0.167)	(0.853)	(0.395)
Mother's Age at child's birth: pre- window	-0.104	-0.034	-0.366	0.848	0.259	0.054	0.121	-0.105	1.265	-0.871
	(0.100)	(0.158)	(0.442)	(1.476)	(0.642)	(0.183)	(0.159)	(0.452)	(2.337)	(1.439)
Mother's Age at child's birth: in window	0.021*	0.064***	0.190***	0.561**	0.489***	0.056***	0.098***	0.205***	1.271***	0.773***
	(0.013)	(0.019)	(0.066)	(0.267)	(0.131)	(0.017)	(0.020)	(0.068)	(0.333)	(0.165)
Mother's Age at child's birth: post- window	0.063***	0.064***	0.355***	1.126***	0.648***	0.063***	0.086***	0.353***	0.803***	0.699***
	(0.010)	(0.015)	(0.058)	(0.197)	(0.103)	(0.014)	(0.016)	(0.061)	(0.268)	(0.126)
R squared	0.095	0.110	0.088	0.100	0.089	0.067	0.072	0.055	0.040	0.068
Obs	3122	3789	4224	3442	3931	3120	3786	4217	3536	3837
Outcome: Mean	5.06	3.14	28.01	49.06	42.43	5.40	3.40	28.05	84.52	40.84
SD	0.79	1.30	4.76	17.02	9.10	1.11	1.36	4.87	21.95	11.00
Treatment % SD	**7.24**	**13.14**	**5.75**	**5.52**	**9.55**	**7.05**	**15.60**	**7.67**	**11.27**	**15.34**

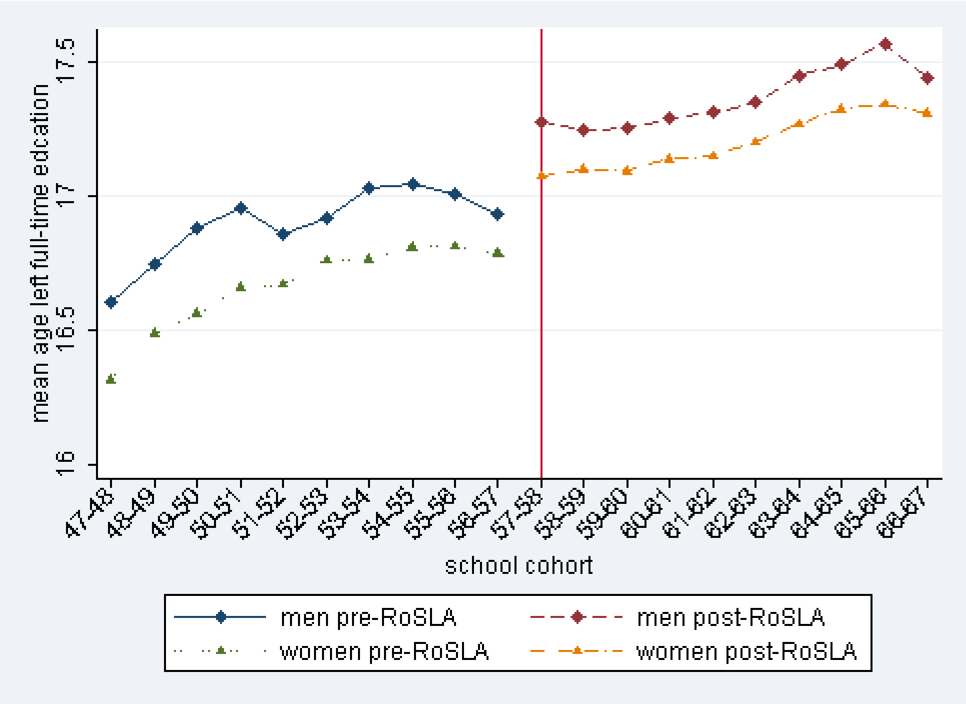
* p<0.10, ** p<0.05, *** p<0.01. Additional controls included: child gender, month of birth, GCSE cohort, parent foreign qualifications, missing education info dummy, dummies for father born pre- or post-sample window.

Table 11: The impact of RoSLA on child Key Stage 4 (age 16) outcomes, parents with lower levels of education, +/-3 year sample window

	(1) Key Stage 4 Total	(2) GCSE Total	(3) GCSE Academic Total
RoSLA Treatment	35.847*** (7.236)	20.567*** (6.427)	17.011*** (4.412)
Mother's Age at child's birth: pre-window	28.264 (22.735)	10.689 (21.610)	-2.257 (12.947)
Mother's Age at child's birth: in window	12.643*** (2.529)	6.460*** (2.301)	7.861*** (1.541)
Mother's Age at child's birth: post-window	8.436*** (2.108)	9.005*** (1.858)	6.007*** (1.304)
Mother has A levels, equivalent or higher	81.260*** (9.658)	90.509*** (8.844)	59.150*** (6.612)
Father has A levels, equivalent or higher	60.502*** (8.634)	71.325*** (7.946)	48.505*** (5.400)
R squared	0.098	0.115	0.098
Observations	2452	2467	2359
Outcome Mean	385.52	302.04	205.03
Outcome SD	147.43	138.85	90.97
Treatment as % of SD	**24.31**	**14.81**	**18.70**

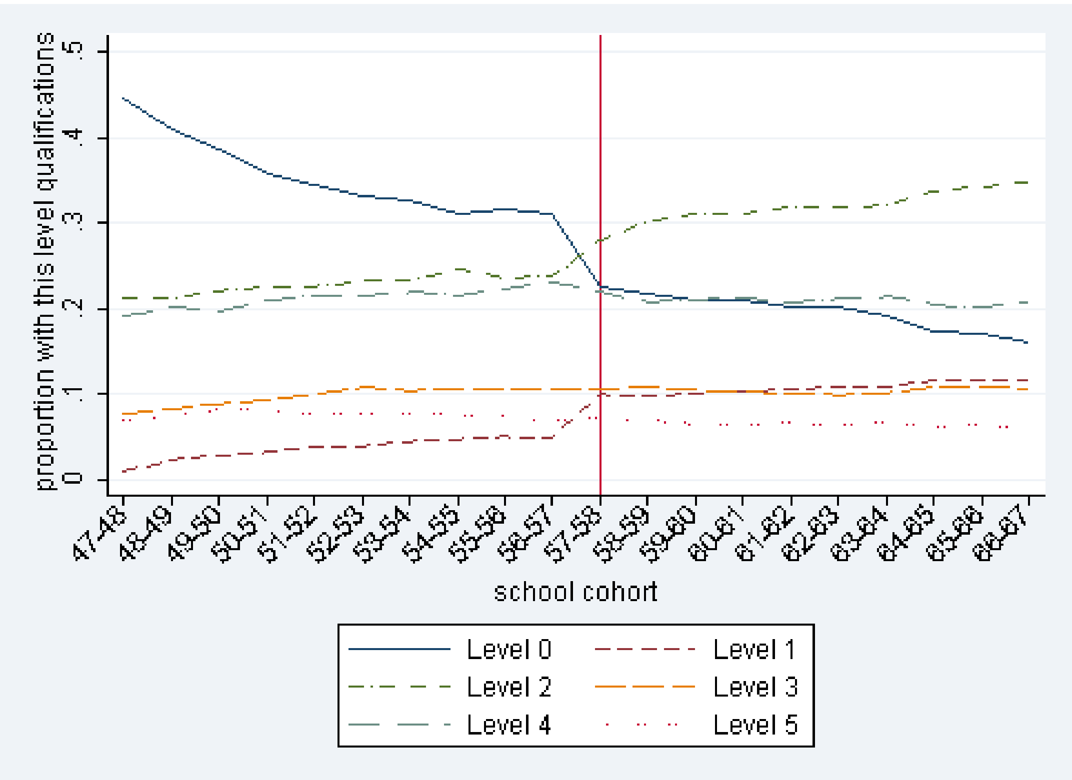
* p<0.10, ** p<0.05, *** p<0.01
Additional controls included: child gender, month of birth, GCSE cohort, parent foreign qualifications, missing education info dummy, dummies for father born pre- or post-sample window.

Figure 1: Mean age left full-time education, by birth (school) cohort and sex



Source: Quarterly Labour Force Survey, pooled 1993q1 to 2010q2

Figure 2 Qualification Attainment Levels in NVQ equivalence scale, by birth (school) cohort



Source: Quarterly Labour Force Survey, pooled 1993q1 to 2010q2

Figure 3: Schematic representation of the overlap in age range for treated and untreated parents

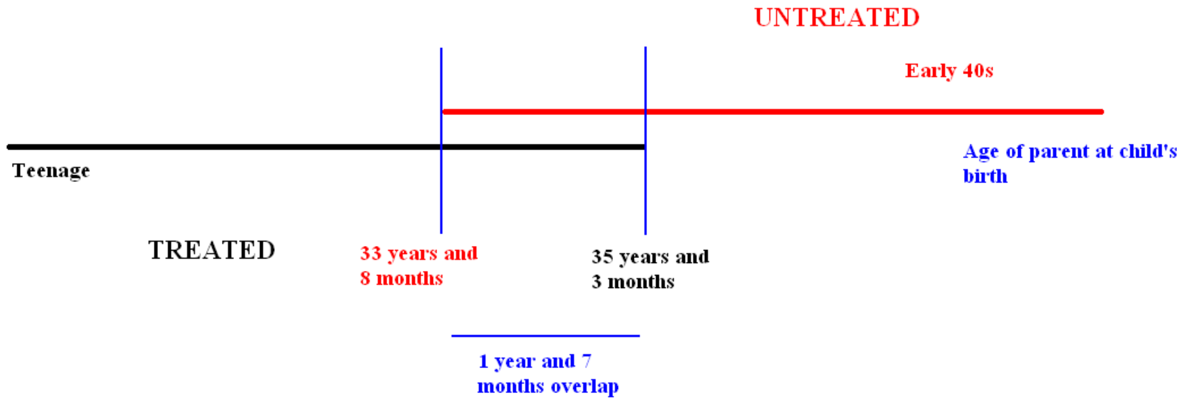
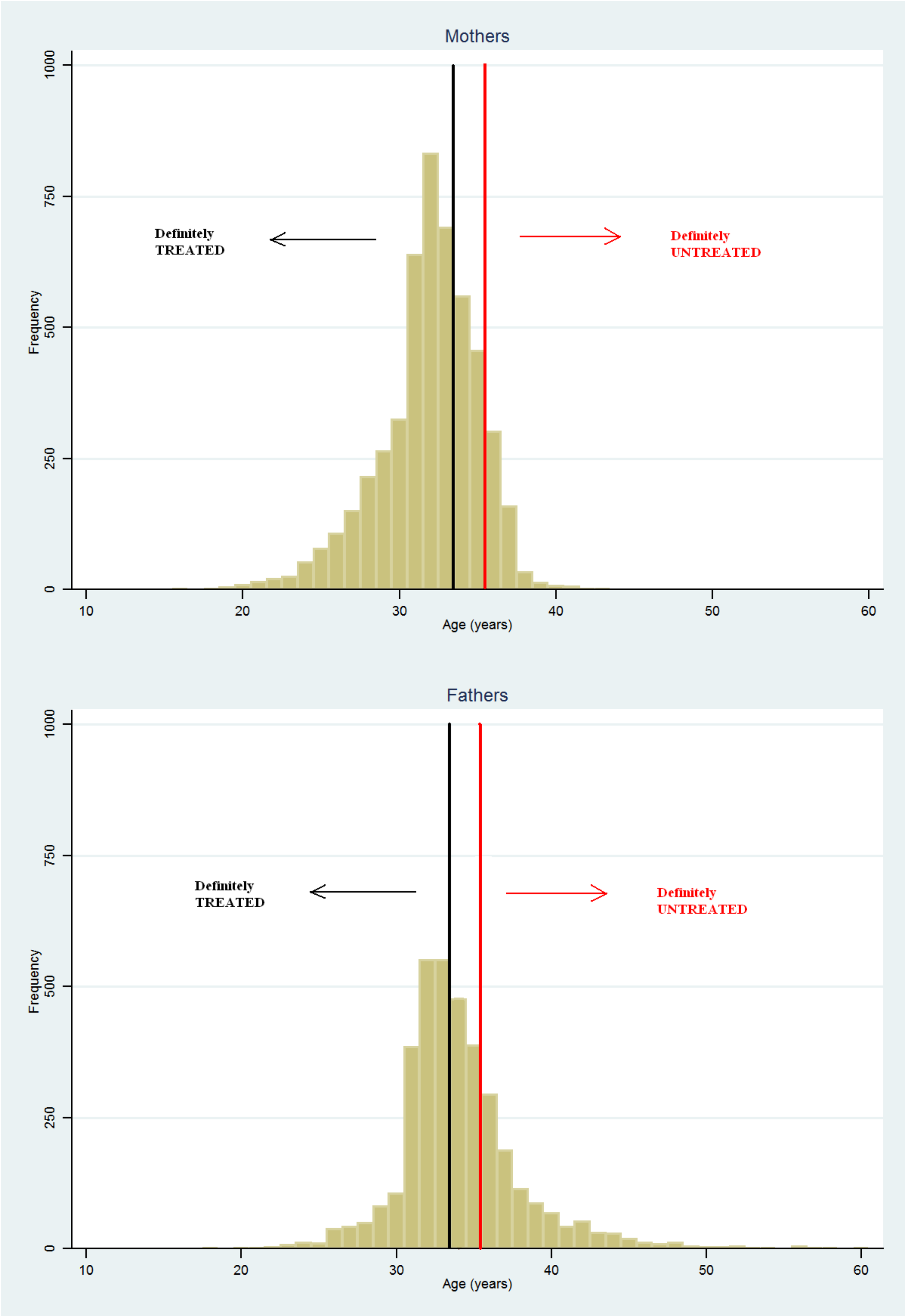


Figure 4: Age of parent at child's birth and mapping to RoSLA treatment status, by parent



Appendix A – Additional Tables

Appendix Table A1: National Vocational Qualifications Equivalent Qualifications Classification

NVQ equivalent	Academic qualification
Level 0	No nationally recognised academic qualifications
Level 1	CSE below grade 1, GCSE below grade C
Level 2	CSE grade 1, O-levels, GCSE grade A-C
Level 3	A-levels, A/S levels, SCE Higher, Scottish certificate of sixth year studies, international baccalaureate
Level 4	First/foundation degree, other degree, diploma in higher education
Level 5	Higher degree

Appendix Table A.2: The impact of RoSLA on child outcomes throughout childhood, parents with lower levels of education, +/-3 year sample window

	(1) Fine Motor Skills (18 and 30 months)	(2) Gross Motor Skills (18 and 30 months)	(3) Performance IQ, aged 8	(4) Entry Assessment Score	(5) Key Stage 1 Score	(6) Key Stage 2 Score	(7) Key Stage 3 Score
RoSLA Treatment	0.400	-0.377	1.112	0.392**	0.817***	1.298**	2.403**
	(0.511)	(0.609)	(1.076)	(0.170)	(0.184)	(0.601)	(1.003)
Mother's Age at child's birth: pre-window	-3.007	-1.292	2.908	-0.569	0.526	1.211	3.984
	(1.996)	(1.658)	(4.542)	(0.959)	(0.484)	(2.329)	(3.543)
Mother's Age at child's birth: in window	0.145	0.143	0.963**	0.164***	0.253***	0.521**	0.978***
	(0.180)	(0.249)	(0.400)	(0.063)	(0.065)	(0.217)	(0.355)
Mother's Age at child's birth: post- window	-0.165	-0.322**	0.590*	0.133***	0.160***	0.650***	0.931***
	(0.148)	(0.155)	(0.335)	(0.050)	(0.054)	(0.193)	(0.292)
Mother has A levels, equivalent or higher	1.981***	-1.663**	8.566***	1.426***	1.920***	6.134***	10.252***
	(0.608)	(0.731)	(1.373)	(0.246)	(0.249)	(0.780)	(1.363)
Father has A levels, equivalent or higher	2.382***	0.457	4.997***	0.837***	1.409***	5.205***	9.622***
	(0.555)	(0.653)	(1.228)	(0.215)	(0.220)	(0.695)	(1.196)
R squared	0.040	0.036	0.056	0.095	0.104	0.071	0.088
Observations	2186	2185	1381	2008	2358	2498	2285
Outcome Mean	81.69	83.91	97.31	20.48	8.82	82.27	104.17
Outcome SD	10.00	11.44	17.11	3.27	3.73	12.43	19.70
Treatment % of SD	**4.00**	**-3.30**	**6.50**	**12.00**	**21.87**	**10.45**	**12.19**

* p<0.10, ** p<0.05, *** p<0.01. Additional controls included: child gender, month of birth, GCSE cohort, parent foreign qualifications, missing education info dummy, dummies for father born pre- or post-sample window. Child age in months is included as an additional covariate for the early development scores.

Appendix Table A.3(i): The impact of RoSLA on child English/literacy outcomes throughout childhood, parents with lower levels of education, +/-3 year sample window

	(1) Entry Assessment Literacy	(2) KS1 Literacy Score	(3) KS2 English Score	(4) KS3 English Score	(5) English Language GCSE
RoSLA Treatment	0.110*** (0.041)	0.251*** (0.064)	0.248 (0.232)	1.326 (0.849)	1.031** (0.436)
Mother's Age at child's birth: pre- window	-0.108 (0.197)	0.066 (0.149)	0.007 (0.792)	4.630** (2.187)	2.452* (1.434)
Mother's Age at child's birth: in window	0.031** (0.015)	0.070*** (0.022)	0.200** (0.084)	0.570* (0.300)	0.480*** (0.162)
Mother's Age at child's birth: post- window	0.042*** (0.012)	0.041** (0.019)	0.215*** (0.076)	0.663*** (0.231)	0.406*** (0.126)
Mother has A levels, equivalent or higher	0.300*** (0.060)	0.615*** (0.087)	2.009*** (0.310)	7.577*** (1.185)	4.889*** (0.564)
Father has A levels, equivalent or higher	0.194*** (0.051)	0.473*** (0.076)	1.610*** (0.272)	6.235*** (1.010)	3.397*** (0.565)
R squared	0.096	0.115	0.085	0.117	0.105
Obs	2007	2356	2479	2119	2316
Outcome Mean	4.92	2.85	26.79	44.62	39.69
Outcome SD	0.79	1.30	4.87	16.26	9.24
Treatment as % of SD	14.02	19.29	5.09	8.16	11.16

* p<0.10, ** p<0.05, *** p<0.01. Additional controls included: child gender, month of birth, GCSE cohort, parent foreign qualifications, missing education info dummy, dummies for father born pre- or post-sample window.

Appendix Table A.3(ii): The impact of RoSLA on child Maths outcomes throughout childhood, parents with lower levels of education, +/-3 year sample window

	(6) Entry Assessment Maths	(7) KS1 Maths Score	(8) KS2 Maths Score	(9) KS3 Maths Score	(10) Maths GCSE
RoSLA Treatment	0.118** (0.058)	0.309*** (0.068)	0.633** (0.249)	2.614** (1.149)	2.394*** (0.548)
Mother's Age at child's birth: pre- window	-0.178 (0.285)	0.389* (0.206)	0.352 (1.117)	2.055 (4.336)	-1.200 (2.952)
Mother's Age at child's birth: in window	0.057*** (0.021)	0.108*** (0.024)	0.202** (0.088)	1.394*** (0.399)	0.904*** (0.201)
Mother's Age at child's birth: post- window	0.035** (0.016)	0.077*** (0.021)	0.235*** (0.081)	0.526 (0.329)	0.437*** (0.155)
Mother has A levels, equivalent or higher	0.468*** (0.087)	0.685*** (0.095)	2.150*** (0.335)	6.833*** (1.634)	6.075*** (0.720)
Father has A levels, equivalent or higher	0.311*** (0.071)	0.481*** (0.082)	1.989*** (0.287)	9.494*** (1.304)	5.899*** (0.629)
R squared	0.068	0.075	0.054	0.046	0.085
Obs	2005	2353	2476	2203	2313
Outcome Mean	5.18	3.13	26.84	80.23	37.58
Outcome SD	1.09	1.37	5.00	21.43	10.98
Treatment as % of SD	10.86	22.50	12.65	12.20	21.80

* p<0.10, ** p<0.05, *** p<0.01. Additional controls included: child gender, month of birth, GCSE cohort, parent foreign qualifications, missing education info dummy, dummies for father born pre- or post-sample window.

Appendix B – Data Description

ALSPAC

We have used data from the Avon Longitudinal Study of Parents and Children (ALSPAC), which consists of children who were expected to be born between 1st April 1991 and 31st December 1992 in the Avon area, an area including and surrounding Bristol in the UK. All mothers with children due during this period in this area were invited to join the study, resulting in 14,062 live born children, 13,971 of whom were alive at 12 months, representing 13,801 mothers.

Additionally eligible children who were found in external education data but who were not in the core ALSPAC sample were added to the datasets. In total we have 19,966 observations including 14,663 children from the core sample and 5,303 eligible children added later, excluding triplets and quadruplets as the external data is unavailable for these children due to confidentiality concerns.

The data from the study includes information from survey questionnaires completed by the mothers, the mothers' partners and the study children. Further to the questionnaires there were several "clinics" during which children completed various types of tests and questionnaires on more sensitive topics; these occurred at ages 7, 8, 9, 10, 11, 12, 15 and 17. Data from other sources has also been linked in, including Annual School Censuses, at school and pupil level and children's school test results for all Key Stages and Entry Assessment.

Variables Constructed: Parental Date of Birth

The treatment of the study children is determined by whether their parents were affected by the "Raising of the School Leaving Age" that affected those turning 15 on or after 1 September 1972, i.e. those born on or after 1 September 1957. To determine treatment status requires parents' date of birth which is not directly recorded in ALSPAC. However, we were able to use answers to other questions to determine parents' year and month of birth. First we calculated a "benchmark" estimate from the good quality, clean data available and then used further information to construct more estimates to compare with the benchmark. If the majority of available further estimates were within a month of the benchmark we considered the benchmark validated. The process was slightly different for maternal and paternal estimates.

Maternal Date of Birth: For the benchmark we use mothers' age in months at their child's delivery. This was available for 15,995 observations, while study child's month and year of delivery is available for all study children. Therefore we could precisely calculate mothers' month and year of birth for all observations and determine whether they were treated or not by RoSLA. There are 3,108 observations with only a benchmark estimate, 12,792 observations with a validated benchmark estimate and 95 observations with a benchmark estimate that was not successfully validated, so the benchmark was used for 15,900 observations, while the date of birth was considered "missing" for the

95 observations with an invalid benchmark; the date of birth was considered missing also for all observations without a benchmark estimate.

Paternal Date of Birth: A reported month and year of birth was included in a questionnaire completed by, or on behalf of, the mothers' partners when the study child was approximately 8 months old. A reported month of birth was also included in questionnaires completed during pregnancy and when the child was 8 weeks old. Where the reported month of birth was the same in the majority of these three datasets, it was used along with the reported year of birth to calculate fathers' date of birth (month and year). This estimate was available for 6,510 observations, of which 6,304 were validated and a further 17 observations had no other available estimates. Like for maternal date of birth, the benchmark was used as the final estimate for all observations where the benchmark was valid or the only available estimate. However since there were many observations where there was no benchmark estimate, for these observations and observations where the benchmark was not validated, we found the median estimate (excluding the benchmark case) and used the same validation process on the median estimate as we had on the benchmark. This gave us a validated estimate for 53 of the invalid-benchmark observations and 2,455 observations with no benchmark estimate.

To see which estimates are used for each parent, see the table below:

Table 1: Type of Date of Birth Estimate

Father	Mother		Total
	Benchmark Estimate	Estimate Missing	
Benchmark Estimate	6,300	21	6,321
Median Estimate	2,601	21	2,622
Estimate Missing	6,999	4,024	11,023
Total	15,900	4,066	19,966

Variables Constructed: Treatment

The parents' treatment group is determined by three conditions:

Date of Birth: A sample window of ± 3 years, ± 6 years or ± 1 year is chosen and based on that, parents are treated, untreated or ineligible for treatment. If their date of birth is outside the sample window, i.e. either more than 3 years, 6 years or 1 year (depending on chosen window) away from the date of birth corresponding with RoSLA (1 September 1957) then the parent is ineligible for treatment. If they are born in the window before September 1957 they are untreated and if they are in the window on/after 1 September 1957 they are treated.

Foreign Education: RoSLA only affected those in the English and Welsh teaching systems so we have tried to identify parents who may have been in foreign education systems. In the child-based questionnaires at ages 4 years 9 months, 5 years 9 months and 6 years 9 months there is information on whether English is the only main language of the mother and their partner. If at any of these ages it is reported that English is not their main language or not their only main language then we believe they may have been educated abroad and so we consider them ineligible for treatment.[23]

Parental relationship conditions (fathers only): Since for the most part the date of birth estimate for fathers (i.e. mothers' partners) is taken early, we want to make sure that this partner is the child's main father figure and has spent most of the child's life in the same household. The mother is asked about the length of her marriage when the child is approximately 10 years old and the length of her cohabiting relationship when the child is approximately 12 years old. Using the resulting variables we can identify cases where the current partner is not the same as when the child was 8 months old (when the benchmark estimate is reported) and so we can make these fathers ineligible for treatment.[24]

The combined treatment variable is the number of treated parents, after considering all three conditions. If neither parent is eligible for treatment then the variable has no value, if both parents are untreated or if one is untreated and the other is ineligible then the treatment value is zero; if one is treated and the other is untreated or ineligible the treatment value is one; if both parents are treated then the treatment value is two.

Variables Constructed: Parent and Child Education Outcomes

Parents

In our first stage regressions we use three dummy variables to measure parents' educational attainment, as reported by the study child's mother. These variables are for whether the parent has any qualifications, has any O Levels and has any A levels. Qualifications included in "Any Qualifications" include CSE, vocational and skill qualifications, apprenticeships, intermediate, full and final City & Guilds, State Enrolled Nurse, State Registered Nurse, teaching qualifications, degrees, O and A levels. Unfortunately there was no information on number of each type of qualification or grades. The information is taken from the mothers' questionnaire during pregnancy (at approximately 32 weeks gestation).

[23] In a robustness check for parents where both are missing information on main language we drop any observations for which either the child or household are reported as having a main language that is not English. The child's language data is teacher-reported in the Pupil Level Annual School Census; the household data is from child-based questionnaires at 3 years 2 months, 4 years 9 months, 5 years 9 months and 6 years 9 months.

[24] In robustness checks we make fathers ineligible if the mother has not been in the same relationship since the child was under a year old. In the "relaxed" robustness test, relationships must last until the child is at least 10 years old; in the "strict" test the relationship must have lasted until the child was at least 12 years old.

Children

IQ: This is taken from the Focus 8+ Clinic, to which all ALSPAC study children were invited at around 8 years of age. The children were measured using the Wechsler Intelligence Scale for Children, specifically the WISC-III ^{UK}, which was the most up-to-date at the time of the clinic. We use the total score, a sum of 10 subscales (split into verbal and performance categories) which are age-adjusted. The verbal subscales are: information, similarities, arithmetic, vocabulary and comprehension. The performance subscales are picture completion, coding, picture arrangement, block design and object assembly.

Early Development: We have mother-reported measures of child development in several areas from the early child-based questionnaires. We use Gross and Fine Motor Skills scores and Communication scores. The Gross and Fine motor skills scores are averages of scores taken from questionnaires when the child is aged 18 and 30 months, scaled between 0 and 100. Communication scores are available for ages 15, 18, 24 and 38 months[25], however the score at 18 months is much less detailed. We rescale these scores so they also range from 0 to 100, and create a mean Early Communication score using the more detailed measures at 15, 24 and 38 months. We do not adjust the scores for age but we do include age when measured in regressions as controls when using these dependent variables.

Entry Assessment: These measures are teacher-assessed in the child's first term of Reception (age 4 to 5 years), generally in late October/early November. These were not compulsory nationally at the time the ALSPAC children were being assessed, but the same system was used in about 80% of schools in the Avon area at the time. The Entry Assessments included both cognitive and behavioural measures, all measured on a scale from 2 to 7. We have constructed a total (prorated) from the results for Reading, Writing, Language and Maths, and have also looked at Maths and Literacy individually, using the mean of Reading and Writing for our Literacy measure[26]. Unfortunately there is no data for Entry Assessments for children who were not in the LEAS of Bristol, South Gloucestershire, Bath and North East Somerset, but the dataset from Bristol LEA included eligible children who were not already in the ALSPAC sample.

Key Stage 1: Key Stage 1 testing occurs when the children are aged 6 to 7 years and at the time the ALSPAC children were being assessed it included components measured by standardised national

[25] For the more detailed Communication measures, the score has the following subscales:

15 months: nonverbal communication, vocabulary, understanding.

24 months: vocabulary, grammar, past tense, plurals.

38 months: vocabulary, combining words, past tense, plurals.

We use the raw sum of these subscales, and then rescale for our final score.

[26] Where only one measure of Reading and Writing was available, that was used for Literacy, i.e. the Literacy score is the mean of the available scores for Reading and Writing.

tests and also teacher assessment. The teacher assessment results were not available for all ALSPAC school years so only the standardised test results are available. These cover Reading, Writing and Maths. As with Entry Assessment we combine the Reading and Writing results to create a Literacy measure, and also create a prorated total score. The results reported are Levels that are dictated by the Department for Children, Schools and Families and are used for Assessment at Key Stages 1 to 3. The table below indicates how the levels should be understood in terms of child development.

Table 2: Key Stage Levels[27]

Key Stage	Range of levels within which most children will work	Target that most children reach by the end of the key stage
1	1 – 3	2
2	2 – 5	4
3	3 – 7	5-6

The results in ALSPAC include a breakdown of level 2, to sublevels 2A, 2B and 2C, where 2A is the highest achievement and 2C is the lowest. Some children reach level 4 in assessment but this is not available to all children because it requires testing with Key Stage 2 materials and is only attained by a few, so the data combines these children with those reaching Level 3. Thus the data is coded:

Table 3: Key Stage 1 Variables

Key Stage Level	Value
Working towards Level 1	0
Level 1	1
Level 2C	2
Level 2B	3
Level 2A	4
Level 3 or Higher	5

Key Stage 2: Key Stage 2 assessment occurs at ages 10 to 11 and again results are in terms of levels, which are coded as follows:

Table 4: Key Stage 1 Variables[28]

Key Stage Level	Value
Working towards Level of Test	15
Not Award A Test Level	15
Level 2	15

[27] Source: ALSPAC dataset documentation, originally taken from Department for Children, Schools and Families (DCSF) website.

[28] http://www.bris.ac.uk/cmpo/plug/support-docs/ks2userguide2011.pdf [Accessed 6 November 2011]

Level 3	21
Level 4	27
Level 5	33

English, Maths and Science are assessed using standardised tests, and we construct a prorated total from the results for these.

Key Stage 3: At age 13 to 14, children face Key Stage 3 assessments using standardised national tests. English is scored out of 100; Maths and Science are both scored out of 150. A prorated Total is constructed from the total supplied in the dataset (used in the value-added calculations) and the reported number of subjects included. In the data this Total ranges from 0 to 141.

Key Stage 4: Assessments occur age 15 to 16, including GCSEs as well as other vocational qualifications, which are designed to be graded equivalently to GCSEs. The following table explains the grading system for GCSEs and their equivalents.

Table 5: GCSE Points Scores[29]

Grade	Points
A*	58
A	52
B	46
C	40
D	34
E	28
F	22
G	16

The measures we consider are total Key Stage 4 points (the sum of all GCSE-equivalent points for all Key Stage 4 qualifications, the total points for GCSEs, the total points for traditional academic GCSEs[30], the score for Maths GCSE and the score for English Language GCSE (as a measure of literacy).

Variables Constructed: Controls

Parent education: While being used dependent variables in the "first stage" of the analysis, parental education is also used as a control. There is a pair of dummy variables for each parent, one dummy for

[29] Table 4.1 in Washbrook, 2010, ***Early Environments and Child Outcomes:*** *An Analysis Commission for the Independent Review on Poverty and Life Chances*, University of Bristol

[30] Included GCSEs: Maths, English Language, English Literature, Geography, History, French, German, Italian, Russian, Spanish, Single/Double award Science, Biology, Chemistry, Physics.

whether the parent has A levels, equivalent or more[31] and a second dummy for whether their education information is missing.

Foreign Education: As outlined in the Treatment section above, RoSLA only affected those in the English and Welsh teaching systems. We have control dummies containing the information in the method above, plus dummies recording whether there is no available information for each parent, which are used as controls so we pick up any effects when one parent is eligible for treatment but the other is believed to have a high probability of a foreign education.

Siblings: Based on information from a child-based questionnaire at age 11 years 8 months we have a set of dummies for the child's siblings: no siblings (i.e. only child), one sibling, more than one sibling, missing information. At this age this constitutes the mother's lifetime fertility for most children. The siblings include all siblings living in the home of the study child, and do not need to be full siblings.

Child demographics: We use child's sex and month of birth (from the Sample Definition dataset which uses information from hospital records) and child's GCSE cohort (taken from the Key Stage 4 dataset), except for the early development dependent variables, when age at questionnaire completion is used instead of month of birth and GCSE cohort.

Date of Birth groups: For each parent we include a set of dummies for whether they were born before the treatment window, after the treatment window, or if they have missing Date of Birth information. These are used to control for these effects where one parent was eligible for treatment (whether treated or untreated) but the other was not because they were not known to be born in the sample window.

Age at child's birth: For each parent there are three main age-at-birth variables, all with the parent's age of birth in whole years, constructed using the parent's date of birth estimate and the child's month and year of birth (from the Sample Definition dataset). The variables are conditional on the parent's date of birth, specifically whether they are born before, during or after the sample window. The values of the variables are zero if the parent was not born in the relevant time period, and is the parent's age at the study child's birth if they were born in the time period. In most of the main regressions only the mother's age is included as a control, but when treatment effects are father-specific then the father's age is used.[32]

[31] This is based on the National Qualifications Framework, where A levels are Level 3 qualifications. The dummy captures whether parents have A levels, degrees, full City & Guild qualifications, teaching qualifications or are a State Enrolled or State Registered Nurse.

[32] We also test for sensitivity to the imposed linearity by including the parent's age-at-birth squared.