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ORIGINAL PAPER

Parental inputs and socio-economic gaps in early child development

Lindsey Macmillan[1] · **Emma Tominey**[2]

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Abstract
Around 1 in 5 individuals in OECD countries leave school without basic qualifications, impacting their own later life outcomes and those of their children. We document the impact of a compulsory schooling reform in England, which raised the education of the marginal mother from leaving school with no qualifications to having at least a basic level of qualifications, on their children's cognitive and socio-emotional outcomes in childhood. We further estimate the causal effect of this reform on a range of parental inputs, which we show are associated with children's human capital development. Our results suggest that family resources and parental investments, including health behaviours during pregnancy and monetary investments at home, are causally impacted by the educational reform and, when coupled with their association with human capital, can each explain between 12 and 60% of the effect of the reform on the second generation's skills.

Keywords Child development · Test scores · Socio-emotional skills · Parental inputs · Decomposition · ALSPAC

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✉ Emma Tominey
emma.tominey@york.ac.uk

Lindsey Macmillan
l.macmillan@ucl.ac.uk

[1] UCL Institute of Education; IFS, London, UK

[2] University of York; HCEO; IZA, York, UK

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1 Introduction

Across OECD countries, 17% of 25–34-year-olds lack a minimum level of upper secondary qualifications (OECD 1998). There is a strong socio-economic gradient where, in the UK for example, 18% of all school leavers and 37% of disadvantaged school leavers lack basic qualifications (Children's Commissioner 2019). The consequence of leaving education with no qualifications is felt by the individuals, but given strong intergenerational links in human capital, may also be borne by their children. Indeed, a well-documented socio-economic gap in the skills of children exists from an early age in terms of both cognitive and socio-emotional skills.[1] These early gaps widen across the child's life and drive early adult outcomes including college attendance, crime, and health.[2][3]

Whilst the effect of parents' qualifications on the next generations' skills has been estimated (see for example Dickson et al. 2016), the literature is "silent about the mechanisms" through which increasing the education of parents may lead to higher skills in their children (Francesconi and Heckman 2016, p.F22). This is where we make a significant contribution to the literature, investigating the causal effect of mothers' educational qualifications on a wide set of parental inputs which themselves drive children's cognitive and socio-emotional outcomes.[4]

When considering which parental inputs respond to mothers' schooling, an obvious place to start is to draw on the extensive literature on the returns to education within a generation. An increase in mothers' education is likely to increase the financial resources available to families, directly through their own capital accumulation and labour market earnings, but also through their choice of partner and therefore their combined income, which can increase child skills (see Dahl and Lochner 2012; and Carneiro et al. 2021). A potentially less obvious impact of the policy is that an increase in mothers' education may also change the type of direct parental investments made in their children, including health behaviours in pregnancy, their home learning environment, and the quantity and quality of time spent with their child.[5][6] Therefore, when considering the channels through which mothers' education raises

[1] Feinstein (2003); Doyle et al. (2009) and Washbrook and Waldfogel (2011) provide evidence for cognitive skills gaps; Kalil (2015); Washbrook and Waldfogel (2011) estimate socio-emotional skills gaps.

[2] see Heckman and Rubinstein (2001); Carneiro and Heckman (2003); Heckman et al. (2006, 2013); Kautz et al. (2014).

[3] A large literature has estimated the causal effect of parents' education on the completed education of their children, dating back to Black et al. (2005) and reviewed in Holmlund et al. (2011). Instead in this paper, we focus on the effect of parents' education on early skills of children.

[4] The effect fathers' education on child skills is an important question but the survey design of our data focuses on the mother and her partner — rather than the father. Fathers enter the analysis firstly by considering how mothers' education drives assortative mating patterns and in Section 7 when we consider the effect on children of "double treatment", where both the mother and her partner were treated by RoSLA.

[5] For the literature on the effect of time investments in children on their skills, see Del Boca et al. (2014); Attanasio et al. (2017)

[6] Doepke and Zilibotti (2017); Moroni et al. (2019) have examined the role of quality of parenting through parenting style on child development.

children's skills, it is important to incorporate both dimensions of potential parental inputs in children — financial resources and parental investments.

Our analysis exploits the rich dataset of the Avon Longitudinal Study of Parents and Children (ALSPAC) to identify the causal effect of mothers' education on parental inputs of family resources and parental investments. We focus on parent inputs at a crucial stage of child development early in the child's lifetime, before they start school (Cunha and Heckman 2008). The family resources we measure include pre-birth human capital (home ownership, marital status, employment history), family income, employment (participation and hours in the labour market of mothers), wellbeing, and measures of assortative mating (partner's education, partner's employment, quality of relationship). We also consider a range of parental investments: mothers' health during pregnancy (smoking and alcohol consumption), monetary investments (purchases such as books and educational toys), time investments, parenting style measures, including permissive, authoritarian and authoritative, and the number of siblings or fertility choices.

To give a causal interpretation to the effect of mothers' schooling on parental inputs, we exploit an exogenous change in education from the raising of the school leaving age (RoSLA). The policy extended the compulsory schooling age in England from 15 to 16 in 1972, enticing those who would otherwise have left school at 15 to stay on for 1 additional year. This additional year coincides with important qualifications taken at the end of secondary school at age 16 in England and hence the treatment not only raised the years of schooling but also lowered the probability of leaving school with no qualifications. The low-educated marginal individuals who were enticed to stay in education for one additional year are a policy relevant group, whose investment behaviour as parents can be improved to lower inequalities in child skills. Crucially, only a subsample of mothers from our ALSPAC dataset was affected by the 1972 RoSLA and exposed to the exogenous increase in education. Previous research has shown that exposure to this policy raised child cognitive outcomes (Dickson et al. 2016). We extend this analysis by also considering the effect of RoSLA on socio-emotional outcomes of children during early schooling and most importantly, analysing whether this policy change impacted a range of parental inputs that are associated with human capital formation.

There is a small existing literature looking at how parental inputs respond causally to mothers' education. Using instrumental variables, Carneiro et al. (2013) exploit exogenous variation across the distribution of mother education on parental inputs. Piopiunik (2014) analyses the effect of a change in compulsory schooling on a small set of parent inputs including parents' value of education and aspirations for their children, whilst Cuartas (2021) considers how a reform to lower the cost of primary schooling in Uganda raised the mothers' stimulation with the child and lowered corporal punishment.[7] However, there has not yet been any study that we know of that focuses on the important margin of mothers' decision to acquire a minimum level of qualifications and includes such a wide set of

[7] An interesting paper documenting cross-section associations between a wide range of parental time investments in children across mothers' education is Guryan et al. (2008).

potential inputs, allowing us to explore more extensively which parental inputs react to a change in mothers' education. This is an important contribution to further open the black box of how a reform to compulsory schooling affects child development in the next generation.

There are three main findings. First, the policy reform raised schooling outcomes for mothers, decreasing their probability of leaving school with no qualifications and increasing their probability of leaving school with high-stake qualifications at age 16. Second, there is a sizeable treatment effect of RoSLA on child outcomes. The children of treated mothers had cognitive skills at school entry and at age 7 that were 0.12–0.14 of a standard deviation higher than untreated mothers. There was no significant treatment effect on children's socio-emotional skills. Third, of the potential mechanisms considered, five were causally changed by the education reform of mothers. Treated mothers had higher family resources than untreated mothers, including higher pre-birth human capital, higher family income, and a higher likelihood of partnering with someone with more education, relative to untreated mothers. Interestingly, treated mothers also made more positive investments, both during pregnancy and in early childhood, including greater monetary investments in the home learning environment and improving health behaviours during pregnancy, relative to untreated mothers. We calculate the proportion of the treatment effect on child cognitive skills explained by parental inputs, by applying (Heckman et al. 2013) but recognising that in this setting our results are descriptive. These tentatively suggest that family resources and parental investments accounted for around 49–60% and 12–13% respectively of the differences in child cognitive skills between treated and untreated mothers.

We do not find any improvements in softer parental investments, including parenting style or time spent with children, as a result of the policy. RoSLA moved mothers from having no qualifications towards a basic set of qualifications, which are still associated with low pay. Income shocks in low-income families have been shown to drive essential purchases such as clothes for children and paying off bills (see for example Gregg et al. 2006). Indeed, mothers impacted by the policy reduce their smoking or drinking during pregnancy and buy more home learning resources for their children, and these in turn are associated with improved child cognitive skills in early childhood. The implications of the effect of raising mothers' qualifications on investment in children are relevant not just to the specific RoSLA policy, but also given contemporaneous policy reforms that extend years of compulsory schooling and increase the share of students who gain qualifications. For example, the UK raised the age of participation in education from 16 to 18 in 2015. During those 2 extra years, individuals must stay in education or training, and they gain qualifications as a result. In addition, around 20% of individuals leave school with no qualifications, and the effects of RoSLA estimated in our paper are relevant for this group of individuals.

The paper is structured as follows. Section 2 explains the educational system in the UK, whilst Section 3 describes the ALSPAC dataset, including a description of how we exploit the RoSLA to create exogenous variation in mothers' education. Section 4 describes the methodology, and Section 5 discusses our results. Section 6 discusses sensitivity analysis, Section 7 considers the effect of a child exposed to

double treatment — both their mother and her partner were exposed to the reform — and Section 8 concludes.

2 Educational system in the UK and the policy reform

In England, children start school in September of the year they turn 4. We analyse the effect of a policy which extended the compulsory schooling age from 15 to 16. Individuals born before August 1957 could leave school after their 15th birthday, whilst those born after August 1957 were required to stay on in school for an additional year until their 16th birthday. Harmon and Walker (1995) provides further details of the reform.

The policy coincided with the first set of educational qualifications which are taken in the summer of the academic year that pupils turn 16. At the end of this school year, students either took CSE (Certificate of Secondary Education) or GCSE (General Certification of Secondary Education) examinations. CSEs were lower quality qualifications compared to the higher level GCSE. A student with satisfactory GCSE qualifications could then progress to higher education, taking A level (Advanced level) qualifications at age 18 — the route through to university entrance. Students with CSE qualifications on the other hand either left school or tended to enter a vocational path.

3 Data

3.1 Sample

Our data comes from the Avon Longitudinal Study of Parents and Children (ALSPAC), a longitudinal cohort study. Pregnant women resident in Avon, in the South West region of England, with expected dates of delivery 1st April 1991 to 31st December 1992 were invited to take part in the study. The initial number of pregnancies enrolled is 14,541, for whom at least one questionnaire has been returned or a"Children in Focus" clinic attended by 19 July 1999. Of these initial pregnancies, there was a total of 14,676 foetuses, resulting in 14,062 live births and 13,988 children who were alive at 1 year of age (Boyd et al. 2013; Fraser et al. 2012). Mothers were interviewed during pregnancy and at frequent intervals after the birth of the child, with follow-up surveys including children and partners. The survey contains very detailed information on the mothers and children, including maternal education and a range of early cognitive and socio-emotional outcome measures. Survey questionnaires related to parents were answered by the mother, who reported information on herself and her partner.[8] The questions did not relate specifically to the father of

[8] The study website contains details of all the data that is available through a fully searchable data dictionary and variable search tool, see http://www.bristol.ac.uk/alspac/researchers/our-data/.

the cohort member and for this reason we focus on the effect of RoSLA on mothers' education.

Crucially, for the purpose of this paper, a number of the mothers in the survey were born both before and after August 1957. As noted in the previous section, the school cohort born from 1 September 1957 onwards was subject to an exogenous policy reform in 1972 whereby the minimum school leaving age was increased from 15 to 16 years old. This meant that those born before September 1957 could leave school at age 15 whilst those born during or after September 1957 had to stay on in school until at least age 16. Moreover, given that this is not a birth cohort study, the window of births means that we have overlapping support of the mothers' age when she gives birth to the cohort members, within both the treatment and control groups. This study therefore combines an exogenous policy shock to mothers within a window of births, resulting in a quasi-experimental design, that separates mothers' age from the policy shock. In the first stage of our analysis, we use information on mother's reported education levels to explore the impact of the exogenous policy shock on her schooling choices.

Our methodology chooses a set of mothers whose date of birth is relatively close to the RoSLA cut off. Sample 1 includes individuals in the treatment or control group if born no more than 6 years either side of the educational reform. Because the cohort members of ALSPAC were born across a 22-month window, when defining treatment status by the mothers' date of birth, there is overlap in support for the mothers' age at birth, which is not the case for example in birth cohort studies. Nevertheless, sample 1 defines treatment status by a variable (mothers' own date of birth) which is correlated with a potential outcome from treatment (the age of the mother at birth). For this reason, we both control for mother's age of birth in all models, and we also show results for sample 2, constructed around a smaller window (+/−1 year) of RoSLA where there was common support across mothers' age at birth. We use statistical tests to check for similarities in our results across our two samples. We face the common quality-quantity trade-off when restricting to a small window around the treatment status — the sample size of children and mothers falls from 5017 (for test score age 6/7 outcomes) for sample 1 to 1035 for sample 2.

3.2 Balance of treatment and control

We check whether our treatment and control samples are balanced in terms of fertility and family background. Table 1 shows balance across covariates in two ways following Dickson et al. (2016), firstly reporting the mean difference for parents in the +/− 1-year window of mother births where there is common support across mothers' age at birth. Second, by reporting regression discontinuity estimates of the effect of RoSLA on each relevant covariate using the wider 6-year window of mother birth dates.

The treatment and control groups are selected to be similar in terms of the mother's date of birth in relation to the RoSLA cut-off, which may be thought of as an outcome of the treatment itself. There may be concern that treatment affects the timing of children so that the birth parity of the child differs across

treatment and control groups. In addition, we check whether our treatment and control groups look similar in terms of pre-treatment characteristics. As treatment in our case refers to a policy change when the mother is age 15 or 16, the pre-treatment variables that we can consider include the education levels and ethnic groups of the mother's parents. Therefore, we analyse whether there are any differences in the types of families that our treated and control group mothers were raised in. Table 1 shows that there are no statistical differences across treatment and control groups in terms of parity or the pre-treatment characteristics at the 5% level.

3.3 Mother's education

The highest qualification of the mother is recorded in ALSPAC when the mother was 32 weeks pregnant with the child. The categories record from lowest to highest; no qualifications; CSE (the Certificate of Secondary Education which is a low level set of qualifications taken at age 16); vocational qualifications; GCSE (the General Certification of Secondary Education which is a higher level set of qualifications taken at age 16); A levels (advanced level set of qualifications taken at age 18); and degree. The mother's years of schooling is defined as 15 if she attains CSE or vocational qualifications, 16 for GCSEs as the highest qualifications, 18 for achieving A'levels, and 21 if she attains a degree.

3.4 Child outcomes

After considering the impact of RoSLA on mother's education, we next estimate the causal effect of RoSLA on child cognitive and socio-emotional skills. Cognitive skills at ages 4/5 are recorded through national school administrative tests called the Entry Assessment Test. These tests are taken by children upon entry to school and all schools within the same Local Education Authority covering the ALSPAC area were administered the same tests. A second cognitive test outcome is recorded at age 6/7 through the Key Stage (KS) 1 test scores.[9] The KS test score is a nationally administered test to all children in state schools and tests the child on reading, writing, spelling, and mathematics. The average of these scores is the final score, used in our analysis. Test score data is obtained from the National Pupil Database, a census of all pupils in England within the state school system, which is matched into ALSPAC.

A measure of socio-emotional skills is derived through questionnaires administered to mothers, which include the Strength and Difficulties Questionnaire (SDQ), an international standardised test set to measure the behaviour and

[9] In the UK education system, children enter schools into the early years level of development at age 4. Key Stage 1 refers to schooling between years 1 and 2 when the child is aged 5–7 and the KS test score is taken at the end of this stage.

Table 1 Balance across treatment and control

	Sample 1-year window						Sample 6-year window		
	Untreated		Treated		Difference	*N*	Difference	SE	*N*
	Mean	SE	Mean	SE					
Birth order	0.951	1.023	0.962	1.029	−0.011	1035	−0.109*	0.058	5017
Grandmother education	1.749	1.511	2.027	1.445	−0.278*	474	−0.150	0.125	2169
Grandfather education	2.276	1.515	2.47	1.39	−0.196	421	−0.194	0.131	2004
Grandmother white	0.953	0.211	0.956	0.21	−0.002	1035	−0.010	0.011	5017
Grandfather white	0.949	0.221	0.95	0.21	−0.003	1035	−0.013	0.012	5017

The 1-year window refers to a window of mother's year of birth 1 year either side of the reform, and the 6-year window refers to a window of 6 years either side of the reform. Grandmother and grandfather education is a discrete variable taking the value of 0 for no qualifications, 1 for CSE level or vocational, 2 for A'level, 3 for degree. **Significant at 1%, *significant at 5%

emotions of children (Goodman 1997, 2001). The SDQ consists of five scales of child behaviour including emotional problems, conduct problems, hyperactivity, peer relationship problems, and pro-social behaviour. Each scale is the composite of five measures as described in Table A.1. For each question, the mother answers from the set "Doesn't apply", "Applies somewhat", and "Certainly applies". Our analysis uses the SDQ scores when the child is 81 months old (6.75 years).

When creating the socio-emotional skills using the SDQ, there are several possibilities for how best to combine the information from different measures. We follow Moroni et al. (2019) and create a latent factor representing internalising skills by combining emotional symptoms and peer problem subscales, and a second factor representing externalising skills by combining the conduct problems and hyperactivity problems. As explained in the psychological literature including (Achenbach 1966), externalising and internalising traits have been shown to represent the latent factor for a large set of psychological traits of individuals. Externalising behaviour in children indicates a child exhibiting externally their emotions, through hitting, shouting, and being generally disruptive. Internalising children on the other hand tend to keep their emotions within themselves and can seem unhappy or withdrawn. Our results are robust to using either the SDQ score or individual factors relating to each subscale.[10]

[10] Results are available on request.

3.5 Parent inputs

Once we have established the impact of RoSLA on the children of mothers impacted by the policy, we consider the impact of RoSLA on a set of parent inputs, which could potentially be driving the relationship between an increase in mother's education and child skills. We observe a large set of parent inputs after the policy treatment and before or early in the child's lifetime. We focus on inputs before the child school starting age because this has been shown to be an important stage of child development and also because the final stage of analysis descriptively decomposes the effect of RoSLA on child outcomes measured at the start of schooling. In this section, we describe the measurement of the parental inputs described in the introduction as those most relevant from the existing literature. Section 3.5.1 describes a set of family resources considered, including human capital of the mother measured before the child's birth (referred to as pre-birth human capital), family income, labour supply of the mother, wellbeing of parents, and assortative mating. Section 3.5.2 describes a set of parental investments considered, including smoking and alcohol consumed during pregnancy, monetary investments, the number of siblings, three parenting style measures, time investments, and the quality of relationships between the child and the mother or partner. With the exception of family income, number of siblings, and permissive parenting, the parental inputs are constructed using factor analysis described in Section A.2.

3.5.1 Family resources

An exogenous shock to education is likely to affect different dimensions of human capital throughout the mother's life before she becomes pregnant, which may be associated with better child outcomes. We define a latent factor for the mothers' human capital before the birth of the cohort member, by combining three questions related to the mothers' home ownership, marital status, and employment status upon discovery of the pregnancy. These factors aim to caputre the circumstances of the mother pre-birth, driven either directly or indirectly from human capital accumulation. Table A.2 reports details of each measure and the factor loadings which construct the latent variable for pre-birth human capital.

Family income is recorded when the children cohort members were aged 2 and 3. We take the log of the average across these two periods to minimise measurement error and transitory variation. A third measure of family resources is the labour supply of the mother after pregnancy and before the child skills are measured. Table A.2 reports factor loadings on four measures including the number of hours worked at 33 months and again at 61 months (around the time of the early test score) and employment status at 47 months and again at 61 months.

A broad definition of family resources should include the mental health or wellbeing of the mothers. Table A.2 reports the factor loadings relating to the latent variable set to capture the mothers' mental health, or wellbeing, recorded when the child was 33 months old. These combine different scales of the mothers' self-esteem (measured through the Bachman self-esteem score), anxiety (measured through Crown Crisp Experimental Index anxiety score which measures anxiety, depression

and somatic symptoms), depression (measured through Edinburgh postnatal depression scale), and enjoyment of and bonding with the child. The enjoyment scale is derived from three questions asking the mother how much she identifies with statements such as "I really enjoy this child" and "I feel confident with my child". The bonding scale is measured from eight statements, again asking the mother how much she identifies with statements such as "Children are fun" and "Having this child has made me feel more fulfilled". The factor loadings show that the subscales indicating mental health issues load negatively and those indicating positive mental health load positively.

Finally, a channel through which RoSLA of the mother might impact the child is through assortative mating or the traits of her spouse. We include a measure of partner's years of schooling to capture assortative mating directly. Using information on partner's qualifications, his years of schooling is constructed as follows. Years of schooling is set equal to 15 if he attains CSE or vocational qualifications, 16 if he attains GCSEs as his highest qualification, 18 if he achieves A'levels, and 21 if he attains a degree. We additionally construct two latent factors for the mothers' partner, to include his employment when the child is ages 21, 33, and 47 months (Table A.2) and a relationship quality measured at 33 months (Table A.2). This latter factor combines three composite measures of their partner's warmth, authority, and communication with the mother. The communication score is derived from six items regarding the frequency that the mother and partner (for example) make plans, talk over feelings, and discuss how their days have gone. The warmth and authority scores are derived from 11 and 13 questions respectively regarding how likely the partner is to be considerate of the mother, is a good companion, and is affectionate, and for authority, how likely the partner is to insist they do exactly as told, seeks to dominate, and is critical of the mother.

3.5.2 Parental investments

Parental investments are broadly defined as inputs made by parents which directly affect the child. Here we are interested in investments that may have been changed by the policy reform, and also potentially are associated with child outcomes. We consider a range of investments that could feasibly be shifted by the RoSLA of mothers. First, we consider the health choices made by mothers during pregnancy. Carter et al. (2019) finds a strong link between years of schooling and health habits, such as smoking. We combine information on the smoking and alcohol habits of the mother during pregnancy, including the number of cigarettes smoked in the first 3 months, whether the mother smoked in the last 2 weeks of pregnancy, and glasses of alcohol consumed in the first 3 months. Table A.3 reports the factor loadings indicating that the factor picks up negative health behaviours during pregnancy.

Second, we measure monetary investments of the household. ALSPAC recorded a set of questions relating to a composite toy score, at ages 24 and 42 months. The questions were the same across the two waves, but the recording of the answers differed. For the child aged 24 months, the mother was asked how

many of each item was owned in the household, including cuddly toys, books, and balls for example. The mother responses were recorded as "None", "One", "2 or 3", and "4 or more". At age 42 months, the responses to the same questions were recoded as "Yes" or "No" and consequently the measures of the toy score at age 42 months tend to distinguish between households who own none of the items versus households who own at least one. What we are able to pick up from this toy score is relevant for analysing the effect of RoSLA, to see whether the reform which raised education from a very low level of education to a slightly higher level affected the home learning environment, through investments in items such as books and toys. The monetary investments score we use is the combination of the score at 24 and 42 months (see Table A.3) and is standardised to mean 0, standard deviation 1.

We capture the quantity of time investments, in part, by the working patterns of the mother and her partner in the early years, which we include as family resources. Another potentially important factor with regard to time available is the household composition, and specifically the total number of siblings in the household, which we measure at 48 months.

The final set of potential mechanisms is intended to capture the quality of interactions between the parents and their child. Doepke and Zilibotti (2017) define three important measures of parenting style to include permissive parenting, which "allows children to make free choices according to their natural inclinations", authoritative parenting where "parents attempt to mold their children's preferences, with the aim of inducing choices that parents view as conducive to success in life" and authoritarian parenting whereby "parents restrict children's choices, that is, the parent directly imposes her will on the child rather than taking the indirect route of molding the child's preferences" (Doepke and Zilibotti 2017, p.1332). Given this, we create an indicator for permissive parenting to take the value of 1 if mothers report that the child dominates the household "usually". Table A.3 details factor loadings for the remaining two measures of the parenting style of the mother at 42 months. Authoritarian parenting is constructed from measures indicating the extent to which mothers implement consequences for poor behaviour which allow the child to reflect on their behaviour, including sending the child to their room when naughty. Authoritarian parenting on the other hand is constructed from measures which suggest a harsh punishment strategy, where factor loadings are highest for the measures of smacking or shouting at the child when naughty.

A further set of measures captures the time investments made by mothers in her child (see Table A.3) at 42 months including whether she sings to, plays with, or cuddles the child. This factor loads heavily on measures indicating interactions between the mother and child and therefore we interpret the measure as picking up quality engagement between parent and child. A comparable index for the partner time investments is recorded in Table A.4. Finally, two factors relating to the mothers' and partners' relationship with the child, detailed in Table A.4, record measures including whether the mother or partner loves the child, gets on their nerves, or has a battle of will with the child.

We have assumed that the measurement system for treatment and control groups is identical. The mean of the latent factors is close to zero in all cases and therefore

suggests that our results are not sensitive to allowing the measurement system to differ for the groups of treatment and control.[11]

4 Methodology

Our aim is to understand the parental inputs which causally respond to an increase in mothers' educational qualifications that are associated with child skills. There are several steps to the analysis. First, we estimate the effect of the policy reform to compulsory education on education of ALSPAC mothers, identifying in particular where in the distribution of education the effect occurs. Second, we estimate the causal effect of RoSLA on child cognitive and socio-emotional skills and third we estimate the causal effect of the policy reform on a range of inputs which may drive the human capital of the next generation, including family resources, labour decisions, parenting behaviours, and parental investments in their children. For the first three stages, we consider that the policy reform changed two dimensions of mothers' education — their years of schooling and an indicator for achieving any qualifications. The higher qualifications would equip the mothers well for labour market entry, likely affecting their income and financial resources. On the other hand, the additional year of schooling can increase the mothers' ability to learn and use information, raise her cognitive skills (Harding et al. 2015), and expand the mothers' social network to include more educated individuals (Choi et al. 2008). Therefore (in the absence of two instrumental variables for each dimension), estimating the reduced form of RoSLA is the best specification to pick up both of these important effects.[12]

Finally, we undertake a descriptive mediation analysis to estimate the extent to which measured parental inputs mediate the treatment effect on child skills.

4.1 Effect of policy reform on mothers' education, parental inputs, and child skills

In our setting, RoSLA raised the compulsory age of schooling for mothers born after a particular date from age 15 to 16. Therefore, treatment status of mother i, D_i is given by exposure to RoSLA, defined by the date of birth of mothers in our sample. Those born before 1 September 1957 were exposed to an education system with a compulsory minimum leaving age of 15 years, whereas those born after the date could not leave school before the age of 16. We select mothers born around a small window of dates around the cut-off, as discussed in Section 3.

[11] Results can be seen in Appendix Table A.5 of an early working paper, Macmillan and Tominey (2020).

[12] When running a 2SLS analysis, instrumenting the presence of qualifications or mothers' years of schooling with the RoSLA variable, RoSLA strongly predicts the presence of qualifications (with a first-stage F-statistic of 15) and less strongly the mothers' years of schooling (F-statistic of 9).

We are therefore interested in estimating the effect of RoSLA on a vector of outcomes including (a) mothers' education, (b) parental inputs, and (c) child cognitive and socio-emotional skills, given by Y in the equation

$$Y_i = \delta_1 + \delta_2 D_i + \delta_2 X_i + \epsilon_i \tag{1}$$

Here X denotes covariates measured before treatment, i.e. before the mothers turned 15 (notably mothers' age), and ϵ is the error term.

4.2 Decomposition with quasi-experimental variation in treatment status

In the final section, we decompose the treatment effect of RoSLA on child outcomes into the effect of mediators and the unexplained component, following Heckman et al. (2013). Whilst we identify the effect of the policy reform on mothers' education, parent inputs, and child skills, we cautiously intrepret the decomposition analysis as descriptive. This is because our parental inputs are potentially correlated with unobserved parent or child inputs, which simultaneously drive child skills.

The potential outcome for each individual is given by the equation

$$Y = DY_1 + (1 - D)Y_0 \tag{2}$$

where Y denotes the child outcome, D a binary treatment indicator for exposure to RoSLA where treated mothers were born between 1 September 1957 and 1 September 1963, and control mothers were born between 31 August 1951 and 31 August 1957. Y_1 and Y_0 refer to the outcome for a child with treated and untreated mothers respectively.

We aim to decompose the intention to treat effect $E(Y_1 - Y_0)$ to understand the channels through which the policy drives early life skills of children. In order to do this, consider the outcome equation defined as follows.

$$Y_d = \kappa_d + \sum_{j \in J} \alpha^j \theta_d^j + \beta X + \tilde{\epsilon_d} \tag{3}$$

where κ_d is an intercept fixed at the level of treatment $d = \{0, 1\}$, θ_d^j denotes the j parental inputs from a set $j \in J$ at the level of treatment d and α^j the associated coefficients. X denotes a set of covariates which are observed prior to the treatment of RoSLA.

It may be that the mediators we observe are only a subset of the full set of mediators for the effect of RoSLA on child skills. If we observe in the data only a subset $j \in J_p$, we can rewrite Eq. 3 to express this.

$$Y_d = \tau_d + \sum_{j \in J_p} \alpha^j \theta_d^j + \beta X + \epsilon_d \tag{4}$$

$\tau_d = \kappa_d + \sum_{j \in J | J_p} \alpha^j E(\theta_d^j)$ and ϵ_d is an error term with mean zero equal to $\tilde{\epsilon_d} + \sum_{j \in J | J_p} \alpha^j (\theta_d^j - E(\theta_d^j))$. Our objective is to decompose the treatment effect of RoSLA on child skills into mediators. Combining Eqs. 2 and 4 leads to the following

$$Y = D\left(\tau_1 + \sum_{j\in J_p} \alpha^j \theta_1^j + \beta X + \epsilon_1\right) + (1-D)\left(\tau_0 + \sum_{j\in J_p} \alpha^j \theta_0^j + \beta X + \epsilon_0\right) = \tau D + \sum_{j\in J_p} \alpha^j \theta^j + \beta X + \epsilon \tag{5}$$

where $\tau = \tau_1 - \tau_0$ defines the role of unmeasured variables on the mean treatment effects, $\epsilon = D\epsilon_1 + (1-D)\epsilon_0$ is a mean zero error term and $\theta^j = D\theta_1^j + (1-D)\theta_0^j$, $j \in J_p$ denotes the measured inputs. We decompose the treatment effect of RoSLA into the different measured components of parental inputs using Eq. 5.

$$E(Y_1 - Y_0|X) = \underbrace{(\tau_1 - \tau_0)}_{\substack{\text{Treatment effect} \\ \text{unmeasured inputs}}} + \underbrace{\sum\nolimits_{j\in J_p} \alpha^j E(\theta_1^j - \theta_0^j|X)}_{\substack{\text{Treatment effect} \\ \text{measured inputs}}} \tag{6}$$

The presence of unobserved mediators which are correlated with both observed mediators and the child outcomes would lead to biased estimates in Eq. 6. Heckman and Pinto (2015) and Heckman et al. (2013) describe the conditions upon which it is possible to relax the assumption that mediators are exogenous for the treatment and control groups, to the assumption that mediators are exogenous for the control group only. The conditions by which it is possible to make the weaker identification assumption are detailed in Appendix Section A.3 of the working paper Macmillan and Tominey (2020).

Even under the weaker identification assumption, there may be concerns that the mediators are not exogenous at least for the control group and for this reason we interpret the mediation analysis as descriptive.

4.3 Measurement error in parental inputs

An additional strength of our analytical approach is that the survey data contains a number of variables at different ages relating to multiple dimensions of parental inputs into child human capital. For example, we observe questions relating to health behaviour during pregnancy through smoking and drinking habits. Whilst each variable measures the latent factor, in this example relating to health behaviour during pregnancy with measurement error, the multiple observations mean that we can use factor analysis to combine the set of measures into a latent factor for each parental input, which is free of measurement error.

The following measurement system is applied to extract a latent factor for each parental input for which we observe multiple measures.[13]

[13] As detailed in Section 3, we do not use factor analysis for family income, the number of siblings or permissive parenting which are observed directly.

$$M^j_{m^j,d} = \underbrace{v^j_{m^j}}_{\substack{\text{measure specific} \\ \text{intercept}}} + \underbrace{\phi^j_{m^j}\theta^j_d}_{\substack{\text{factor} \\ \text{loadings}}} + \underbrace{\eta^j_{m^j}}_{\substack{\text{mean 0 error} \\ \text{independent of } \theta_d}} \tag{7}$$

for $j \in J_p$ and $m^j \in M^j$ is measure m from the set 1, .., M related to each latent parental input j. For identification of the latent factor, we normalise the location and scale of the factors similarly to Cunha and Heckman (2008). That is, we set the intercept for the first measure in the system equal to zero (i.e. $v^j_{m^j} = 0$ for measure $m = 1$ for each $j \in J_p$) and the factor loading for the first measure equal one (i.e. $\phi^j_{m^j} = 1$ for measure $m = 1$ for each $j \in J_p$). The specific measures to include in each measurement equation were derived by exploratory factor analysis. A description of the factor analysis along with the factor loadings for each factor is reported in Section A.2 and Tables A.2–A.4.

4.4 Missing data

Our empirical analysis is demanding in the sense of requiring information on the treatment status of mothers (determined by their date of birth), test scores and socio-emotional skills of children, and a total of 17 mediators, measured across a period of up to 8 years, including the period of pregnancy. We exploit quasi-experimental variation in our treatment status using longitudinal secondary data which, unlike in a randomised control trial for example, was not collected for this purpose directly. As a result, as is typical in longitudinal cohort data analysis, information is available for the majority of variables in our model, but there is missing information on a small number of variables. Our sample is defined as mothers with a reported date of birth,[14] a recorded cognitive test score or socio-emotional skill and at least one mediator.[15] For individuals with missing mediators, we impute the missing data using the following method from Carneiro et al. (2021).

There are a set of households with complete data on the treatment status, child outcome, and all 17 mediators making up 66% of the sample. Using the set of households with complete data, we regress

$$\theta^j_i = \delta^j + \sum_{k \neq j} \gamma^j_k \theta^k_i + \mu^j X^j_i + u^j_i \tag{8}$$

where θ^j_i is the latent factor relating to mediator j with $j = 1, .., 17$, for household i. Included in the regression are the remaining 16 mediators ($k \neq j$) and covariates X including treatment status and mothers' age at birth. That is, for households with complete data, we run 17 regressions with the dependent variable equal to the mediator θ^j_i regressed on the remaining variables in our model. The estimated coefficients

[14] 99% of the sample

[15] Conditional on observing treatment and child outcomes, 66% of households have no mediators missing, 6% have 1 mediator missing, 5% have 2 mediators missing, 5% have 3 mediators missing, 3.5% have 4–5, and 14% have more than 5 mediators missing.

γ_k^j tell us the relationship between a mediator j and the remaining mediators $k \neq j$, whilst the vector of coefficients μ^j informs of the relationship between mediator j and the set of covariates X. Fitted values were calculated for each j regression for the total sample parents. These predicted values give the imputed level of the mediator for the households whose mediator is missing.[16]

In order to evaluate the sensitivity of our strategy to deal with missing values for a small number of mediators, Section 6 shows that our results are robust to two alternative strategies of (i) limiting the sample to households with at least two-thirds of mediators non-missing and (ii) an alternative mean replacement strategy including dummy variables to indicate missing data in our models. In both cases, the estimated coefficients are very similar and therefore we conclude that our results are not sensitive to our methods to deal with missing data.

Finally, we test for systematic attrition across treatment status by regressing a binary indicator for the household being present in one of the samples for our four outcomes on the treatment indicator and a control for mothers' age. The dependent variable takes the value of 1 if the mother is in our final estimation sample for either of the four outcomes and 0 otherwise. The analysis shows that households who are not included in our final sample due to attrition are not systematically different across treatment status.[17]

5 Results

5.1 Effect of RoSLA on education of mother

We begin by exploring the impact of RoSLA on mothers' education in Table 2. The first column of Table 2 considers the impact of RoSLA on the mother's years of schooling (proxied with the age she left school) whilst columns 2–5 consider outcomes across the distribution of mothers' education of no qualifications; low-quality qualifications (which include either CSE or vocational qualifications); GCSEs; and A levels.[18] Panel a) presents the results for sample 1, the broader definition of RoSLA whilst panel b) presents the results for a more restricted window around the policy implementation of sample 2 (with common support across mothers' age at birth).

[16] The *R*-squared values from each of the 17 regressions are as follows: pre-birth capital (0.1295); log average family income (0.3235); mother's labour supply (0.1112); partner's years of education (0.1883); partner's employment (0.1688); mother's wellbeing (0.2125); partner's wellbeing (0.2451); health during pregnancy (0.0568); monetary inputs (0.0769); no. of siblings (0.1140); mother's permissive parenting style (0.0546); mother's authoritarian parenting style (0.3161); mother's authoritative parenting style (0.2808); mother's time investments (0.1729); partner's time investments (0.2594); mother-child relationship (0.0737); partner-child relationship (0.1666). These indicate relatively large explanatory power.

[17] The coefficient (standard error) on the treatment indicator is 0.024 (0.017).

[18] Recall that CSEs are examinations taken at the age of 16 with a relatively low quality compared to GCSEs which are also taken at the age of 16. A levels are examinations taken at the end of high school at the age of 18.

Focusing on panel a), the impact of the policy is to increase the age that mothers left school by 0.291 years on average. This is consistent with national estimates of the impact of RoSLA from external data sources such as the Labour Force Survey as shown in Fig. 1. Columns 2–5 show that RoSLA impacts differentially across the distribution of mothers' education. RoSLA mothers have a reduced probability of leaving school with no qualifications by 4.7 percentage points. This is because the control mothers could leave school at age 15 before taking formal examinations at age 16. There is no change in the probability of obtaining the low-quality qualifications, but an increase in the probability of attaining the higher quality GCSE qualifications by 6.2 percentage points. The results suggest that the margin through which mothers' educated was affected by RoSLA was a shift from leaving with no qualifications to achieving GCSE qualifications at the age of 16. In this case, when we consider the effect of RoSLA on children, the policy does not just represent an increase in a proportion of a year in schooling, but in addition reflects an increased probability of leaving school with some qualifications compared to no qualifications.

Panel b) illustrates that the results are very similar for our more restricted sample. The estimated effect sizes are qualitatively similar, with treated mothers less likely to attain no qualifications and more likely to attain GCSEs. The final row of Table 2 reports the test statistic for the hypothesis that the coefficients in sample 2 are not statistically different to the coefficients in sample 1. For each measure of mothers' education, the z-statistics are low and we cannot reject the hypothesis of equality of coefficients.

5.2 Effect of RoSLA on child development

Given that RoSLA has a positive impact on maternal education, we next look at the reduced form impact of RoSLA on the skills of the next generation — the children of the mothers who were born close to the 1972 policy reform. Table 3 shows the impact of RoSLA on a range of cognitive (columns 1 and 2) and socio-emotional (columns 3 and 4) skills of children between ages 4 and 7. Panel a) again presents the results for sample 1 whilst panel b) presents the results for a more restricted window around the implementation of RoSLA in sample 2. The results show that children of mothers affected by RoSLA have higher cognitive test scores at age 4/5 by 13.9% of a standard deviation, and age 6/7 by 11.7% of a standard deviation. This is consistent with the findings of Dickson et al. (2016).

The analysis next considers for the first time the impact of RoSLA also on child socio-emotional skills. We find no significant impact of the policy on the externalising or internalising skills of children (columns 3 and 4) at age 81 months (6.75 years). The results are consistent across sample 2 in panel b) of Table 3. The final row provides z-statistics for the test of equality of coefficients which indicate that we cannot reject the hypothesis of equal coefficients for any of the outcomes, in sample 2 compared to sample 1.

A potential reason for the policy having no impact on the socio-emotional skills of children is that the inputs which responded to the policy were those more likely to drive cognitive skills of children. Research has shown that socio-emotional skills

Table 2 Effect of RoSLA on mothers' education

	(1) Years of ed.	(2) No quals.	(3) Low-quality quals.	(4) GCSE	(5) A level
A. +/− 6 yrs					
RoSLA	0.291**	−0.047**	−0.015	0.062*	0.053
	(0.112)	(0.013)	(0.024)	(0.026)	(0.028)
Mother's age	0.125**	0.002	−0.010**	0.008*	0.031**
	(0.015)	(0.002)	(0.003)	(0.003)	(0.004)
Observations	5017	5017	5017	5017	5017
R-squared	0.021	0.011	0.000	0.001	0.023
B. +/− 1 yr					
RoSLA	0.290	−0.064**	−0.007	0.071	0.054
	(0.211)	(0.024)	(0.039)	(0.043)	(0.050)
Mother's age	0.192	−0.002	−0.010	0.012	0.049
	(0.128)	(0.014)	(0.024)	(0.026)	(0.030)
Observations	1035	1035	1035	1035	1035
R-squared	0.002	0.016	0.000	0.004	0.003
Z test	0.00	0.62	−0.17	−0.18	−0.02

Sample 1 in panel A consists of of a window of mothers' year of birth 6 years either side of the reform to compulsory schooling; sample 2 in panel B restricts to common support across mothers' age at the timing of the cohort member's birth within treatment and control. Regressions control for mothers' age at birth. Low-quality quals refer to CSE examinations or vocational qualifications. GCSEs are the relatively high-quality examinations taken at the age of 16 whilst A levels are taken at age 18. **Significant at 1%, *significant at 5%

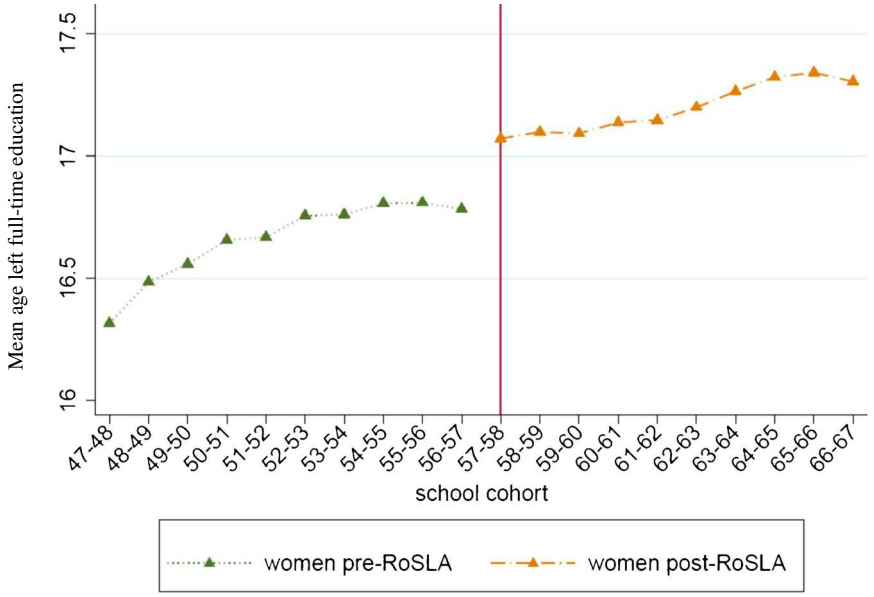
of children respond to "softer" inputs such as parenting style and mothers' wellbeing rather than inputs such as family income. As we will show in the next section, these softer inputs were not changed by the policy, so this could explain why there was no significant effect on externalising or internalising skills.[19][20]

5.3 Parent inputs

Our main contribution is to consider the potential mechanisms through which RoSLA improves child skills by estimating the effect of RoSLA on a wide range of parent inputs, which are in turn associated with child development. Individual regressions were run for each input controlling for mothers' age, in order to ascertain whether the input is affected by RoSLA, i.e. if for a parental input j, $E(\theta_1^j - \theta_0^j|X) \neq 0$ in Eq. 6. This further extends the existing literature by exploring

[19] See for example Moroni et al. 2019

[20] Note that we tried many different methods to construct socio-emotional skills: taking the SDQ score from ALSPAC, using individual components of the score separately, and running a factor analysis on all subscales. In no case was the effect of RoSLA on the socio-emotional skill measure statistically significant.



Notes: Source Dickson et al. (2016). RoSLA (raising of the school leaving age) is the reform of school leaving age

Fig. 1 Impact of RoSLA on women across cohorts from the Labour Force Survey

the possible routes through which RoSLA impacted children's cognitive skills, but had little impact on their socio-emotional development.

Table 4 reports the estimated coefficients of the impact of RoSLA of mothers on all potential parental inputs. We estimate the effect of RoSLA on each potential mediator using two samples — the sample of test scores at age 4/5 (columns 1–3) and the sample of test scores at age 6/7 (columns 4–6) and across our two windows of mother's age at birth (sample 1 results are reported in columns 1 and 4, whilst sample 2 results in columns 2 and 5), reporting test statistics on the equality of coefficients across these windows (columns 3 and 6).

In terms of the potential mediators of family resources for sample 1 (column 1), the estimates indicate that mothers who were exposed to RoSLA have 16.6% of a standard deviation higher pre-birth human capital, 7.5 percentage points higher average family income, and have a partner whose education is 0.4 years higher than mothers who were not exposed to RoSLA. The coefficients on these mediators are statistically significant. For parental investments, RoSLA has a negative impact on poor health during pregnancy — mothers impacted by RoSLA scored 17.6% of a SD lower in terms of smoking and drinking in pregnancy. They were also likely to have 17.4% of a SD higher monetary investments in the home learning environment, compared to those not affected by RoSLA. This suggests that on top of the expected family resource and assortative mating channels, treated mothers had better health habits during pregnancy and higher levels of investments in the home

Table 3 Effect of RoSLA on child outcomes

	(1) Test score 4/5	(2) Test score 6/7	(3) Externalising 6.75	(4) Internalising 6.75
A. +/− 6 yrs				
RoSLA	0.139*	0.117*	0.067	−0.071
	(0.063)	(0.056)	(0.058)	(0.058)
Mother's age	0.027**	0.028**	−0.004	−0.004
	(0.008)	(0.008)	(0.008)	(0.008)
Observations	4248	5017	4694	4575
R-squared	0.003	0.003	0.002	0.000
B. +/− 1 yr				
RoSLA	0.190	0.214*	0.072	−0.025
	(0.106)	(0.098)	(0.098)	(0.102)
Mother's age	0.090	0.099	−0.050	0.010
	(0.064)	(0.059)	(0.059)	(0.062)
Observations	872	1035	1014	988
R-squared	0.004	0.005	0.005	0.000
Z stat.	−0.414	−0.859	−0.044	−0.392

Sample 1 in panel A consists of a window of mothers' year of birth 6 years either side of the reform to compulsory schooling; sample 2 in panel B restricts to common support across mothers' age within treatment and control; regressions control for mothers' age at birth. **Significant at 1%, *significant at 5%

learning environment. The estimated effect of RoSLA on the other mediators was close to zero in many cases and imprecisely estimated. The estimates are broadly similar qualitatively for sample 2 compared to sample 1 and in column 3 we cannot reject the hypothesis that coefficients are equal between the benchmark sample 1 and the more restrictive sample which narrows the window around the implementation of RoSLA.

Columns 4–5 show that when we consider the larger sample of observations for the test score at age 6/7, the parental inputs identified as responding to RoSLA are similar, with the addition of a negative impact on mothers' labour supply and a positive impact in terms of the partner-child relationship. The maternal labour supply effect, when taken in the context of the positive coefficient on family income, may suggest that RoSLA raised the wage of mothers whilst lowering their hours worked in the labour market. The positive partner-child relationship and large impact on partner's education suggest a strong positive assortative mating channel. Again there is no statistically significant difference across estimates in samples 1 and 2, with the exception of partner's well being.

A large set of parental inputs is not impacted by RoSLA. For example, we find no impact on a set of family resources including partner's labour supply or wellbeing of the mother or partner. In addition, the parental investments including number of siblings, parenting style measures, time investments (measuring parental engagement

Table 4 Effect of RoSLA on potential mediators

	Test score 4/5			Test score 6/7		
	(1)	(2)	(3)	(4)	(5)	(6)
	RoSLA (+/− 6 yrs)	RoSLA (+/− 1 yr)	*Z*-score	RoSLA (+/− 6 yrs)	RoSLA (+/− 1 yr)	*Z*-score
A. Family resources						
Pre-birth capital	0.166***	0.104	0.677	0.149***	0.140*	0.105
	(0.051)	(0.076)		(0.047)	(0.072)	
Log average family income	0.075**	0.043	0.565	0.071**	0.033	0.695
	(0.030)	(0.048)		(0.028)	(0.047)	
Mother's labour supply	−0.063	−0.011	−0.443	−0.112**	−0.047	−0.571
	(0.060)	(0.101)		(0.056)	(0.099)	
Partner's years of education	0.412***	0.213	0.753	0.286**	0.009	1.090
	(0.130)	(0.230)		(0.122)	(0.223)	
Partner's employment	0.027	0.087	−0.548	0.014	−0.009	0.224
	(0.056)	(0.094)		(0.051)	(0.089)	
Mother's wellbeing	0.008	0.062	−0.480	−0.006	0.045	−1.728
	(0.057)	(0.097)		(0.053)	(0.091)	
Partner's wellbeing	0.034	0.131	−0.869	0.031	0.176*	2.061
	(0.057)	(0.096)		(0.053)	(0.091)	
B. Parental investments						
Health during pregnancy	−0.176***	−0.268***	0.909	−0.132***	−0.186**	0.554
	(0.055)	(0.085)		(0.051)	(0.083)	
Monetary inputs	0.174***	0.177*	−0.034	0.163***	0.173*	−0.091
	(0.061)	(0.101)		(0.056)	(0.094)	
No. of siblings	0.014	−0.064	0.652	0.006	−0.051	0.513
	(0.059)	(0.104)		(0.054)	(0.097)	
Mother's permissive parenting	−0.046	0.175	−1.809	−0.087	0.114	−1.769
	(0.059)	(0.107)		(0.054)	(0.100)	
Mother's authoritative parenting	−0.034	−0.050	0.144	−0.071	−0.121	0.485
	(0.056)	(0.096)		(0.052)	(0.089)	
Mother's authoritarian parenting	0.012	0.008	0.036	−0.031	−0.047	0.154
	(0.056)	(0.095)		(0.052)	(0.090)	
Mother's time investments	0.048	0.010	0.336	0.036	−0.012	0.456
	(0.058)	(0.097)		(0.053)	(0.091)	
Partner's time investments	0.067	0.005	0.558	0.033	−0.015	0.462
	(0.056)	(0.096)		(0.052)	(0.090)	
Mother-child relationship	0.086	−0.032	1.076	0.074	−0.019	0.975
	(0.053)	(0.096)		(0.047)	(0.083)	
Partner-child relationship	0.058	0.007	0.462	0.115**	0.035	0.776
	(0.056)	(0.095)		(0.052)	(0.089)	
Observations	4248	872		5017	1035	

Coefficients of a regression of RoSLA on each mediator, controlling for mothers' age at birth. The analysis restricts to the sample for which test scores were observed at ages 4–5 (columns 1–3) and 6–7 (columns 4–6). **Significant at 1%, *significant at 5%

with the child), or factors for the relationship with the child did not vary statistically significantly across RoSLA status.[21] Our interpretation is that the policy change increased basic levels of education, which led to an increase in the more fundamental inputs such as family income, smoking or drinking in pregnancy, and the purchase of educational toys. Instead, the mediators remaining in Table 4 which do not respond to RoSLA in a statistically significant manner may be more responsive to changes higher up the education distribution.[22]

Given that some of our potential mediators in Table 4 were significantly affected by the RoSLA policy, in the final stages of our analysis, we consider whether they might account for the direct effect of RoSLA on those skills. We note a cautious descriptive interpretation of this analysis, given that any unobserved inputs may be correlated with both our included inputs and child skills. We begin by showing the association between our mediators and child skills in Table 5, conditional on the direct effect of RoSLA on the child skills.

Columns 1–3 of Table 5 present the results for cognitive skills of the early test score at age 4/5 and columns 4–6 for the cognitive skills measured through a test score at age 6/7. In columns 1 and 4, we report results for the wide sample window of sample 1 and in columns 2 and 5 for sample 2. Columns 3 and 6 report the z-score relating to the test of equal coefficients across samples 1 and 2. We do not show the results for socio-emotional outcomes here as there is no policy effect to decompose.

All parental inputs which were affected by RoSLA are also associated with child cognitive skills at ages 4/5 and 6/7. The inclusion of these potential mediators in the child skills equation significantly reduces the direct impact of RoSLA on these skills. This provides suggestive evidence that these observed skills may be partially responsible for the impact of RoSLA on child skills. Starting with the family resources, an increase in the pre-birth human capital of mothers by one standard deviation is associated with an increase in test score at age 4/5 (6/7) by 8.7% (11.1%) of a standard deviation, whilst a raising average family income by 1% in pre-school is associated with a 25.5% (27.2%) of a standard deviation increase in child cognitive skill at age 4/5 (6/7). Finally, increasing partners' years of schooling by 1 year is associated with an increase in test scores at age 4/5 (6/7) by 7.4% (8.8%) of a standard deviation. Similarly, an increase in the parental investments of (poor) health during pregnancy by one standard deviation is associated with a decline in test scores of the second generation by 3.1% (3.9%), whilst a SD increase in monetary inputs is associated with an 10.7% (8.6%) of a standard deviation increase in test scores at the age of 4/5 (6/7). Many other inputs also drive child skills, including mothers' wellbeing, authoritarian and authoritative parenting styles, and the number of siblings, but rather than mediators can be considered additional controls as they were not affected by RoSLA.

[21] RoSLA has no statistically significant effect on parenting style measured either as just one factor from all measures or indicators for parenting style taking particularly high values to indicate very strict or very relaxed parenting. Results are available on request.

[22] Section A5 of the working paper (Macmillan and Tominey 2020) tests whether RoSLA drives divorce status of mothers, concluding that there is no statistically significant effect on divorce.

To analyse more directly the potential mediating role of the parental inputs, Table 6 reports a descriptive decomposition analysis, combining the results from Tables 4 and 5 as described in Eq. 6. Reported in Table 6 is the proportion of the estimated treatment effect in Table 3 explained by each mediator. Figure 2 provides a graphical representation of the decomposition analysis and illustrates that 61% and 73% of the total impact of RoSLA on cognitive outcomes at age 4/5 and 6/7 respectively can potentially be accounted for by these mediators. Family resources make the biggest contribution, accounting for 49–60% of the total RoSLA effect. This is not surprising given the extensive literature which has established a causal relationship between family resources and child outcomes. Interestingly, even conditional on a range of measures of family resources, RoSLA is in part mediated by the investment behaviour of parents from pregnancy into early years. Together the parental investments account for 12% (13%) of the treatment effect on the test score at age 4/5 (age 6/7).[23]

The coefficients in Table 5 are similar in the more restrictive sample. Despite this, Fig. A.1 shows that we are able to account for a smaller proportion of the treatment effect on cognitive test scores (31–36%). A closer inspection shows that the difference in the explained treatment gap comes just from the relatively limited role of family resources in explaining the cognitive test score gaps in our more restricted sample. The parental investments have a similar impact in the two samples, accounting for 8% and 18% of the treatment effect on age 4/5 and 6/7 test scores respectively.

In summary, our results have identified some important parental inputs which are both impacted by the RoSLA reform, and associated with child cognitive outcomes. Of the total treatment effect of RoSLA on the cognitive skills, family resources account for the largest portion. However on top of the more obvious family and assortative mating channels, direct investments, such as improving health behaviours in pregnancy and improving the home learning environment, are also drivers of the cognitive skills gaps.

6 Sensitivity

Inherent in longitudinal panel datasets such as ALSPAC is a degree of missing information across the waves of data. In our case, of the 17 parental inputs which potentially mediate the effect of RoSLA on child skills, 66% of households have no missing mediators. For the sample of households with a missing mediator, we imputed the value of the parental input as described in Section 4.4. To check the sensitivity of our results to the imputation, we firstly tighten the conditions by which households are included in our sample, to those with at least two-thirds of mediators non-missing. This reduces the sample to 84% of our benchmark sample. Tables

[23] Included in the decomposition analysis is the mediation through all parental inputs, even those not statistically significant in Table 4. However, it is evident in Table 6 that our decomposition results are very similar if we exclude these inputs from the decomposition, as their contribution to the average treatment effect is close to zero.

Table 5 RoSLA on child outcomes, conditional on mediators

	Test score 4/5			Test score 6/7		
	(+/− 6 yrs)	(+/− 1 yr)		(+/− 6 yrs)	(+/− 1 yr)	
	Entry ass.	Entry ass.	*Z*-score	Key Stage 1	Key Stage 1	*Z*-score
Conditional RoSLA	0.055	0.132	−0.668	0.032	0.136	−1.001
	(0.059)	(0.099)		(0.052)	(0.090)	
A. Family resources						
Pre-birth capital	0.087***	0.022	1.207	0.111***	0.076*	0.751
	(0.020)	(0.050)		(0.018)	(0.043)	
Log average family income	0.255***	0.440***	−1.878	0.272***	0.344***	−0.861
	(0.040)	(0.090)		(0.035)	(0.076)	
Mother's labour supply	−0.039**	−0.030	−0.234	-0.073***	−0.089***	0.483
	(0.016)	(0.035)		(0.014)	(0.030)	
Partner's years of education	0.074***	0.061***	0.727	0.088***	0.089***	−0.064
	(0.008)	(0.016)		(0.007)	(0.014)	
Partner's employment	0.007	−0.058	1.452	−0.002	−0.075**	1.853
	(0.018)	(0.041)		(0.016)	(0.036)	
Mother's wellbeing	0.032*	0.111***	−1.801	0.041***	0.153***	−2.843
	(0.018)	(0.040)		(0.016)	(0.036)	
Partner's wellbeing	0.016	0.052	−0.788	0.019	0.025	−0.152
	(0.018)	(0.042)		(0.016)	(0.036)	
B. Parental investments						
Health during pregnancy	−0.031*	0.060	−2.050	−0.039**	−0.012	−0.709
	(0.017)	(0.041)		(0.015)	(0.035)	
Monetary inputs	0.107***	0.128***	−0.546	0.086***	0.123***	−1.059
	(0.016)	(0.035)		(0.014)	(0.032)	
No. of siblings	−0.137***	−0.200***	1.637	−0.107***	-0.159***	1.510
	(0.016)	(0.035)		(0.015)	(0.031)	
Mother's permissive parenting	−0.000	−0.007	0.198	−0.004	0.050*	−1.725
	(0.015)	(0.032)		(0.014)	(0.028)	
Mother's authoritative parenting	0.070***	0.033	0.819	0.027	−0.007	0.854
	(0.019)	(0.041)		(0.017)	(0.036)	
Mother's authoritarian parenting	−0.060***	−0.005	−1.170	−0.052***	−0.036	−0.393
	(0.019)	(0.043)		(0.017)	(0.037)	
Mother's time investments	0.011	−0.006	0.408	0.030*	0.084**	−1.490
	(0.017)	(0.038)		(0.015)	(0.033)	
Partner's time investments	−0.016	0.001	−0.384	−0.043***	−0.074**	0.779
	(0.019)	(0.040)		(0.017)	(0.036)	
Mother-child relationship	−0.014	0.016	−0.745	−0.019	0.007	−0.692
	(0.018)	(0.036)		(0.016)	(0.034)	
Partner-child relationship	−0.013	−0.032	0.433	−0.009	−0.010	0.026
	(0.018)	(0.040)		(0.016)	(0.035)	
Observations	4248	872		5017	1035	

Sample 1 in columns 1 and 4 consists of births within a 6-year window of the mother's date of birth around RoSLA; sample 2 in columns 2 and 5 restricts to common support across mothers' age within treatment and control. Regressions control for mothers' age at birth. **Significant at 1%, *significant at 5%

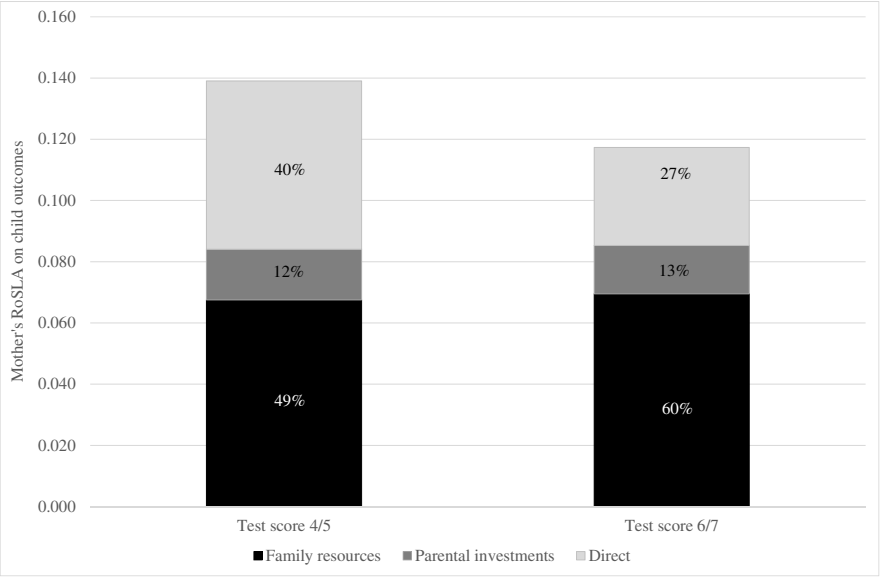
Table 6 Decomposing effect of RoSLA on child cognitive skills into the proportion of the treatment effect explained by each potential mediator

	(1) RoSLA (+/− 6 yrs)	(2) RoSLA (+/− 1 yr)	(3) RoSLA (+/− 6 yrs)	(4) RoSLA (+/− 1 yr)
A. Family resources				
Pre-birth capital	0.014	0.002	0.017	0.011
Log average family income	0.019	0.019	0.019	0.011
Mother's labour supply	0.002	0.000	0.008	0.004
Partner's years of education	0.030	0.013	0.025	0.001
Partner's employment	0.000	−0.005	0.000	0.001
Mother's wellbeing	0.000	0.007	0.000	0.007
Partner's wellbeing	0.001	0.007	0.001	0.004
Total through family resources	**0.068**	**0.043**	**0.070**	**0.039**
B. Parental investments				
Health during pregnancy	0.005	−0.016	0.005	0.002
Monetary inputs	0.019	0.023	0.014	0.021
No. of siblings	−0.002	0.013	−0.001	0.008
Mother's permissive parenting	0.000	−0.001	0.000	0.006
Mother's authoritative parenting	−0.002	−0.002	−0.002	0.001
Mother's authoritarian parenting	−0.001	0.000	0.002	0.002
Mother's time investments	0.001	0.000	0.001	−0.001
Partner's time investments	−0.001	0.000	−0.001	0.001
Mother-child relationship	−0.001	−0.001	−0.001	0.000
Partner-child relationship	−0.001	0.000	−0.001	0.000
Total through parental inputs	**0.017**	**0.016**	**0.016**	**0.039**
Total through mediators	**0.084**	**0.059**	**0.085**	**0.078**
Direct	**0.055**	**0.132**	**0.032**	**0.136**
Total	**0.139**	**0.190**	**0.117**	**0.214**

Sample 1 in columns 1 and 3 consists of births within a 6-year window of the mother's date of birth around RoSLA; sample 2 in columns 2 and 4 restricts to common support across mothers' age within treatment and control. Regressions control for mothers' age at birth. The bold text highlights the totals across groups and overall

A.8–A.9 in Appendix A.4 of the working paper version of our paper (Macmillan and Tominey 2020) report the results for the restricted sample, which are very similar to our benchmark analysis.

Next, we take the full benchmark sample and create a dummy variable for each mediator to take the value of 1 if the mediator is missing and 0 otherwise. The value of the mediator, if missing, is then replaced with the observed mean value. Interestingly our results, reported in Tables A.10–A.11 of Macmillan and Tominey (2020), are again very similar to our benchmark analysis, and suggest that the small sample of households who have incomplete information on meditators does not create a bias in our results.



Notes: Figure relating to Equation 6 and results in Table 6. Column 1 (2) decomposes the total effect of RoSLA on cognitive skills at ages 4-5 (test scores at ages 6-7) into the effect explained by family resources, parental investments and the unexplained (direct) component.

Fig. 2 Decomposition of the effect of RoSLA on child cognitive skills

7 Double treatment effects

Table 4 identified one potential channel for the effect of the educational reform of mothers on child skills was through the traits of her partner. We complement this analysis by considering that there may be a double treatment status within some households, if both the mother and her partner were exposed to the 1972 educational reform. This section estimates the effect of exposure to RoSLA, differentiating between households with no treated parents, 1 treated parent, and 2 treated parents. It could be that the indirect effects of RoSLA on children's ability are stronger in households where both spouses received the treatment, than in households where only one or neither spouse was treated. This is an interesting analysis; however, it is not possible to cleanly identify the double treatment effect. The reason is that whilst RoSLA of one individual can be interpreted as exogenous, who that individual partners with is not. Therefore, the following analysis is given a descriptive interpretation.

Defining the double treatment to equal 0 if neither spouse was treated, 1 if one spouse was treated and 2 if both spouses were treated, Table A.5 estimates the effect of double treatment on the test score at age 4/5 (column 1), age 6/7 (column 2), externalising (column 3), and internalising behaviours (column 4). All regressions control for mothers' age, as in our benchmark specification and additionally the partner's age when the cohort member was born. The sample size is somewhat smaller

than our benchmark due to missing data on the partner's date of birth (necessary to assign treatment status). The results suggest that compared to two treated spouses, having no treated spouse lowers child test scores by 19.4–22.6% of a standard deviation and raises the incidence of internalising behaviour by 15.9% of a standard deviation. There are also statistically significant differences in child skills when comparing the double treatment spouses to single treatment spouses. Child test scores are 15.7–25.5% of a standard deviation lower in single treatment households, and socio-emotional skills worse by around 10% of a standard deviation compared to double treatment households.

In addition, Table A.6 reports that having two treated spouses raises inputs in children, compared to having just a single treated spouse. Pre-birth human capital, household income, partner's education, hours, and parenting scores are all lower in single treatment households, whilst the mother is more likely to smoke and have more children. Interestingly time investments of both mother and partner with the child are higher in the single compared to double treatment households. Having a double treatment household does seem to be beneficial for child skills and for parental inputs, although note that a possibility is that this double treatment effect is picked up in our main analysis, through the higher education identified of the mothers' partner.

8 Conclusion

Across many developed countries, inequalities exist in the skills of young children, by the socio-economic status of their parents. In this paper, we have shown that at least part of this difference in early skill accumulation is causally driven by mothers' education. In particular, a reform which raised the compulsory schooling leaving age in the UK from 15 to 16 led to a shift from no qualifications attained towards some high-stake qualifications and a rise in cognitive skills of the children of affected mothers. On the other hand, there was no statistically significant effect on child socio-emotional skills.

Our main contribution is to understand how RoSLA for mothers — or more generally the decision to gain a minimum level of qualifications — impacts their own education, the skills of their children, and crucially, their parental inputs associated with child skills. Given the large proportion of individuals who leave school with no qualifications in OECD countries, understanding the benefit of gaining qualifications on the next generation's skills is crucial. Using rich longitudinal data, our analysis includes a wide set of potential parental inputs to allow us to pinpoint whether mothers' education drives child development through a solely financial channel, through assortative mating of the spouse or whether there is any role in the investment behaviours into child human capital. We are interested in which resources and inputs of parents change in response to the exogenous increase in education, which then drive child skill accumulation.

We interpret the results as follows. The reform to mothers' schooling raised the education of the marginal mother from leaving school with no qualifications to having at least a basic level of qualifications. This, in turn, impacted children's cognitive skill development suggesting

that a policy should strongly encourage individuals to stay in education until they gain important qualifications. Of the wide set of family resources and parental investments considered, there were five important inputs identified which were affected by the education reform. At the time of birth, treated mothers had accumulated more human capital, had matched with a higher quality partner in terms of his education and labour market attachment, and during the next 3 years of the child's life earned a higher household income. These variables were also associated with the cognitive skills of the child. The descriptive decomposition analysis shows that a significant proportion of the treatment effect of mothers' education on child cognitive skills was driven by these mechanisms, with family resources accounting for up to 60% of the total treatment effect.

Interestingly, we found also an important role for parental investments, over and above family resources, with monetary and health inputs accounting for a further 12–13% of the total treatment effect of RoSLA on age 4/5 and 6/7 cognitive child skills. Treated mothers smoked and drank less alcohol during pregnancy and invested more in educational toys and books at home, which raised cognitive skills of their children at school starting age. These inputs were again associated with child cognitive skill development. These results are robust to a number of alternative specifications, across a range of windows of mothers' birth dates, and suggest that improving education has wider ranging impacts, over and above the standard impact on financial channels.

How comparable are our estimates of the effect of RoSLA to papers in the wider literature? Piopiunik (2014) is the most similar to ours, evaluating an increase in education from a low level of 8 to 9 years of schooling in Germany. An increase in mothers' education through the reform raised the sons' probability to attain a middle school degree and translated into 0.79 more years of schooling — a large effect which authors suggest is due to the reform shifting the education track of children — set in Germany early in life. Carneiro et al. (2013) instead estimate more modest effects that raising mothers' education by 1 year raised test scores by around 8% of a standard deviation. We would expect our effects to be in between these two estimates, as unlike Piopiunik (2014) the UK does not have an educational tracking system and unlike Carneiro et al. (2013) the UK reform changed not just years of schooling but also qualifications of the treated mothers.

The average treatment effect of mothers' years of schooling from our estimates translates into an increase of 41% of a standard deviation in the test score.[24] We can translate this into years of schooling by correlating the test score measures at age 4/5 or 6/7 with a measure of the cohort members' completed years of schooling measured at age 26.[25] A 1 standard deviation increase in the test score at age 4/5 (6/7) is associated with an increase in years of schooling by 0.43 (0.67) years. Translating our estimates of RoSLA into the outcome years of schooling gives an estimate in between Piopiunik (2014) and Carneiro et al. (2013) of 0.41*0.43=0.18 (0.41*0.67=0.27) years of schooling.

We did not find effects of RoSLA on parent time investments, which may seem at odds with an influential paper by Guryan et al. (2008) which suggested instead a high correlation. However, the high correlation in Guryan et al. (2008) was true only when comparing mothers without a high school education (who spend 12.1

[24] RoSLA raised years of schooling by 0.29 years and raised test scores by 0.12 standard deviations, giving an average treatment effect of mothers' schooling of 0.12/0.29=0.41

[25] Note the smaller sample for this question of 2338–2779 individuals

h on average per week in total childcare), to someone with a college degree (who spends 17 h per week on average). More related to our paper is the comparison of the first group of low educated mothers, to a mother with high school degree — who in Guryan et al. (2008) spends 12.6 h per week on average on total childcare — a very small difference comparable to our estimate. Whilst large maternal education differences are required to generate meaningful differences in inputs such as time investments in Guryan et al. (2008), we can see instead that a relatively small change in education estimated in our paper — moving from no qualifications to some qualifications — generates changes in other inputs such as financial resources and smoking habits during pregnancy, which then improve second-generation outcomes. This itself highlights the importance of gaining at least basic qualifications.

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Declarations

Competing interests The authors declare no competing interests.

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