Evaluating a demand-side approach to expanding free preschool education

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Abstract

Using a large administrative data set on all state schools in England, this paper studies the effect of free part-time preschool education at age 3 on child outcomes in primary school at ages 5, 7 and 11. To do this it exploits the staggered implementation of free preschool places across Local Education Authorities in England. We demonstrate that the policy led to a substantial transfer to parents as 3 of every 4 places funded were already being paid for privately, while only one genuinely new place was created. Despite this large crowd-out of parental investments the policy had some small beneficial effects at age 5 with a 10pp increase in the proportion of 3-year-olds covered by free places improving individual school outcomes by around 2% of a standard deviation. Effects are somewhat larger for boys and for more disadvantaged children. Impacts are twice as large for children in LEAs where more new places were created, which implies that benefits came from additional participation, not from income effects. Effects of the policy at age 7 are very small, with no benefits at age 11.

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1 Introduction

High-quality formal childcare is thought to improve outcomes for children, and, if it targets children from deprived backgrounds, to reduce inequalities in society. Recent research in the economics of human capital production has emphasised the importance of timely investments into child development, as differences in children's cognitive and non-cognitive development emerge at early ages and early investments have multiplier effects into the future (Almond and Currie, 2011; Carneiro and Heckman, 2004; Cunha and Heckman, 2008). Preschool education is one area where early investments can take place outside the family, so it may be particularly important for children from disadvantaged backgrounds who may receive lower parental investments. State support for childcare is thought to lead to a "double dividend" (Strategy Unit, 2002, p.29), by both promoting children's development and encouraging maternal employment (Brewer and Crawford, 2010; Fitzpatrick, 2010; Berlinski et al., 2011) which not only provides the foundation for broad and resilient tax bases, but also helps in advancing gender equality.

The research evidence on the importance of early years has helped to build a consensus that Government should have an important role in making early investments through childcare. In OECD countries (including the UK) state funding takes a variety of forms from cash subsidies and/or tax breaks, which reduce the cost of childcare, to funding places through direct provision or subsidies to private providers. In the last few decades many countries have introduced publicly funded, universal preschool programmes and momentum continues to build. For example, in the US President Obama used his 2013 State of the Union address to announce Pre-School-For-All, a Federal and State Programme to ensure full time pre-kindergarten access for all 4 year olds (The White House, 2013). However, the growing body of literature evaluating these universal programs has produced a wide range of estimates of the impact of free childcare on childrens cognitive and non-cognitive outcomes in the short and long term, and the evidence on the effectiveness of such programs is mostly positive but there are notable exceptions (see for example Baker et al., 2008, Fitzpatrick, 2008; Dumas and Lefranc, 2010; Andrews et al., 2012; Black et al., 2012; Havnes and Mogstad, 2011; Felfe and Lalive, 2014; Felfe et al., 2014; Dustmann et al., 2013). The international literature indicates that while early years education has the potential to be beneficial the specifics of the policy matter.

In England, all three and four year-olds are entitled to a free part-time nursery place during the school year, and similar policies are in place in Scotland and Wales. This is a popular policy and from 2013 it has been extended to disadvantaged two year-olds. In the run-up to the 2015 general election all political parities appear to be committed to its continuation or expansion (Brewer et al., 2014). From lagging well behind most European countries in the early 1990s, the UK is now one of the highest spenders on pre-primary services in Europe (OECD 2008). Despite the considerable funds invested into preschool education in England and the policy interest accompanying this, little is known about its effects. Previous evidence for England suggests that high-quality formal childcare is associated with improved outcomes for children (Sylva et al., 2004).[1] At an aggregate level, the performance of English children at the start of school showed no improvement in cognitive development in the early 2000s - when the provision for 4 year olds was already universal, while the provision for 3 year olds was still being rolled out - and no closing of the gap between children from different social backgrounds (Merrell and Tymms 2011). Stewart (2013) observes a narrowing of the gap in age 5 school outcomes between 2006 and 2011 which is the time-period after the roll-out of the free entitlement for 3 year olds is largely complete.

The English arrangements for providing free early education differ from the arrangements made in most other European countries. Before the free entitlment came into effect public nurseries and nursery classes in primary schools (known as the maintained sector) covered almost 40% of three year olds. Universalism was achieved by paying private providers a fixed amount for all eligible children in their care, (this started at £1,130 a year in 2000-2001 for 12.5 hours a week, or £2.74 an hour, see DfEE,1999). The new free places were therefore rather different from those already established; the private, voluntary or independent settings which provided them were subject to slightly lower quality regulations than the maintained sector and in many cases they were profit-making enterprises. The public funding was demand-led rather than the usual supply-led funding. Understanding the impact of this approach is particularly interesting in the context of the current expansion of pre-school in the US. State programmes in the US take different forms. For example, Oklahoma provides education through public provision while in Georgia the money follows the

[1]The Effective Provision of Pre-School Education (EPPE) study is much-cited in this area (Sylva et al., 2004). The results of this study are not comparable to those of this paper because it 1) refers to an earlier time-period; 2) was based on parents who chose to send their children to nursery; 3) oversampled nurseries in the public sector which on average are of higher quality than the private settings we focus on in this paper.

child to certified providers, as in the UK. Future expansion seems likely to follow the same mode of heterogeneity, and it is therefore extremely informative to understand the impacts of a demand-funded approach.

This is the first paper to evaluate the causal effect of free preschool education for 3-year-olds on child outcomes in England.[2] We exploit the fact that the entitlement for 3 year olds was phased in differentially across Local Education Authorities (LEAs), with the proportion of 3 year-olds holding a free part-time nursery place varying substantially between LEAs and rising from 36% to 87%, on average, between 1999 and 2007. This means that we can use both temporal and geographic variation to identify the effects of interest. We use a large administrative data set covering all students in state schools in England (92% of students) in which we can observe outcomes at age 5, at the end of reception year, at age 7 (year 2) and age 11 (year 6), which marks the last year of primary schooling. This dataset has the advantage that i) it covers the universe of childen in state schools, allowing us to detect even small effects precisely, ii) it is free of attrition, iii) we observe outcomes evaluated by teachers rather than arguably more subjective parental assessments, iv) we have measures at age 5, 7 and 11, which allows us to analyse whether any early positive effects persist, v) we have a number of background characteristics that allow us to study heterogeneity of the effects by gender and family background. In our data set we cannot, however, observe individual childcare participation, but rather we relate individual outcomes to childcare participation at the LEA level as measured by the proportion of children aged 3 attending preschool.

We estimate the effect of availability of free childcare places for 3-year-olds in nurseries and other registered settings on child outcomes. For our research design to work we must be confident that we control for all local characteristics that might be correlated with the build-up of places and with child outcomes. To do this, we model child outcomes as a function of free childcare availability, LEA and cohort fixed effects, LEA-level trends, and the characteristics of children and LEAs.

Our reduced-form estimates indicate that a 10pp increase in the proportion of 3-year-olds covered by free places improves cognitive and non-cognitive outcomes at age 5 by around

[2]There is a companion paper evaluating the causal effect of free preschool education for 3-year-olds on child outcomes using date-of-birth discontinuities in entitlement, see Blanden et al. 2014 (work in progress). Preliminary results are in line with those presented here. Another related paper is Brewer et al. (2014) which evaluates the effect of free preschool on maternal employment.

2% of a standard deviation. There are larger point estimates for boys than girls and for some children from lower socio-economic backgrounds, but these differences are mostly not statistically significant. The free entitlement increased the coverage of free places by about 50pp between 1999 and 2007, so the total effect of the policy may have been (up to) five times greater[3]. At age 7 the positive effects are small and by age 11 no effects can be detected. We estimate that among four 3-year-olds for whom free places were made available, roughly one takes up a new childcare place while three receive funding for a place they would have taken, and paid for, in absence of the policy. The policy therefore has substantial deadweight.

Ideally we would like to estimate the effect of *attending* childcare on outcomes later in school. Making available free childcare for 3 year olds could affect child outcomes through various channels in our set-up, however, including increased childcare participation, income effects accruing to parents, improved childcare quality and increased maternal labour supply. If the only effect of the policy had been to increase childcare attendance, our results suggest that a 10pp increase in the proportion of 3-year-olds attending childcare would increase age 5 outcomes by 8% of a standard deviation.

The paper proceeds as follows. In the next section we review the existing evidence on the impact of early education, specifically universal pre-school. Next, in section 3 we outline the institutional background of the paper, explaining the policy and providing descriptive evidence of its roll-out. Section 4 describes the empirical strategy and section 5 the data we use. Section 6 contains the main results of paper, robustness checks and assesses the impact of preschool on different subgroups. Section 7 explores the factors driving our results and section 8 concludes.

2 Literature review

Investigating the causal link between early education attendance and future outcomes is challenging. Pre-school enrolment is usually determined by parental choice and might be correlated with family and child characteristics. It is therefore likely to be endogenous to child outcomes. As a consequence a positive coefficient on pre-school in a model of outcomes does not indicate a causal effect, even in regression models which condition on a rich set of

[3]In our sample we observe years 2002 to 2007 only.

observable characteristics. Random assignment trials provide the ideal evaluation evidence, and in the preschool literature two studies in particular have been frequently used to make the argument that investment in the early years pays off. The Perry Preschool project carried out in the early 1960s in Ypsilanti, Michigan provided 2.5 hours of preschool per day over two years plus weekly home visits from a teacher. The sample size for both treatment and controls was 123 (Berrueta-Clement et al., 1984, Table 2). Initially large improvements in IQ at age 5 do not persist as children age (Schweinhart et al, 2005), but Heckman et al. (2010) demonstrate that this particular programme had returns of 7 to 10% for each dollar spent primarily through reductions in crime.

The Abecadarian project in Chapel Hill, North Carolina was more intensive still, providing full time childcare with a strong developmental element for children from babyhood up to school age. Once again the sample sizes were small, consisting of 111 children. The benefits from the Abecadarian project were concentrated on cognitive and educational outcomes with participants doing better in reading and math scores in young adulthood and having higher final educational attaintment (Campbell et al, 2002, Barnett and Masse, 2007). Both of these programmes were highly targetted at the most disadvantaged children where home care or alternative childcare arrangements were likely to be poorest. The results of these experiments are therefore not readily generalisable to universal programmes.

In parallel with the growth in public provision of universal pre-school across many nations, the literature evaluating the impact of these policies on children's outcomes has grown. Many of these papers rely, as we do, on differential policy implemenation across areas for identificiation. Positive short-term effects of preschool education on educational outcomes are found by, Cascio and Whitmore Schanzenbach (2013), among others, in the US, Felfe and Lalive (2014) for Germany[4], and Berlinski et al. (2009) in Argentina. Longer term benefits are revealed by Havnes and Mogstad (2011) for Norway, Berlinski et al. (2008) for Uruguay, Dumas and LeFranc (2012) for France and Felfe et al. (2014) for Spain.

A number of studies consider the heterogeneity of outcomes for different population subgroups; impacts are often found to be larger for more disadvantaged children. Dustmann

[4]Interestingly Felfe and Lalive (2014) find that access to preschool leads to mother's being more likely to undertake high quality activities with their children, a mechanism not explored elsewhere.

et al. (2013) find that impacts are concentrated on the children of migrants (for one German region). Havnes and Mogstad (2011), Felfe et al. (2014) and Cascio and Whitmore Schanzenbach (2013) all find impacts concentrated on children of lower educated or lower income mothers. There are two possible explanations for this: relative quality and crowd-out. Stronger impacts are likely for groups where the quality of the programme is higher than the counterfactual experienced by children and this is usually assumed to be the case for poorer children. The importance of quality is emphasied by findings from Baker et al. (2008) who find negative effects of $5 a day childcare in Quebec on a number of social development and health outcomes. Despite public regulation the quality of the childcare received appears low with only 60% of centres meeting minimum quality standards (Japel, Tremblay, and Côté, 2005). As poorer families were already receiving a subsidy much of the response to this policy came from better-off parents for whom subsidised care was likely to be a relatively poorer option compared to the counterfactual.

Understanding how big participation effects are, compared to the crowd out of privately-funded provision, also help us to understand sub-group effects. Benefits will be larger for families who were not already accessing preschool care from another source. This seems to be the case in Dustmann at al. (2013) and Felfe et al. (2014). Bassok et al. (2012) consider the extent to which public provision leads to crowd-out in pre-K programmes in Oklahoma and Georgia. In Georgia a wide range of providers contributed to the pre-K policy while in Oklahoma provision was through the public school system. Crowd-out is found to be much more pervasive in Georgia with little crowd-out occurring in Oklahoma. Other studies indicate that the impact of universal pre-K was greater in Oklahoma (Gormley and Gayer, 2005) compared with Georgia (Fitzpatrick, 2008). As discussed in the introduction the English example considered here has stronger parallels with the Georgian case than with the way the policy was implemented in Oklahoma.

3 Institutional background

The 1944 Education Act gave Local Education Authorities the duty to 'have regard to the need for' nursery education, this effectively gave Local Education Authorities (LEAs) the choice over whether they provide nursery education (in nursery schools or within nursery

classes in primary schools in the so-called maintained sector). Public provision was more likely to be found in politically left-of-centre inner-city areas (Lewis and Lee, 2002) and was mainly targeted at children from the most deprived families (DES 1990). Funding was low compared with mainland Europe (Pugh 1996). By the year 1999, 37% of 3-year-olds had a publicly funded childcare place, with large variation between LEAs.

In the 1970s and 1980s provision by voluntary short-session playgroups and private day nurseries expanded, with some support from Government in the form of employer tax breaks and subsidies for those on low incomes.[5] In 1997 the Conservative Government introduced a voucher scheme for nursery school education for all 4-year-olds. In 1998 the incoming Labour Government morphed the voucher into the Nursery Education Grant which provided universal free part-time preschool for 4-year-olds, achieved by 2000. The offer was for 12.5 hours per week of childcare, during 33 weeks in the year, in 2.5 hour daily sessions. A commitment to expanding the free entitlement to three year olds was to be achieved by 2004. In the roll-out of the entitlement for 3-year-olds, the policy of interest in this paper, the Department for Education initially provided funds for childcare places in 65 Local Education Authorities in 1999-2000 and across the country from 2000-2001. In 2008 the entitlement extended to 38 weeks of the year, and in 2010 it rose to 15 hours per week. As the number of hours rose the flexibility of the free entitlement has increased so that hours could be taken over a minimum of three days, and in 2012 this was reduced again to two days.

At the time of the introduction of free nursery places for 3-year-olds, children of this age-group could already be receiving preschoool in two ways; in free, publicly provided childcare or in privately paid for childcare offered in private settings. Figure 1 gives an overview of the development of preschool education of children aged 3 between 1999, the year before the age 3 roll-out began, and 2007. The most striking aspect of this graph is that in the year 2000 most three year olds (82%) were already receiving some type of preschool education/care, which meant that the increase in access was not very large; between 2000 and 2007 the total proportion of children in any type of childcare increased by just 14.4 percentage points. However, by the end of the period many more children were receiving a free place; this percentage increased from 37.0% to 87.9% between 1999 and 2007. Immediately we see substantial evidence of crowd-out. The Figure also shows that publicly provided places

[5]This sector is known as the private, voluntary and independent (PVI) sector, and we will refer to it as the private sector for brevity.

remained relatively stable over the time period with a small increase from 37.0% in 1999 to 38.4% in 2007. The variation in the percentage of free places available to 3-year-olds comes from places created in the private sector.[6] This indicates that a considerable proportion of private sector childcare was substituted for free private places.

As a consequence many children continued to experience early childcare in the same places as before, although in order to obtain funding private settings had to meet a quality threshold. In 2000 this was codified as the Curriculum Guidance to the Foundation Stage, which emphasised learning through play, ensuring that a range of stimulating activities are provided and that children's development across a range of areas is encouraged. Childcare workers were required to plan learning activities and to observe and document children's progress towards early learning goals.[7] Since 2001 all settings are also subject to registration and inspection by the Government regulator Ofsted (Office for Standards in Education). Ofsted inspects early years settings to judge the quality of the provision, including the quality of the teaching and learning, and grades providers ona 4 point scale, from 'inadequate' to 'outstanding'.[8]

Despite existence of a national curriculum the type of early education experience that registered settings offer will vary depending on where children take up their place. Funding rates in the public sector are higher than for private providers (NAO, 2012). Moreover, providers from different sectors have to comply with differing child to staff ratios and requirements regarding minimum qualification of staff. Nursery schools and classes in public provision require that a qualified teacher is present, and have an adult-child ratio of 1:13 to reflect that well qualified staff are employed. In the private sector, on the other hand, requirements for qualifications are lower, but if there is no qualified teacher present then the ratio of adult per child is increased to 1:8 (Gambaro et al., 2013).

There is also a lot of variation between childcare settings in terms of the duration of a preschool day. Public provision will usually be relatively restrictive in terms of hours

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

[7]From 2003/4 the Foundation Stage was assessed at the end of children's first year in school through the Early Years Foundation Stage, used as an outcome variable in this paper. From 2008 the Foundation Stage was combined with guidance for younger children to form the Early Years Foundation Stage, but the broad goals have not changed.

[8]There has been some criticism of the regulator focus on health, safety and environment rather than pedagogical quality (NAO, 2004 and Mathers et al., 2012).

available, often either five mornings or five afternoons, and usually will not extend outside school hours. Private day nurseries often focus on full-time care, so that the entitlement to free places acts only as a discount on fees, with few part-time places available. Pre-schools which evolved from community play-groups, on the other hand, generally offer care over more restricted hours, mostly spanning no longer than from 9am to 3pm.

To evaluate the impact of the free entitlement we need information on outcomes; and information on school achievementsat age 5 is only available from 2002/3. This restricts our analysis to considering children who would have been eligible for a free place from 2001-2007. Using this time period rather than the period from when the policy started in 1999/2000 has some advantages. Between 1999 and 2002, the first years of the policy build-up, only 6 per cent of the funded places were genuinely new capacity. In addition, it took some time for the quality requirements on providers of the free entitlement to be enforced. For both reasons it is cleaner to focus on later years when assessing the impact of offering free part-time early education places on child development.

The identification strategy used in this paper relies on variation over time and across space in the availability of free childcare. Figure 2 shows how preschool education developed from 1999 to 2002 and through to 2007. Over the whole period we can see a subtantial increase in free places in this time-period, and this was not uniformly distributed across LEAs: 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, and there was still substantial 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. Figure 3 follows analysis in Brewer et al. (2014b) and gives more detail on the trajectory of the build up. LEAs are split into four quartiles based on the increase between 2002 and 2007 in the proportion of three year olds with access to a place and we plot the average coverage for each group in all years from 1999 to 2007. It is clear that the group with the largest build up over our years of observation has the lowest level of coverage in 2002. The importance of the baseline level can be exploited to check our identification assumptions,

and we will return to this in our robustness checks. However, notice that buildup trajectories are rather different in the first years of the policy. Between 1999 and 2000, in particular, the strongest growth was found in areas with the most existing provision, this is due to an explicit focus on serving the most disadvantaged areas first.

4 Empirical strategy

In this paper we are interested in estimating the effect of making available free pre-school education places for 3-year-olds on child development. We consider the following reduced form model:

$$Y_{icl} = \beta_1 F_{cl} + \beta_2 X_{icl} + \beta_3 Z_{cl} + \gamma_c + \mu_l + \mu_l * t + e_{icl}, \quad (1)$$

where:

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

2. F_{cl} is an indicator of the availability of free places in a LEA of residence for a given cohort of children. More precisely, it is the proportion of the population of 3-year-olds for whom a free place is available.

3. X_{icl} is a vector of child characteristics measured at age 7 including gender, ethnicity (7 categories), free school meal status, language spoken at home, decile of neighborhood deprivation and the month of birth to control for relative age at test effects.[9]

4. Z_{cl} is a vector of LEA-level characteristics that may affect child outcomes and are time-variant. We include controls for economic conditions that may favourably or adversely affect children and for other early years intiatives active at LEA-level in the same time-period.

[9]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 age 3, as research shows that children that children who are eligible for free meals in any year will be affected by low income over longer periods of time. Likewise, changes in neighborhood deprivation through moves tend to occur around birth of a child, but the neighborhood quality tends to remain stable throughout the early years and starting school, see Rabe and Taylor, 2010.

5. γ_c is a cohort fixed effect which controls for unobserved factors affecting particular cohorts. This is implemented by including cohort dummies in the regressions.

6. μ_l is a LEA fixed effect that controls for time-invariant LEA characteristics. In particular, it controls for the fact that the build-up of the program was systematically related to pre-treatment levels of free childcare and therefore, indirectly, to relative deprivation at the LEA level.[10]

7. $\mu_l * t$ are LEA specific trends, these enable us to distinguish any general trends in outcomes within LEAs from those specifically related to the timing of the build-up in places.

8. e_{icl} is an idiosyncratic error term.

This equation estimates the Intention To Treat (ITT) parameter, β_1, of the availability of funded places in an area at a particular point in time. As we are asuming that the funded places were exogenously assigned conditional on the controls in the model, we give a causal interpretation to the ITT effect.

The estimated ITT effect potentially captures a number of things. For one, there is the participation effect. This needs to be considered in the context of the counterfactual care arrangement that might have been displaced by taking up a free part-time childcare place in the private sector. The counterfactual in this case is parental care and informal care by family and friends.[11] We expect positive (negative) effects on child outcomes if parental or informal care is of lower (higher) quality than pre-school education provided in formal settings. Participation effects can also arise at the intensive margin if parents increase hours of childcare use as a result of the policy. Second, there is the possibility of an income effect. This would accrue to families who were already using private childcare and continue to do so, with the free entitlement effectively providing an income subsidy. To the extent that parents invest the income subsidy into their child, this can improve child

[10] While it is quite plausible to assume that the proportion of 3-year-olds covered by free pre-school places is exogenous to the characteristics of an individual child i, we have to allow for the possibility that assignment of free places to LEAs was not random. In fact, evidence indicates that places were first created in the most deprived LEAs which did not already have maintained provision, followed by those areas which were better off.

[11] We think that the switch from public to private provision is likely to be very small, as the proportion of three year olds covered by public places did not change over the time-period we consider.

outcomes. Third, there may have been a quality effect, as settings which were eligible for funding also had to subscribe to the Early Years Curriculum. This may have improved quality when funding was introduced even if children attended the same setting as before. Fourth, the policy may have had a maternal employment effect. One of the aims of the policy was to increase maternal labor supply, 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 treatment effect estimated using equation (1) is a weighted average of a number of possible implicit treatment effects with the weights given by the number of children/families affected by each effect.

In addition to the ITT effect which informs about the overall impact on child outcomes of introducing free pre-school for 3-year-olds, we would like to estimate the impact of actual preschool participation. The usual procedure would be to weight the reduced form effect with the first stage, i.e. the increase in participation as a result of the free entitlement, using coverage with free places as an instrument. However, this would assume that child outcomes were affected by participation only and that the potential income, quality and employment effects are zero. We devote section 7 to exploring the mechanisms underlying the ITT effect.

5 Data

The 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. The NPD is a longitudinal register dataset for all children in state school in England, covering roughly 92% of pupils. It combines pupil level attainment data with pupil characteristics and school identifiers as they progress through primary and secondary school.

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 generally begin at age 4 in the academic year they turn 5. From birth to the end of reception year, at age 5, the Early Years Foundation Stage sets standards for the learning, development and care of children in schools and pre-school settings in England. At the end of Reception Year 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 personal, social and emotional development; communication, language and literacy; problem solving, reasoning and numeracy; knowledge and understanding of the world; physical development and creative development. 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 standardised separately by academic year.

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. The pupils affected by the roll-out of the free entitlement policy when they were nursery age have to date been tested at ages 7 and 11. At age 7 (Key Stage 1) test scores are not available, and following standard practice we transform National Curriculum levels achieved in Reading, Writing and Mathematics into point scores using Department for Education point scales. At age 11 we have comparable test scores for Reading and Maths for the cohorts we consider. We standardise age 7 point scores and age 11 test scores separately by academic year and subject.

In the NPD we can 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.

LEA-level controls

In our model we control for LEA fixed effects to account for time-invariant LEA characteristics, and for LEA-specific trends to distinguish general trends in outcomes from those related to the timing of the buildup of places. We also include in our model two sets of time-varying variables at the LEA level which might be associated with child outcomes. The first vector captures economic conditions that may favourably or adversely affect children through parental income and employment, for example. We include information from the

Labour Force Survey measured at the LEA level on the proportion of working-age individuals with certain qualification levels (NVQ3 and NVQ4, roughly equivalent to High School and College), and the share that are working and claiming unemployment-related benefits. We enter these separately for men and women and we lag employment and claimant rates to account for the fact that childcare availability might affect current employment rates. We also include the mean hourly and weekly mean pay from the NOMIS workplace analysis as controls.

The second set of LEA-level controls captures other early years initiatives that were active in the same time-period. 2002-2007 was a busy period for early childhood investment by the state, funds were allocated to a number of intitiatives aimed at improving childcare quality and offering services that improve child outcomes (see Stewart, 2013, for more details). Sure Start Centres were a flagship policy during this period; these provided help and advice on child and family health, parenting, money, training and employment as well as play sessions and (in some cases) childcare (Eisenstadt, 2011). Sure Start Centres were a very local policy, designed to be within 'pram-pushing' distance of disadvantaged families. In order to capture the likely exposure of children we count the number of Sure Start Centres available within each LEA in each year and weight these by the population catered for by Sure Start Centres (0-4 year olds).[12][13] To control for other policies, including Neighbourbood Nursery places (Smith et al., 2007) we use data from DfE which provides information on all spending on Early Years Services that was routed through Local Education Authorities (not including Sure Start and spending on the free entitlement itself). Again we weight this with the population of 0-4 year olds in each LEA. For both Sure Start Centres and spending on other initiatives we construct a three-year average around the year in which the child would be aged 3 to account for the fact that children would be able to benefit from these policies roughly between ages 2 and 4. Finally, the 15 hour extension of the free entitlement was piloted in some LEAs, and we include an indicator variable that captures pilot LEAs for the affected cohorts.

Measures of free childcare availability and childcare take-up

[12]The population figures are population estimates from the Office of National Statistics.

[13]Of course the specific location of the Sure Start Centre, in later years known as Sure Start Local Partnerships, will matter to their likely effect on individual children's outcomes, but similarly to our measure of free places we capture LEA average effects.

Annual headcounts of children aged 3 receiving free childcare by LEA are available from the Department for Education (Department for Education, various years) with separate counts of children in public provision (nurseries and nursery classes in primary schools) and in the private sector. The data is available from 1999, the year before the free provision for 3-year-olds was gradually introduced. Our measure of free part-time pre-school places is the sum of publicly provided places and free places in the PVI sector, divided by the population of 3-year-olds in each LEA.

To assess the overall increase in childcare participation in each LEA we again rely on Department for Education data. Headcounts of children taking up places in the private sector, including both free and privately funded places, are availble for years 2000 to 2007. There are some issues with the quality of these data which require us to carry out data adjustments.[14] 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 3-year-olds.

We scale all childcare measures so that a unit change represents a 10 percentage point increase in the children covered by free nursery places and taking up nursery places, respectively. We merge these data to children observed in the National Pupil Database using their LEA of residence at age 7. All children that were aged three in the month a headcount was taken are assigned the corresponding measure. Children turning 3 after that month are assigned the following year's measurement.

Estimation sample

This 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,

[14]During data collection, in some years not all providers returned data to the Department for Education (DfE), so that DfE revised the figures by assigning the average number of children of the providers that did return data to the missing providers. This ocurred in the years 2003-2007, with an estimated 3-4% of children missing in 2004, 2006 and 2007, and 14% (8%) missing in 2003 (2005). The data broken down by LEA were not revised by DfE, and we therefore adjust the data for 2003 and 2005 by interpolation and the data for 2003-2007 by increasing the counts in each LEA proportionally to the rate of unreported children in that year. More precisely, we first apply linear interpolation between the preceding and following year for years 2003 and 2005 and replace the data for a LEA if the interpolation leads to a higher count than the recorded count. This reduces the proportion of missing children to 5.4% in 2003 and 3.8% in 2005. We then increase the counts in each LEA by the proportion of children deemed by DfE to be missing in the returns overall, so that the count of children across all LEAs coincides with figures published by DfE.

the Foundation Stage Profile at age 5 is not available for earlier years.[15] Our sample therefore includes six cohorts of children aged 3 in the years 2001-2007 with observations at age 5 relating to academic years 2002/03 to 2007/08, observations at age 7 relating to academic years 2004/05 to 2009/10 and at age 11 to academic years 2008/09-2013/14.

From this sample we remove children living in Scotland or Wales and 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.[16] Finally, we retain only pupils for whom we have non-missing outcomes and background characteristics. The main estimation sample includes 6 cohorts of children with 3.2 million observations.

Table 1 shows descriptive statistics for child outcomes at ages 5, 7 and 11. We display mean raw Foundation Stage Profile point scores at age 5, raw point scores in Reading, Writing and Maths at age 7 and raw test scores in Reading and Maths 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 are outperforming boys in all outcome measures at all three ages, with the exception of Mathematics at ages 7 and 11 where boys are better than girls on average. Even 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 11 to 15% lower than those of children from higher income families. Summary statistics of individual and LEA-level controls are given in appendix Table A.

6 Results

In this section we present our main results, robustness checks and analysis by sub-group.

[15] In 2002/03-2005/06 FSP data were collected only for a 10% sample of school children, and we calculate and use weights to ensure the subsample is representative of the full population.

[16] Note that there is no grade repetition in the UK.

6.1 Main results

Our main set of results examines the effect of availability of free part-time preschool education for 3-year-olds on child outcomes measured in school at ages 5, 7 and 11. Table 2 presents our baseline results in column (4). The coefficients are estimated using linear regressions with LEA and cohort fixed effects and LEA-specific time trends as well as individual and LEA-level controls (see equation 1).[17] The top panel shows effects on standardised point scores in the Foundation Stage Profile (FSP) and the learning areas within the FSP at age 5. The middle panels of Table 2 shows effects of free childcare availability on standardised point scores in Reading, Writing and Maths at age 7, and the bottom panel displays the effect on standardised scores in Reading and Maths at age 11.

Looking first at the age 5 outcomes in the top panel of Table 2, column (4), the results show that availability of free childcare has a positive effect on several outcome measures: A 10 percentage point increase in the proportion of 3-year-olds for whom free preschool is available leads to an increase in the FSP score of 1.8% of a standard deviation. Positive and statistically significant effects of around 1-2% of a standard deviation are also found for the learning areas Literacy (communication, language and literacy), Numeracy (problem solving, reasoning and numeracy) and Social development (personal, social and emotional development).

To assess the magnitude of these effects, we can consider that the free entitlement policy increased the fraction of 3-year-olds receiving free early education by around 50 percentage points (from an average of 37% to 88% between 1999 and 2007), so if we assume a linear effect we can extrapolate and say that the policy change improved children's outcomes by an average of 9% of a standard deviation overall. By way of comparison, a 9% of a standard deviation improvement in scores is equivalent to being 1.5 months older within the academic cohort, and compares to a FSM "penalty" of 59% of a standard deviation in Foundation

[17] The effect of adding fixed effects and time trends in our model is explored in columns (1)-(3) of the Table. Column (1) shows results for a model that controls only for cohort fixed effects and individual characteristics. Adding LEA fixed effects to control for persistent LEA characteristics reduces point estimates slightly for most outcomes (column 2), and controlling for a LEA time trend again changes point estimates only by a little (column 3). This reassures us that by adding a LEA time trend we are not absorbing most of the variation in the build-up of childcare places that we are expoilting for identification. Adding time-varying LEA level controls, including economic conditions and early years initiatives again reduces point estimates somewhat for most outcomes, but not hugely, see column (4). We will explore in the robustness checks what happens when we further control flexibly for intitial levels of childcare coverage.

Stage Profile scores. This indicates that the impact of the policy is 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 a point increase of 1.7 FSP points or a 1.9% improvement in points on average - again a small effect.

Focusing now on the results at age 7, displayed in column (4) in the middle panel of Table 2, our reults show a positive and statistically significant effect of free childcare availability on Reading (at the 10% level) and Writing, but not Maths point scores. At just 0.3% of a standard deviation for a 10% point increase in childcare availability the increases are again small, however. For example, this would increase point scores achieved in Writing by just 0.01% compared to the average. Scaling this up to the increase in available places of the policy overall (50%), means that the policy has increased scores in Writing by 0.4% on average. Clearly, this is a very small effect.

Results for outcomes at age 11 can be found in the bottom panel of Table 2, column (4). We can see that by age 11 we find no statistically significant effects of access to free places on test results in Reading and Maths. In other words, the small gains in children's academic outcomes at younger ages are not sustained until the end of primary school.[18]

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 for boys than girls for all outcomes, indicating that boys benefit more from access to free places than girls do. Differences in the effect of nursery attendance by gender are a fairly common finding in the literature (see for example Havnes and Mogstad, 2011; Felfe et al. 2014; Datta Gupta and Simonsen 2010), but most authors find that girls benefit more from early education than boys. However, none of the gender differences we find are statistically significant (only in Literacy is the gender difference significant at the 10% level). At ages 7 and 11 the point estimates are very similar between boys and girls, and any differences are not statistically significant.

[18]This 'fade out' is a common empirical finding for 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 the fade out.

6.2 Robustness checks

Before proceeding further with the analysis by sub-group and exploring possible mechanisms, we test the robustness of our main results, concentrating on outcomes observed at age 5. We address a number of concerns. First, it may be the case that children sort into schools of different quality depending on whether they have accessed childcare at age 3. For example, if children with pre-school experience sorted into worse schools, the small positive impact of childcare we observe at the end of the reception year in school at age 5 could be the result of bad schools undoing the benefits of pre-school education. If children with pre-school experience sort into better schools, our estimated effect of pre-school availability will be biased upwards. To address this concern we estimate our models using school rather than LEA-level fixed effects. This should eliminate all the school-level differences between children. Table 3 displays the results of this exercise for our four outcomes at age 5 in column (2), whereas column (1) displays our baseline estimates. We can see that the point estimates in the models based on LEA and school-level fixed effects are very similar, indicating that sorting into different quality schools is not driving the effect.

Our second robustness check looks at how sensitive our results at age 5 are to the weights we apply to account for the fact that we only observe FSP outcomes for a 10% sample of students in the earlier years of our data (see section 5 for details). The results are presented in column (3) of Table 3. The unweighted results are identical to baseline results for the total FSP score. There are some minor differences for the results obtained for the different areas of learning, but these differences are not statistically or substantively significant.

Thirdly, we want to check whether our results change when we choose a different method for assigning available free places to the children in our sample. The data on places is collected only annually at the turn of the year, and needs to be merged to children within each academic cohort depending on when they turn 3. In our baseline estimates we assign the annual headcount of free places to all children within a school cohort that were three at the time of the count, and for children born later in the academic year we use places from the following year's census. Therefore we assign each child the census that would conceivably include that child. This method is not ideal, in that children born in September to December become eligible for their free entitlement just after the count is taken, so our assumption is that places available just before this are a good proxy for their availability.

We check the senstivity of our results to matching children to places in a different way. Here we match the following year's census to all children in an academic cohort, assuming that September to December borns will not access the places before January and that the following census therefore better captures availability. The results are displayed in column (4) of Table 3. Reassuringly, they show that substantively the estimates are very similar, although the point estimates are slightly higher when using the alternative measure, with higher standard errors. It is possible that the wide-spacing of the place data leads to some attenuation bias; the effects we find could be a lower bound estimate of the impact of place availability.

Fourth, we want to carry out a placebo test. Ideally we want to see whether effects disappear, as they should, when assigning the build-up of places to earlier or later cohorts, or conversely, when assigning the cohorts included in our sample places of a different time-period. Unfortunately data limitations (number of cohorts for whom we oberve outcomes and number of years for which we have data on places) mean that we cannot match cohorts to places that were completely irrelevant for them. We can, however, assign the children in our sample next year's and previous year's places instead of the year relevant to them. We expect the estimates to be smaller but not necessarily zero, as there will be a correlation of places in adjacent years. Columns (5) and (6) of Table 3 show the results. Compared to our baseline estimates displayed in column (1) the point estimates when assigning next year's or previous year's places are indeed lower, and they are not statistically significant for several outcomes.

Finally, the identification strategy we employ in this paper relies on us controlling sufficiently for all unobserved factors that could be potentially correlated both with the roll-out of free places and with child outcomes. By controlling for LEA and cohort fixed effects and LEA time trends, as well as time-varying LEA characteristics, we are going further than most comparable papers which often do not control for regional trends or characteristics. However, we want to check whether there are unobserved factors associated with the pre-policy level of public childcare provision. This is because we know that the roll out of further free places was mechanically related to pre-reform levels as the scope for increasing free places was higher in LEAs with lower pre-policy levels, and we want to make sure that this in itself is not responsible for the change in outcomes. It might also be that the pre-policy level of

provision reflects unobserved factors which also impact child outcomes (political preferences, for example). Following Duflo (2001) we check that our estimates are robust to allowing for the existence of variation over years that is different according to the values assumed by the pre-reform levels of free childcare. We do this by entering in our model interactions of the 1999 LEA levels of free childcare with academic year dummies. Following the same line of reasoning we also estimate a model including interactions with levels of free childcare in 2002 with academic year interactions, as 2002 is the first year of the observation period we use in this paper. Results of this robustness check can be found in columns (7) and (8) of Table 3. We can see that flexibly controlling for initial levels of free childcare availability in 1999 or 2002 does not substantively affect the estimates for any of the outcomes at age 5.

6.3 Heterogeneity

Our baseline results indicate a small positive impact of access to free childcare places at early ages and no impact remaining at the end of primary school. However, it may be that larger positive effects are concentrated on subgroups of the population. In particular we are interested to see whether children from deprived backgrounds stand to gain from the policy as this could contribute to closing socio-economic gaps in children's outcomes - one of the aims of providing free early education. We might expect children from lower socio-economic backgrounds to benefit more from an improvement in access to childcare as their alternatives (maternal and informal care) are likely to be, on average, worse than for more privileged children. Therefore in this section we present results for different subgroups, where we capture disadvantage by free school meal eligibility, neighborhood deprivation and by whether English is an additional language for the child. These three measures of disadvantage each reflect slightly different things, with free school meal status capturing low family income[19] and neighborhood deprivation capturing income deprivation of the area, dividing neighborhoods 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 (see Dustmann et al., 2013).

[19]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 (2007) for some shortcomings.

Table 4 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 sub-group 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 age 5, but not at the later ages. None of the differences are statistically significant; the estimated difference in standardised FSP scores between children with and without free school meal eligibility is 0.008, with a confidence interval of (-0.02 to 0.04). Columns (4)-(6) show results by neighborhood deprivation tertile. Here the differences in point estimates at age 5 are quite pronounced, with no statistically significant effect of pre-school availability estimated for affluent and middle neighborhoods, and positive and statistically significant effects accruing in the 33% most deprived neighborhoods. The differences between the affluent and deprived neighborhoods are statistically significant at 10% or higher for all 4 outcomes measured at age 5. At ages 7 and 11 the differences between children living in deprived and affluent neighborhoods disappear. Finally, columns (7) and (8) display results by language spoken at home. At age 5 point estimates are higher for children who do not speak English at home, indicating that they benefitted more from childcare access than English speaking children. However, standard errors are relatively large and group differences not statistically significant. The estimated difference in standardised FSP scores between children who do and do not speak English at home is 0.010 with a confidence interval of (-0.02 to 0.04). Again, there are no differences between children by language spoken at home at later ages.

In summary, we find only slight evidence that children from deprived or lower socio-economic backgrounds have benefited more from an improvement in access to childcare places than children from less deprived backgrounds. Although there are some small differences in the point estimates at age 5 that suggest a larger benefit for deprived children (statistically significant when comparing most and least deprived neighborhoods), these are not sustained into the later ages. Based on the measures available it seems that the equity goals of the policy have largely not been met, because the benefits are not sustained. The next section of the paper explores the mechanisms that may be driving the small results we find for the policy overall, and we consider these with a view to children from different backgrounds.

7 Mechanisms

In this section we explore the mechanisms that may be driving the small effect sizes. To this end we first look at the relationship between availability of free places and actual childcare participation. Further, we assess whether the increase in childcare participation differed by child background; we do this to try and explain the weak evidence that more deprived children benefit slightly more at age 5. Finally, we look at other possible mechanisms, in particular maternal employment and childcare quality.

7.1 Participation and crowd-out

The descriptive evidence presented in Figure 1 already suggested that the increase of free places has not led to an equivalent increase in the number of 3-year-olds participating in childcare. About eighty percent of three year olds were accessing some form of childcare in 1999 when the age 3 entitlement came into effect. Rougly half of these places were funded by parents, the other half by the state through providing places. By 2007 free places had increased by roughly 50 percentage points, but childcare participation by only about 14 percentage points. In Table 5 we present LEA-level estimates of the 'first stage' relationship, i.e. the effect of the availability of free places on childcare participation.

We present in column (1) results for the whole sample, based on regressions that control for LEA level means of individual charatceristics, LEA and cohort fixed effects as well as LEA-specific time trends. The estimated coefficient shows that 2.4 genuinely new places were created for every 10 places that were funded. This shows that the policy has crowded out parental investments into pre-school education to a large extent, and has primarily worked as a transfer of resources to parents who would have used childcare in absence of the policy. 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. To provide context, the mean income of parents with three year olds in 2003 was just under £17,000 a year (Ward et al., 2007, p.171). Even if they would have received free early education without the policy, children might also have benefitted from improved quality as settings registered themselves for funded status.

Given the large crowd-out we want to consider whether the effects from access to free places on child outcomes are a result of income transfers to parents (who might invest the transfers into the development of their children, e.g. by buying books) and quality improvements or if they come from participation in preschool. To investigate this further we split the LEAs in our sample into LEAs where a high proportion of funded places led to increase in childcare participation ('complier LEAs') and LEAs where the crowd-out of parental investments was high ('non-complier LEAs').[20] Table 5 shows the first stage relationship for complier and non-complier LEAs in columns (2) and (3). In complier LEAs almost half (43%) of the increase in funded places led to increased childcare participation, whereas in non-complier LEAs only around 17% of the increase in funded places translated into increased childcare participation. We would expect that any observed effects in complier LEAs are driven primarily by more children participating in preschool. In non-complier LEAs positive effects would be more likely driven by income effects and/or improved quality; it is not possible to distinguish between these two. We therefore estimate the effect of availability of free places using equation (1) with full interactions for complier status.

Table 6 displays the results of our estimates by LEA complier status. Comparing columns (2) and (3) we can see that at age 5 the positive effect of the free entitlement is concentrated on complier LEAs. The point estimates in complier LEAs are roughly double the size of the effects found overall (column 1), whereas in non-complier LEAs the effects are near zero and not statistically significant. The differences between complier and non-complier LEAs are statistically significant for all outcomes at age 5 at 10% or higher. This indicates that the effects of childcare availability are driven by participation more than by income effects. 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 8-11% of a standard deviation (dividing the point estimates in columns (1) or (2) of Table 6 by the corresponding first stage estimates in Table 5). For an individual taking up childcare who would not otherwise have done so, this implies an impact of up to 110% of a standard deviation, although this is likely to be an overestimate as it relies on the strong assumption of no income, quality and other indirect effects. We can conclude that the small overall effects

[20]Specifically we split the LEAs in our sample in half based on the ratio between change in all childcare over our sample years and change in subsidized childcare over our sample years.

of the free entitlement policy are largely caused by crowd-out.[21] This positive perspective must be tempered by the finding that effects do not last to age 11, even in complier LEAs.

7.2 Participation by background

Our expectation was that children from lower socio-economics status families might benefit more from childcare availability than other children. This could occur for two reasons; because these families were less likely to have been accessing early education before the free entitlement and/or because accessing early education is more beneficial in situations where mothers or other care givers do not provide high quality investments. We have shown that our hypothesis is only weakly supported by the results. We use evidence from a survey of parents to try and understand how usage patterns changed by parental characteristics over the period.[22] Table 7 shows the proportion of 3-year-olds in formal, center-based childcare in the years marking the beginning and end of our sample period (2001 and 2007), as well as the change between these years. The first row giving these figures for all children shows that the level of childcare use is lower in both years 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. The second to fourth panels of Table 7 show changes in formal childcare use for children by background. Here we distinguish children by their family's social class, mother's education and deprivation of neighborhood as measured using the Index of Multiple Deprivation. Looking first at the differences by social class, the Table shows that children in families belonging to the lower two social classes (semi-skilled and unskilled; skilled manual) have increased their use of formal childcare by twice as much as children from the upper two social classes (skilled non-manual; professional and managerial). Looking at this by maternal education paints a different picture, however. Here it is the children at both extremes, from low and high rather than medium educated mothers who have increased participation most. Finally, the split by neighborhood deprivation shown in the bottom panel of Table 7 reveals

[21] Of course it may be that there were reactions to the policy not only at the extensive but also the intensive margin. We look at changes in childcare hours over the policy buildup period below.

[22] From 1997 the Department for Education and Employment began to commission surveys to discover more about parents' use and perception of childcare and early years services, under a number of different names. We use the 2001 Parents Demand for Childcare Survey and the 2007 Childcare and Early Years Provision: Parents Survey. Data was collected from 5,416 households in 2001 and 7,136 households in 2007. Information was collected on household demographics including parental qualification and employment history, patterns of childcare use in the last year and previous week.

that increase in use of formal childcare between 2001 and 2007 was largest for children living in the middle quintiles of deprivation and smaller in the least and most deprived neighborhoods. Taken together, it is hard to see a compelling story that children from poorer backgrounds increased their access to early education and care more than others.

In the next table, Table 8, we use the same ways of capturing children's socio-economic background. This time we look at changes in formal care between 2001 and 2007, as before, plus changes in other forms of care, including informal care by family and friends, exclusive parental care and other forms of care such as childminders and nannies. This helps us to understand the types of care that were substituted by formal care for different types of children, and consider their likely quality.

Table 8 shows that the overall increase in formal childcare from 2001 to 2007 was accompanied by a reduction in exclusive parental care, whereas there was no statistically significant change in the other forms of childcare (informal care and other care). Note that children taking up formal childcare on a part-time basis will still require other forms of care, so we do not expect the increase in formal care to be mirrored in equivalent decreases in other types of care. Looking at the changes by social class, Table 8 shows that for children in the two lower social classes the increase in formal care was accompanied by a reduction in both informal care and in exclusive parental care. This may imply that children from lower socio-economic backgrounds received a relative quality improvement as a consequence of the free entitlement compared to their more advantaged peers.

The analysis by mother's education and neighborhood deprivation mostly confirms this. In particular children with low educated mothers experienced a stark reduction in informal care of 23 percentage points, and children from the 20% most deprived neighborhoods a significant reduction in informal care of 11 percentage points. However, the reduction in exclusive maternal care seems to be concentrated on high educated mothers and children from middle to low deprived neighborhoods although the reduction in exclusive parental care was highest among children with high educated mothers (9 percentage points). In summary, children from poor backgrounds were likely to switch out of care by family and friends, whereas children from more affluent families that were previous exclusively cared for by their parents received more formal childcare as a result of the policy.

In summary, there is no strong evidence that children from poorer backgrounds entered formal childcare more as a result of the policy. This goes some way to explaining why there are no large impacts of the policy in terms of narrowing the socio-economic gap between children. There is, however, evidence that children from poorer backgrounds were more likely to shift out of informal care as the free entitlement was rolled out, but this does not seem to have a strong effect on children's outcomes. This implies no large quality difference between informal care and the early education settings this group experienced.

Another possible mechanism mentioned in Section 4 is that parents whose child already attends nursery might react to the free part-time place by increasing the number of hours attended. Datta-Guppta and Simonsen (2010) find evidence that longer hours in non-parental care leads to poorer non-cognitive outcomes for Danish children. If long hours are a consequence of the policy for a sub-group of children, this might explain why impacts are muted overall. Table 9 uses the same survey data as before to show the hours used, conditional on participation, both for high users of childcare (at the 75th percentile of the hours distribution) and for the mean of all users. It demonstrates that although there has been an increase in hours used, the child from the highest participation group is still using only 15-20 hours a week in 2007 and children overall are using 13-16 hours a week, depending on the education of the mother. This does not seem to indicate a long hours culture for childcare, so that it seems unlikey that increased hours should negatively affect children.

7.3 Maternal labor market behavior

One factor that could be responsible for the small impact of free place availability on child outcomes could be maternal labour supply. Increasing maternal labor supply was one of the aims of the policy, and 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 10 we present estimates of the effect of childcare availability on different measures of maternal labour market behavior. These estimates are based on data from the Labour Force Survey for the time period 2002-2007 and focus on mothers with three year old children. As we can see, a 10 percentage point increase in coverage with free places has a very small effect on all of the measures of maternal labor market behavior considered in the Table and standard errors are large, i.e. any effects are not statistically

significant. This indicates that there is no sizeable effect of the free entitlement on child outcomes operating through maternal employment.

In a related paper Brewer et al. (2014b) estimate the effect of the free entitlement in England on maternal employment over a longer period (2000-2008) and using 2 year olds as control group in a difference-in-difference framework. They find slightly larger effects on labor force participation of 0.004, but these are not statistically significant. When focusing the analysis on mothers whose youngest child is aged 3 the effects are larger (0.006) and statistically significant. This indicates that a subgroup of children might be affected in their outcomes by mothers' working, but this should not have a large impact on mean outcomes of all children.

7.4 Childcare quality

We have so far found it hard to explain why the impact of free nursery places in England has been small and short-lived. We know that crowd-out provides part of the explanation, but even in areas where crowd-out is lower the impact of the policy does not persist. A unique feature of universal early education in the UK is the reliance on private nurseries to provide almost all of the new places. We have already stated that these nurseries have lower qualifications requirements for staff than do public nurseries, and Gambaro et al. (2013) show that less than 40 per cent of children in private nurseries have a teacher present, compared to 100 per cent in public nurseries. 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 which gained a graduate leader were able to improve the quality offered significantly compared with those who did not (Ranns et al., 2011).

Public nurseries also have higher quality based on detailed observation of classroom practice and adult-child interactions (Sylva et al. 2004). Although there has been some improvement in quality along this criterion since the free entitlement came into effect, Mathers et al. (2007) show that gaps between publicly provided childcare and private setting remain. The lack of long-term effects we find in this paper might be a consequence of insufficient attention to the quallity of the newly funded private places.

A close examination of the distribution of quality 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., 2013). However, as the new places funded under the free entitlement were private we need to understand how this sector works for those who are less privileged. Gambaro et al. (2013) show that private nurseries serving less advantaged children are likely to obtain worse quality inspection ratings than average. This picture is confirmed by Mathers and Smees (2014) using researcher-observed measures of quality, which show that disadvantaged children are experiencing pre-schools with poorer staff-child interactions and less support for language development. The presence of a graduate leader ameliorated this gap.

8 Conclusions

The UK government spends almost £2 billion every year to provide universal part-time preschool education to children aged 3 and 4. 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 the universalism was achieved by paying private providers a fixed amount for all eligible children in their care; there is demand-led rather than supply-led funding. This paper exploits the staggered introduction of the entitlement to free preschool for 3-year-olds in England to investigate the effect of the policy on child outcomes at ages 5, 7 and 11. We find that a 10pp increase in the proportion of 3-year-olds covered by free places improves cognitive and non-cognitive outcomes at age 5 by 2% of a standard deviation, with larger effects for boys than girls, and some weak evidence that effects are larger for those from lower rather than higher socio-economic backgrounds.

Crowd-out is substantial with only 1 in every 4 newly funded places between 2002 and 2007 providing a genuine new place. The other 3 simply switched funding from private to public provision. Effects at age 5 are larger when we focus on areas where more new places were created. Crowd-out can therefore explain the limited impact of the policy to some extent. However, even in areas where crowd-out was limited, impacts do not last through to ages 7 and 11. It could be that our results can be explained by the fact that all the new

places that resulted from the policy were in the private sector. This sector is subject to less regulation in the UK compared to publicly provided childcare, it has fewer qualified teachers and is less good in terms of pedagogical quality. There is also evidence that private nurseries which serve poorer children are differentially bad on these measures. This could help to explain why the policy had so little success in reducing gaps in cognitive development between children from different backgrounds. Of course, our conclusions are based on the outcomes in cognitive achievement reported by primary schools. The Perry Preschool experiment found long lasting impacts on non-cognitive skills manifested in reduced crime rates later in life. It is therefore possible that other benefits will accrue to children who benefitted from the provision of free childcare entitlement, which will not become evident until the children age.

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Figure 1: Percentage of 3-year-olds in preschool education

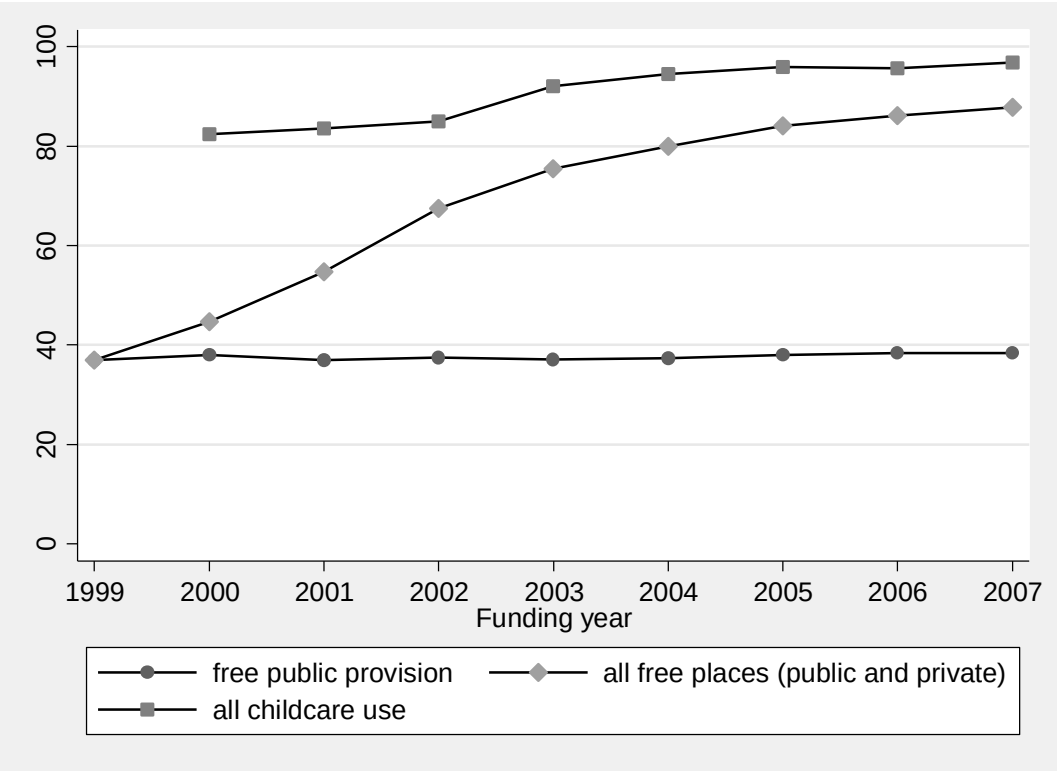

Figure 2: Free nursery places for 3-year-olds across England, 1999, 2002 and 2007

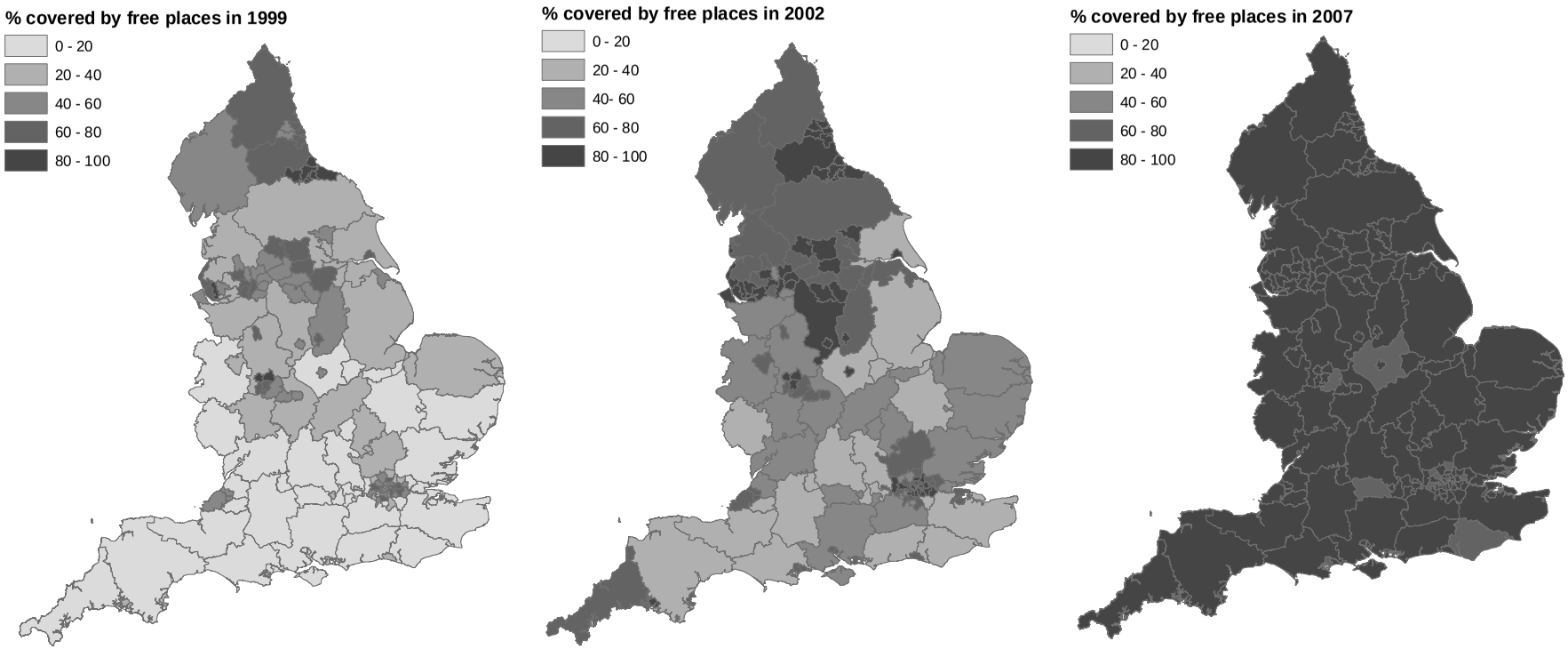

Figure 3: Expansion of free nursery places for 3-year-olds across England, 1999-2007

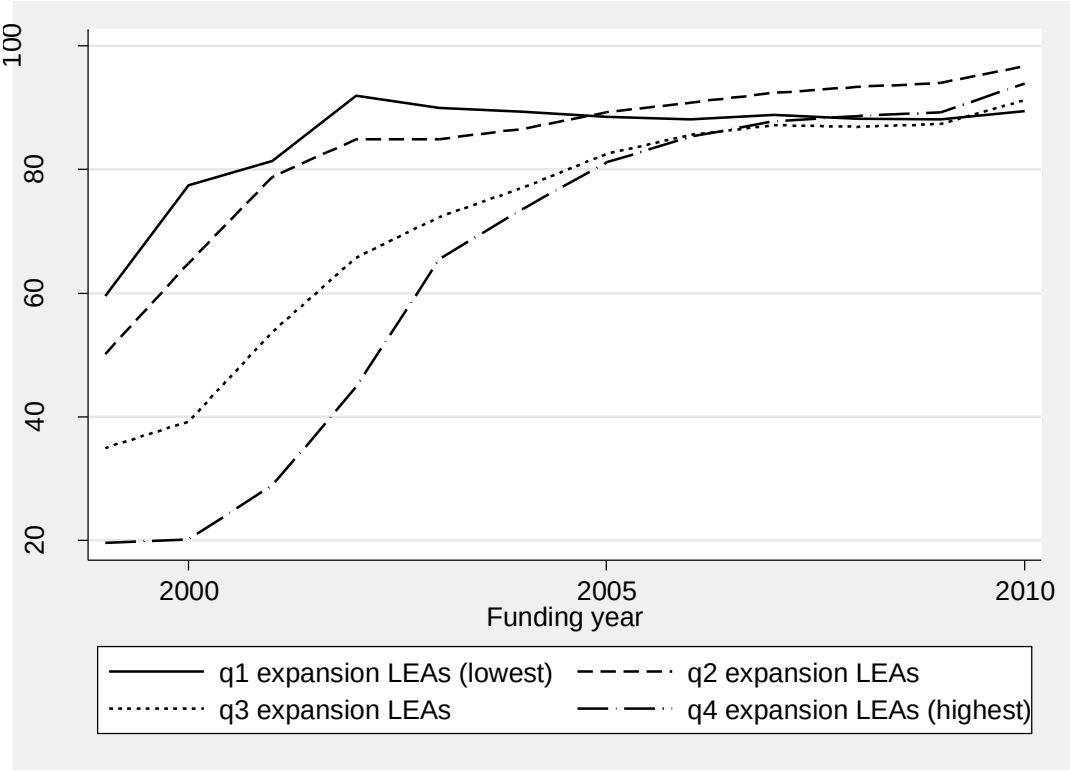

Table 1: Summary statistics for child outcomes at ages 5, 7 and 11

	All		Girls		Boys		FSM		not FSM	
	mean	std dev	mean	std dev	mean	std dev	mean	std dev	mean	std dev
Raw Foundation Stage Profile (FSP) point scores (age 5)										
FSP total	87.48	18.58	90.17	17.46	84.84	19.32	78.44	20.13	89.36	17.71
literacy	25.22	7.02	26.35	6.65	24.13	7.21	21.77	7.31	25.95	6.74
numeracy	20.42	4.77	20.72	4.50	20.11	5.01	18.18	5.37	20.89	4.50
social	21.09	4.33	21.80	3.99	20.40	4.54	19.42	4.68	21.44	4.18
Raw Key Stage 1 point scores (age 7)										
Reading	15.78	3.86	16.08	3.95	14.99	4.27	13.57	4.35	16.00	3.91
Writing	14.72	3.59	15.14	3.67	13.82	4.01	12.70	4.26	14.87	3.63
Maths	15.87	3.24	15.60	3.28	15.70	3.76	14.21	3.68	15.98	3.39
Raw Key Stage 2 point scores (age 11)										
Reading	30.99	9.29	32.03	9.07	29.98	9.40	27.06	9.44	31.80	9.05
Maths	68.94	20.44	67.67	20.37	70.19	20.42	60.81	21.13	70.61	19.88

Notes: National Pupil Database, 2003-2010. FSP weights applied. FSM is eligible for free school meals. 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.

Table 2: Effect of availability of free preschool: reduced form estimates

	All cohort FE (1)	All + LEA FE (2)	All + LEA trend (3)	All + LEA controls (4)	Girls (5)	Boys (6)
Standardised Foundation Stage Profile (FSP) point scores (age 5)						
FSP total	0.025**	0.025**	0.022**	0.018**	0.011	0.026**
	(0.009)	(0.007)	(0.006)	(0.007)	(0.008)	(0.008)
literacy	0.025**	0.016**	0.016**	0.013*	0.005	0.021**
	(0.008)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
numeracy	0.023**	0.022**	0.020**	0.017**	0.009	0.024**
	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
social	0.024**	0.022**	0.017**	0.014*	0.007	0.022**
	(0.009)	(0.007)	(0.006)	(0.007)	(0.008)	(0.007)
N	1.2 m					
Standardised Key Stage 1 point scores (age 7)						
Reading	0.007*	0.003	0.004**	0.003+	0.002	0.003*
	(0.004)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Writing	0.010*	0.004+	0.005**	0.003**	0.001	0.006**
	(0.004)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Maths	0.004	0.002	0.001	0.001	-0.001	0.002
	(0.004)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
N	3.2 m					
Standardised Key Stage 2 point scores (age 11)						
Reading	0.019**	-0.000	-0.001	-0.003+	-0.004*	-0.002
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Maths	0.027**	-0.002	0.001	0.001	-0.001	0.003
	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
N	2.9 m					
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
LEA FE	No	Yes	Yes	Yes	Yes	Yes
LEA time trend	No	No	Yes	Yes	Yes	Yes
Controls						
individual	Yes	Yes	Yes	Yes	Yes	Yes
economic (LEA)	No	No	No	Yes	Yes	Yes
early years (LEA)	No	No	No	Yes	Yes	Yes

Notes: National Pupil Database, 2003-2010. Estimates include LEA and cohort fixed effects and LEA-specific time trends. FSP weights applied. Availability of free preschool 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. 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 < .10, * p < .05, ** p < .01.

Table 3: Robustness checks

	baseline estimate	school FE	no weights	alternat. measure of places	next year's places	last year's places	control places 1999	control places 2002
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Standardised Foundation Stage Profile (FSP) point scores (age 5)								
FSP total	0.018**	0.017**	0.018*	0.020+	0.010	0.011*	0.018*	0.019**
	(0.007)	(0.007)	(0.007)	(0.011)	(0.009)	(0.005)	(0.008)	(0.007)
literacy	0.013*	0.012*	0.009+	0.015+	0.005	0.011*	0.013*	0.014*
	(0.006)	(0.006)	(0.004)	(0.009)	(0.006)	(0.004)	(0.006)	(0.006)
numeracy	0.017**	0.015**	0.012*	0.015+	0.013+	0.012**	0.016**	0.016**
	(0.006)	(0.006)	(0.005)	(0.009)	(0.007)	(0.004)	(0.006)	(0.006)
social	0.014*	0.013*	0.013*	0.014	0.002	0.009+	0.014*	0.015*
	(0.007)	(0.006)	(0.006)	(0.010)	(0.009)	(0.004)	(0.007)	(0.007)
N	1.2 m							

Notes: National Pupil Database, 2003-2010. Estimates include LEA and cohort fixed effects and LEA-specific time trends. FSP weights applied. Availability of free preschool 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. 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 < .10, * p < .05, ** p < .01.

Table 4: Effect of availability of free preschool: sub-group analysis

	All	FSM	not FSM	affluent nbh	middle nbh	deprived nbh	EAL	not EAL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Standardised Foundation Stage Profile (FSP) point scores (age 5)								
FSP total	0.018**	0.024+	0.015*	0.003	0.013	0.025**	0.025+	0.014*
	(0.007)	(0.014)	(0.007)	(0.007)	(0.009)	(0.009)	(0.013)	(0.007)
Literacy	0.013*	0.018	0.011+	0.001	0.008	0.022*	0.030**	0.008
	(0.006)	(0.012)	(0.006)	(0.006)	(0.009)	(0.009)	(0.013)	(0.006)
Numeracy	0.017**	0.023	0.013*	0.003	0.008	0.023*	0.016	0.013*
	(0.006)	(0.014)	(0.006)	(0.006)	(0.008)	(0.009)	(0.015)	(0.006)
Social	0.014*	0.018	0.013+	-0.001	0.015+	0.019*	0.018	0.011
	(0.007)	(0.014)	(0.007)	(0.008)	(0.008)	(0.009)	(0.014)	(0.007)
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	0.003+	0.002	0.002+	0.002	0.001	0.002	-0.002	0.002+
	(0.001)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.001)
Writing	0.003**	0.002	0.003*	0.002	0.001	0.002	-0.001	0.003*
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.001)
Maths	0.001	0.000	0.003	0.000	-0.001	0.002	-0.002	0.001
	(0.001)	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)	(0.001)
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	-0.003+	-0.002	-0.002	-0.001	-0.002	-0.000	-0.002	-0.002
	(0.002)	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.002)
Maths	0.001	0.003	0.000	0.000	0.001	0.002	-0.002	0.001
	(0.001)	(0.003)	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)	(0.001)
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, 2003-2010. Estimates include LEA and cohort fixed effects and LEA-specific time trends. FSP weights applied. Availability of free preschool 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. 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 < .10, * p < .05, ** p < .01.

Table 5: First stage: effect of free places on childcare participation

	(1)	(2)	(3)
	all LEAs	complier LEAs	non-complier LEAs
free places	0.237**	0.425*	0.169**
	(0.050)	(0.164)	(0.055)
N	888	888	

Notes: Department for Education data, 2001-2007. Estimates at LEA level include LEA and cohort fixed effects and LEA-specific time trends, no further controls. Standard errors clustered at LEA level. + p < .10, * p < .05, ** p < .01.

Table 6: Income versus participation effects

	All	Complier LEAs	Non-complier LEAs
	(1)	(2)	(3)
Standardised Foundation Stage Profile (FSP) point scores (age 5)			
FSP total	0.018**	0.046**	0.007
	(0.007)	(0.015)	(0.008)
literacy	0.013*	0.025**	0.006
	(0.006)	(0.009)	(0.007)
numeracy	0.017**	0.032**	0.009
	(0.006)	(0.008)	(0.007)
social	0.014*	0.030**	0.007
	(0.007)	(0.010)	(0.008)
N	1.2 m	0.5 m	0.7 m
Standardised Key Stage 1 point scores (age 7)			
Reading	0.003+	0.005	0.001
	(0.001)	(0.003)	(0.001)
Writing	0.003**	0.005+	0.002
	(0.001)	(0.003)	(0.002)
Maths	0.001	0.002	-0.000
	(0.001)	(0.003)	(0.001)
N	3.2 m	1.4 m	1.8 m
Standardised Key Stage 2 point scores (age 11)			
Reading	-0.003+	0.006+	-0.004*
	(0.002)	(0.003)	(0.002)
Maths	0.001	0.005+	-0.001
	(0.001)	(0.003)	(0.002)
N	2.9 m	1.3 m	1.6 m

Notes: National Pupil Database, 2003-2010. Estimates include LEA and cohort fixed effects and LEA-specific time trends. FSP weights applied. Availability of free preschool 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. 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 < .10, * p < .05, ** p < .01.

Table 7: Formal childcare use 2001-2007 by parental background

	Proportion of 3-year-olds in formal, center-based childcare		
	2001	2007	Change
All	63.7	80.8	17.1***
			(2.272)
Social class			
Semi-skilled and unskilled	50.4	78.1	27.6***
			(5.212)
Skilled manual	45.6	79.7	34.0***
			(8.477)
Skilled non-manual	67.9	82.5	14.6***
			(3.754)
Professional and managerial	75.8	89.1	13.3**
			(5.846)
Mothers education			
Low educated	54.7	70.5	15.8***
			(5.166)
Medium educated	72.7	83.5	10.9***
			(4.064)
High educated	71.5	87.2	15.8***
			(4.053)
Index of Multiple Deprivation			
Most deprived quintile	51.3	68.2	16.9***
			(5.1095)
Fourth quintile	59.6	76.7	17.1***
			(5.154)
Middle quintile	64.6	85.8	21.2***
			(5.008)
Second quintile	72.3	90.5	18.2***
			(4.545)
Least deprived quintile	75.9	90.6	14.6***
			(4.301)
N	592	1,242	

Notes: Parents Demand for Childcare Survey 2001 and Childcare and Early Years Provision: Parents Survey 2007. Sampling weights applied. + p < .10, * p < .05, ** p < .01. Standard errors between parenthesis.

Table 8: Changes in childcare use 2001-2007 by parental background

	Δ formal care (centre based)	Δ informal care (family and friends)	Δ exclusive parental care	Δ other care (childminder, nanny, etc.)
	(1)	(2)	(3)	(4)
All	17.1***	-1.7	-7.2***	1.6
	(2.272)	(2.239)	(1.889)	(1.654)
Social class				
Semi-skilled and unskilled	27.6***	0.0	-13.4**	-1.6
	(5.212)	(5.138)	(4.692)	(3.104)
Skilled manual	34.0***	-19.4**	-13.2*	4.8
	(8.477)	(8.203)	(7.768)	(4.7271)
Skilled non-manual	14.6***	0.4	-9.0***	6.4
	(3.754)	(4.024)	(3.065)	(2.662)
Professional and managerial	13.3**	1.9	-7.0**	-0.5
	(5.846)	(4.434)	(2.697)	(3.759)
Mothers education				
Low educated	15.8***	-22.9***	1.1	-4.2
	(5.166)	(4.991)	(4.401)	(3.1278)
Medium educated	10.9***	4.0	-4.5*	4.4**
	(4.064)	(4.488)	(3.196)	(2.736)
High educated	15.8***	6.1	-8.9***	1.8
	(4.053)	(4.410)	(3.172)	(3.791)
Index of Multiple Deprivation				
Most deprived quintile	16.9***	-11.2**	-3.2	1.5
	(5.1095)	(4.780)	(4.553)	(2.507)
Fourth quintile	17.1***	-4.2	-4.8	3.7
	(5.154)	(5.138)	(4.269)	(3.338)
Middle quintile	21.2***	-4.4	-11.2***	-0.3
	(5.008)	(5.408)	(4.101)	(3.750)
Second quintile	18.2***	7.2	-12.2***	-3.2
	(4.545)	(5.222)	(3.746)	(4.277)
Least deprived quintile	14.6***	7.9	-8.3**	7.8**
	(4.301)	(5.844)	(3.645)	(4.764)

Notes: Parents Demand for Childcare Survey 2001 and Childcare and Early Years Provision: Parents Survey 2007. Sampling weights applied. + p < .10, * p < .05, ** p < .01. Standard errors between parenthesis.

Table 9: Changes in hours in formal childcare among those participating, 2001-2007

	Hours in formal childcare per week					
	75th percentile			mean		
	2001	2007	Change	2001	2007	Change
Low educated mother	12.5	15	2.5**	8.8	13.3	4.5**
			(0.211)			(1.127)
Medium educated mother	12.5	16	3.5**	9.7	14.5	4.8**
			(0.197)			(0.981)
High educated mother	17.5	20	2.5**	12.8	16.4	3.6**
			(0.362)			(1.094)

Notes: Parents Demand for Childcare Survey 2001 and Childcare and Early Years Provision: Parents Survey 2007. Sampling weights applied. + p < .10, * p < .05, ** p < .01. Standard errors between parenthesis. In 2007 participation was collected in full hours only.

Table 10: Effect of childcare availability on maternal labor market behavior

	Participates in labour force	Employed	Self employed	Works part time	Works full time	Usual weekly hours
Free places (10ppt)	0.001	0.000	-0.002	-0.001	0.001	-0.015
	(0.044)	(0.042)	(0.030)	(0.045)	(0.033)	(1.427)
controls						
individual	Yes	Yes	Yes	Yes	Yes	Yes
LEA level	Yes	Yes	Yes	Yes	Yes	Yes
N	22,777	22,777	22,777	22,606	22,606	22,606

Notes: We thank Sarah Cattan, IFS, for producing this Table for us. Labour Force Survey, 2002-2007. The Table shows the effect of a 10 ppt increase in coverage of 3 year olds with funded places. Sample includes mothers observed between the beginning of the term after which their child turns 3 and the child's fourth birthday. Estimates include LEA, year and month fixed effects and LEA-specific time trends. Control variables includes 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 number of younger siblings, the total number of children aged 0-2, 3-4, 5-9, 10-15, and 16-19 in the household, and dummies for the child's month of birth. LEA-level economic controls include average unemployment rate, employment rate and hourly wage level in the LEA of residence and in the quarter preceeding the quarter of observation. Contains the same early years controls as Table 2. + p < .10, * p < .05, ** p < .01. Standard errors clustered at LEA level in parenthesis.

Appendix Table A: Summary statistics, Individual and LEA-level characteristics

	mean	std. deviation
Individual characteristics		
Female	0.49	
Eligible for free school meals	0.18	
No. of months older than August-born	5.48	3.48
English additional language	0.14	
White British	0.80	
Indian	0.02	
Chinese	0.00	
Black	0.05	
Pakistani/Bangladeshi	0.05	
Mixed ethnicity	0.04	
Other ethnicity	0.03	
IDACI deprivation score	0.23	0.19
LEA-level characteristics		
% 3-year olds with free childcare place/10	7.83	1.60
% qualified at NVQ4 level or higher (16-64)	25.19	6.58
% qualified at NVQ3 level or higher (16-64)	44.95	6.52
Employment rate (16-64)	73.70	6.00
Employment rate (16-64), women	67.98	6.45
Claimant rate (16-64)	2.42	1.25
Claimant rate (16-64), women	1.21	0.58
Mean gross hourly pay (2005 £)	12.03	1.92
Mean gross weekly pay (2005 £)	411.29	70.66
Number of Sure Start Centres, moving average	0.24	0.21
Other early years initiatives (2013 £), moving average	121.60	56.06
Extension funding pilot LEAs	0.04	

Notes: National Pupil Database, 2003-2010. Deprivation is controlled for in the estimates using deciles.