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The Impact of Free Early Childhood Education and Care on Educational Achievement: A Discontinuity Approach Investigating Both Quantity and Quality of Provision

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The Impact of Free Early Childhood Education and Care on Educational Achievement: a Discontinuity Approach Investigating Both Quantity and Quality of Provision

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Abstract

In this paper we analyse whether entitlement to free part-time early childhood education and care at 3 years old affects educational attainment in the first year of primary school. Our identification strategy exploits date-of-birth discontinuities that lead to some children born just a few days apart being entitled to different amounts of free pre-school (up to 3.5 months) while starting school at the same time and within the same cohort. Using administrative data on all state school pupils in England, we carry out a regression discontinuity analysis and find that eligibility to free part-time early education and care results in a zero overall effect on educational achievement at age 5. This is true for advantaged and disadvantaged groups and for children attending high and low quality provision.

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1. Introduction

In 2002 the President of the European Union set a policy goal for countries to provide childcare to at least 90 percent of children between age three and school start by 2010 (EU, 2002). This goal, and other shifts in national policy towards more early years investment, is supported by a body of evidence that shows investment in early education is both positively related to school readiness and early cognitive development (Cascio and Schazenbach, 2013; Felfe et al., 2015; Hansen and Hawkes, 2009; Havnes and Mogstad, 2011; Mathers and Sylva, 2007) and an effective way of reducing early cognitive gaps between children from different socio-economic backgrounds (Barnett, 1995; Cunha et al., 2006; Felfe and Lalive, 2014; Felfe et al., 2015; Heckman et al., 2010; Havnes and Mogstad, 2012; Karoly et al., 2006; Knudsen et al., 2006).

In the UK the most important, and expensive, form of early years' investment is the free entitlement to part-time early education (hereafter the "free entitlement").[1] This policy was rolled out across England in the early 2000s, and 94 percent of children now benefit from part-time early childhood education and care (ECEC) at age 3 (National Audit Office, 2016). Prior to its introduction 37 percent of children benefited from publicly provided nursery education at the discretion of their local authority (Blanden et al., 2016). Currently all 3 and 4 year olds are entitled to 15 hours of free care, a provision which costs the government around £2 billion per year (Department for Education, 2013).

In earlier work, we carried out a comprehensive evaluation of this policy (Blanden et al., 2016). In order to detect its causal effect we exploited geographic and temporal variation in the availability of free places during its implementation phase. We found that the provision of the free entitlement was associated with substantial crowding-out, i.e. it largely replaced private with public investment in early education, with only 2.7 genuinely new places created for every 10 new free places funded. Partly because of this, we found only very limited impacts on educational achievements at age 5, although effects were larger in areas where participation changed most in response to the roll-out. Surprisingly however, we could not detect any significant effects at age 7 even among groups with the largest impacts at age 5.

One of the most consistent findings in the ECEC literature is that it is "high quality" childcare that matters (Cascio, 2015; Ulferts and Anders; 2016). In Blanden et al. (2016) we speculate that the quality of early education may not have been high enough to generate long-lasting improvements in educational achievement. The recent expansion of free childcare

[1] For a summary of other recent investments in early years in the UK see Stewart (2013).

places in England was achieved through private rather than public provision, and private provision has been shown to be very heterogeneous in terms of quality and of low quality on average during the roll-out period (Gambaro et al., 2015; Mathers et al., 2007). However it is hard to evaluate the importance of quality. This is in part because of the limited data that is routinely collected on nurseries and pre-schools and because it is hard to find a research design such that quality is exogenously determined and therefore independent of parental characteristics and choices.

Blanden et al. (2017) investigate the associations between various measures of quality in private sector settings[2] and outcomes, revealing that there are very weak correlations between outcomes at age 5 and the presence of staff with degree-level qualifications in the setting, and the same is true for inspection ratings by the government regulator for standards in education (Ofsted). An alternative approach is to regress outcomes on observable child and provider characteristics and a dummy variable for the setting attended. The coefficient on the variable for each setting (the setting fixed effect) can be seen as picking up unobservable factors which contribute to higher achievement and we find that it has a wide variance; implying that there are differences in quality between settings that are hard to capture using available measures. However, although controls are included for the child's characteristics and the school that they go on to attend we cannot completely rule out selection bias caused by endogenous matching between children and settings. It may be that in Blanden et al. (2017) we are measuring the association between outcomes and some combination of quality and the child's characteristics.

This paper provides further evidence on the impact of the UK free entitlement on educational attainment building on the approaches and findings of Blanden et al. (2016) and Blanden et al. (2017). In the current paper we analyse whether the educational impacts of the free entitlement vary by the quality of the provision while employing an identification strategy that allows us to overcome issues of selection. Specifically, we make use of variation in eligibility to a free place due to date of birth discontinuities created by strictly-enforced administrative rules. These mean that children born just a few days apart are entitled to different amounts of free early education (around 3 and a half months, or a term) while starting school at the same time and within the same school-cohort.

[2] The data used only allows us to distinguish quality among settings in the private (PVI) sector. This is true in both Blanden, Hansen and McNally (2017) and here.

Using administrative data from the National Pupil Database we measure the effects of the eligibility discontinuity on teacher-assessed measures of academic and social skills (the Foundation Stage Profile) recorded at the end of the first year of school (the Reception year). We have information on the precise date of birth for children born 4 weeks before and 4 weeks after the two relevant eligibility cut-off dates (31st December and 31st March) for cohorts of children who started school from 2008/9 to 2011/12, providing us with over 600,000 observations in our analysis. Our findings show no significant benefit from an additional term of eligibility; and no suggestion that this varies according to the quality and characteristics of the setting attended.

An important contribution of our analysis is the investigation of how the impact of the length of time spent in early education varies with the quality of the experience. We use two established structural quality indicators, staff qualifications and pre-school quality ratings by the Government regulator for standards in education (Ofsted). Moreover, we attempt to capture quality indirectly by distinguishing settings by sector (public vs. private) and by type (pre-school vs. day nursery) and by studying settings with high fixed effects. Although none of these measures are perfect indicators of quality, we believe that by using all of them we are most likely to pick up complementarity between quality and quantity if it does exist. However, our findings show no significant benefit from an additional term of eligibility and no suggestion that this varies according to the quality and characteristics of the setting attended as captured by our different available measures. Given our large number of observations this zero effect is precisely estimated.

Our treatment is the effect of being entitled to one extra term of part-time education and care, whereas many other studies in this area show impacts for longer periods of full-time provision. We must therefore consider whether we would expect to find any significant effects of this additional dose. Results from a closely related study by Cornelissen et al. (2013) indicate that substantial effects on age 5 educational achievement can be found from each month of full-time education at age 4, in the order of 6 to 9 percent of a standard deviation. Assuming these effects are linear in the number of months, we would expect our treatment to generate an increase in educational achievement between 21 and 32 percent of a standard deviation (a term is about 3.5 months).[3] However, as already discussed, we know

[3] This result is obtained by multiplying the effect of 0.06-0.09 of a standard deviation for one month found in Cornelissen et al (2013) by the 3.5 months in an average term. It assumes that the developmental benefits of the free entitlement are the same as the benefits of the switch from the free entitlement to full-time school (this is defensible on the basis that it is a similar change in terms of number of hours).

that the free entitlement generated substantial crowding out (Blanden et al., 2016) and the likelihood of finding an impact of eligibility will depend on the extent to which attendance responds to the policy. Our analysis on an additional dataset, the Family Resources Survey, shows that attendance at formal childcare settings increases by 10 percentage points with eligibility, and for some children there are additional anticipation effects as they start nursery in the term before they become eligible. Given this, we would expect an intention to treat estimate of at least 0.021 or 0.032, which would be still statistically distinguishable from the null of a zero impact given our large sample.

The next Section discusses the related literature, considering research on the impact of universal early education and the importance of its quality. Section 3 describes the institutional background to the English education and childcare sector, and more specifically, the free entitlement. Section 4 lays out our empirical strategy, based on a regression discontinuity design, while Section 5 provides information on the data used. Section 6 describes our results, and Section 7 concludes.

2. Related Literature

There is by now a large body of work examining the effects of early interventions and pre-school programmes on children's outcomes. Evidence from small scale, randomized interventions targeted at disadvantaged children found positive effects on children's attainment during the school years that, in some cases, extended into adulthood (Barnett, 1995; Heckman et al., 2010; Karoly et al., 2005).

International evidence on the impact of universal provision is more mixed. For example, Gormley and Gayer (2005) use an age-based discontinuity design and find a large positive effect of a pre-K program implemented in Oklahoma on cognitive scores and language scores of children in the following year. Positive effects of childcare on children's short-run outcomes are also found by Fitzpatrick (2008) and Cascio and Schazenbach (2013) in the US and Berlinski et al. (2008) in Uruguay, Havnes and Mogstad (2011) for Norway and Felfe et al. (2015) in Spain. A range of other studies find no or even negative effects of universal childcare on child outcomes. Baker et al. (2008) and Herbst and Tekin (2008) find negative effects of the introduction of subsidised universal childcare in Quebec and of subsidies for child care provided to working mothers in the US, respectively, on various measures of early attainment, but, in particular on aspects of social and emotional development. More recently, Datta Gupta and Simonsen (2010) find no effects of pre-school enrolment on outcomes at age 7 in Denmark.

The literature suggests that the impact of provision might be heterogeneous across the population, with some indication that disadvantaged groups benefit more (Felfe et al., 2015; Felfe and Lalive, 2014; Havnes and Mogstad, 2011, 2012;). For example Gormley and Gayer (2005) show that the most positive effects of the pre-K program in Oklahoma were felt by Hispanics and Blacks, while for the white majority the impacts were largely non-significant. Havnes and Mogstad (2011) find that an increase in publicly provided child-care in the 1970s in Norway had larger long-term benefits for children of low educated parents, while Dustmann et al. (2013) document large effects of free public child-care places on school readiness measures of children from immigrant families, with no effects on children of natives in Germany. Notably, Blanden et al. (2016) find only limited evidence that the educational benefits of the free entitlement were larger for the most disadvantaged groups.

In a paper closely related to ours, Cornelissen et al. (2013) seek to understand the impact of early formal schooling in the English institutional setting. This paper uses data on children who started school in 2005 when variation in school starting age by term of birth was relatively common. This variation is used to identify the combined effect of the age at starting school and the length of time spent in the first year of school (the Reception year). An additional month in school with the earlier start that it implies leads to an improvement of 6 to 9 percent of a standard deviation in the Foundation Stage Profile, a teacher-assessed measure of child performance in several areas of academic, social and physical development, with effects felt through to age 7.

Ours is the first study to consider the impact on educational development of differences in the number of terms of the free entitlement a child experiences, although others have looked at the impact of the quantity of early education provided in other institutional settings (a number are reviewed in Ulferts and Anders, 2016). Of particular relevance to our analysis is the Effective Provision of Preschool Education (EPPE) study which followed a sample of children from their nursery experience in the late 1990s through to the end of compulsory schooling. Sylva et al. (2004) and Sammons et al. (2007) find that an earlier start at nursery improved progress up to age 7 but not beyond this. Although the EPPE study is able to condition on test scores at entry to nursery and a large number of background factors, variations in duration of ECEC are a consequence of parental choice, so it is not possible to give a causal interpretation to these results. More similar in spirit to this paper is a new study by Külne and Oberfichtner (2017) who exploit a fuzzy RDD in Germany that leads to a five month difference in the age at start/number months of ECEC received. A variety of outcomes are studied and no short- or medium- impacts are found.

An important contribution of our analysis is the investigation of how the impact of the length of time spent in early education varies with the quality of the experience. Within the economics literature the conclusion that quality matters is generally reached by comparing the features of programmes which show substantive benefits such as those in Norway (Havnes and Mogstad, 2011), Spain (Felfe et al. 2015) Oklahoma and Georgia (Cascio and Schazenbach, 2013), with those with no benefits such as Quebec (Baker et al., 2008), and Denmark (Datta-Gupta, 2010). Cascio (2016) draws on a comparison among US states and finds that universal systems have much greater benefits for disadvantaged children than targeted programmes. However, she finds it hard to identify the precise features of universal systems that lead to their success.

The educational literature on early education and childcare quality distinguishes structural quality from process quality and often measures the link between quality and outcomes for children within a particular national ECEC system. Structural quality consists of the more easily observable aspects of the setting such as space, staff-child ratios, group size and qualifications of staff. Process quality is measured by trained observers using inventories that record children's experiences and interactions in the setting; these can result in scales that provide an overall measure of quality or be focused on how nurseries encourage particular pre-academic skills. Ulferts and Anders (2016) provide a meta-analysis of European longitudinal studies of the impact of various aspects of both structural and process quality on child development; including the results from EPPE. They find that measures of process quality are consistently related to children's outcomes. However, structural quality has a weaker association with outcomes; with the exception of staff qualifications which do have predictive power across a number of studies.

Blanden et al. (2017) explore the associations between children's school achievement at age 5 and various measures of the quality of the free entitlement. The presence of staff qualified to degree-level, staff-child ratios, group size, nursery type and cohort size are examined as measures of structural quality, and the outcome of the most recent regulatory inspection report is considered to potentially pick up aspects of both structural and process quality (see Blanden et al. 2017 for further discussion of this). It is found that these standard measures of quality are only weakly related to children's outcomes. Despite this there is evidence of substantial unexplainable differences in outcomes between nurseries.

The current literature clearly indicates that universal early education has the potential to have beneficial effects on children's development. However, less is known about the impact of varying the number of months of participation, the margin we investigate here. In addition, as the evidence leads us to believe that the benefits of early education are higher in high quality settings, we consider the interaction of the quality of provision with the quantity of provision.

3. Institutional Background

Since 2004 all English Local Authorities have been funded to provide universal part-time early years education and care for children from the term after their third birthday. For the cohorts we study here this was 12.5 hours for 38 weeks a year until 2010, extending to 15 hours per week from September 2010 onwards[4]. Since 2013, disadvantaged two year olds have also been offered 15 hours of free care, but this did not affect the sample of children in our analysis.

In England all children enter primary school in the academic year in which they turn 5 (the Reception year). While in the past many schools operated different intake policies, usually allowing younger children to start later in the year, in more recent years most schools have adopted a unique intake date in September. This implies that irrespective of their date of birth, all children within a school-cohort (going from 1st September to 31st August) start formal schooling at the same time (but at a different age). By contrast, eligibility to free part-time pre-school care changes discontinuously across the year; children born between 1st September and 31st December are entitled to claim their free hours from the following January, children born between 1st January and 31st March from April, while those born between 1st April and 31st August are allowed to claim their entitlement only from September of the following school year. To the extent that children's participation is governed by their entitlement, children who experience more months in ECEC will also start at a younger age. Our analysis considers only children born around the 31st December and 31st Match cut-offs, who are different in respect of their eligibility for free early education and care but start school at the same time and belong to the same school cohort.

Prior to the introduction of the free entitlement local authorities had a choice to provide free early education in nursery classes attached to schools, or more rarely, through stand-alone nursery schools. This type of state provision was more common in urban areas with Labour-controlled councils and was often targeted at low-income households in receipt of means-tested or unemployment benefits. The expansion of the policy to universal coverage

[4] This change will affect the last cohort in our sample, however there is no evidence that results are different for this group.

was primarily made through funding places in the private[5] sector. This means that around half of children are provided their free place in the state sector and the other half in the private sector, with eligibility rules being the same across both sectors.

Although the policy entitles all children to the same number of hours, the type of early education experience the child will have will vary depending on where they take up their place. State provision is usually more restrictive in terms of hours available, often either five mornings or five afternoons, and usually does not extend outside school hours. It is a requirement that a qualified teacher is present in the maintained sector, with an adult-child ratio of 1:13. Requirements for qualifications are lower in private settings, but if there is no qualified teacher or Early Years Professional (see below) present then the ratio of adult per child is increased to 1:8 (Gambaro et al., 2015). There is also substantial variation within the private sector, with day nurseries focusing on full-time care, so that often the entitlement acts only as a discount on fees. Pre-schools, which often evolved from community play-groups, usually offer care only during school hours and terms and more commonly on a not-for-profit basis. If state settings are not available in the area it is more likely that those not already in childcare will take up their entitlement at a pre-school as the two have common characteristics and can be therefore seen as substitutes.

Although the structural aspects of provision vary, all providers are required to follow a common curriculum, the Early Years Foundation Stage. The curriculum emphasises learning through play, ensures that a range of stimulating activities are provided and that children's development across a range of areas is encouraged and monitored.

All settings are subject to inspection by the Government regulator Ofsted (Office for Standards in Education), roughly every four years. Settings are evaluated against their ability to deliver the Early Years Foundation Stage; and this covers both structural aspects such as space and resources, and an evaluation of the activities the children take part in. Ofsted awards an overall grade ranging from 1, Outstanding to 4, Inadequate for overall effectiveness, as well as subgrades across a number of areas including: leadership and management, quality of provision, and outcomes for children (Mathers et al., 2012). Mathers et al. (2012) compared measures of process quality with Ofsted ratings and found only weak relationships. The authors note that differences may be partly explained by the fact that

[5] What we name here the private sector is actually more completely described as the Private, Voluntary and Independent (PVI) sector as it also includes settings run on a not-for-profit and nurseries attached to independent (fee-paying) schools.

Ofsted inspections are based on broad criteria and on the whole-setting level, whereas process quality measures relate to the activity in a particular room.

Our Early Years Census data - available for the private sector only - provides information on how many staff are qualified teachers and, from 2008/9 onwards, how many are accredited as having Early Years Professional Status (EYPS). [6] As mentioned previously, it is a requirement for staff in the state sector to have at least one qualified teacher – a degree level qualification - per class; but there is no such requirement in private settings; although the regulations on ratios give private nurseries an incentive to have a member of staff qualified to degree level on staff; either a QTS or EYPS. Notably, the EYPS does not qualify individuals to work as a nursery teacher in the maintained sector, implying that the two qualifications are not viewed as comparable.

Previous work has drawn attention to differences in the quality of childcare experienced by children making use of the free entitlement. Gambaro et al. (2015) and Blanden et al. (2017) examination of the Early Years Census and the National Pupil Database suggests that disadvantaged children are likely to have access to more qualified staff as they tend to be more concentrated in the state sector, but those in the private sector are found in settings with fewer staff qualified to degree level and which receive lower quality scores in Ofsted inspections. These findings are supported by Mathers and Smees (2014) who demonstrate that there are inequalities in process quality, with lower quality found in nurseries in more disadvantaged areas. Given these differences it is particularly important to gather as much evidence as possible on the impact of quality, and we add to this evidence here.

4. Empirical Strategy

In England access to free part-time early education and care is based on strict date-of-birth rules. The cut-offs which define eligibility can be shown to be randomly assigned with respect to the observed determinants of test scores, and are assumed to be unrelated to their unobservable determinants. We can therefore pursue a Regression Discontinuity Design (RDD) to identify the impact of eligibility on educational achievement (Imbens and Lemieux,

[6] The EYPS was created in 2006 as an alternative to Qualified Teacher Status for leaders in this field, and both qualifications are considered as degree-level qualifications. In order to be awarded EYPS individuals are required to demonstrate that they meet 38 professional standards when working with children from 0 to 5 years old. Training routes vary and accreditation can take from four months part-time to one year full-time depending on the experience of the individual (Mathers et al., 2012). Even the long route is considerably shorter than QTS training which usually takes three years full-time.

2008; Thistlethwaite and Campbell, 1960). We define an indicator variable which takes value 1 when the child is born before a certain cut-off date $\bar{t}$ (and is therefore entitled to start at $\bar{t}$+1) and 0 otherwise. This variable effectively defines eligibility to free part-time early education and care over the next school term; we will call it $T_{i,}$, and write it as:

$$T_i = I\{t_i < \bar{t}\} \tag{1}$$

where t_i is the date of birth of the child.

In this institutional setting eligibility may affect child development through a number of mechanisms. We might expect it to influence (i) how many terms the child attends early education for and the age at which he/she starts, (ii) the number of hours of early education a child is exposed to, (iii) the type of setting a child attends (if for example on becoming eligible a child is transferred from a place in the private sector to a public place), and (iv) the disposable income of the family.

Bearing these potential mechanisms in mind we estimate the reduced form equation[7] of test scores in the first year of school on the eligibility to an additional term of pre-school, $T_{i,}$, while controlling for individual characteristics X_i:

$$Y_i = \beta T_i + \Pi X_i + \varepsilon_i \tag{2}$$

Our variable of interest, eligibility, is a function of date of birth. In a system like the English educational system, where children take their assessments at the same time, date of birth determines age at test. As there is a well-documented positive relationship between age at test and test scores (Crawford et al., 2014; Leuven et al., 2010), simply introducing an indicator variable for eligibility status will not be sufficient to ensure the parameter we estimate is the effect of eligibility only. Eligible children will be, on average, older than non-eligible children and therefore will have (by virtue of the positive relationship between test scores and age at test) better outcomes. It is therefore essential that we capture the eligibility effect by the impact of the discontinuity *conditional* on a flexible function of date of birth. Our main equation is therefore specified as follows:

$$Y_i = \beta T_i + f(t_i) + \Pi X_i + \varepsilon_i \tag{3}$$

The graphical analysis presented in the next section suggests that $f(t_i)$ is a linear function of date of birth and we use this formulation in most of our results, however we also run models where $f(t_i)$ is specified as a quadratic function of date of birth or as a linear function whose slope is allowed to change at the cut-off.

[7] It might seem natural to use eligibility as an instrument for participation in a 2SLS framework. However, this would not be legitimate as there are other mechanisms through which eligibility could plausibly affect attainment.

Our data includes information on two different cut-off dates: 31st December and 31st March. To test if the effect of eligibility is different by cut-off, in some of our analysis we will additionally include interaction terms between the eligibility indicator, T_i, and the March cut-off as well as interactions between the cut-off and f(t). Any differences could come from a difference in the size of the participation effect from the cut-off or because of differences in the age at school start. Those born just before the cut-off will start very close to age three in both cases. However those born just after the December cut-off will start in April at age 3 and 3 months, while those born just after the March cut-off will start in September at age 3 and 5 months. The extent to which eligibility effects differ between these groups provides an indication of the degree to which starting age might be an important driver of our results.

5. Data on children educational outcomes and eligibility

National Pupil Database

Our analysis is based on data from the National Pupil Database (NPD). This is an administrative dataset containing information on all children attending state (public) schools in England, and covering about 93 per cent of all pupils in the country. The dataset combines individual data on attainment and a limited set of characteristics of the children which are recorded by the school. The dataset is longitudinal, in that it follows each child over the primary and secondary school years, and contains school and Local Education Authority (equivalent to districts in the US) identifiers.[8]

We focus our analysis on children's educational attainment at age 5 because this is where we expect to see the clearest evidence of our treatment. At the end of Reception year children are assessed by their teacher in the different areas covered by the Foundation Stage Profile curriculum. For the children in the cohorts we examine this involved 13 assessment scales, each with a range between 1 and 9. The 13 assessment scales are grouped into six areas of learning: personal, social and emotional development; communication, language and literacy; problem solving, reasoning and numeracy; knowledge and understanding of the world; physical development, and creative development. Children who score between 1 and 3 points are deemed to be working “towards” the Early Learning Goals (ELGs), children scoring between 4 to 8 points are working “within" the ELGs, a score of 6 or above is considered as working “securely within” the ELGs, and a 9 point score is regarded as

[8] There are around 150 Local Education Authorities – which are responsible for setting admissions policies – in England.

working “beyond” the ELGs (Department for Education, 2012). As assessments are conducted by teachers in schools we control for school fixed effects in all our analyses. Standard errors are clustered at the date of birth level, as this variable defines our treatment, i.e. eligibility to an extra term of early childhood education and care.

Our main outcome measure is the standardised total points score in the FSP; where standardization is by academic year to take into account concerns about grade inflation. We also make use of the thresholds mentioned above and consider binary indicators for meeting the expected level in communication, language and literacy (*literacy*), problem solving, reasoning and numeracy (*numeracy*) and social and emotional development (*social development*) as well as achieving a score of 6 or above in all thirteen areas of learning, which we call ‘*working within the expected level’.*

In the NPD we also observe a number of individual background variables. These are gender, eligibility for Free School Meals (FSM), ethnicity, whether the first language spoken at home is English, whether the child has been defined by the school as having Special Educational Needs, and the level of income deprivation in the area around the child's postcode of residence. As families entitled to FSM are usually in receipt of means tested benefits and/or with one if not both jobless parents, this indicator will be used to distinguish low- from high-income families, although it is not always a good proxy (Hobbs and Vignoles, 2010). The data also contains date of birth, and the date at which the child was enrolled in the school. We use this information to define school entry policies, as described below.

NPD Estimation Sample

In order to perform our analysis we need information on precise date of birth of the child. This information is usually restricted, but we obtained access to NPD data with date of birth for several cohorts, including children starting school between the academic years 2008/09 and 2011/12. These data can be matched with their age 5 (Foundation Stage Profile) outcomes. Because of the confidential nature of the data, we only obtained information on a subsample of all children within the year, and can only analyse data on children born up to 4 weeks before and after 31st December and 31st March cut-offs. This means that we have information on children born in 16 weeks of the year and do not have information on children born in the month of February or May, for example.

We apply the usual checks on the data and remove pupils with duplicate data entries or with missing data on school-identifiers, but keep children with missing information on some of the background and outcome variables. Our checks also show that the proportion of children from non-White British families and the share of children who speak English as an additional language is very high among those born on January 1st. We think this is due to administrative data issues, whereby children from immigrant families are registered as having a January 1st date of birth. As these children usually score lower on standard educational tests, including them in our analysis would lead us to overstate the effects of eligibility. We therefore exclude any child born on January 1st from our analysis.[9] We also exclude "special schools" that exclusively cater for children with specific needs, for example because of physical disabilities or learning difficulties, as well as schools specifically focused on children with emotional and/or behavioural difficulties (less than 1 percent of pupils).

Taking into account differences in school entry policies is very important in our analysis. In England children start formal schooling at age 4, in the academic year in which they turn 5.[10] The school cohort consists of all children born between the month of September of one year and August of the following year. Most children start school in September irrespective of their date of birth, but in some schools children born later in the school year are allowed (or even encouraged) to start school in the second or third term (i.e. in January or April, respectively). School entry policies can differ quite markedly across the country, and this variation in school entry age has been exploited in previous work which looked at the effect of month of birth (Crawford et al., 2014) or early schooling (Cornelissen et al., 2013) on educational outcomes. Differences in school starting ages are potentially problematic for us as they can make it difficult to distinguish the impact of eligibility for free ECEC and length of formal schooling. We therefore use information on date of birth and date of enrolment to identify the schools where a significant proportion of children starts in January or April. Children attending these schools are excluded from our analysis, but as this policy as become less common in recent years this affects just 4 percent of pupils.[11] The remaining sample contains 620,568 pupils with non-missing observations on the total FSP score.

Early Years Census

Children who experience their nursery education in the state sector can be observed in the NPD in the year before they start school, but very little information on the quality of these

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[9] In order to balance out the number of observations around the 31st December and 31st March cut-off, we also exclude children born on April 1st.

[10] About 96% of children in England are educated through the public school system, and it is very rare for a child to delay for one year or start earlier than age 4.

[11] Our initial checks reveal that our analysis is robust to including/excluding the schools for which we cannot define a school entry policy.

settings is available. However, we can match in information from the Early Years' Census (EYC), for the 52 per cent of pupils attending private sector settings. Completion of the Early Years Census is compulsory for private sector settings that receive state funding for providing the free entitlement. We match children in the NPD with their record in the EYC in the year before they start Reception.[12]

The EYC collects data at both the setting-level and the child-level. We are particularly interested in two variables collected at the setting level. The EYC reports the type of setting attended, allowing us to distinguish between day nurseries (where care is available full-time) and pre-schools where care is available for half-days or school-hours only. Information is available on staff who are qualified teachers (QTS) and have Early Years Professional Status (EYPS). Questions on qualifications are asked in respect of all staff and also more specifically about those carers working with the children who receive the free entitlement.[13] In addition, information is also collected about the total number of staff (again in the whole setting and working with 3 and 4 year olds) and the total number of children.

Both the NPD and the EYC use the same unique child identifiers, which enables us to match children between the datasets. This allows us to identify children who appear in the EYC and the NPD in the year before they start school. Where a child attends more than one pre-school setting, we keep the observation for the setting where he/she attends most hours (which is necessary for less than 1 per cent of observations).

We are able to complement the information from the Early Years with information on Ofsted ratings. We have data on all assessments made for Early Years Settings between 2005 and 2011, and we match to each child and their setting the rating that is closest in time to their attendance. We are able to do this for 80 percent of children who attend in the private sector. The exception to this is children who attend settings linked to fee-paying schools; in this case only half can be matched to an Ofsted inspection.[14]

Descriptive Statistics

[12] All children should be receiving the free entitlement at this point, regardless of their date of birth.
[13] Analysis in Blanden et al. (2017) indicates this distinction is not important in practice, so we use 'graduate present' in the setting as our variable of interest here.
[14] Our use of the Ofsted data is complicated by a change in the inspection regime in 2008. In the 2005-2008 cycle, childcare settings were inspected on quality of care with judgements based solely on the 14 National Standards for Daycare. Where the childcare provider was eligible to deliver the free early education entitlement, they were also inspected on quality of nursery education against the criteria in the Curriculum Guidance for the Foundation Stage. Post 2008 all settings where judged on their delivery of the EYFS. Previous analysis in Blanden et al (2017) indicates that this change does not matter.

The descriptive statistics for child outcomes at age 5 are shown in Appendix Table 1 where we report the standardised scores in the FSP overall, and for individual areas as well as the proportions of children achieving a good level of development in all areas. We present statistics for the whole sample, and by gender and free school meal status. As is common, girls out-perform boys in all outcome measures, with the gap being generally smaller in numeracy than in literacy. Even larger differences in attainment can be found between children eligible for free school meals and all the other children. In our sample years 55 percent of all children are judged as working within the expected levels across all areas, but only 38 percent of those on free school meals achieve this target.

The characteristics of the children in our sample and of the nurseries they attend in the year before they start school are shown in Appendix Table 2. We observe that almost 18 percent of children are eligible for free school meals in Reception[15], 65 percent of children are from a white British background and 17 percent speak English as an additional language. As was the case in the mid-2000s (Blanden et al., 2016) we observe around half of the children in nurseries in the state sector and the rest in the private sector. Of those in the private sector roughly half are educated in day nurseries (43 percent) and half in pre-schools (46 percent) which focus on shorter hours care and are more likely to be provided by not-for profit organisations. As highlighted by Gambaro et al. (2015), the proportion of children in private settings with at least one qualified teacher is low, at just under 30 percent. Twelve percent of children are attending a setting rated Outstanding. As discussed in Blanden et al. (2017) the majority of other settings are rated Good, with 22 percent rated Satisfactory or Inadequate, this is why we focus on the consequence of attending an Outstanding setting.

Is Eligibility Randomly Assigned?

As standard in analyses based on a Regression Discontinuity research design, we start by plotting the distribution of births (date of birth is our *running variable*) either side of the two cut-offs. This is in order to investigate whether the entitlement date had any effect on the day on which a child was born. One could think, for example, that parents who were aware of the importance of the eligibility rule (because they were well-informed or because they had an older child) might have timed the birth of their child so to receive more free part-time child

[15] From September 2014 all children in Reception and Years 1 and 2 receive school meals for free, but this is outside our sample period.

care. If so, we would expect to see relatively more births in the days preceding the 31st December or the 31st March, and fewer births in the first few days after these dates.

The first panel of Figure 1A plots the relationship between date of birth and number of children born on each day for the four weeks around the December cut-off. The first line shows the raw number of births on each day. Although there is no systematic pattern with regards to the cut-off we do see some non-random patterns here. In particular there is a clear weekly pattern in the number of births - with fewer of them occurring at weekends - and also we see a sharp drop around Christmas, these patterns are likely to be driven primarily by the timing of planned caesarean sections. We also plot residuals from a regression of the number of births on days of the week dummies and controls for the date falling within the weekend, during festivities (Christmas and Boxing Day, or Easter) or other public holidays. The pattern of births is now much smoother over time with no relationship between the number of births and the cut-off. The same is true when looking separately at each year in our sample in the second panel of this Figure, and for the March eligibility cut-off shown in the two panels of Figure 1B.

Another way to test for non-randomness in eligibility is to check that individual characteristics are not correlated with eligibility status. We do this in two ways. Firstly in Figure 2, we plot visual evidence about the distribution of the individual characteristics we observe by day of birth of the child. We find no evidence that individual characteristics are correlated with eligibility. Secondly, we run regression analyses where we test for the presence of a discontinuity in observable characteristics either side of the cut-off using a similar model to equation (4), but using individual characteristics as outcomes. Results (shown in Appendix Table 3) show no effect of eligibility on child observable characteristics, with the exception of SEN status. Here we find that children born before the cut-off date are more likely to be registered as having Special Educational Needs; where eligibility results in a 0.3 percentage point increase (or an almost 3 per cent increase over the mean) in the probability of being identified as a child with difficulties in learning or having social and behavioural problems.

The effect of eligibility on SEN status could be a causal effect of the policy. For example, it is possible that by being exposed to ECEC earlier than comparable children, eligible children develop more problematic behaviour and are therefore more likely to require a statement of Special Education Needs. Alternatively, it is also possible that eligible children are more likely to be identified as having learning or behavioural difficulties simply because they are observed by qualified staff for a longer period of time. Either way, SEN status appears to be affected by eligibility status, and as such it will be excluded from our set of controls in the regression analysis which follows.

6. Participation in early childhood education and care

The National Pupil Database and Early Years Census do not contain information about the date that a child starts at a setting, we only know if they are present at the turn of the calendar year. This means we cannot relate eligibility directly to start date and we cannot estimate the impact of attendance on early educational achievement. Rather we estimate the impact of eligibility; the parameter estimated by equation (3) is an intention to treat effect.

This parameter is of interest per se, as it encompasses effects of the policy which might work through other channels (e.g. income effects), nonetheless it is still useful to analyse the relationship between eligibility and attendance as this allows us to understand how much of any effect might be due to participation. To do so, we use another dataset, the Family Resources Survey. This is an annual cross-sectional survey of UK households with interviews continuously running throughout the year. We use the years 2005-06 to 2012-13 and select children living in England.

In these data, we observe the date of interview and the month of birth of the child, so that we can define the child's age in months (rather than days). Because of the rules determining when children become entitled to free early education, we divide children into three groups according to their month of birth, denoting those born in September through December as "Autumn born" (eligible for free early education from the January after they turn 3), those born January through March as "Spring born" (eligible from the April after they turn 3), and those born April through August as "Summer born" (eligible from the September after they turn 3). We consider children as attending childcare in the reference week if the parent reports they are cared for in a day nursery or pre-school. The data unfortunately does not allow us to distinguish between private and state settings.

The fact that we do not know the child's precise date of birth (and we have a much smaller sample size) means we cannot use the same RD design we adopt for our main analysis and need to use a different empirical strategy. Specifically, we model children's participation in ECEC as a function of their term of birth (Autumn, Spring or Summer) and their eligibility, where the latter is defined by the age of the child at interview (eligibility takes value 1 if the child is observed after becoming eligible for the free entitlement and 0 otherwise). We then construct interactions between term of birth and eligibility. This is equivalent to a difference-in-difference design, where we allow each group of children – as defined by their term of birth – to be affected by the free entitlement policy depending on their age at interview.

In the regression we also control for date of interview (month and year) and some family characteristics such as the age and education of the main carer and the number of siblings in the family. It is important that we control for age, as children will be more likely to attend a nursery or a pre-school as they become older, independent of their eligibility status. This means that if we use a very short window of data (say children between 30 and 40 months of age) our eligibility variable might simply capture the effect of age at interview. In order to address this issue, we include in our regression children from a wider age spectrum (i.e. from 24 to 60 months) and control for a flexible function of the child's age in months. This way we are more confident that we can separate the effect of age from the effect of eligibility, as indeed the different specifications in Table 1 show.

The first panel of Table 1 shows our results. Eligibility to the free entitlement increases the use of childcare by 11 to 13 percentage points for the Summer-born, 10 to 11 percentage points for the Autumn-born, and 8 to10 percentage points for the Spring-born, although these coefficients are not statistically different from each other. Our specification also includes a dummy for the term before a child become eligible to capture anticipation effects. It is possible that families are prepared to enter their child into an early education setting a few months before the child becomes eligible in order to take advantage of available spaces for example. We expect this effect to be larger for children born in the Autumn term, who become eligible in January but might start attending in the September of the year before, at the start of the academic year. Indeed, we find that the Autumn born children experience an increase of about 6 percentage points in attendance to ECEC the term before eligibility. This implies that the treatment effect for these children might be as much as two terms of additional early education and care, so we might expect to find higher impacts of the 31st December cut-off as compared to the 31st March cut-off on educational outcomes.

As discussed in Section 4, we might expect families to respond to eligibility in ways other than a change in attendance at the extensive margin. The second panel provides information on the impact of eligibility on hours of attendance, showing that eligibility increases the average hours used per week by about 2 (higher for the Autumn-borns than Spring-borns). Notice that the sample here is confined to those who attend, so this shows how those already enrolled change their hours once the universal subsidy is applied.

Brewer et al. (2017) conduct a very similar analysis as background to their assessment of the impact of the free entitlement on parental employment. Their main findings are very similar to ours, and they make two additional relevant points. First, they show that children often switch from informal to formal childcare on becoming eligible. Second, they show that expenditure on childcare falls by just £7 a week when a child becomes entitled, suggesting that we should not expect income effects to be a relevant mechanism through which the free entitlement operates.

Table 2 provides further results on the impact of eligibility on attendance, this time disaggregated by family background. This analysis can help us to understand differences in the reduced form effects of eligibility, for example stronger eligibility effects in poorer groups could be explained by strong impacts of eligibility on participation for those who are likely to be financially constrained. Blanden et al. (2016) note that the participation effect from the roll-out was larger among more disadvantaged groups, and there is some evidence of comparable effects here. Many of the effects observed for the full sample seem to be confined to those who are in the middle 50 percent of the income distribution, rather than the richest or poorest quartiles, but there is evidence of slightly stronger effects among those who are eligible for means tested benefits, with eligibility effects for those born in the Autumn term of almost 14 percentage points, and anticipation effects for this group of 10 percentage points, compared to 13 and 6 percentage points, respectively, that we estimate across all Autumn-borns.

These results demonstrate that eligibility for free early education does cause changes in childcare use; but that there is substantial crowd-out, with only around 10 percent of children taking up eligible care in the term they become entitled. As the effects of eligibility on hours and expenditure are small we would expect most of any educational impacts detected to come through the effect of eligibility on participation. As already discussed in the introduction, using the results from Cornelissen et al. (2013) as a benchmark we might expect that an additional term of early education would lead to between 0.21 and 0.32 of a standard deviation improvement in the FSP if there was full compliance and no additional effects from income. Adjusting this by the participation effect of around 10 percent we might expect to find impacts of between 0.02 and 0.03 of a standard deviation; slightly higher effects for those on free school meals could be explicable by a higher first stage for this group.

7. Main results

As a first piece of evidence about the impact of eligibility on educational attainment at age 5, the upper panel of Figure 3 plots standardised FSP scores for girls and boys either side of the two cut-offs, adjusting for both the day of the week and festivity effects discussed earlier and for average differences across schools (this is particularly important as these assessments are conducted by teachers). Each figure also plots two regression lines showing the estimated linear relationship between the outcome and the birth date separately for the period before and after the cut-offs in order to facilitate the visual interpretation of the results. We see that during the 4 weeks before and after the cut-off dates (31st December in the top panel and 31st March in the lower panel) there is a clear negative relationship between the outcome and the date of birth. However we do not see a discontinuity at the eligibility date. Figure 4 shows similar plots, now distinguishing between children who are, and who are not, eligible for Free School Meals (girls and boys are combined). Again, we see a clear negative relationship between the outcome and the test score in both cases, but no discontinuity. These graphs provide the first evidence that eligibility has no obvious impact on outcomes, despite the fact that it changes attendance by about 10 percentage points as shown in our analysis of the Family Resources Survey.

The graphical analysis also reveals some important features of the data which guide the specification of our regression models. First, we see evidence of a mostly *linear* relationship between the outcome and the birth date. Although we will consider also quadratic specifications, there is no suggestion that significant non-linearities are at work within the 4-week window before and after each cut-off. Also, the graphs do not show any substantial differences between the December and March cut-off points, or by gender, so we will begin with analysis which pools data across these dimensions, relaxing these assumptions in later specifications.

We run four specifications of our main model, as outlined in section 3, we take into account date of birth in the 4 weeks around each cut-off using: (i) a linear term (specification 1), (ii) a quadratic term (specification 2), (iii) a linear term which is allowed to change at the cut-off point (specification 3), and finally (iv) a version of specification 3 that allows the age and eligibility effects to vary across the two cut-offs (specification 4). Level differences in outcomes between children born around the 31st of December and children born around the 31st March are captured by a separate dummy. All our regressions also control for a full set of dummies for year, festivity and bank holiday dummies, free school meal status, ethnicity, language spoken at home, and area of deprivation deciles. Finally, we include school-level fixed effects and cluster the standard errors by date of birth.

The results of these models are shown in Table 3 indicating no effect of eligibility on the total FSP score and confirming the previous graphical analysis. . This is true for specifications using different sub-components of the score as the dependent variable and is the case when using a binary rather than a continuous indicator (i.e. being "within the expected level in all areas of assessment"). Furthermore, there is no substantive difference between the impact of eligibility at the December and March cut-offs. Differences in the impact of eligibility by cut-off date could be due to differences in age at enrolment, as just missing the March eligibility deadline means a greater difference in the age at eligibility than missing the December deadline[16], or could come about because of the more substantial effects on attendance at the December cut-off. There is no evidence that the combined effects of these mechanisms are important.

It is possible that the impact of an additional term of ECEC might differ by the child's characteristics. We examine this possibility in Table 4 where we test the hypothesis that impacts are larger for children who are more disadvantaged; either as a result of a stronger effect on participation, or because participation is more beneficial. If anything, the evidence goes in the opposite direction to what we might expect, as we see that an additional term of eligibility raises total FSP score by 1 percent of a standard deviation for children who live in the least disadvantaged areas (significant at the 10 percent confidence level). Comparing across the columns indicates that the estimated effect is similar across all specifications. Results for all other subgroups are insignificantly different from zero.

As previously discussed we may expect our results to vary by childcare setting. So in Table 5 we explore the question of heterogeneity in an alternative way focusing on whether effects differ by setting characteristics. Panel A focuses on settings with observable markers of quality, these include the presence of a QTS/EYP, the Ofsted rating and the setting type. As noted previously, day nurseries tend to provide all day care and primarily aim their

[16] Those born just after the December cut-off will start in April at age 3 and 3 months, while those born just after the March cut-off will start in September at age 3 and 5 months. If starting early has positive effects on outcomes, then we would expect the impact of eligibility to be higher for the March than the December cut-off. On the other hand, if starting later is more beneficial, then the effects should be smaller (in absolute value) for the March than the December cut-off.

services at working parents while pre-schools provide less flexible hours. The setting type therefore offers a proxy for a bundle of different characteristics which might affect the quality of the child's experience. Once again, however, no statistically significant effects are found. The largest effect quantitatively is for private sector nurseries with an Outstanding Ofsted rating, where attendance for an additional term leads to an improvement in FSP of 0.022 of a standard deviation. However, the standard error is too large for us to confidently differentiate this effect from zero. We have tried a variety of alternative specifications using interactions to see if these reveal a stronger result, but there is no evidence of this.

Finally, Panel B takes a different approach to demarcating quality by focusing on nurseries with high and low values of the fixed effects as estimated in Blanden et al. (2017); potentially these indicate unobservable quality. These fixed effects identify nurseries where children attending go on to do better in the FSP, conditional on the school they attend, their own characteristics and the average characteristics of all children in the child's nursery cohort. The effects are obtained from running high-dimension fixed effects models of the total FSP score on the child's characteristics, their peers' characteristics and fixed effects for both their school and ECEC setting. Blanden et al. (2017) provide further technical details. Specifications (1) and (2) divide private nurseries into top and bottom quartiles based on these fixed effects while (3) and (4) do this on the basis of residual fixed effects net of nursery characteristics (type, the number of children attending, the staff-child ratio and the presence of a QRS/EYP). Results are in the right direction with positive effects for an additional term of eligibility when attending a high quality setting and negative effects in low-quality settings, but none of the coefficients are significantly different from zero.

8. Conclusions

Previous research evidence encourages us to anticipate that the provision of universal free part-time early education and care will have beneficial outcomes for child development at young ages. An evaluation of the initial roll-out of the free entitlement in England (Blanden et al., 2016) indicates that the policy did not initially deliver child development benefits. This paper evaluates the same policy using a different research design, and asks whether accessing the free entitlement one term earlier (and therefore getting an additional term of ECEC before school start) has any educational benefits. Unlike most of the previous literature, we can analyse whether the effects of an extra term of ECEC differs by the quality of the care provided in the setting, using both observed and unobserved measures of quality. Previous literature suggests only good quality early education will be beneficial, and we seek evidence that some types of settings are generating educational impacts.

We find that the entitlement to free early education and care has large crowd-out effects, with only just over 10 percent of children starting nursery when they become eligible. Nonetheless, if the effects of one extra month in pre-school education are similar to those observed in reception classes (Cornelissen et al., 2013) we should still detect feasible effects of the free entitlement policy due to our large sample size. Perhaps disappointingly, we estimate that overall the effects of the policy are statistically indistinguishable from zero, implying that the development benefits of informal early education are not as large as those for infant school.

There are a number of explanations for this finding. First, it could be that children benefit more from early education at age 4 rather than age 3 but this seems unlikely given the body of evidence suggesting that earlier interventions are most effective (Heckman and Mosso, 2014). Second, it could be that the families who participate when they become eligible are not those who benefit the most from participation, although this seems unlikely as we find higher behavioural responses among disadvantaged groups, who are usually thought to benefit more from ECEC.

Third, and we would argue most plausibly, it could be that the quality of formal education and ECEC in the UK are not comparable. This seems likely given the wide variation in nursery experiences of children in England and the gap in funding and resources between ECEC and formal schooling.[17] The addition of data from the Early Years Census enables us to probe this story further and analyse whether the effects of the free entitlement vary by observed or unobserved quality measures of the setting. If only high quality ECEC matters, then we should be able to find some evidence that the effects of an extra term are higher in state provided settings, in settings with a higher number of qualified teachers, which receive better Ofsted ratings, or which score best according to their unobservable characteristics. Once again, even for these groups, we do not find any significant effects.

It is possible that our measures of quality are not well suited to capture important differences in the early education experience of young children, or it is possible that the quality of early education and child care provision in the UK is overall too low compared to

[17] Belfield, Crawford and Sibieta (2017) show that average spending per pupil in the Early Years is £1,700 a year, less than half of the average annual spending per pupil in primary school.

that experienced in other countries. This would perhaps explain why we do not find any significant return to an extra term of ECEC even in the best quality setting.

Another possibility is that the quantity is simply not sufficient. In other words, it could be that part-time provision is simply too little to have any substantive developmental benefit, and it is not until children attend full-time hours that they really begin to gain from early education. Unfortunately we do not have any way to assess this issue, but evidence from the extension of the free entitlement to 30 hours will soon provide new insights on this hypothesis.

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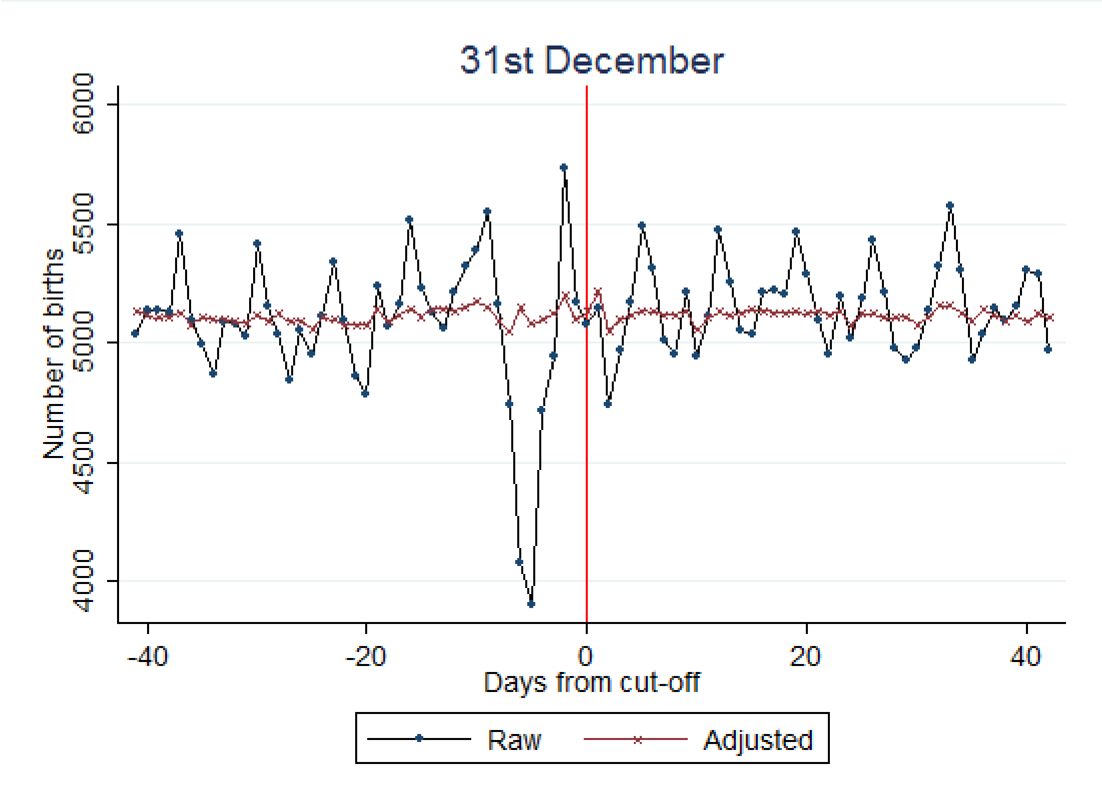
Figure 1a: Number of births before and after 31st December



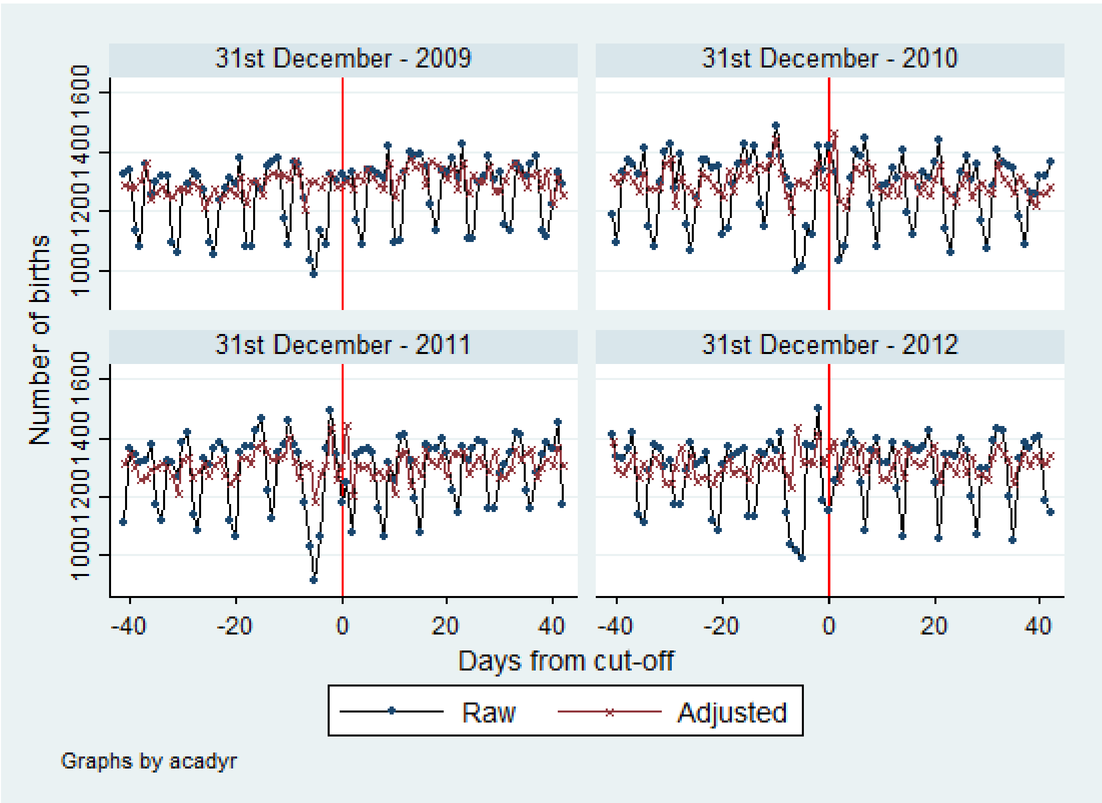
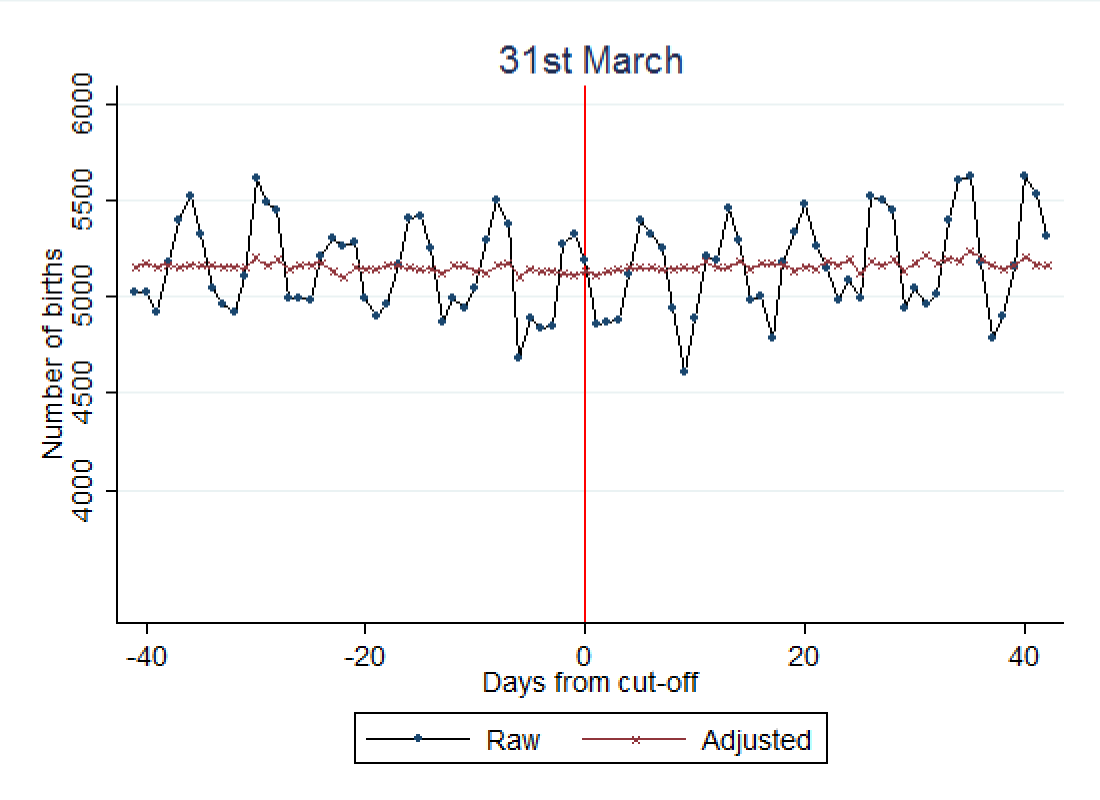


Figure 1b: Number of births before and after 31st March



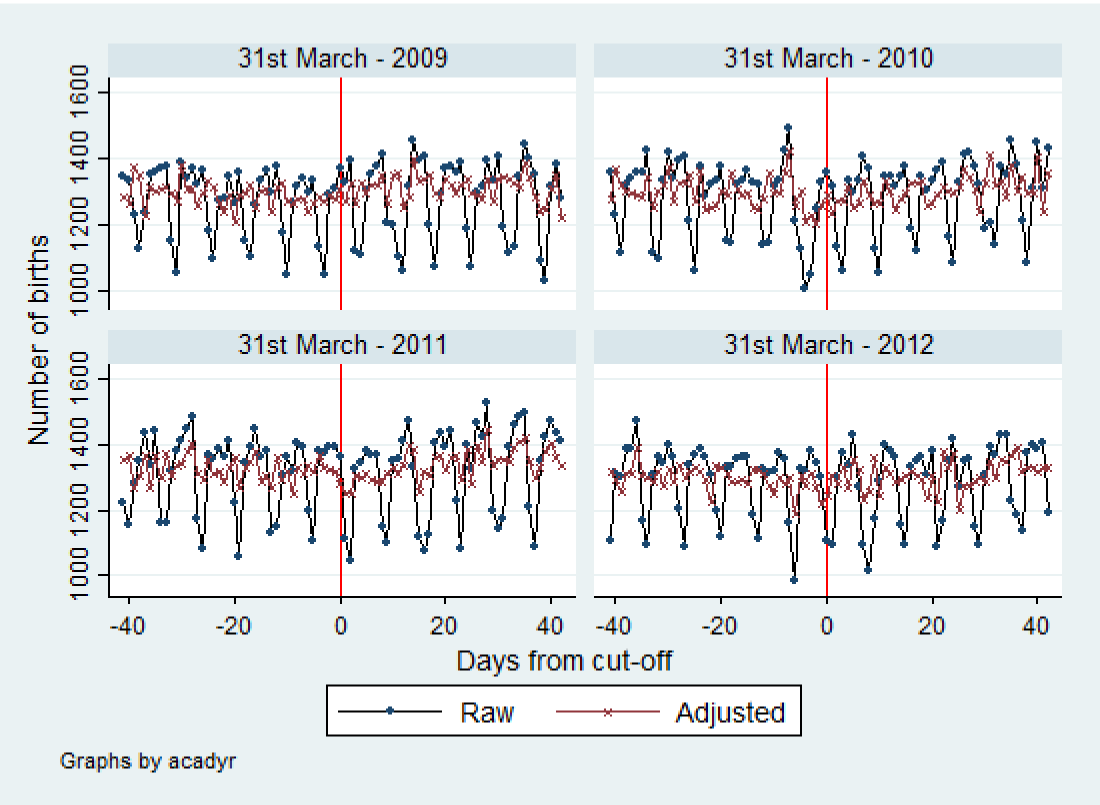


Figure 2: Observable characteristics by day of birth before and after cut-off dates

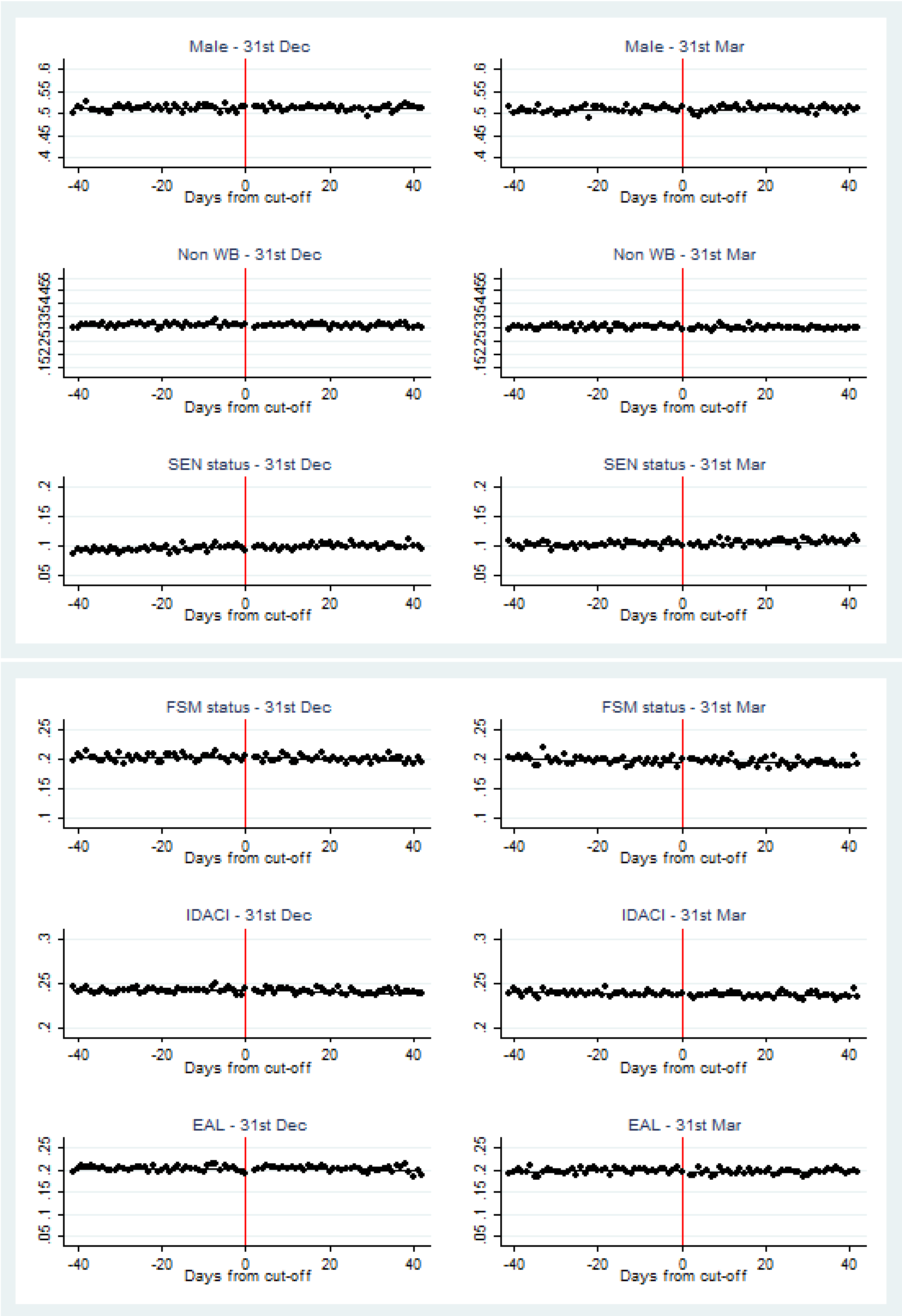


Figure 3: Effect of eligibility on total score in Foundation Stage Score, by gender

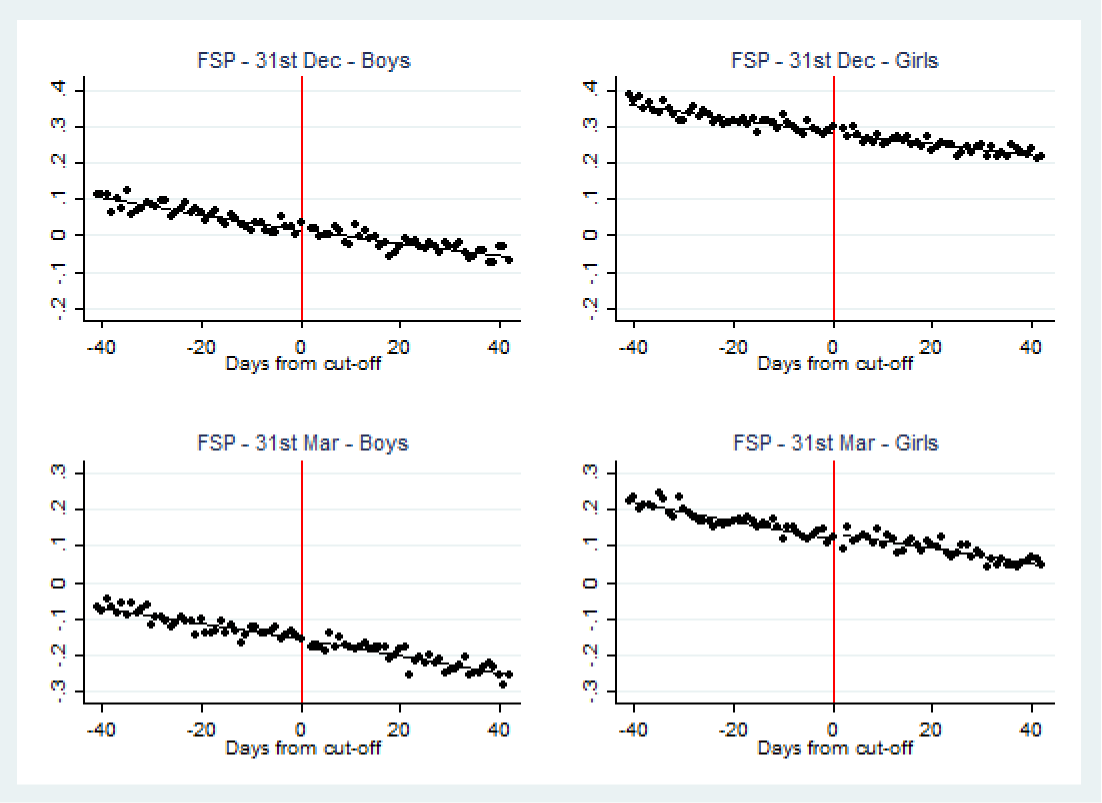


Figure 4: Effect of eligibility on working within the expected level for the Foundation Stage Score, by FSM status

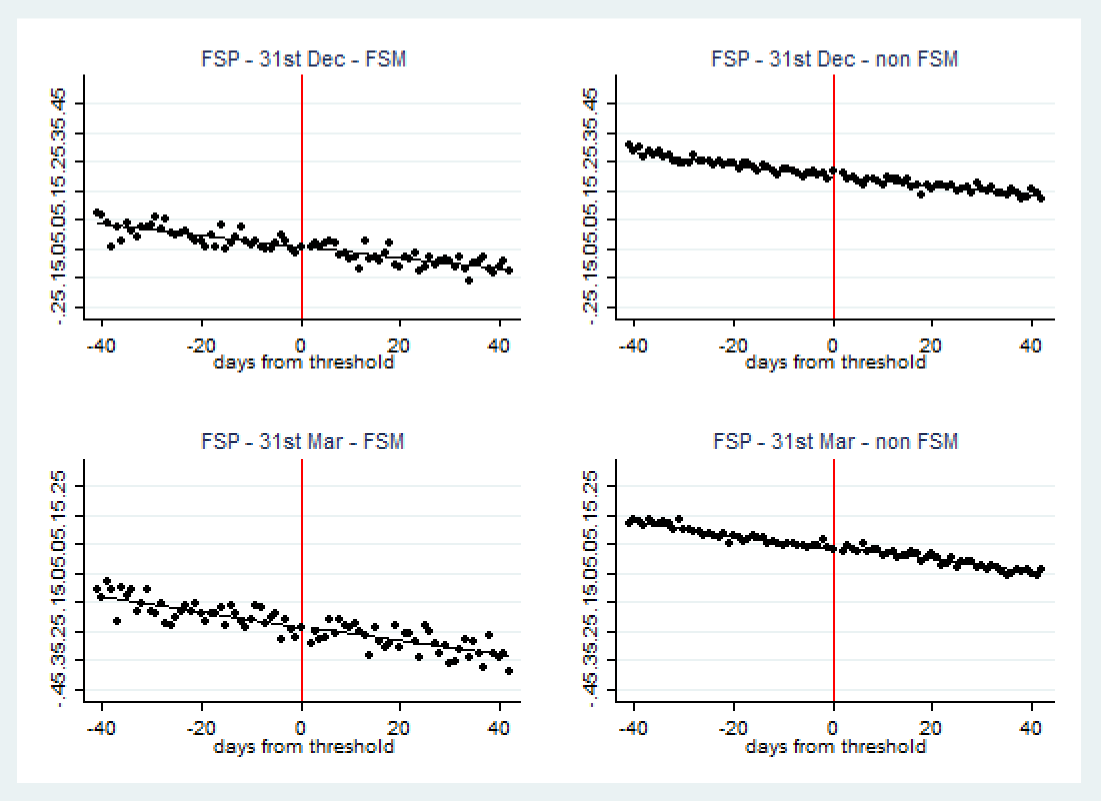


Table 1: Childcare use by eligibility status, Family Resources Survey

	Attendance at eligible setting			Hours conditional on attendance		
	Quadratic	Cubic	4th polynomial	Quadratic	Cubic	4th polynomial
Summer born	0.00436	0.00224	0.00216	0.137	0.0854	0.109
	(0.0175)	(0.0179)	(0.0178)	(0.393)	(0.401)	(0.406)
Autumn born	-0.0314*	-0.0305*	-0.0304*	-0.648	-0.627	-0.646
	(0.0158)	(0.0160)	(0.0158)	(0.494)	(0.497)	(0.500)
Spring born - eligible	0.0980***	0.0844***	0.0866**	2.650***	2.319***	1.636**
	(0.0275)	(0.0288)	(0.0341)	(0.623)	(0.637)	(0.648)
Summer born - eligible	0.113***	0.103***	0.105***	3.145***	2.886***	2.137**
	(0.0278)	(0.0279)	(0.0362)	(0.712)	(0.705)	(0.793)
Autumn born - eligible	0.133***	0.119***	0.122***	3.537***	3.204***	2.558***
	(0.0290)	(0.0320)	(0.0410)	(0.701)	(0.717)	(0.639)
Spring born – antic.	-0.0323	-0.0329	-0.0317	-0.557	-0.571	-0.937
	(0.0243)	(0.0237)	(0.0241)	(0.662)	(0.653)	(0.728)
Summer born – antic.	0.00890	0.00989	0.0112	-0.112	-0.0886	-0.476
	(0.0280)	(0.0284)	(0.0306)	(0.466)	(0.481)	(0.543)
Autumn born – antic.	0.0625**	0.0622**	0.0633**	1.118*	1.108*	0.759
	(0.0259)	(0.0251)	(0.0264)	(0.599)	(0.583)	(0.502)
Observations	13,423	13,423	13,423	13,423	13,423	13,423
R-squared	0.183	0.183	0.183	0.198	0.198	0.198

Note: Table shows coefficents from linear regression of use of eligible childcare regressed on variables related to quarter of birth, interactions between quarter of birth and a dummy for eligibility, and interactions between quarter of birth and a dummy for term before eligibility. Standard errors clustered by month of age in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Sample is children aged between 24 and 60 months.

Table 2: Childcare attendance by eligibility status, comparing by family background, Family Resources Survey

	High income (25%)	Middle income (50%)	Low income (25%)	Family receives means-tested benefits	Family not in receipt of means-tested benefits
Summer born	0.00982	-0.00456	0.0324	0.00839	-0.0196
	(0.0301)	(0.0202)	(0.0289)	(0.0164)	(0.033)
Autumn born	-0.0288	-0.0375*	-0.00924	-0.0218	-0.0432
	(0.0339)	(0.0227)	(0.0319)	(0.0184)	(0.037)
Spring born - eligible	0.0981	0.0710	0.0902	0.110***	0.0472
	(0.0745)	(0.0520)	(0.0757)	(0.0418)	(0.085)
Summer born - eligible	0.0843	0.103**	0.0534	0.117***	0.0764
	(0.0744)	(0.0514)	(0.0741)	(0.0413)	(0.085)
Autumn born - eligible	0.0907	0.124**	0.0739	0.136***	0.0667
	(0.0730)	(0.0501)	(0.0719)	(0.0403)	(0.083)
Spring born – antic.	-0.109	-0.00690	0.0161	-0.0196	-0.0699
	(0.0714)	(0.0498)	(0.0727)	(0.0398)	(0.085)
Summer born – antic.	-0.0827	0.0448	-0.0176	0.0567*	-0.121**
	(0.0542)	(0.0377)	(0.0535)	(0.0303)	(0.061)
Autumn born – antic.	0.0138	0.0554	0.0613	0.0902***	-0.0541
	(0.0626)	(0.0415)	(0.0601)	(0.0337)	(0.070)
Observations	3,326	7,158	2,906	10,814	2,609
R-squared	0.156	0.143	0.097	0.115	0.187

Note: Table shows coefficents from linear regression of use of eligible childcare regressed on variables related to quarter of birth, interactions between quarter of birth and a dummy for eligibility, and interactions between quarter of birth and a dummy for term before eligibility. Standard errors clustered by month of age in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Sample is children aged between 24 and 60 months.

Table 3: Effect of eligibility on educational outcomes at age 5

	Regression of dependent variable on eligibility and age controls specified as:				
Dependent variable	Linear (1)	Quadratic (2)	Kinked (3)	Linear-changing at cut-off (4)	
				Main effect	March interaction
Total FSP					
Eligibility effect	0.007*	0.007*	0.006	0.010	-0.004
	(0.004)	(0.004)	(0.004)	(0.006)	(0.009)
Literacy					
Eligibility effect	0.009*	0.009*	0.007	0.011	-0.004
	(0.005)	(0.005)	(0.004)	(0.006)	(0.009)
Numeracy					
Eligibility effect	0.007	0.007	0.005	0.009	-0.004
	(0.005)	(0.005)	(0.004)	(0.006)	(0.009)
Social development					
Eligibility effect	0.004	0.004	0.002	0.007	-0.006
	(0.004)	(0.004)	(0.004)	(0.006)	(0.009)
Within expected level in all areas					
Eligibility effect	0.001	0.001	-0.000	-0.001	0.003
	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)

Notes:

1. FSP Total is the sum of all areas of development reported as part of the Foundation Stage Profile. Literacy is shorthand for Communication, Language and Literacy which is the sum of scores on three measures of development in these areas. Numeracy is shorthand for problem solving, reasoning and numeracy which is the sum of scores on the three measures of development in these areas. Social Development is the score for personal, social and emotional Development which is the total across these three measures.
2. Children are classified as "Within expected level in all areas" if they have scores of 6 and above in all the 13 scales used in the FSP.
3. FSP Total, Numeracy, Literacy and Social Development measures are all standardised within cohort.
4. All regression models control for the child's year of birth, sex, free school meal status, ethnicity, whether they speak English as an additional language and the deprivation of the area where they live measured by the decile of the neighbourhood of residence on the Income Deprivation Affecting Children (IDACI) scale.
5. In addition to the age controls described in the column headings and in the text all models control for the day of the week the child was born on and whether they were born on a bank holiday or during another festive period.
   * indicates that p<.010. ** indicates that p<0.005. *** indicates that p<0.001.
6. Sample sizes are 620,568 for total FSP, 620,541 for literacy, 620,502 for numeracy and 620,555 for social development.

Table 4: Effect of eligibility on educational outcomes at age 5, by child characteristics

	Boys (1)	Free School Meals (2)	Living in third most deprived area (3)	Living in middle third deprived area (4)	Living in third least deprived area (5)
Eligibility effect	0.010	-0.003	0.002	0.012	0.012*
	(0.007)	(0.014)	(0.009)	(0.008)	(0.007)
Sample	317,588	91,018	199,137	200,936	202,096

See notes for Table 3.

Table 5: Effect of eligibility on educational outcomes at age 5, by setting characteristics

	Panel A: Subgroups defined by observable setting characteristics					
	Private setting (1)	Maintained setting (2)	Private Setting with QTS/EYP staff (3)	Private Setting rated Outstanding (4)	Private pre-school (5)	Private day nursery (6)
Eligibility effect	0.004	0.007	-0.007	0.022	0.011	-0.007
	(0.006)	(0.007)	(0.012)	(0.023)	(0.009)	(0.007)
Sample	286,518	284,114	84,125	27,890	132,675	123,768
	Panel B: Subgroups defined by setting fixed effects calculated on the basis of differences in FSP performance					
	Top 75% of private settings (1)	Bottom 25% of private settings (2)	Top 75% of private nurseries – residual FE (3)	Bottom 25% of private nurseries – residual FE (4)		
Eligibility effect	0.016	-0.010	0.012	-0.004		
	(0.013)	(0.018)	(0.013)	(0.018)		
Sample	60,257	54,860	60,447	54,400		

Notes:

1. See Notes for Table 3.
2. The setting fixed effect in Panel B specifications (1) and (2) is derived from a regression on FSP total on school and setting fixed effects, conditional on individual characteristics and the mean of individual characteristics in the child's cohort in the setting.
3. The setting fixed effect in Panel B specifications (3) and (4) is derived from further regressing the fixed effect on observable characteristics of the nursery: its type; the number of children attending; the staff-child ratio and the presence of a graduate.

Appendix Table 1: Summary Statistics for outcomes at age 5

	All	Boys	Girls	Free School Meals	Not Free School Meals
FSP total	0.0607	-0.0802	0.208	-0.336	0.159
	(0.931)	(0.973)	(0.861)	(0.965)	(0.884)
Literacy	0.0594	-0.101	0.227	-0.338	0.158
	(0.945)	(0.979)	(0.876)	(0.970)	(0.901)
Numeracy	0.0580	0.00324	0.115	-0.315	0.150
	(0.928)	(0.983)	(0.862)	(0.996)	(0.875)
Social development	0.0419	-0.121	0.212	-0.306	0.127
	(0.948)	(0.991)	(0.869)	(0.969)	(0.915)
Working within the expected level in all areas	0.547	0.461	0.636	0.383	0.585
	(0.498)	(0.498)	(0.481)	(0.486)	(0.493)
Sample size	620,493	317,588	302,980	109,209	506,774

Notes:

1. FSP Total is the sum of all areas of development reported as part of the Foundation Stage Profile. Literacy is shorthand for Communication, Language and Literacy which is the sum of scores on three measures of development in these areas. Numeracy is shorthand for problem solving, reasoning and numeracy which is the sum of scores on the three measures of development in these areas. Social Development is the score for personal, social and emotional Development which is the total across these three measures.
2. Children are classified as “Within expected level in all areas” if they have scores of 6 and above in all the 13 scales used in the FSP.
3. FSP Total, Numeracy, Literacy and Social Development measures are all standardised within cohort.
4. The gender and free school meals status is based on data reported in the Reception year.

Appendix Table 2: Sample characteristics

	% of Estimation Sample
Child characteristics	
Male	51.2
Free School Meals at age 5	17.7
White British	64.6
English as an additional language	17.1
Special Educational Needs	9.29
Most deprived third of areas at age 5	33.1
Middle deprived third of areas at age 5	33.4
Least deprived third of areas at age 5	33.6
Nursery characteristics	
Attended nursery in pre-school year	91.9
Of those who attend nursery	
Attend maintained setting	49.8
Attend PVI setting	50.2
Of those who attend a PVI setting:	
Attend PVI day nursery	43.2
Attend PVI pre-school	46.3
Attend PVI setting with QTS/EYP present	29.4
Attend PVI setting with Outstanding OFSTED rating	11.6
Sample	621,017

Appendix Table 3: Effect of eligibility on observable variables at age 5

	Regression of dependent variable on eligibility and age controls specified as:				
	Linear	Quadratic	Kinked	Linear changing	
Dependent variable				Treatment effect	March interaction
Male	0.001 (0.003)	0.001 (0.003)	0.001 (0.002)	-0.004 (0.004)	0.010+ (0.005)
FSM	-0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)	0.002 (0.003)	-0.004 (0.004)
White British	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.004 (0.003)	0.004 (0.004)
SEN	0.003* (0.002)	0.003* (0.002)	0.004* (0.002)	0.003 (0.002)	0.001 (0.003)
EAL	0.002 (0.002)	0.002 (0.002)	0.002 (0.002)	-0.001 (0.003)	0.007+ (0.003)
Most deprived	-0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)	-0.000 (0.003)	-0.000 (0.004)
Least deprived	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)	0.000 (0.003)	-0.004 (0.004)

Notes: All regression models control for the child's year of birth and in addition to the age controls described in the column headings and in the text all models control for the day of the week the child was born on and whether they were born on a bank holiday or during another festive period. + indicates that p<.010. * indicates that p<0.005. ** indicates that p<0.001. Sample sizes are 621,017 for sex, 616,282 for FSM, 615,936 for white, 616,282 for SEN, 532,005 for EAL and 602,481 for the most and least deprived.