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UNIVERSAL PRE-SCHOOL EDUCATION: THE CASE OF PUBLIC FUNDING WITH PRIVATE PROVISION*

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This article studies the effect of free pre-school education on child outcomes in primary school. We exploit the staggered implementation of free part-time pre-school for three year olds across Local Education Authorities in England in the early 2000s. The policy led to small improvements in attainment at age 5, with no apparent benefits by age 11. We argue that this is because the expansion of free places largely crowded out privately paid care, with small changes in total participation, and was achieved through an increase in private provision, where quality is lower on average than in the public sector.

Over the last several decades, many governments around the world have sponsored the expansion of early education and childcare. There is a dual rationale behind these policies: to support maternal employment and to promote child development, particularly that of disadvantaged children. This article focuses on the UK, specifically England,[1] a relatively late adopter of universal early education compared to other European nations. The social and economic context prevailing in the UK in the early 2000s makes the British expansion stand out. First, maternal employment was high and supported by an established childcare market of private and public providers; second, the expansion relied entirely on the private sector to provide additional free places which were funded and regulated by the state.

In this article, we evaluate the effect of the dramatic increase in free provision of early education and childcare for three year olds in England on early school outcomes.[2] This policy was part of a general shift towards intervention in the early years through both universal and targeted programmes. From 2000, fully subsidised part-time nursery places (hereafter 'free places') were rolled out across England for all three year olds. Despite considerable state funds invested (£2 billion per year, National Audit Office, 2012), this is the first rigorous study attempting to evaluate the effects of this policy. There are studies that offer an evaluation of similar policies in other countries.

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[1] We focus on England as education is one of the devolved responsibilities of the home nations of the UK. While similar policies exist in Wales and Scotland, differences in the detail of the policies and the availability of data mean we focus on England only.

[2] While the policy was clearly intended to provide early education, for ease of refence we use the shorthand 'childcare' for the remainder of the article.

However, the impacts may vary across contexts for many reasons. In particular, the positive effects on childrens' outcomes of the universal childcare provision introduced in Norway and France in the 1960s and 1970s (Havnes and Mogstad, 2011; Dumas and Lefranc, 2012) may not necessarily translate to a context where maternal labour market participation is already high and childcare markets are well-established. In addition, as the general level of education is higher in the 2000s than in previous decades, maternal care may not be such a poor substitute for pre-school as it was for earlier expansions.

A priori, one might think that early education has benefits for children's development. For example, research in the economics of human capital production has emphasised the importance of timely investments in child development, as differences in children's cognitive and non-cognitive skills emerge at early ages and early investments provide the foundations for later learning (Carneiro and Heckman, 2004; Cunha and Heckman, 2008; Almond and Currie, 2011). The strongest evidence in support of early years interventions is based on the favourable long-term evaluations of intensive programmes targeted at low income families, such as the Perry pre-school and Abecedarian projects in the US (Barnett, 1995; Karoly *et al.*, 2005; Heckman *et al.*, 2010). However, evaluation of policies designed to increase early childcare across larger populations have not always found positive results and the literature gives little guidance as to the best type of policy. For example, some evidence points to the harmful effects of long hours (Baker *et al.*, 2008; Datta Gupta and Simonsen, 2010), while other studies (Havnes and Mogstad, 2011) indicate that longer hours designed to accommodate working parents can have impressive long run benefits for child outcomes. One consistent finding is that effects are strong for disadvantaged or minority groups but small or even negligible for the rest of the population (e.g. Felfe and Lalive (2014) and Cornelissen *et al.* (2015) for Germany; Gormley and Gayer (2005) for the US state of Oklahoma; Fitzpatrick (2008) for the US state of Georgia). There are few studies in Europe about the effect of universal childcare policies on recent cohorts of children, Cornelissen *et al.* (2015) for Germany and Felfe *et al.* (2015) for Spain are exceptions. Both papers find positive effects, at least among specific groups of the population.[3]

In this article, we exploit the staggered implementation of free universal part-time pre-school education for three year olds across Local Education Authorities (LEAs) in England in the early 2000s. We have the advantage of access to census-level information of all children in state primary schools, with outcomes that are measured by teachers at age 5 and 7 following criteria and standards that are implemented nationally, and information on the results of national tests at age 11. We explicitly take account of the effect of other early years' initiatives, as well as child characteristics, area and cohort-fixed effects, and a vector of time-varying characteristics of the area which capture economic, health and political conditions that might affect child outcomes. We offer a number of checks showing that our results are robust to alternative ways of taking into account unobserved and time-varying area-specific factors.[4]

[3] The study by Black *et al.* (2014) on the effect of providing childcare subsidies in Norway is also of relevance, even though this is not aimed explicitly at universal provision of childcare in the same way as these other studies. They find positive effects on early educational achievement that operate through income effects.

[4] Dickson (2008) uses LEA-level data to investigate the early roll-out of the same policy, his results are in line with ours.

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Whether or not an expansion of childcare brings about developmental gains to children might be considered a function of at least three factors:

(*i*) the intensity of the additional provision;
(*ii*) the quality of the childcare being provided on account of the policy;
(*iii*) the quality of counterfactual care.

The effects of intensity are difficult to predict as this might be negative if young children spend too much time away from their parents or positive if it facilitates full-time employment for the parents (and thus improves their economic situation). Regarding quality, one would expect the effects of the policy to be positive if the quality of the counterfactual is low and/or if the quality of additional childcare is high. We provide a comprehensive analysis of the universal childcare policy in England by offering new empirical evidence regarding each of these aspects. Therefore, our article goes further than previous analyses in explaining the relationship between early childcare and child development.

We show that the childcare expansion of the early 2000s led to an increase in the free provision of places (possibly topped up by parental contributions) rather than to a large increase in children attending pre-school. Over the time period of interest (2002–7), only 2.7 genuinely new places were created for every 10 places funded. The policy was not high-intensity: it provided 12.5 hours a week of free childcare, with a very modest increase in hours across all children. Thus, the change in the quantity of pre-school education experienced by English children was fairly limited overall. In other words, there was a lot of crowd-out of private expenditure.

In view of these features, it is perhaps not entirely surprising that we find only a small effect of the policy on children's outcomes at age 5. Specifically, we estimate that a 10 percentage point increase in the proportion of three year olds covered by free places improves literacy and numeracy scores at age 5 by <2% of a standard deviation. Although the point estimates suggest that the effects are larger for children from more economically disadvantaged families (they are twice as large for children who are eligible for free school meals than for children who are not), the differences are usually not statistically significant and so small as to be economically negligible. Finally, we show that effects are short-lived for all groups, becoming essentially zero by age 7. This finding is mirrored in the school effectiveness literature where interventions such as class size reductions and improvements in teacher quality are found to diminish over time (Kane and Staiger, 2008; Chetty *et al.*, 2011).

These small effects are found despite evidence that the most disadvantaged families changed their behaviour as a consequence of the policy, increasing pre-school participation and reducing the use of informal care arrangements and parental care. Our assessment is that the new places, provided entirely in the private sector (in stark contrast to what happened in other European countries), were of insufficient quality to lead to lasting benefits. We show that the expansion of childcare took place in private settings of which only 10–20% employed staff holding a degree, compared to nearly full coverage in the public sector. The presence of graduate staff is considered as a key driver of observed quality in the previous literature (Mathers and Smees, 2014) and we support this claim with analysis which directly links the presence of graduate staff to better school outcomes for children.

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This study is one of the few analyses able to look at the effects of expanding childcare provision on recent cohorts of children. It shows the effects of a low-intensity policy in a context where parents were already using formal childcare in very large numbers before free provision was made universal. These conditions are likely to hold in many economies that are considering whether to invest more in childcare (like the UK and US) or less (in the light of austerity measures). To our knowledge, this study is unique in that it considers the effects of an expansion of childcare provision delivered almost exclusively by the private sector. This helps inform the debate on the potential benefits of for-profit providers in early child education. Our study relates to the literature on the private provision of public services which emphasises the importance of optimal contracts (Hart *et al.*, 1996; Jensen and Stonecash, 2005, *inter alia*). In line with this earlier theoretical literature our results point to the need to ensure strict quality thresholds are met by participating providers.

The remainder of the article is structured as follows. Section 1 provides information about the institutional context in which the policy operated, and describes the main features of the expansion. Section 2 outlines our empirical strategy and, in particular, discusses the importance of controlling for differences in the baseline level of places. Section 3 describes the data that we use, while Section 4 outlines our main results and robustness checks. In Section 5 we explore the extent to which childcare take-up was affected in terms of hours and places and differences across groups. In Section 6, we discuss the quality of childcare experiences that children receive and relate this to their outcomes. Section 7 concludes.

1. Institutional Background

1.1. *Childcare Policy in the UK and the Free Entitlement*

Unlike many of its European neighbours, the UK failed to establish a universal system of publicly provided early education and childcare during the 1960s and 1970s. Despite experiencing increases in the levels of female labour force participation comparable to those in Sweden, Norway and France, successive governments missed several opportunities to expand publicly-funded childcare during the post-war years. This was partly a consequence of an implicit commitment to the ideal of the male bread-winner model and partly due to unwillingness to extend the universal aspects of the welfare state in an era of tight budgets (Lewis, 2013).

Until major changes took place in the late 1990s, childcare and pre-school education were seen as quite distinct arenas. While the Department of Health oversaw full daycare provision for a strictly limited group of mothers in need, Local Education Authorities (LEAs) were responsible for pre-school education which they provided in nursery classes in schools and in stand-alone nursery schools. As LEAs were free to choose the level of coverage of such places, these were more likely to be found in Labour-dominated inner-city areas and targeted at the poorest families living within them (Lewis and Lee, 2002). Services were generally available for children in the year before starting school, i.e. primarily for four year olds.

Insufficient expansion in the public sector meant that the provision of childcare in the private, voluntary and independent (from here on simply ‘private’) sector grew.

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This consisted of private day nurseries suitable for working mothers and short-session playgroups. From the late 1980s, some financial support was available from the government in the form of employer tax breaks and subsidies for those on low incomes. Before the roll-out of universal childcare the sector had therefore evolved into a diverse mixed market with huge variation between different areas of the country. By the year 1999, only 37% of three year olds had a publicly funded childcare place, and the majority of children were being catered for by the private sector. The level of public funding was very low compared with mainland Europe (Pugh, 1996).

The New Labour Government that took office in 1997 brought in a number of changes to the *status quo*, extending universal education down the age range and focusing for the first time on the welfare of young children and their families. As well as announcing in 1998 the entitlement to free part-time pre-school education for all three and four year olds, the Government also launched Sure Start centres which were set up to provide help and advice on child and family health, parenting, money, training and employment as well as play sessions and (in some cases) childcare for families living in disadvantaged areas with pre-school children (Eisenstadt, 2011). Further, it extended maternity and paternity leave and subsidised childcare for working parents through the Working Families Tax Credit. The total spend on services for under fives rose from 0.24% of GDP to 0.71% of GDP. Additional changes in taxes and benefits made the system more generous towards families with young children, increasing the welfare spend on families with children by 1% of GDP (Stewart, 2013).

There were several motivations behind these policies. The first was to improve the life chances of young children as a growing body of research showed that gaps in development open up very early in life (Feinstein, 2003). In addition it was felt that providing help to mothers who wanted to return to work would reduce the number of children living in poverty, and halving the latter by 2010 (from the 1998 baseline) was a specific policy target set by that Government. Connected to this was a desire to promote gender equality. The policies pursued were a mix of targeted, means-tested and universal programmes. The childcare expansion, along with the extension to maternity and paternity leave, is an example of the latter approach.

The first step in the expansion of pre-school education was to establish universal free part-time pre-school for all four year olds. This consisted of 12.5 hours per week of childcare, during 33 weeks of the year, in 2.5 hour daily sessions, and was universally available by 2000.[5] The second step was to do the same for all three year olds. This happened more gradually. In the academic year 2000 the Department for Education provided funds for childcare places for three year olds in 65 LEAs, and from 2001 places were rolled out across the country. The aim was to achieve universal coverage of all three year olds by 2004.[6]

Panel (*a*) of Figure 1 gives an overview of the development of pre-school education of children aged three between 1999 (the year before the age 3 roll-out began) and

[5] After the period analysed in this article, the entitlement was made more generous. In 2008, it was extended to 38 weeks per year, in 2010 to 15 hours per week with a gradual shift towards families being able to organise their free hours entitlement over fewer days.

[6] The academic year in England runs from September of one calendar year to August of the following year. Throughout the article, we will refer to the later of the two calendar years for brevity. For example 2001 indicates the academic year 2000–1.

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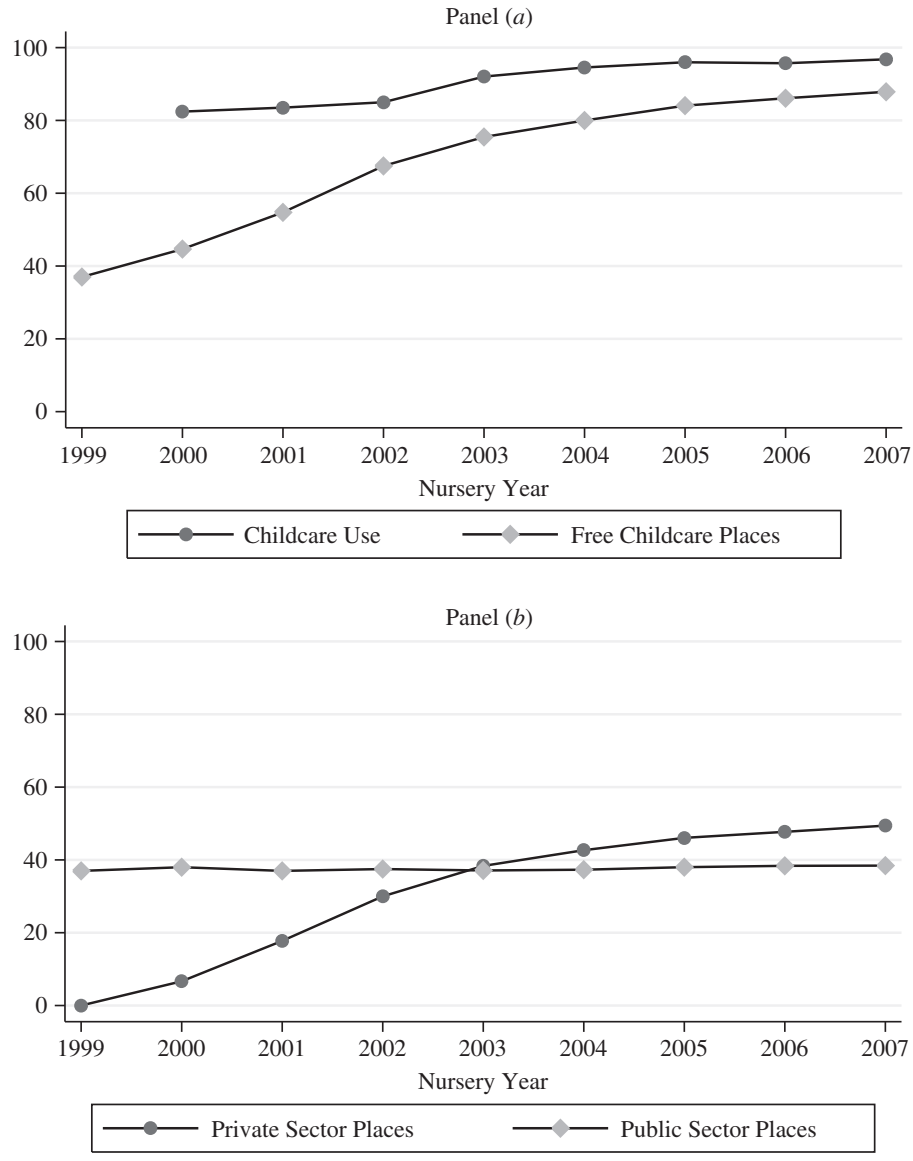


Fig. 1. *Percentage of Three Year Olds in Childcare*
Notes. Department for Education LEA-level data, aggregated using Office for National Statistics population weights. Childcare use includes free places and privately funded places.

2007. A striking aspect of this graph is that in the year 2000 most three year olds (82%) were already attending some type of pre-school education, which meant that the increase in children attending pre-school was not as dramatic as one might first expect. Indeed, between 2000 and 2007 the total proportion of three year old children in a childcare setting increased by only 14.4 percentage points.[7] The really big change was

[7] Data on all places is not available for 1999.

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that the number of children accessing a free place increased dramatically from 37.0% to 87.9% between 1999 and 2007.

Given that the introduction of the free entitlement was one of several policy changes being introduced by New Labour in the early 2000s we need to consider whether other policies could confound identification of the effect of free childcare for three year olds. The existence of other policies is a concern to us only if they are correlated with the expansion of free places across areas and time, as this is the variation we exploit for our identification strategy. Policies such as tax breaks for childcare costs are national policies that are uncorrelated with the roll-out of the free entitlement. The most important policy that was locally implemented at a similar time as the free entitlement were Sure Start centres. This was a policy based around small areas that were within 'pram-pushing' distance of around 800 families with children under five (Eisenstadt, 2011). The allocation of the funding for Sure Start Local Partnerships was strongly focused on the most disadvantaged areas, with the Government making repeated commitments to cover the poorest 20% of wards.[8] Although the allocation of Sure Start centres was in principle independent of the expansion of free places, we have gained access to area-level expenditure data on all policies benefiting pre-school children, including Sure Start, so that in our empirical analysis we can control explicitly for the existence of other policies. We will demonstrate that the inclusion of these expenditures does not change our results.

1.2. *Different Types of Pre-school Education*

The expansion of free places could have been achieved through direct funding of public sector places. Instead, the Government opted to fund private and voluntary settings to provide free early education places.[9] The rationale behind this approach (and other similar policies at the time) is that the private sector would offer parents more choice (Besley and Ghatak, 2003). Panel (*b*) of Figure 1 shows that the expansion of free places happened entirely in the private sector. Indeed, the proportion of three year olds in publicly provided places remained relatively stable over the period with a very small increase from 37.0% in 1999 to 38.4% in 2007.

The Government required all private settings receiving public funds to follow a standardised curriculum, the Foundation Stage. This curriculum emphasises learning through play, ensuring that a range of stimulating activities are provided and that children's development across a whole range of areas is encouraged.[10] However, despite the existence of a standardised curriculum, the type of early education

[8] By 2004 Sure Start Local Partnerships had become Children's Centres and the specific focus on disadvantaged areas was diluted with a pledge to make coverage universal by 2010, although services would be reduced in more affluent areas. By 2006 Children's Centres had a more explicit goal of providing childcare but it seems that, up to late 2006, Children's Centres where not much more than a rebranding of existing provision with over 90% emerging from previous Sure Start and Neighbourhood Nurseries (National Audit Office, 2006).

[9] Initially funding was allocated according to a fixed amount per pupil (£1,130 a year in 2000–1 for 12.5 hours a week, or £2.74 an hour, see Department of Education and Employment, 1999) and then, from 2003–4, it was included in the LEAs' general schools budget.

[10] From 2002/3 children's performance in the Foundation Stage was assessed at the end of children's first year in school through the Early Years Foundation Stage Profile, used as an outcome variable in this article.

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Table 1

Characteristics of Childcare Providers

	Public sector	Private sector
Type of setting	Nursery schools and nursery classes within primary schools	Day nurseries and playgroups run by private, voluntary or independent providers
Hours	Sessional care*	Day nurseries: full-time care Playgroups: sessional care*
Qualification of staff	Qualified teacher (degree-level)	50% of staff must hold level 2 qualification (two years post-compulsory schooling)
Adult-child ratio	1:13	1:8 if no qualified teacher 1:13 if qualified teacher

Notes. Main source Gambaro *et al.* (2015). *Sessional care is a morning or afternoon session of no more than four hours.

experience will vary depending on where children take up their place. Funding per hour is higher in the public than the private sector (National Audit Office, 2012) and there is variation in terms of the duration of a pre-school day. As Table 1 shows, public provision will usually be restrictive in terms of hours available – often either five mornings or five afternoons – and usually will not extend outside school hours. Private day nurseries by contrast often focus on full-time care, so that in practice the entitlement to free places acts as a discount on fees, with fewer part-time places provided. Private settings which evolved from community playgroups, on the other hand, generally offer care over more restricted hours, mostly spanning no longer than a school day.[11]

Most importantly, there are marked differences between public and private sector providers in respect of staff qualification requirements and adult-child ratios. Nursery schools and nursery classes within primary schools require that a teacher with a degree-level qualification is always present and have an adult-child ratio of 1:13. In the private sector, there is only a general requirement that at least 50% of staff must hold a relevant level 2 qualification, which corresponds to approximately two years of post-compulsory schooling. When there is no qualified teacher present, however, the adult-child ratio is increased to 1:8 (Gambaro *et al.*, 2015). One hypothesis is that the limited benefits we find can be explained by the choice to provide new free places through the private sector which offered lower quality childcare. We will evaluate this hypothesis in Section 7.

1.3. *The Roll-out of the Policy*

The identification strategy used in this article relies on variation over time and across areas in the availability of free pre-school education. Here we provide detail on the roll-out of the policy. For reasons of data availability our main analysis focuses on the period 2002–7, although we show estimates covering the time period since the start of the policy, i.e. 1999–2007, for some outcomes. Figure 2 shows how coverage developed

[11] Childminders are also considered among private providers but we omit them here, as very few of them registered with an LEA and were able to offer free places.

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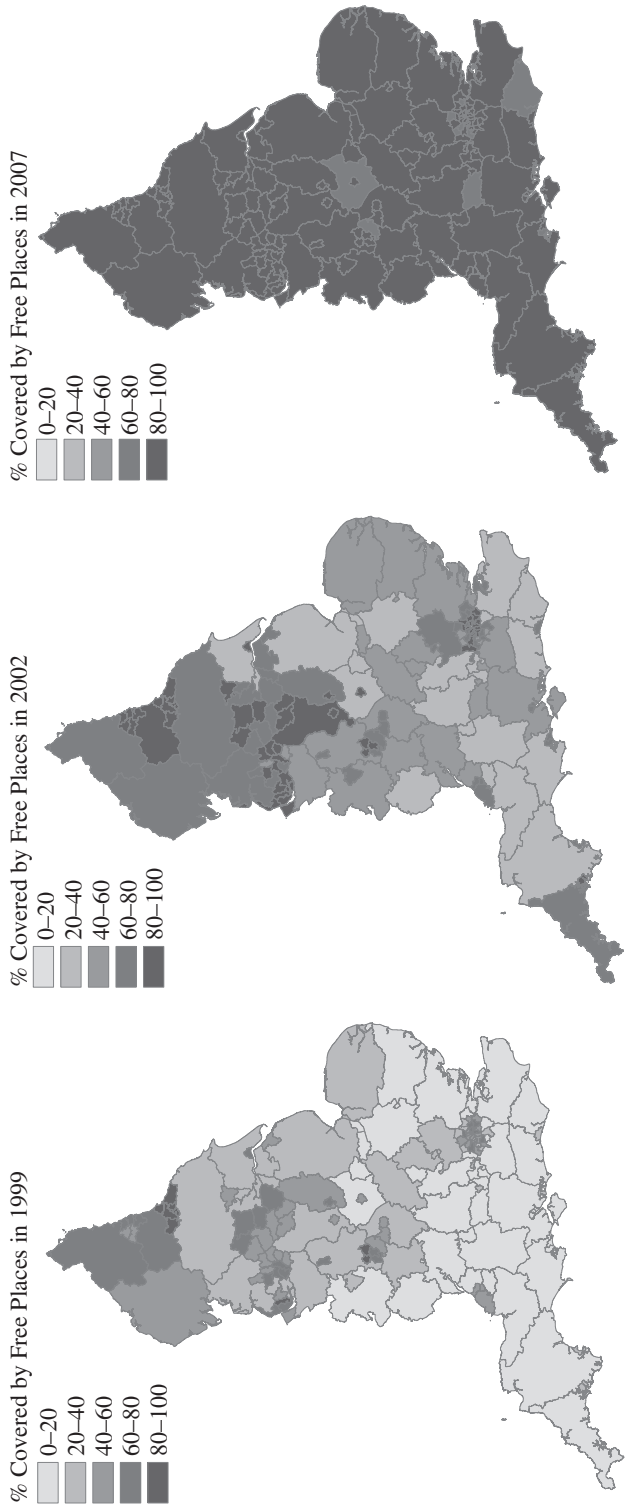


Fig. 2. *Free Childcare for Three Year Olds Across England, 1999, 2002 and 2007*
Note. Department for Education LEA-level data.

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from 1999 to 2002 and through to 2007. Over the whole period we can see a substantial increase in free places, and that this increase was not uniformly distributed across areas. While the North of England and areas around London as well as Cornwall already had a fairly high coverage in 1999 (implying high levels of existing public childcare), the increase for most Southern areas of England was from a low level of coverage, in the 0–20% bracket. Looking at the changes between 1999 and 2002, it seems that the largest increases occurred in the North but there was still considerable variation in the availability of places across the country in 2002. Substantial catch up occurred from 2002 to 2007 to ensure full coverage was achieved, with the largest increases in coverage experienced in the South.

What were the factors driving the variation in the roll-out? The expansion of free places was determined by a complex mixture of elements, including parental demand for pre-school places (determined by their wages and employment opportunities) and existing levels of provision. According to accounts available of the build-up period, however, the key factor determining the speed of the implementation of the policy was the local ability to create new places (Harries *et al.*, 2004). This depended in particular on the staffing of so-called Early Years Development and Childcare Partnerships (EYDCPs) which gave advice to existing and new providers applying to receive the government funding. The EYDCPs were composed of representatives from local government authorities, Training and Enterprise Councils, employers, parents, and other early education providers including out-of-school clubs, schools and churches. They had responsibility for drawing yearly plans for the implementation of the free entitlement and for submitting these plans to the Department for Education and Employment for approval. The effectiveness of the EYDCPs and the way they operated within the Local Authority – according to 'integrated' or 'collaborative' modes, for example – differed widely across areas (Osgood and Sharp, 2000) and explains why the availability of free pre-school places grew more rapidly in some areas than others.

Given that the roll-out did not occur homogenously throughout the country, we need to consider whether there are other significant differences between areas that saw a large expansion and areas that did not. To ease this comparison we follow Havnes and Mogstad (2011) in defining treatment and comparison LEAs (but for our main analysis we use the variation in the timing of the expansion across all LEAs rather than relying on a binary categorisation). Here we divide the LEAs in England into two groups of equal number according to the increase in coverage with free childcare places for three year olds, so that 'treated' LEAs are the 50% of LEAs with the highest expansion of places, whereas 'comparison' LEAs are the 50% of LEAs with the lowest expansion of places. Figure 3 shows the trajectory of the roll-out for treatment and comparison LEAs from 1999 to 2007. It is clear that the larger build-up in treated areas coincides with lower levels of initial coverage in both 1999 and 2002. As discussed more fully in the next Section, we take account of the importance of the baseline level of coverage by controlling for baseline-by-cohort effects in our empirical model.

In Appendix Table A1, we provide a comparison of child and area level characteristics between treatment and comparison LEAs in 2002, the base year of most of our analysis. This is a preview of our data which we will describe in detail in Section 3. The Table shows that treatment LEAs were quite different to comparison

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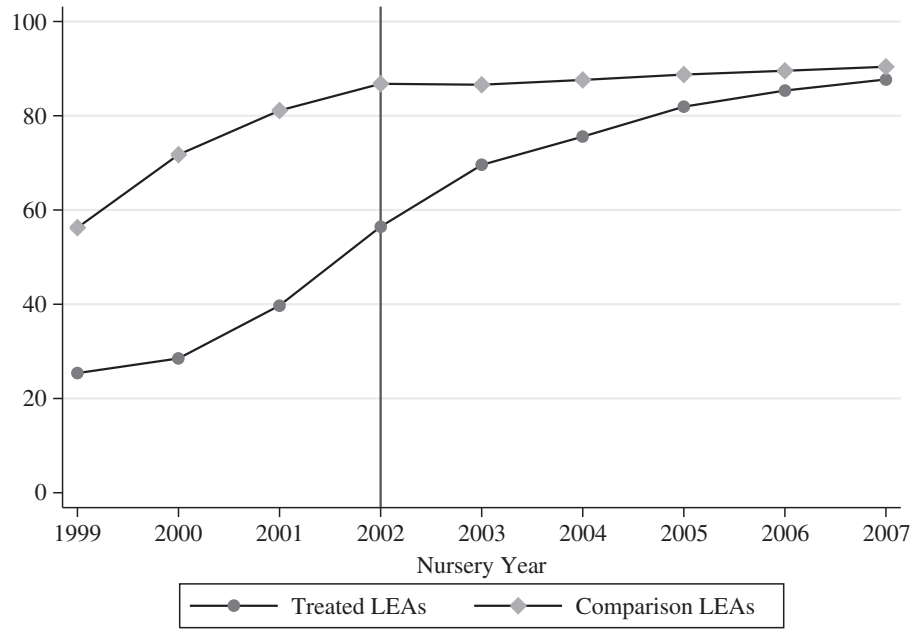


Fig. 3. *Expansion of Childcare Places for Three Year Olds across England, 1999–2007*
Notes. Department for Education data. The graph shows the build-up of free places for treatment and comparison LEAs. Treatment (comparison) LEAs are the 50% areas where free places coverage increased most (least) between 2002 and 2007. The vertical line indicates nursery year 2002, the base year of our analysis.

LEAs; generally they were less deprived. For example, treatment LEAs had fewer three year olds who later claim free school meals in school, speak English as an additional language and live in the most deprived neighbourhoods than comparison LEAs. At the area level, we are interested in factors that might affect child outcomes. We find that treatment areas had on average more favourable economic conditions (e.g. higher qualification levels and female employment rates) and better health services (as proxied by low birth weight rates) than comparison areas. They were more often controlled by Conservative rather than Labour administrations and less money was spent per child on other early years initiatives. And in line with Figure 3, we see that coverage with free childcare places was 30 percentage points lower in treatment than comparison LEAs in 2002.

We also want to see whether there were differential trends in area characteristics over the expansion period that may have affected children's outcomes. Figure 4 shows trends in characteristics for treated and comparison LEAs between 1999 and 2007. We have indexed the data to be 1 in both areas in 2002 to ease comparison. The Figure shows that treatment and control LEAs display very similar trends in characteristics apart from the percentage of LEAs that are controlled by Conservative administrations, which is a binary category and quite likely to change over time. We will control for these key LEA characteristics in our estimation, although we will show that inclusion of these characteristics does not affect results once we control flexibly for baseline levels of free places for three year olds.

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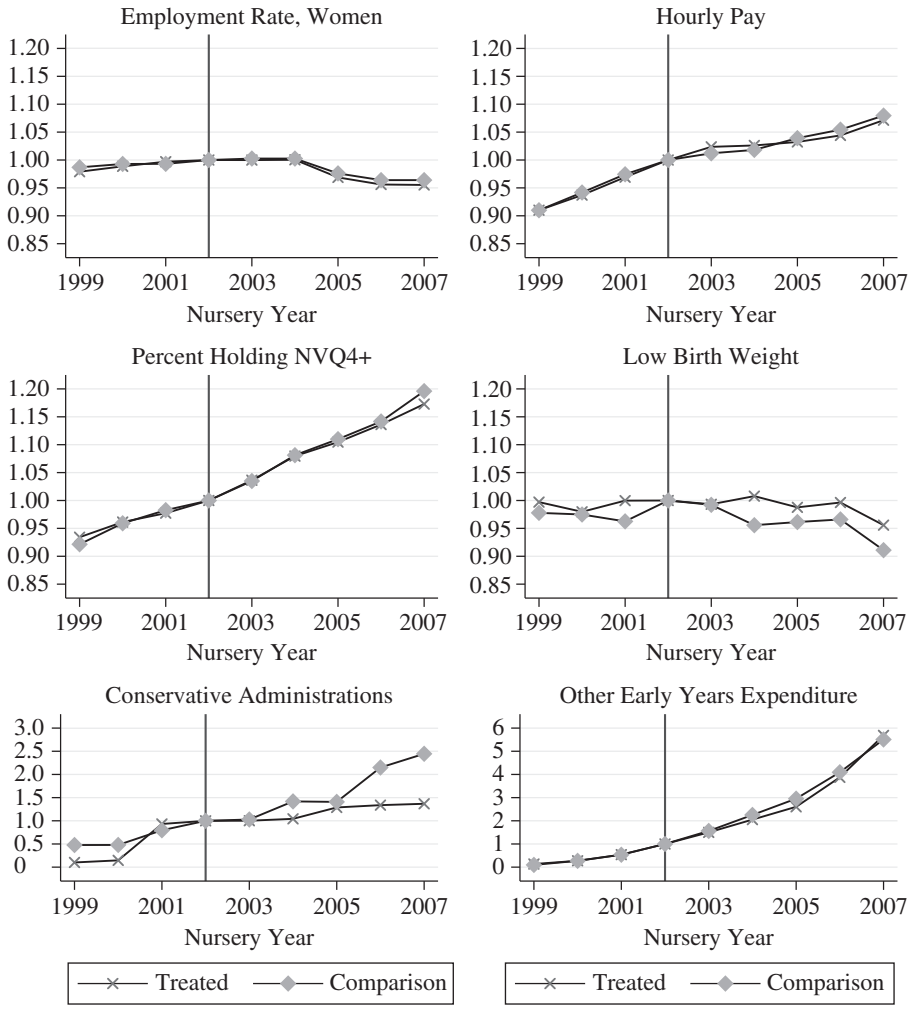


Fig. 4. *Trends in LEA Characteristics, 1999–2007*
Notes. Office of National Statistics and the Election Centre, Plymouth University. See notes to Figure 3 for the definition of treated and comparison areas. The vertical line indicates nursery year 2002, the base year of our analysis.

Finally, we examine trends in our outcome variables before and after the treatment. In particular we want to establish whether treatment areas had different trends in outcomes with respect to comparison areas even before the policy was introduced. School outcomes at ages 7 and 11 can be first observed for children who were aged three in 1995. However, age 5 school outcomes are available from nursery year 2002 only. Figure 5 shows mean raw point scores in numeracy and literacy at age 5, in mathematics and reading at age 7, and in mathematics and reading at age 11 across treatment and control LEAs. We can see that all outcomes follow approximately parallel trends in the years leading up to the introduction of free childcare for three year olds in 1999, with children in the more affluent treatment areas

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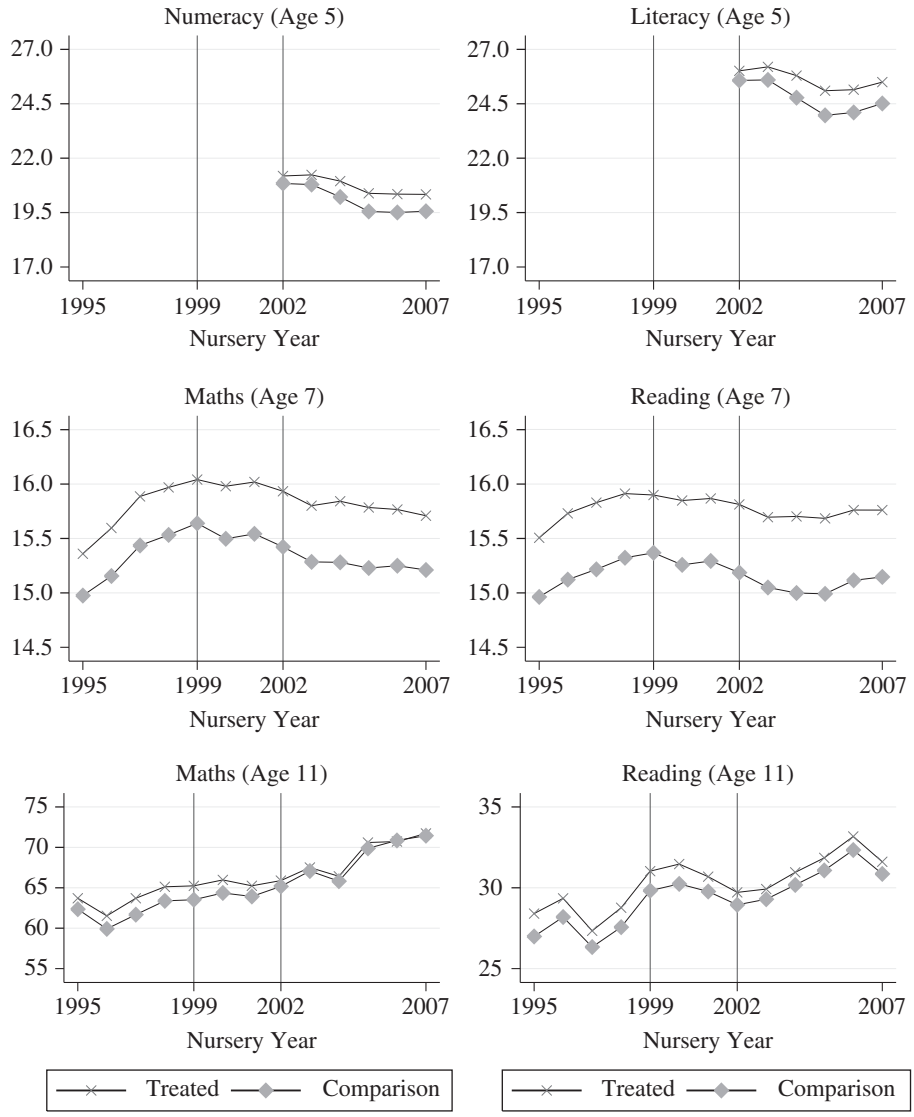


Fig. 5. *Trends in School Outcomes at Ages 5, 7 and 11, Nursery Years 1995–2007*
Notes. National Pupil Database. See notes to Figure 3 for the definition of treated and comparison areas. The vertical lines indicate the year 1999, when the expansion of free places started, and the year 2002, the base year of our analysis.

consistently outperforming children in the relatively disadvantaged comparison LEAs. The trends continue to be parallel through the expansion years, with the exception of mathematics scores at age 11, which seem to converge between treatment and control areas, and the age 5 scores in numeracy and literacy, where the gap between outcomes seems to increase. The latter is our first suggestive evidence of a small positive impact of free places at age 5.

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In summary, our examination of the roll-out of the policy indicates that we need to take account of differences between LEAs that saw different rates of expansion from different levels of initial free place coverage. The descriptive analysis of the roll-out does not indicate that we need to be concerned about differing trends in characteristics or prior trends in outcomes, although we will investigate this more formally by entering appropriate controls in our empirical model and by performing a placebo test.

2. Empirical Strategy

To estimate the effect of the expansion of free pre-school places on children's outcomes we rely on a difference-in-difference research design, exploiting the fact that the availability of free places grew more rapidly in some areas than others. This identification strategy rests on the assumption that the expansion of free places across LEAs and over time is independent of other time-varying and area-specific factors that might affect child outcomes. It is therefore essential that we control as comprehensively as possible for differences across time and areas.

Our main regression model, estimated by OLS over the sample of children who were aged three between 2002 and 2007, is defined as follows:

$$Y_{icl} = \beta_1 F_{cl} + \boldsymbol{\beta}_2 \boldsymbol{X}_{icl} + \gamma_c \times F_{2002_l} + \boldsymbol{\beta}_3 \boldsymbol{Z}_{cl} + \mu_l + \gamma_c + e_{icl}, \quad (1)$$

where:

(*i*) Y_{icl} is the outcome of interest for child i in cohort c and area l measured at ages 5, 7 and 11, respectively.

(*ii*) F_{cl} is an indicator of the availability of free pre-school places in an area of residence for a given cohort of children. More precisely, it is the proportion of the population of three year olds in a free place in a specific area, which we take as our measure of availability.

(*iii*) $\boldsymbol{X}_{icl}$ is a vector of child characteristics including gender, ethnicity (seven categories), free school meal status, language spoken at home, decile of neighbourhood deprivation and month of birth (to control for relative age at test effects).[12]

(*iv*) $\gamma_c \times F_{2002_l}$ interacts cohort dummies with levels of free pre-school places in the year 2002, the first year in our observation period. By adding this term we control for the fact that the build-up of the programme could be systematically related to the pre-treatment levels of free childcare in a way that is time-variant (see Duflo (2001) for a similar approach). For example, we know that more affluent areas saw a more rapid expansion of free places, as they had lower starting levels of free provision than other areas, and, if these areas are also on a more favourable trajectory regarding child development, this would cause our effect of interest to be biased upwards.

[12] Child characteristics are measured at age 7 as information for the whole sample is not available at age 5. Free school meal eligibility at age 7 is a good proxy for low income at earlier ages, as research shows that children who are eligible for free school meals in any year will be affected by low income over longer periods of time. Likewise, neighbourhood deprivation tends to remain stable even if families move (Rabe and Taylor, 2010).

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(*v*) $\mathbf{Z}_{cl}$ is a vector of area-specific characteristics that may affect child outcomes and are time-variant. We include labour market and local economic conditions that may favourably or adversely affect children, spending on other early years intiatives active in each area in the same period, an indicator of the quality of health services for mothers and children and information on political control.

(*vi*) μ_l are area dummies which control for time-invariant unobserved area characteristics. These may for example take into account the fact that the build-up of the policy was systematically related to pre-treatment levels of free childcare (in a way that is constant over time).

(*vii*) γ_c are cohort dummies which control for unobserved factors affecting each cohort.

(*viii*) e_{icl} is an idiosyncratic error term.

Although the variation in free places we exploit is at the area-level, (1) will be estimated at the individual (child) level. This is in order to exploit as far as possible the information we have about the characteristics of individuals in $\boldsymbol{X}_{icl}$, as these are important explanatory variables in an education production function. We cluster standard errors at LEA-level.

An important question to ask is whether – after including the comprehensive set of controls just described – our key explanatory variable (free childcare places as a proportion of three year olds) has enough variation left to identify the effect of interest. We show in Table 2 the raw variation in available pre-school places in the first row. Rows 2–5 summarise the residuals of regressions of free places on the listed controls. As we can see, over the period of our analysis the mean availability of free places is 80.14%, with a standard deviation of 13.55. Individual-level characteristics do not seem to explain a lot of the variation, while area and cohort fixed-effects eat up almost half of it. Our flexible way of controlling for pre-treatment availability of free places interacted with cohort fixed effects brings the standard deviation down to a third of the initial amount, 4.53, which is still a substantial and meaningful variation. Notably, controlling for a wide range of time-varying LEA characteristics capturing

Table 2

Identifying Variation in Free Places

	Mean	SD	Min	Max
Free places	80.14	13.55	25.68	114.09
net of individual characteristics	0.00	13.17	−57.85	38.15
net of cohort and LEA FE	0.00	7.48	−53.87	60.44
net of $\text{places}_{2002} \times$ cohort FE	0.00	4.53	−78.55	81.09
net of LEA characteristics	0.00	4.47	−77.19	83.90
N	3.2 m			

Notes. National Pupil Database. The first row summarises our measure of free places. Rows 2–5 summarise the residuals obtained by regressing the proportion of free places on: individual pupil characteristics; plus cohort and LEA dummies; plus the availaibility of free places in 2002 interacted with cohort dummies; plus LEA-level controls, respectively.

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economic conditions, health services, other early years initiatives and political control does not reduce the available variation in free places.

This is a first indication that flexibly controlling for pre-treatment availability of free places takes out most of the LEA-level variation that could affect educational outcomes and is correlated with the expansion of places. We present different specifications of our estimation model in Section 4 and show that, once we control flexibly for differences in levels of free childcare at the start of the expansion, the further inclusion of our set of LEA-level controls does not change results. This reassures us that our results do not rest on the requirement to capture all time varying factors at the LEA level which might be correlated with places and outcomes. Further, in our robustness analysis, we show that our results are not sensitive to different ways of controlling for unobserved time-varying and area-specific characteristics. In short, we interpret the remaining variation in free places as idiosyncratic and not systematically related to any other variable (other than the timing of childcare expansion) which is related to educational outcomes.[13]

The estimated parameter β_1 captures the overall impact of the introduction of free early education, including a number of different behavioural responses. We offer a detailed analysis of each of these. For one, there is the participation effect. We have already seen that the increase in attendance (the extensive margin) was fairly small and we estimate this 'first stage' relationship more formally, as well as extending the analysis to look at changes in hours of attendance (the intensive margin). Second, it is possible that child development could be affected by the expansion of free places even if participation effects are small. For parents already using childcare, the policy led to a cash transfer from the Government that they could use to invest in their children (e.g. buying more books) or reduce financial stress and therefore potentially improve child outcomes (Black *et al.*, 2014). Among those who changed their childcare arrangements, the impact of the policy is also influenced by the relative quality of care provided compared with the counterfactual care children would have received. We expect positive (negative) effects on child outcomes if parental or informal care is of lower (higher) quality than the pre-school education provided in formal settings. The likelihood of this will depend on variation in policy responses across socio-economic groups and on the quality of care provided under the policy. Fourth, the policy may have had a maternal employment effect. One of the aims of the policy was to increase maternal labour supply (Strategy Unit, 2002) and, if this was successful, it could have effects on child outcomes through reduced maternal time available for child investments and/or an increase in available income. Therefore, the effect estimated using (1) is a weighted average of a number of possible implicit effects with the weights given by the number of children/families affected by each. We analyse these mechanisms systematically in Sections 5 and 6.

[13] Our identification strategy might be under threat if families were moving in response to the availability of free places. The Millennium Cohort Study (MCS) gives us an indication of the extent of house moves in children's early years. The MCS shows that 30% of families do move when their children are between nine months and age three, however moves motivated by the child's education are limited, with only 10% of movers indicating that this is a factor, and this includes moves for future schools. It therefore seems very unlikely that moves to secure a free place are significant.

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3. Data

Our empirical analysis is based on the National Pupil Database (NPD), which is available from the English Department for Education and has been widely used for education research.[14] The NPD is a longitudinal register data set for all children in state schools in England (a low proportion of children go to private schools at this age). It combines student-level attainment data with pupil characteristics as they progress through primary and secondary school.

3.1. *Outcomes and Observed Background*

We study the effect of early education on children at ages 5, 7 and 11. Primary school in England begins with the reception year, which children start at age 4 in the academic year they turn five. From birth to the end of reception, at age 5, the Early Years Foundation Stage sets standards for the learning, development and care of children in schools and pre-school settings. At the end of reception, children are assessed by their teacher according to the Foundation Stage Profile. This measures achievements of children aged five against 13 assessment scales, with 9 points within each scale. The 13 assessment scales are grouped into six areas of learning which include: communication, language and literacy (hereafter Literacy); problem solving, reasoning and numeracy (hereafter Numeracy); personal, social and emotional development (hereafter Social Development); knowledge and understanding of the world; physical development and creative development. We use as the age 5 outcome the standardised point scores in the main learning areas: Literacy, Numeracy and Social Development, as well as of the sum of the points in all assessment scales (Foundation Stage Profile total); all the scores are standardised separately by academic year.[15]

School education from age 5 to 16 is divided into four Key Stages, and at the end of each Key Stage pupils are assessed against the National Curriculum. To date, the pupils affected by the roll-out of the free entitlement have been tested at ages 7 and 11. At age 7 (end of Key Stage 1) teachers assess pupils according to carefully defined criteria which are the same across all primary schools. Teacher assessments are externally moderated and subject to scrutiny by the schools regulator Ofsted. Following standard practice, we transform National Curriculum levels (7 distinct levels) achieved in Reading, Writing and Mathematics into point scores using Department for Education point scales. We also show results for models where the dependent variable is a binary indicator measuring whether the pupil has reached a particular level of attainment to take into account the discrete nature of the outcome.[16] At age 11 we have test scores from national examinations that are externally set and marked for Reading and

[14] We provide details of where to obtain all the data sets used in this article as well as the code used in our analysis in an online Appendix C.

[15] Further details of the assessment framework can be found in Department for Children, Schools and Families (2008). Snowling *et al.* (2011) find that age 5 assessments are reliable measures of children's development and are highly correlated with outcomes later in the child's school life.

[16] Further information on Key Stage 1 assessment can be found at https://www.gov.uk/government/publications/key-stage-1-assessment-and- reporting-arrangements-ara/end-of-key-stage-1-assessment-arrangements with information on Key Stage 2 available at https://www.gov.uk/government/publications/key-stage-2-assessment-and- reporting-arrangements-ara.

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Mathematics for the cohorts we consider. As before, we standardise age 7 and age 11 scores separately by academic year and subject.

In the NPD, we observe some basic individual background variables, and we use these in our regressions to control for gender, eligibility for free school meals, ethnicity (White British, Indian, Chinese, Black, Pakistani/Bangladeshi, mixed and other), area deprivation deciles as measured by the Income Deprivation Affecting Children (IDACI) score of the neighbourhood of residence and whether the child speaks English as the first language at home. We also control for birth month to account for relative age at test. We perform subgroup analysis by neighbourhood deprivation, free school meal eligibility and language spoken at home.

3.2. *LEA-level Controls*

In our model, we control for a range of time-varying variables at the area level which might be associated with child outcomes. Economic conditions may favourably or adversely affect children through parental income and employment. We include the proportion of working-age individuals with certain qualification levels (NVQ3 and NVQ4, roughly equivalent to High School and College) and the female employment rate derived from the Labour Force Survey. We lag the employment rates to account for the fact that childcare availability might affect current employment rates. We also include the mean hourly pay from the Annual Survey on Hours and Earnings (ASHE).[17] Further, we use the proportion of children of low birth weight (below 2,500 g) to proxy the quality of health services for mothers and children, and we include binary indicators for the political control of LEAs (Conservative, Labour, Liberal Democrats) using data from the Elections Centre, Plymouth University.

We capture other early years initiatives that were active over the same time period as the roll-out of free pre-school education by including expenditure data obtained from the Department for Education. This includes expenditure on Sure Start centres and other policies that were smaller in scale, including the Neighbourbood Nursery Initiative. We use data on all spending on early years services that can be attributed to LEAs, not including spending on the free entitlement itself.[18] We weight this using the population of 0–4 year olds in each area and construct a three-year average around the year in which the child would be aged three to account for the fact that children would be able to benefit from these policies over several years.

3.3. *Measures of Free Childcare Availability and Childcare Take-up*

Annual headcounts of three year olds receiving free childcare by LEA are available from the Department for Education with separate counts of children in public

[17] Area-level measures of these variables are publicly available through NOMIS, the official repository of labour force statistics of the Office for National Statistics (https://www.nomisweb.co.uk/).

[18] The information on expenditure by LEA was collated from several different sources. Cross-checks with alternative sources on the total spend across England indicate that our data approximates national trends well.

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provision (nurseries and nursery classes in primary schools) and in the private sector at the end of each calendar year. The data are available from 1999, the year before the free provision for three year olds was introduced. Our measure of free part-time pre-school places is the sum of publicly provided places and free places in the private sector, divided by the population of three year olds in each LEA.

Information on total childcare participation (privately funded as well as free) in each LEA is also available from the Department for Education. Headcounts of children taking up places in the private sector, including both free and privately funded places, are available for years 2000–7.[19] Our measure of childcare take-up is the sum of public sector places and all places taken up in the private sector, divided by the population of three year olds.

We scale all childcare measures so that a unit change represents a 10 percentage point increase in the proportion of children for whom a pre-school place is available. We merge these data to children observed in the National Pupil Database using their LEA of residence at age 7, as residence at age 5 is not available for all of them. To all children in an academic cohort we match the childcare census of the December after they turned three.[20]

3.4. *Estimation Sample*

The analysis focuses on children attending early education from 2002 to 2007, as we can observe outcomes for these six cohorts of children at ages 5, 7 and 11. As mentioned above, the Foundation Stage Profile (FSP) at age 5 is not available for earlier years.[21] Our sample therefore includes six cohorts of children aged three in academic years 2002–7 with observations at age 5 relating to academic years 2003–8, observations at age 11 relating to academic years 2005–10 and at age 11 to academic years 2009–14.

From this sample, we remove children living in Scotland or Wales but attending school in England, children in 'special schools' that exclusively cater for children with specific needs, for example those who have physical disabilities or severe learning difficulties. Moreover, we exclude a small number of children who are younger or older than the children expected to belong to a particular school cohort (there is no grade repetition in the UK). Finally, we retain only pupils for whom we have no-missing outcomes and background characteristics. The main estimation sample includes six cohorts of children with 3.2 million observations.

Table C1 provided in online Appendix C shows descriptive statistics for child outcomes at various ages. We display mean raw Foundation Stage Profile point scores

[19] In some years, data returns were missing from some childcare providers, leading the Department for Education to revise the national figures. We carry out analogous data adjustments on the LEA-level data which line up with the national figures.

[20] For September to December-borns of each cohort, we could alternatively assign the previous year's census when these children are aged three. However, these children are only eligible to access free places from January so that the following census better captures availability.

[21] In 2003–6 FSP data were collected only for a 10% sample of school children, and we calculate and use weights to ensure this subsample is representative of the full population. The weights are the inverse of the probability of sample inclusion, predicted using a probit model which regresses sample inclusion on a large number of characteristics observed for the whole sample at age 7.

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at age 5, raw point scores in Reading, Writing and Mathematics at age 7 and raw test scores in Reading and Mathematics at age 11. We show this separately for the whole sample, as well as by gender and free school meal status. The Table shows that girls outperform boys in all outcome measures at all three ages, with the exception of Mathematics at ages 7 and 11 where boys perform better than girls on average. Larger differences can be found between children eligible for free school meals and other children. At age 5 the mean Foundation Stage Profile score of children on free school meals is 12% lower than that for children who are not eligible. At ages 7 and 11, mean point scores of children from low-income families are about 10–14% lower than those of children from more affluent families. Summary statistics of individual and LEA-level controls are given in online Appendix Table C2.

4. Results

4.1. *Main Results*

Our main set of results examines the effect of the availability of free part-time pre-school education for three year olds on educational attainment at ages 5, 7 and 11. The top panel of Table 3 shows effects on standardised point scores in the Foundation Stage Profile (FSP) and the three key learning areas within it. The second panel shows effects of free childcare availability on standardised point scores in Reading, Writing and Mathematics at age 7, while the bottom panel displays the effect on standardised scores in Reading and Mathematics at age 11. Column (1) presents results of a model that controls for cohort and area fixed effects as well as individual characteristics. There is a positive and statistically significant effect of increasing availability of free places by 10 percentage points on various outcomes at age 5 but not much evidence of an effect at later ages. In column (2) we control for the interaction of cohort fixed effects with starting levels of free places in 2002 to net out differences in child outcomes correlated with differences in the speed of expansion due to unequal levels of initial provision across LEAs. Point estimates decrease for most outcomes, indicating that, as we have shown, starting levels of provision of free places are correlated with the expansion of free places. In column (3) we add in LEA-level controls capturing economic conditions, health services, political control and other early years' initiatives in the observation period. These are our baseline results; full results listing coefficients and standard errors for all controls for a model of total FSP scores are given in online Appendix Table C3. The results change only marginally between columns (2) and (3). This indicates that flexibly controlling for LEA differences in starting levels of free childcare takes account of area characteristics that may be correlated with the expansion and that affect educational outcomes.

The baseline results for age 5 outcomes, displayed in column (3) of the top panel of Table 3, show that availability of free childcare has a positive effect on several measures. Specifically, a 10 percentage point increase in the proportion of three year olds for whom free pre-school is available leads to an increase in the FSP score of 1.8% of a standard deviation. Positive effects of around 1.5% of a standard deviation are also found for Literacy, Numeracy, and Social Development, but only for the first two of these outcomes are the effects statistically significant at the 10% level.

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Table 3

Effect of Availability of Free Childcare Places on Children's Educational Outcomes

	All indiv. X's (1)	All + Places$_{2002}$ × cohort FE (2)	All + LEA controls 'Baseline' (3)	Girls (4)	Boys (5)
Standardised Foundation Stage Profile (FSP) point scores (age 5)					
FSP total	0.042**	0.018*	0.018**	0.009	0.026***
	(0.021)	(0.010)	(0.009)	(0.010)	(0.010)
Literacy	0.018***	0.012	0.014*	0.003	0.024**
	(0.007)	(0.008)	(0.008)	(0.008)	(0.010)
Numeracy	0.022***	0.014*	0.014*	0.006	0.023**
	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)
Social Development	0.025***	0.014	0.016	0.010	0.021**
	(0.008)	(0.010)	(0.010)	(0.010)	(0.011)
N	1.2 m				
Standardised Key Stage 1 point scores (age 7)					
Reading	0.004	−0.000	0.000	0.002	−0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Writing	0.005*	0.001	0.001	0.001	0.002
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
Mathematics	0.004	−0.002	−0.001	−0.003	−0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
N	3.2 m				
Standardised Key Stage 2 point scores (age 11)					
Reading	0.001	0.006**	0.006**	0.007***	0.005*
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Mathematics	−0.003	−0.002	−0.002	−0.002	−0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
N	2.9 m				
Cohort & LEA FE	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes
Places 2002 × cohort FE	No	Yes	Yes	Yes	Yes
LEA-level controls	No	No	Yes	Yes	Yes

Notes. National Pupil Database. Results are from separate linear regressions, with weights applied for age 5 outcomes. Results for girls and boys are estimated jointly using complete gender interations. Availability of free pre-school is scaled from 0 to 10, so that a unit increase indicates a 10% point increase in available places. Literacy refers to the learning 'area communication, language and literacy'; Numeracy is 'problem solving, reasoning and numeracy'; Social Development is 'personal, social and emotional development'. Individual and LEA-level controls are listed in online Appendix Table C2. Standard errors clustered at LEA level. *p < 0.10, **p < 0.05, ***p < 0.01.

To assess the magnitude of these effects, we can consider that the free entitlement policy increased the fraction of three year olds receiving free early education by around 50 percentage points (from an average of 37% to 88% between 1999 and 2007). If we assume linearity, we can extrapolate and say that the policy change improved children's total FSP score by an average of 9% of a standard deviation overall. This compares to a 'penalty' of being eligible for free school meals of 41% of a standard deviation in Foundation Stage Profile scores (see online Appendix Table C3) and reveals that the overall impact of the policy was small. This is also true if we look at the increase in FSP points. On average children obtain 87.5 points out of 117 possible points. An increase of 9% of a standard deviation corresponds to an increase of 1.7 FSP points or a 1.9% improvement in points on average – again a small effect.

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At age 7 (middle panel of Table 3), our point estimates show very small (and slightly negative in the case of Mathematics) effects that are not statistically different from zero. Results for outcomes at age 11 can be found at the bottom of Table 3. Here we see a positive and statistically significant effect of access to free places on test results in Reading and a zero effect on Mathematics. The effect on Reading amounts to a 0.6% increase in writing scores for a 10 percentage point increase in free places. This is a very small improvement. Overall, these results indicate that the gains in children's academic outcomes at age 5 are not sustained through to the end of primary school.[22,23]

Columns (5) and (6) of the table show results when cutting the data by gender. We can see at age 5 that point estimates are higher and more precisely estimated for boys than girls for all outcomes, indicating that boys benefit more from access to free places than girls. Differences in the effect of nursery attendance by gender are a fairly common finding in the literature (Datta Gupta and Simonsen, 2010; Havnes and Mogstad, 2011; Felfe *et al.*, 2015) but most authors find that girls benefit more from early education than boys. However, none of the gender differences we find are statistically significant. At ages 7 and 11, the point estimates are very similar between boys and girls, and any differences are not statistically significant.[24]

In Table 4, we present results from linear regressions on binary indicators measuring whether the pupil has reached a particular level of attainment. This allows us to assess whether the policy had impacts on parts of the achievement distribution other than the mean. It also allows us to address the issue that outcomes at age 7 are not truly continuous. We define the thresholds as below/at least at/above nationally expected levels of attainment (level 2 at age 7 and level 4 at age 11, specific point score combinations at age 5, see notes to the table for details). The results show that at age 5 the effects are driven by pupils who are at the margin of exceeding expectations, although point estimates also indicate an increase in the likelihood of reaching expected levels and a reduction in the probability of being below expected levels. At age 7, we observe no effect of the policy on increasing the probability of exceeding the expected level of attainment at this age. At age 11, in the bottom panel of the Table, we

[22] This fade-out is a common empirical finding in many studies analysing early educational interventions. Cascio and Staiger (2012) investigate whether this is an artifact of the standardisation of test scores in a situation where the distribution of skills widens with age but do not find evidence that this can fully account for it.

[23] Although children's attainment over the primary school years is not measured on the same scales, the correlation of the test scores over time is very high. For example, the raw correlation between Numeracy at age 5 and Mathematics at age 7 is 0.60, while the correlation between Literacy at age 5 and Reading at age 11 is 0.65.

[24] In Appendix Table A2, we display results for the other learning areas assessed at age 5 (knowledge and understanding of the world, physical development and creative development). Point estimates suggest that the expansion in the availability of free places had a positive impact on these outcomes, but the size of the effect is once again very small and statistically not different from zero. We also consider whether the policy affected the likelihood that a child would be identified by the school as having Special Education Needs (SEN) because of learning difficulties or behavioural problems. As shown in the lower panel, there is little evidence that this was the case throughout the primary school years, apart from a small reduction in that probability at age 5.

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Table 4

Effect of Childcare Availability on Educational Thresholds

	Below expected level (1)	At least expected level (2)	Above expected level (3)
Foundation Stage Profile (age 5)			
FSP total	−0.001	0.004	0.008**
	(0.002)	(0.004)	(0.004)
Literacy	−0.002	0.004	0.005
	(0.002)	(0.004)	(0.003)
Numeracy	−0.001	0.003	0.008**
	(0.002)	(0.003)	(0.003)
Social Development	−0.000	0.003	0.008*
	(0.001)	(0.003)	(0.004)
N	1.2 m		
Key Stage 1 (age 7)			
Reading	−0.000	0.000	−0.000
	(0.001)	(0.001)	(0.001)
Writing	−0.001	0.001	−0.000
	(0.001)	(0.001)	(0.001)
Mathematics	−0.001	0.001	−0.001
	(0.001)	(0.001)	(0.001)
N	3.2 m		
Key Stage 2 (age 11)			
Reading	−0.004**	−0.003	0.001
	(0.002)	(0.004)	(0.001)
Mathematics	−0.001	−0.005	−0.003
	(0.001)	(0.004)	(0.002)
N	3.2 m		

Notes. National Pupil Database. Results are from separate linear regressions, controlling for cohort and LEA dummies, 2002 levels of free places interacted with cohort dummies and individual and LEA-level controls listed in online Appendix Table C2. Weights are applied for age 5 outcomes. At age 5 'below expected level' is defined as achieving a score between 1 and 3 in at least one area of assessment; 'at least expected level' is defined as achieving a score of 6–9 points in each assessment area; 'above expected level' is defined as achieving 6–9 points in each assessment area and a score of 9 in at least one area. At age 7 (age 11) 'below expected level' is working at a national curriculum level below 2 (below 4); 'at least expected level' is a national curriculum level of 2 or higher (4 or higher); 'above expected level' is a national curriculum level of 3 or higher (5 or higher). At age 11 the levels are based on teacher assessments. Availability of free pre-school is scaled from 0 to 10, so that a unit increase indicates a 10% point increase in available places. Standard errors clustered at LEA level. *p < 0.10, **p < 0.05, ***p < 0.01.

see that the small positive effect on Reading we saw in Table 3 is driven by a reduction in the likelihood of being below the expected level.

4.2. *Robustness Checks*

Before proceeding further with the analysis by subgroup and exploring possible mechanisms, we test the robustness of our main results. Our first set of tests scrutinises our main identifying assumption that the gradual roll-out of places is not related to anything else that predicts children's educational outcomes, given the included controls. One way to check this is to carry out a placebo test. Here we take two approaches. First, we assign to the cohorts of children in our sample free places two years into the future, i.e. we use places for the years 2004–9 instead of 2002–7. A child's

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performance should not be related to the availability of future pre-school places – apart from the fact that there is naturally serial correlation in availability of places over time when identifying estimates using a policy expansion. Therefore, if we find statistically significant effects of future places on age 5 outcomes, this could indicate that other factors that we are not sufficiently controlling for are biasing our results. Table 5 displays results for the placebo test in column (2), whereas column (1) displays our baseline estimates. We see that the effects of the availability of free places on child outcomes are no longer statistically significant when assigning future places, and all point estimates are considerably reduced, particularly for the Foundation Stage Profile score.

Our second placebo test is to assign free places for the years 2002–7 to older cohorts of children who could not have been exposed to the policy. This supplements the first placebo test and addresses the possible concern there is insufficient variation in places for the expansion years 2004–9. As outcomes at age 5 are not observed for older

Table 5

Robustness Checks

	Baseline estimate (1)	Placebo: future places (2)	Placebo: places in past (3)	$Places_{2002}$ × economic controls (4)	Controlling for London trend (5)	Using FSP sample (6)	Policy years 1999–2007 (7)
Standardised Foundation Stage Profile (FSP) point scores (age 5)							
FSP total	0.018**	−0.003	n/a	0.016*	0.018*	0.018**	n/a
	(0.009)	(0.019)		(0.009)	(0.009)	(0.009)	
Literacy	0.014*	0.007	n/a	0.015*	0.015*	0.014*	n/a
	(0.008)	(0.011)		(0.008)	(0.009)	(0.008)	
Numeracy	0.014*	0.007	n/a	0.015*	0.015*	0.014*	n/a
	(0.008)	(0.011)		(0.008)	(0.008)	(0.008)	
Social Dev.	0.016	0.003	n/a	0.014	0.016	0.016	n/a
	(0.010)	(0.014)		(0.010)	(0.010)	(0.010)	
N	1.2 m	1.2 m		1.2 m	1.2 m	1.2 m	
Standardised Key Stage 1 point scores (age 7)							
Reading	0.000	−0.001	0.002	0.005*	0.003	−0.001	0.002
	(0.003)	(−0.004)	(0.003)	(0.003)	(0.003)	(0.005)	(0.003)
Writing	0.001	0.000	0.002	0.006**	0.003	0.002	−0.000
	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)
Mathematics	−0.001	−0.001	0.009**	0.002	0.000	−0.002	−0.002
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)
N	3.2 m	3.2 m	3.5 m	3.2 m	3.2 m	1.2 m	4.9 m
Standardised Key Stage 2 point scores (age 11)							
Reading	0.006**	0.003	−0.005*	0.006**	0.008***	0.009**	−0.000
	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.005)	(0.002)
Mathematics	−0.002	−0.000	−0.003	0.001	0.001	−0.001	−0.003**
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.005)	(0.002)
N	2.9 m	2.9 m	3.3 m	2.9 m	2.9 m	1.1 m	4.4 m

Notes. National Pupil Database. Baseline results are from separate linear regressions, controlling for cohort and LEA dummies, 2002 levels of free places interacted with cohort dummies and individual and LEA-level controls listed in online Appendix Table C2. Weights are applied for age 5 outcomes and for estimates in column (6). Availability of free pre-school is scaled from 0 to 10, so that a unit increase indicates a 10% point increase in available places. Literacy refers to the learning area 'communication, language and literacy'; Numeracy is 'problem solving, reasoning and numeracy'; Social Development is 'personal, social and emotional development'. Standard errors clustered at LEA level. *p < 0.10, **p < 0.05, ***p < 0.01.

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cohorts, we can perform this placebo tests on age 7 and 11 outcomes only. We shift free places as far back in time as the data allow us, which is seven cohorts for age 7 outcomes and nine cohorts for age 11 outcomes. We estimate the same empirical model as for our main results and include cohort fixed effects interacted with places in 2002 and our 2002–7 LEA controls shifted back in time. Results are displayed in column (3) of Table 5. They show a small positive effect on reading at age 7 and a small negative effect on mathematics at age 11 (statistically significant at the 10% level) but as these go in opposite directions we conclude that there is no systematic relationship between future places and child outcomes at ages 7 and 11. This supports the argument that other unobserved time-varying changes that were coincident with the timing of childcare expansion were not influencing educational achievement.

We further experiment with an even more rigorous way of controlling for area-level time-varying characteristics and trends. In column (4) we present results of estimates that control – in addition to all the controls included in the baseline model – for a full set of interactions between the starting levels of our area-level economic controls (i.e. 2002 levels of female employment rates, hourly pay, proportion of working-age individuals qualified at NVQ3 (high school) and NVQ4 (college) level) and cohort fixed-effects. We find that our results hold, being very similar in magnitude to the baseline results in column (1) at age 5 and only slightly higher at age 7.

Next, we check the influence of London. Recent work by Blanden *et al.* (2015) reveals sharp improvements in end-of-primary school tests from 1999 onwards in London. Given that we are relying on area-level changes in outcomes, it is important to ensure that our results are not biased by these strong secular changes. We therefore allow for differential trends in outcomes among children educated in London by including in our model an interaction between a London dummy and a year trend. As column (6) of Table 5 shows, our results are robust to this check.

The next two robustness checks consist of performing our analysis on different samples to investigate whether sampling decisions are driving our results. Our estimates for outcomes at age 5 use a subsample of 1.2 million children and we apply weights to account for possible sample selection. In column (6), we present results where estimates at ages 7 and 11 are carried out on the same sample of 1.2 million students, applying the same sample weights. We find that results are nearly identical to those estimated on the complete sample of children aged 7 and 11 which we use for our baseline results.

Our main results are based on the years 2002–7 because age 5 scores are unavailable for earlier years. Although we have shown that there is substantial variation in the provision of free childcare during this time-period (see Table 2), we want to check whether we get similar results when including the early expansion period from 1999 to 2002. In column (7) of Table 5 we show estimates for policy years 1999–2007 where we observe children's outcomes at ages 7 and 11. We see that there are no differences in the results for age 7 outcomes, and the differences at age 11 are insubstantial.

5. Changes in Childcare Use

In this Section, we investigate one of the important mechanisms that may be driving our results: the extent to which the expansion of free childcare changed childcare use.

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Understanding this is crucial to appreciate the potential developmental gains to children. We look at changes in childcare use at both the intensive and extensive margin and consider variation by social background.[25]

5.1. *Changes in Hours of Childcare Use*

The descriptive evidence presented in Figure 1 showed that the increase of free places in the early 2000s was not accompanied by an equivalent increase in the number of three year olds participating in pre-school education. While free places went up by 20.4 percentage points between 2002 and 2007, our sample period, childcare participation increased by only 11.8 percentage points. This is because a considerable proportion of children were already using childcare before the introduction of the policy. It is possible, however, that parents who were already using childcare reacted to the introduction of free places by increasing the hours their child spent in childcare, i.e. an increase of childcare use at the intensive margin.

We turn to the Parents' Demand for Childcare Survey[26] to examine changes in the hours of childcare use. Table 6 compares hours of childcare use in 2001 with 2007. The top panel looks at all children, including those with zero hours, whereas the bottom panel focuses on children who are in formal childcare only. We see that on average 3.3 more hours of formal childcare were used per child. The increase was a bit lower (2.5 hours) among children at the 75th percentile of hours in childcare. As can be seen in the bottom panel of the Table, among children that were using childcare the number

Table 6

Hours in Formal Childcare 2001–7

	Weekly hours in formal childcare					
	Mean			75th percentile		
	2001 (1)	2007 (2)	change (3)	2001 (4)	2007 (5)	change (6)
All children						
Hours	8.6	11.9	3.33***	12.5	15	2.50***
			(0.54)			(0.50)
N	592	1,242		592	1,242	
Children in formal childcare						
Hours	14.3	14.9	0.63	16.5	17.0	0.50
			(0.62)			(1.37)
N	370	1,003		370	1,003	

Notes. Parents' Demand for Childcare Survey 2001 and Childcare and Early Years Provision: Parents' Survey 2007. Sampling weights applied. Standard errors between parentheses. *p < 0.10, **p < 0.05, ***p < 0.01.

[25] Note that another important mechanism that may impact on child development is maternal employment. We explored whether the policy led to increases in maternal labour supply, which was a declared aim of the free entitlement. We find that there was no overall impact on maternal employment in the time-period 2002–7 on mothers of eligible children. The results of this analysis are presented in Appendix B.

[26] We use the 2001 Parents' Demand for Childcare Survey and the 2007 Childcare and Early Years Provision: Parents' Survey. Data were collected from 5,416 households in 2001 and 7,136 households in 2007.

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of hours stayed constant over the policy period. The estimated increase of about half an hour both at the mean and the 75th percentile is not statistically different from zero. This indicates that any effects of free childcare were driven by more children taking up childcare rather than an increase in hours for those already in childcare.[27]

5.2. *Changes in Take-up of Childcare*

We now examine the first stage relationship between childcare use and availability of free places within areas. We use the same model as our benchmark model, with the only difference that the regression is at area level, because the outcome (pre-school participation) is observed at LEA rather than individual level. We use the same set of controls as before, listed in online Appendix Table C2, aggregating individual characteristics to LEA level by creating population-weighted means and including LEA and cohort dummies as well as cohort dummies interacted with starting levels of free childcare in 2002.

We present results for the whole sample in column (1) of Table 7. The estimated coefficient shows that between 2002 and 2007 only 2.7 genuinely new places were created for every 10 places that were funded. This confirms that the policy crowded out parental investments in pre-school education to a large extent, and primarily worked as a transfer of resources to parents who would have used paid-for childcare in the absence of the policy, giving rise to possible income effects.[28]

Given the large crowd-out and the fact that there was no increase in childcare use at the intensive margin, we consider whether the effects from access to free places on child outcomes are a result of income transfers to parents or if they are primarily participation effects. To investigate this we split the areas in our sample into two halves, a group where a high proportion of funded places led to increase in childcare participation ('complier LEAs') and a group where the crowd-out of parental

Table 7

First Stage: Effect of Free Places on Childcare Participation

	All LEAs (1)	Complier LEAs (2)	Crowd-out LEAs (3)
Free places	0.272***	0.390***	0.241***
	(0.043)	(0.136)	(0.047)
N	888	888	

Notes. Department for Education data. Estimates are linear regressions at LEA level and include LEA and cohort dummies, population weighted LEA-means of individual characteristics and LEA controls listed in online Appendix Table C2. Complier (Crowd-out) LEAs are the 50% of areas where the ratio between change in all childcare with respect to the change in subsidised childcare was highest (lowest) over our sample years. Results by complier status are estimated jointly using complete interactions. Standard errors clustered at LEA level. *p < 0.10, **p < 0.05, ***p < 0.01.

[27] We have investigated whether there were different changes in hours of childcare use for families from different socio-economic backgrounds and found that this was not the case.

[28] In the first year of the roll-out funders received £1,130 per child, but the benefit to parents depends upon the fees which they would have paid under the previous private arrangement; these were likely to be greater than the funding received in most settings.

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investments was high ('crowd-out LEAs'). Specifically, we split the areas in our sample into halves based on the ratio between change in all childcare with respect to the change in subsidised childcare over our sample years. Table 7 shows the first stage relationship for complier and crowd-out LEAs in columns (2) and (3). In complier LEAs 39% of the increase in funded places led to increased pre-school participation, whereas in crowd-out LEAs 24% of the expansion in funded places translated into higher take-up, a difference of 15ppts. We would expect that any observed effects in complier areas are driven by more children participating in pre-school while in crowd-out areas positive effects would be more likely driven by income effects. Note that complier LEAs differ from crowd-out LEAs in key characteristics, with compliers being on average more deprived (see online Appendix Table C4 for details on these differences). As we control comprehensively for differences in area characteristics this should not affect the comparison.

Table 8 displays the results of our estimates by LEA complier status, using a fully interacted model. Column (1) displays our baseline estimates, column (2) shows

Table 8

Examining Participation Effects: Results from Complier LEAs

	All (1)	Complier LEAs (2)	Crowd-out LEAs (3)
Standardised Foundation Stage Profile (FSP) point scores (age 5)			
FSP total	0.018**	0.067**	0.001
	(0.009)	(0.028)	(0.011)
Literacy	0.014*	0.034***	0.003
	(0.008)	(0.013)	(0.010)
Numeracy	0.014*	0.043***	−0.001
	(0.008)	(0.013)	(0.010)
Social Development	0.016	0.047***	−0.003
	(0.010)	(0.015)	(0.012)
N	1.2 m	0.5 m	0.7 m
Standardised Key Stage 1 point scores (age 7)			
Reading	0.000	0.001	0.001
	(0.003)	(0.005)	(0.004)
Writing	0.001	0.002	0.002
	(0.003)	(0.005)	(0.004)
Mathematics	−0.001	−0.000	−0.001
	(0.003)	(0.005)	(0.004)
N	3.2 m	1.4 m	1.8 m
Standardised Key Stage 2 point scores (age 11)			
Reading	0.006**	0.011**	0.003
	(0.002)	(0.004)	(0.003)
Mathematics	−0.002	0.001	−0.004
	(0.003)	(0.005)	(0.003)
N	2.9 m	1.3 m	1.6 m

Notes. National Pupil Database. Results are from linear regressions, controlling for cohort and LEA dummies, 2002 levels of free places interacted with cohort dummies and individual and LEA-level controls listed in online Appendix Table C2. Weights are applied for age 5 outcomes. Results by complier status are estimated jointly using complete interactions. Availability of free pre-school is scaled from 0 to 10, so that a unit increase indicates a 10% point increase in available places. Literacy refers to the learning area 'communication, language and literacy'; 'Numeracy' is problem solving, reasoning and numeracy; 'Social Development' is personal, social and emotional development. See notes to Table 7 for a definition of complier and crowd-out LEAs. Standard errors clustered at LEA level. *p < 0.10, **p < 0.05, ***p < 0.01.

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results for children living in complier LEAs and column (3) for children living in crowd-out LEAs. Comparing columns (2) and (3) we can see that point estimates are generally higher in complier than crowd-out areas. At age 5 results for these areas are sizable and precisely estimated, while results for the LEAs that did not translate free places into additonal take-up to the same extent (high crowd-out areas) show no improvement in child outcomes as a result of expanding free places. The differences between complier and crowd-out LEAs are statistically significant for all outcomes at 10% or higher at age 5. This suggests that the overall impact of the policy of making pre-school free are more likely to be driven by participation effects rather than income effects.[29] However, the results in Table 8 also highlight that the positive effects seen at age 5 fade out very quickly, even in complier LEAs.

5.3. *Childcare Use by Family Background and Heterogenous Effects*

We have established that the expansion of free childcare in England did not lead to a large increase in childcare use overall but it may be that it increased the use among particular groups of children. This is important to investigate, as presumably the quality of counterfactual care received by children at home differs by background and we expect effects to be larger for children from families of lower socio-economic status. Table 9 is again based on the Parents' Demand for Childcare Survey and shows changes in the use of formal, informal, exclusive parental and other types of care in the years marking the beginning and end of our sample period (2001 and 2007). The first row shows that the overall proportion of three year olds in formal, centre-based childcare increased by 18 percentage points in the time-period considered, while the proportion of children being exclusively looked after by their parents decreased by 7 percentage points.[30]

The second to fourth panels of Table 9 show changes in childcare use by background. The data allow us to distinguish children by their family's income, housing tenure and social class. Looking first at the differences by family income, the Table shows that children from families in the lowest income band increased the use of formal childcare considerably more than other children, and this was accompanied by a reduction in exclusive parental care. Children from the second lowest income band switched from informal childcare by family and friends to formal childcare. The increase in formal care among the third and the highest income band were very similar to that of children in the second income band, however. Housing tenure is another way of distinguishing the economic position of children, with renting families being less advantaged than home owners, in particular if they rent social housing. The

[29] Using the first stage estimates to scale up results (and therefore assuming that all effects were through participation), we can conclude that in absence of crowd-out a 10 percentage point increase in childcare availability would have increased the total FSP score by about 6.7% of a standard deviation (dividing the point estimates in column (1) of Table 8 by the corresponding first stage estimate in Table 7). For an individual taking up childcare who would not otherwise have done so, this implies an impact of 67% of a standard deviation; a large effect.

[30] Note that children can be in more than one form of childcare, so that increases in formal care are not mirrored by decreases in other forms of care. The level of formal childcare use underlying the change is lower in both years (63% and 81%) than that derived based on data from the Department for Education (which is 84% and 97%) but the change between the years is quite comparable.

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Table 9

Changes in Childcare Use 2001–7 by Parental Background

	Δ formal care (centre based) (1)	Δ informal care (family and friends) (2)	Δ exclusive parental care (3)	Δ other care (childminder, nanny etc.) (4)
All	18.3***	−1.7	−7.1***	1.3
	(2.27)	(2.33)	(1.92)	(1.64)
For comparison:				
Mean childcare use in 2001	62.9%	32.5%	20.3%	11.9%
By family income				
Lowest income band	25.8***	0.7	−13.6**	2.0
	(5.85)	(5.77)	(5.43)	(2.70)
2nd income band	15.3***	−10.9**	−1.3	−2.9
	(4.67)	(4.63)	(3.95)	(3.20)
3rd income band	14.1***	0.7	−3.6	1.6
	(4.77)	(5.09)	(3.95)	(3.60)
Highest income band	15.6***	3.7	−8.2***	3.4
	(3.78)	(4.50)	(2.76)	(3.66)
Income missing	24.4***	−6.2	−8.7	−1.6
	(8.82)	(8.15)	(8.05)	(4.82)
By housing tenure				
Social or private renter	24.9***	−4.5	−10.9***	1.2
	(4.35)	(3.94)	(4.03)	(2.53)
Home owner	16.9***	1.2	−7.2***	2.4
	(2.57)	(2.92)	(2.04)	(2.12)
By social class				
Semi-skilled and unskilled	28.1***	0.2	−13.7***	−0.3
	(5.23)	(5.18)	(4.81)	(2.99)
Skilled manual	34.6***	−19.4**	−12.4*	4.8
	(8.53)	(8.20)	(7.80)	(4.73)
Skilled non-manual	16.0***	0.4	−9.5***	4.6*
	(3.78)	(4.02)	(3.09)	(2.68)
Professional and managerial	14.6***	1.9	−6.0**	−0.1
	(3.85)	(4.43)	(2.75)	(3.70)
Social class missing	2.7	−7.4	15.4***	−4.0
	(9.05)	(6.98)	(7.87)	(5.22)

Notes. Parents' Demand for Childcare Survey 2001 and Childcare and Early Years Provision: Parents' Survey 2007. Sample size is 592 in 2001 and 1,242 in 2007. Sampling weights applied. Income bands in 2001 are <£10,400; £10,400–£20,799; £20,800–£31,999; £32,000+. Income bands in 2007 are <£10,000; £10,000–£19,999; £20,000–£29,999; £30,000+. Standard errors between parentheses. *p < 0.10, **p < 0.05, ***p < 0.01.

third panel shows a substantially larger increase in formal childcare among children from families living in rented accommodation than those living in owned houses, and a larger decrease in exclusive parental care. Finally, the fourth panel of Table 9 distinguishes families by their social class, based on occupation, and confirms the rough pattern seen for the other classifications: a higher increase in formal childcare and higher decrease in informal and exclusive parental care among children from the lower two social classes (semi-skilled and unskilled; skilled manual) compared to children from the upper two social classes (skilled non-manual; professional and managerial).

Although the association is not completely monotonic, the Table generally suggests that the expansion of the free entitlement was re-distributional, with disadvantaged

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children switching out of informal care by family and friends and out of exclusive parental care into pre-school more often than their more affluent peers. Under the assumption that the quality of care received is higher in formal care than the counterfactual care delivered by parents, family and friends for these children, we expect treatment efffects to be higher for children from families of lower socio-economic status.

To test this, we perform subgroup analysis on our estimation model, making use of available indicators of family background. These include free school meal eligibility, neighbourhood deprivation and language spoken at home. These measures each reflect slightly different things, with free school meal status capturing low family income[31] and neighbourhood deprivation capturing income deprivation of the area of residence, dividing neighbourhoods into tertiles. Families that do not speak English at home are not necessarily income deprived but the children are likely to have difficulties with English that pre-school participation could address (Cornelissen *et al.*, 2015).

Table 10 shows the baseline estimates for outcomes at ages 5, 7 and 11 in column (1). Columns (2) and (3) present the results when splitting the sample by free school meal status. We estimate subgroup effects by entering complete interaction terms into our model. We can see that point estimates are slightly higher for children from lower income families at ages 5 and 11 (but not at age 7). None of the differences is statistically significant, however, and most are quantitatively small. For example, the estimated difference in standardised FSP scores between children with and without free school meal eligibility is 0.014, with a 95% confidence interval of (−0.02 to 0.05). Columns (4)–(6) show results by neighbourhood deprivation tertile. Note that these neighbourhoods are small geographical units with between 1,000 and 3,000 residents, and are nested in the much larger LEAs which house around 220 neighbourhoods on average. The point estimates suggest higher effects of the policy on outcomes of children living in more deprived neighbourhoods (with the exception of age 7) but these differences are again not statistically significant.

Finally, columns (7) and (8) display results by language spoken at home. At all ages, the point estimates are higher for children who do not speak English at home and the group differences are higher than for the other group comparisons. For example, the estimated difference in standardised FSP scores between children who do and do not speak English at home is 0.026 with a confidence interval of (−0.01 to 0.06). In Literacy and Social development at age 5, the benefits accruing to children for whom English is an additional language are four times higher than for children who speak English as their first language and these differences are statistically significant at the 10% level. All other group differences are not statistically significant.

In summary, we find no strong evidence that children from lower socio-economic backgrounds have benefited more from an improvement in access to childcare

[31] Free school meal eligibility is linked to parents' receipt of means-tested benefits such as income support and income-based job seeker's allowance and has been used in many studies as low-income marker, however see Hobbs and Vignoles (2010) for some shortcomings.

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Table 10

Effect of Availability of Free Childcare: Subgroup Analysis

	All (1)	FSM (2)	Not FSM (3)	Affluent nbh (4)	Middle nbh (5)	Deprived nbh (6)	EAL (7)	Not EAL (8)
Standardised Foundation Stage Profile (FSP) point scores (age 5)								
FSP total	0.018**	0.029**	0.016*	0.007	0.016	0.022*	0.040**	0.014*
	(0.009)	(0.014)	(0.009)	(0.010)	(0.010)	(0.013)	(0.018)	(0.009)
Literacy	0.014*	0.020	0.013	0.003	0.017*	0.020	0.043***	0.011
	(0.008)	(0.013)	(0.008)	(0.009)	(0.009)	(0.012)	(0.017)	(0.008)
Numeracy	0.014*	0.025*	0.013	0.003	0.016*	0.020	0.033*	0.012
	(0.008)	(0.014)	(0.008)	(0.008)	(0.009)	(0.013)	(0.018)	(0.008)
Social Dev.	0.016	0.030**	0.013	0.004	0.015	0.017	0.044**	0.011
	(0.010)	(0.014)	(0.010)	(0.011)	(0.011)	(0.013)	(0.017)	(0.010)
N	1.2 m	0.2 m	1.0 m	0.4 m	0.4 m	0.4 m	0.2 m	1.0 m
Standardised Key Stage 1 point scores (age 7)								
Reading	0.000	−0.004	0.002	0.005*	0.000	0.001	0.007	0.003
	(0.003)	(0.005)	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.003)
Writing	0.001	−0.001	0.002	0.005	0.002	0.001	0.010	0.003
	(0.003)	(0.005)	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)	(0.003)
Mathematics	−0.001	−0.002	−0.001	0.001	−0.003	−0.000	0.009	−0.001
	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.003)
N	3.2 m	0.6 m	2.7 m	1.1 m	1.1 m	1.1 m	0.5 m	1.8 m
Standardised Key Stage 2 point scores (age 11)								
Reading	0.006**	0.008*	0.006***	0.005*	0.006*	0.012***	0.007	0.007***
	(0.002)	(0.005)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.002)
Mathematics	−0.002	0.003	−0.002	−0.001	−0.001	0.003	0.005	0.000
	(0.003)	(0.005)	(0.002)	(0.003)	(0.003)	(0.004)	(0.006)	(0.003)
N	2.9 m	0.5 m	2.4 m	1.0 m	1.0 m	1.0 m	0.4 m	2.5 m

Notes. National Pupil Database. Results are from separate linear regressions, controlling for cohort and LEA dummies, 2002 levels of free places interacted with cohort dummies and individual and LEA-level controls listed in online Appendix Table C2. Weights are applied for age 5 outcomes. Results for groups are estimated jointly using complete interactions. Availability of free pre-school is scaled from 0 to 10, so that a unit increase indicates a 10% point increase in available places. FSM is eligible for free school meals. EAL stands for English as additional language, nbh stands for neighbourhood. Literacy refers to the learning area 'communication, language and literacy'; Numeracy is 'problem solving, reasoning and numeracy'; Social D. is 'personal, social and emotional development'. Standard errors clustered at LEA level. *p < 0.10, **p < 0.05, ***p < 0.01.

places than children from less deprived backgrounds. Despite the fact that the availability of a large estimation sample allows us to consider the effects for relatively small groups of the population, there are no statistically significant or economically meaningful differences to highlight. This is not what we expected, based on our evidence of changes in childcare use and results from the previous literature (Havnes and Mogstad, 2011; Cornelissen *et al.*, 2015; Felfe *et al.*, 2015).[32] The next Section explores whether the quality of the childcare received may be driving these results.

[32] We also check whether in the areas where childcare use increased most (complier areas as defined above) the benefits of the policy are concentrated on the most disadvantaged children by performing subgroup analysis on children in complier areas only. Results for age 5 outcomes are shown in Appendix Table A3, and demonstrate that patterns are similar as for the whole sample, with point estimates somewhat higher for more disadvantaged than less disadvantaged children but differences mostly not statistically different from zero.

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6. Childcare Quality

Our results show that the impact of free nursery places in England has been short-lived and did not deliver the anticipated narrowing of socio-economic gaps in attainment. Here we provide evidence that childcare quality, as proxied by staff qualifications, was considerably lower in the private than the public sector, and we link staff qualifications to child outcomes, providing suggestive evidence that childcare quality was in part responsible for the small effect of the free entitlement.

A unique feature of the free entitlement in England is that the expansion relied exclusively on private settings to provide the new places. Education researchers have devised rating scales of the quality of children's experiences based on systematic observations of classroom practice and adult-child interactions. Using these measures of process quality private nurseries are found to be of lower quality than public ones prior to the free entitlement (Sylva *et al.*, 2004) and, although quality in the private sector did improve as the number of free places expanded, the inequality between the sectors is still found towards the end of the build-up period (Mathers *et al.*, 2007).

To provide a link between our results and childcare quality we examine staff qualifications, which have been shown to be linked to process quality (Mathers and Smees, 2014). The presence of a graduate is a key driver of observed quality, a finding confirmed by the evaluation of the Graduate Leader Fund which demonstrates that private settings that gained a graduate leader were able to improve the quality offered significantly compared with those who did not (Ranns *et al.*, 2011). The qualification of staff in a setting is fairly easy to measure, although it is clearly only a proxy for process quality.

We are able to show the stark difference in staff qualifications between public and private childcare providers during the roll-out period by examining data from the Childcare and Early Years Workforce Survey 2002–3. This covers public providers through a survey of nursery schools and nursery and reception classes in primary schools[33] and the private sector through separate surveys of playgroups (and a small number of pre-schools) and day nurseries. Table 11 shows ratios between senior and total paid staff as well as qualification levels for senior managers, supervisory staff and other childcare staff who cannot be left in charge of children on their own. It demonstrates three things. First, the ratio of senior to total paid staff is always higher among public providers. Second, the senior manager level of qualification is much higher in the public than the private sector, with about 80% of senior managers holding a level 4 qualification (degree level or above) in the public sector against only 10% or 20% in the private sector. Third, and most significantly, the highest level of qualification of supervisory staff – which is the staff in charge of the children and in direct contact with them – is significantly higher in nursery schools and nursery and reception classes in primary schools than in any of the private sector providers.[34] This

[33] Unfortunately the survey does not allow us to distinguish nursery classes from reception classes.

[34] Given that the regulations indicate that public sector providers must have at least one qualified teacher it is strange to see this is true only in 90% of nursery schools and 85% of nursery and reception classes in primary schools. The most likely explanation is that in the remaining cases, the senior manager has not just a managerial role but is part of the staff who looks after the children.

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Table 11

Staff Qualifications of Public and Private Sector Providers, 2003

	Nursery schools	Nursery and reception classes in schools	Playgroups	Day nurseries
Number of settings	*197*	*835*	*814*	*797*
Ratio senior/total paid staff	0.83	0.74	0.54	0.63
	(0.16)	(0.20)	(0.33)	(0.28)
Senior manager qualification				
Level 2 or below	0.02	0.01	0.19	0.14
Level 3	0.17	0.17	0.66	0.64
Level 4 (degree) or above	0.77	0.80	0.10	0.20
Missing	0.05	0.02	0.05	0.02
Supervisory staff highest qual				
Level 2 or below	0.00	0.02	0.19	0.05
Level 3	0.10	0.13	0.68	0.80
Level 4 (degree) or above	0.90	0.85	0.10	0.15
Missing	0.00	0.01	0.03	0.01
No. with at least 1 supervisory staff member	197	833	744	781
Other childcare staff highest qual				
Level 2 or below	0.49	0.53	0.75	0.60
Level 3	0.24	0.22	0.18	0.35
Level 4 (degree) or above	0.14	0.07	0.03	0.04
Missing	0.14	0.18	0.04	0.02
No. with at least 1 other childcare staff	125	613	602	637

Notes. Childcare and Early Years Workforce Survey, unweighted. Standard deviation in parentheses.

suggests that process quality is considerably lower in privately than publicly provided childcare.

It is difficult to identify the causal impact of childcare quality on children's educational outcomes as exogenous variation is hard to find. In our UK context descriptive evidence indicates that children in settings with better process quality exhibit better cognitive development (Sylva *et al.*, 2012) and that staff qualifications are important for children's outcomes (Sylva *et al.*, 2004).[35]

We develop this evidence base by relating staff qualifications (presence of staff qualified to degree level, or not) directly to child outcomes. We match information from the Early Years Census (EYC) on the private settings children attend in their preschool year with their outcomes in the Foundation Stage Profile at age 5. As the detailed Early Years Census is only available from 2008, we consider more than 800,000 children who attended preschool in the academic years 2008–10. This is instructive if we assume that the relationship between staff qualifications and child outcomes was similar during the policy roll-out period. As private providers are chosen by parents, selection will affect the distribution of children across settings. In order to get closer to an understanding of the causal relationship between staff

[35] Similarly Felfe and Lalive's (2014) analysis of the Marginal Treatment Effects of an expansion of childcare for under 2s in Germany demonstrates that impacts are greater when staff are older and better trained.

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Table 12

Effect of Graduate Staff on Child Outcomes

	Cohort fixed effects only (1)	Plus child characteristics (2)	Plus pre-school peer charact. (3)	Plus school fixed effects (4)	*N* (5)
FSP total	0.067***	0.053***	0.047***	0.023***	819,771
	(0.002)	(0.002)	(0.002)	(0.002)	
Literacy	0.070***	0.059***	0.053***	0.029***	819,752
	(0.002)	(0.002)	(0.002)	(0.002)	
Numeracy	0.060***	0.048***	0.042***	0.024***	819,708
	(0.002)	(0.002)	(0.002)	(0.002)	
Social Development	0.054***	0.041***	0.035***	0.015***	819,763
	(0.002)	(0.002)	(0.002)	(0.002)	

Notes. Pupil Level Early Years Census 2008–10; National Pupil Database 2009–11. Child characteristics include gender, the child's free school meal eligibility, ethnicity, month of birth and whether child speaks English as an additional language. Pre-school peer characteristics are the means of these variables in the setting and year the child attended pre-school, excluding the child him/herself. *p < 0.10, **p < 0.05, ***p < 0.01.

qualifications and children's outcomes we include school fixed effects in our regressions as well as controlling for the socio-economic background of the child and his or her pre-school peers. Results are displayed in Table 12. We find a statistically significant positive association between the presence of a qualified teacher or Early Years Professional (EYP, trained to degree level) and all outcomes at age 5 among children in the same school in their reception year. The presence of graduate staff raises children's performance on the FSP score and the subscores Literacy, Numeracy and Social Development by around 2–3% of a standard deviation. The presence of a teacher or EYP is a very blunt indicator of quality, so this effect is likely to be downward biased.

An examination of the distribution of quality among private settings can also help to explain why the policy failed to close gaps by socio-economic status. Poorer children are somewhat protected in the UK because they are more likely to access high quality public early education (Gambaro *et al.*, 2015). However, in the context of the expansion, the more relevant issue is how well the private sector caters for those who are less privileged. Gambaro *et al.* (2015) show that private nurseries serving less advantaged children are likely to obtain worse quality inspection ratings than average and children are less likely to have contact with a graduate. Mathers and Smees (2014) show a similar picture using researcher-observed measures of quality and demonstrating that disadvantaged children are usually in private pre-schools with poorer staff-child interactions and less support for language development.

Taken together, evidence on the structure of the English early years sector and the importance of quality for children's outcomes indicates that if the new places had been of higher quality, this may have led to greater benefits for children's educational outcomes. In addition, more consistency in quality across settings accessed by children from different family backgrounds may have made it easier to close the gap in attainment between socio-economic groups.

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7. Conclusions

The UK government spends almost £2 billion every year to provide universal part-time pre-school education to children aged three and four. Like many other OECD countries that have introduced universal childcare, the government is hoping to improve child outcomes, narrow attainment gaps between children from different backgrounds, and increase female labour participation. In contrast to many other countries, universalism was achieved in England by paying private providers a fixed amount for all eligible children in their care, rather than by providing public childcare directly.

This article exploits the staggered introduction of the entitlement to free pre-school for three year olds in England to investigate the effect of the policy on child outcomes at ages 5, 7 and 11 based on a census of children in primary state schools. In our empirical model, we control extensively for differences between areas that might be correlated with the build up of free places and at the same time influence child outcomes. We find that a 10 percentage point increase in the proportion of three year olds covered by free places improves cognitive and non-cognitive outcomes at age 5 by 2% of a standard deviation. There are no apparent benefits by age 11. However, it is important to appreciate that our outcomes mostly measure educational attainment, so that it is possible that effects on other outcomes went unnoticed and that longer-term outcomes such as reduced crime (Heckman *et al.*, 2010) may emerge in the future.

The positive effects observed at age 5 capture the full impact of the availability of free places, and we investigate in detail the mechanisms underlying the effects. We observe that crowding out of privately paid formal care is substantial with <1 in every three newly funded places between 2002 and 2007 providing a genuinely new place. It is possible that positive effects come about because families have more money at their disposal, but evidence suggests this is not the case. Rather, the positive age 5 effects seem to be driven by increases in formal childcare use. The effects are small, in part, because participation did not increase very much.

We document that lower socio-economic status children have increased participation in formal pre-school most, switching out of care by parents, friends and family. However, contrary to expectations, disadvantaged children do not benefit substantively more from the free entitlement than their more affluent peers. One likely explanation is the fact that all the new places resulting from the policy were created in the private sector which is subject to less regulation in England compared to publicly provided childcare. In particular, we show that there were far fewer qualified teachers in private settings in 2003 (10–20%, compared to nearly full coverage in the public sector). Graduate teachers are a key driver of observed quality of settings, and we show that child school outcomes at age 5 are linked to the presence of graduate staff. There is evidence that private nurseries which serve poorer children are particularly bad on these measures, helping to explain why the policy did not have the expected success in reducing gaps in cognitive development between children from different backgrounds. Quality issues could also be responsible for the substantial fade-out of the initial benefits observed at age 5 by ages 7 and 11. We

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conclude that if universal childcare is to be achieved through expansion of private sector provision, as in our case, it is of paramount importance to set high quality requirements for participating providers.

In the 2015 General Election, all the main UK political parties pledged to extend free childcare. The winning Conservative party committed to extending the free entitlement to 30 hours for children whose parents are working. This will provide a considerable transfer to this group. Our analysis indicates that such a policy is unlikely to generate substantial positive impacts for affected children's outcomes. Indeed, unless high quality settings expand significantly, increasing provision to 30 hours for some children may reduce access to high quality provision for other groups. The likelihood of sufficient new high quality expansion will depend upon the precise details of the policy, in particular the extent to which funding mechanisms are linked to quality.

Appendix A. Supplementary Tables

Table A1

Characteristics of Treated and Comparison LEAs

	(1) Treated LEAs	(2) Comparison LEAs	(3) Difference (1)–(2) (p-value)
Mean student characteristics, 2002 cohort			
Eligible for free school meals	0.14	0.27	−0.133*** (0.00)
No. of months older than August-born	5.45	5.48	−0.022* (0.08)
English additional language	0.08	0.21	−0.129*** (0.00)
White British	0.86	0.72	0.144*** (0.00)
Indian	0.02	0.03	−0.017** (0.02)
Chinese	0.00	0.00	−0.001 (0.27)
Black	0.02	0.08	−0.060*** (0.00)
Pakistani/Bangladeshi	0.02	0.08	−0.053*** (0.00)
Mixed ethnicity	0.03	0.04	−0.011** (0.02)
Other ethnicity	0.04	0.04	−0.003 (0.69)
Female	0.49	0.49	−0.003 (0.16)
IDACI deprivation decile (affluent)	0.13	0.05	0.076*** (0.00)
IDACI deprivation decile 2	0.12	0.06	0.057*** (0.00)

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Table A1
(*Continued*)

	(1) Treated LEAs	(2) Comparison LEAs	(3) Difference (1)–(2) (p-value)
IDACI deprivation decile 3	0.12	0.06	0.059*** (0.00)
IDACI deprivation decile 4	0.12	0.06	0.056*** (0.00)
IDACI deprivation decile 5	0.12	0.08	0.038*** (0.00)
IDACI deprivation decile 6	0.11	0.09	0.024*** (0.00)
IDACI deprivation decile 7	0.09	0.11	−0.014** (0.03)
IDACI deprivation decile 8	0.09	0.13	−0.048*** (0.00)
IDACI deprivation decile 9	0.07	0.16	−0.089*** (0.00)
IDACI deprivation decile (deprived)	0.05	0.20	−0.159*** (0.00)
LEA characteristics, 2002			
Other early years initiatives (£/child aged 0–4), moving average	58.58	90.71	−32.13*** (0.00)
Free places (%)	56.46	86.78	−30.32*** (0.00)
Mean gross hourly pay (£)	11.95	12.03	−0.080 (0.84)
% qualified at NVQ4 level or higher (16–64)	25.04	22.36	2.680** (0.03)
% qualified at NVQ3 level or higher (16–64)	44.28	40.67	3.610*** (0.00)
Employment rate (16–64), women	71.99	64.66	7.330*** (0.00)
Low birth weight rate	7.01	8.46	−1.444*** (0.00)
Conservative	0.37	0.08	0.290*** (0.00)
Labour	0.23	0.67	−0.434*** (0.00)
Liberal Democrats	0.08	0.03	0.056 (0.14)

Notes. National Pupil Database; Department for Education; Office for National Statistics; NOMIS workplace analysis and the Election Centre, Plymouth University. IDACI is Income Deprivation Affecting Children Index. *p < 0.10, **p < 0.05, ***p < 0.01.

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Table A2

Effect of Availability of Free Childcare on Other Outcomes

	All indiv. X's (1)	All + Places$_{2002}$ × cohort FE (2)	All + LEA controls 'Baseline' (3)	Girls (4)	Boys (5)
Standardised Foundation Stage Profile (FSP) point scores (age 5)					
Knowledge	0.020**	0.009	0.010	0.000	0.019*
	(0.009)	(0.010)	(0.010)	(0.011)	(0.010)
Physical development	0.020**	0.011	0.011	0.003	0.020**
	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)
Creative development	0.023**	0.015	0.016	0.011	0.022*
	(0.009)	(0.010)	(0.010)	(0.010)	(0.011)
Not having Special Educational Needs					
Not SEN (age 5)	0.003***	0.002**	0.002*	0.001	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Not SEN (age 7)	0.003***	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Not SEN (age 11)	0.000	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Cohort & LEA FE	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes
Places 2002 × cohort FE	No	Yes	Yes	Yes	Yes
LEA-level controls	No	No	Yes	Yes	Yes

Notes. National Pupil Database. Results are from separate linear regressions, with weights applied for age 5 outcomes. Results for girls and boys are estimated jointly using complete gender interactions. Availability of free pre-school is scaled from 0 to 10, so that a unit increase indicates a 10% point increase in available places. Knowledge refers to learning area 'knowledge and understanding of the world'. SEN is Special Educational Needs. Individual and LEA-level controls are listed in online Appendix Table C2. Standard errors clustered at LEA level. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A3

Subgroup Analysis for Complier LEAs, Age 5 Outcomes

	All (1)	FSM (2)	Not FSM (3)	Affluent nbh (4)	Middle nbh (5)	Deprived nbh (6)	EAL (7)	Not EAL (8)
Standardised Foundation Stage Profile (FSP) point scores (age 5)								
FSP total	0.067**	0.081***	0.062**	0.050	0.051*	0.071***	0.093***	0.056*
	(0.028)	(0.026)	(0.029)	(0.040)	(0.028)	(0.023)	(0.027)	(0.030)
Literacy	0.034***	0.044**	0.031**	0.008	0.030**	0.047***	0.079***	0.025**
	(0.013)	(0.018)	(0.013)	(0.016)	(0.014)	(0.018)	(0.024)	(0.012)
Numeracy	0.043***	0.057***	0.040***	0.016	0.037***	0.058***	0.079***	0.036***
	(0.013)	(0.021)	(0.013)	(0.014)	(0.014)	(0.020)	(0.027)	(0.012)
Social dev.	0.047***	0.071***	0.041***	0.016	0.039**	0.060***	0.109***	0.033**
	(0.015)	(0.020)	(0.015)	(0.018)	(0.017)	(0.021)	(0.028)	(0.015)
N	0.5 m							

Notes. National Pupil Database. Sample includes Complier LEAs only, for a definition see notes to Table 7. Results are from separate linear regressions, controlling for cohort and LEA dummies, 2002 levels of free places interacted with cohort dummies and individual and LEA-level controls listed in online Appendix Table C2. Weights for sample selection are applied. Results for groups are estimated jointly using complete interactions. Availability of free pre-school is scaled from 0 to 10, so that a unit increase indicates a 10% point increase in available places. FSM is eligible for free school meals. EAL is English as additional language. Literacy refers to the learning area 'communication, language and literacy'; Numeracy is 'problem solving, reasoning and numeracy'; Social dev. is 'personal, social and emotional development'. Standard errors clustered at LEA level. *p < 0.10, **p < 0.05, ***p < 0.01.

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Appendix B. Maternal Labour Market Behaviour

A possible mechanism underlying the overall impact of the free entitlement is maternal employment. Increasing maternal employment was one of the aims of the policy. In principle, this could affect child outcomes negatively through a reduction in mothers' time available for child investments and/or positively through an associated increase in available income. In Table B1, we present estimates of the effect of childcare availability on different measures of maternal labour market behaviour. These estimates are based on data from the Labour Force Survey for the time period 2002–7 and focus on mothers with three year old children. As we can see, a 10 percentage point increase in coverage due to the expansion in free places has a very small effect on all of the measures of maternal labour market behaviour considered in the Table and standard errors are large, i.e. any effects are not statistically significant. This is evidence that there are no significant or sizeable effects of the free entitlement on child outcomes operating through maternal employment (see Brewer *et al.*, 2016 for a related article exploiting date of birth cut-offs for eligibility to childcare).

Table B1

Effect of Childcare Availability on Maternal Labour Market Behaviour

	Participates in labour force (1)	Employed (2)	Works part time (3)	Works full time (4)	Actual weekly working hours (if employed) (5)
Free places (10ppt)	−0.003	−0.005	−0.002	−0.004	−0.196
	(0.067)	(0.070)	(0.079)	(0.059)	(2.982)
Cohort, month & LEA FE	Yes	Yes	Yes	Yes	Yes
Individual & LEA controls	Yes	Yes	Yes	Yes	Yes
Places 2002 × cohort FE	Yes	Yes	Yes	Yes	Yes
N	20,395	20,395	20,244	20,244	10,595

Notes. We thank Sarah Cattan, IFS, for producing this Table for us. Labour Force Survey. The Table shows the effect of a 10 ppt increase in coverage of three year olds with funded places. Sample includes mothers observed between the beginning of the term after which their child turns three and the child's fourth birthday. Estimates include LEA, year and month of observation dummies and interactions of 2002 levels of free places with cohort dummies. Control variables include the mother's age and age squared, a dummy for whether the mother lives with a partner, dummies for mother's ethnicity, dummies for mother's highest educational qualification, the total number of children aged 0–2, 3–4, 5–9, 10–15 and 16–19 in the household. LEA-level economic controls include average unemployment rate, employment rate and hourly wage level in the LEA of residence in the quarter preceding the quarter of observation. Standard errors clustered at LEA level in parentheses. *p < 0.10, **p < 0.05, ***p < 0.01.

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Additional Supporting Information may be found in the online version of this article:

Appendix C. Supporting Tables.
Data S1.

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