Starting school and leaving welfare: the impact of public education on lone parents' welfare receipt[1]

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July 2010

Childcare costs are often viewed as one of the biggest barriers to work, particularly among lone parents on low incomes. Children in England are typically eligible to start school – and thus access a number of hours of free public education – on 1 September after they turn four. This means that children born one day apart may start school up to one year apart. We exploit this discontinuity to investigate the impact of youngest child being eligible for full-time primary education (relative to part-time nursery education) on welfare receipt and employment patterns amongst lone parents receiving welfare. In contrast to previous studies, we are able to estimate the precise timing (relative to the date when full-time education begins) of any impact on labour supply, by using rich administrative data. Amongst those receiving welfare when their youngest child is aged approximately three and a half, we find a small but significant effect on both employment and welfare receipt (of around 2 percentage points, or 10-15 per cent), which peaks eight to nine months after the child becomes eligible (aged approximately 4 years and 9 months). This suggests that the expansion of public education programmes to younger disadvantaged children may only encourage a small number of low income lone parents to return to work.

JEL classifications: I21, J22

Key words: labour supply, school entry, regression discontinuity, lone parents, welfare receipt

[1] The authors gratefully acknowledge funding from the ESRC Centre for the Microeconomic Analysis of Public Policy at the Institute for Fiscal Studies. They would like to thank the Department for Work and Pensions for granting permission to use their data, and to Kris Chapman and Margaret Hersee who have managed the project over its lifetime. They would also like to thank Tom Crossley, Mike Daly, Lorraine Dearden, Jennifer Fleming and seminar participants at the Department for Work and Pensions and the Institute for Fiscal Studies for useful comments and advice. All errors remain the responsibility of the authors.

1 Introduction

The link between the availability or price of formal childcare and maternal employment is of considerable policy interest, but understanding it poses significant technical challenges. [2]

The policy interest arises because most OECD countries provide significant financial support for childcare (as well as regulating care providers to ensure minimum standards), particularly of pre-school children, through direct provision of group childcare, subsidies to private and not-for-profit providers, or cash payments or tax breaks to parents using childcare.[3] The policy goals vary across countries, but will usually be a combination of promoting children's development and allowing parents to engage in paid work. So policy-makers need to know not just how childcare can affect children's development but how the availability or price of formal childcare relates to parental employment.

But estimating the link between the availability or price of formal childcare and parental employment poses significant technical challenges. Brewer & Paull (2004), drawing on Blau (2003) and Blau & Currie (2004), argue that this is due to a combination of inadequate data (the quality of formal care is rarely observed in large-scale surveys, and the quality, cost and availability of informal care never observed), misspecification of parents' underlying choice set (many studies reviewed in Blau & Currie (2004) assume parents choose between freely-available informal care and formal care, whereas many childcare users use both formal and informal care, and some have no informal care options), and a lack of plausible instruments to overcome selection bias (wages are observed only for those who work, and childcare prices and quantities are observed only for those who choose to use formal childcare).

Despite these difficulties, many studies have sought to estimate the elasticity of maternal employment with respect to the price of formal childcare, mainly using regional variations in the price of childcare as an instrument.[4] Anderson & Levine (1999) report that "these studies do uniformly find a negative relationship between childcare costs and mothers' employment". But, reviewing much the same studies,

[2] By childcare, we mean what is referred to in the UK as "formal childcare", i.e. care provided by someone who is not a relative or family friend. This could be group care (or centre-based care), or care provided by a qualified carer in the carer's own home (or, in the UK, care provided by a qualified person in the child's own home).

[3] For example, the UK government provides financial support for childcare amongst working parents through the childcare element of its Working Tax Credit (see http://taxcredits.hmrc.gov.uk/Qualify/WhatAreTaxCredits.aspx), and also offers direct provision, particularly targeted at disadvantaged families, through Sure Start Children's Centres (http://www.dcsf.gov.uk/everychildmatters/earlyyears/surestart/whatsurestartdoes/). See Section 2.2 for more details.

[4] Note that most studies refer almost exclusively to the effect of childcare on maternal (rather than parental) employment.

Blau & Currie (2004) find that neither sample composition nor data sources can explain the considerable variation in the existing estimates, implying that "specification and estimation issues most likely play an important role in producing variation in the estimates". They conclude that "it is risky to generalize from only three studies, but the fact that the studies that accounted for unpaid [i.e. informal] child care in ways consistent with the existence of an informal care option [Ribar (1995), Blau & Hagy (1998), Tekin (2003)] produced small elasticities [of mothers' employment with respect to the price of childcare] suggests that the true elasticity may be small". There is also little consensus as to whether such elasticities are greater for lone mothers or married women.

Given these difficulties, economists have tried to use policy variation as a way of generating instruments for the price or availability of formal childcare. For example, Lefebvre & Merrigan (2008) and Baker et al (2008) both study an expansion of universal, highly-subsidized childcare for pre-school children that took place in Quebec in the 1990s, using other provinces in Canada as a comparison group. Using different data sources, both find a significant impact on maternal employment.

A related strand of literature has sought to estimate the impact of children beginning full- or part-time education on maternal labour supply. This is of interest in its own right (for example, in estimating the costs and benefits of publicly-funded education systems, but also in thinking about when to apply work-search requirements to lone parents receiving welfare, as we discuss in the conclusion), but such impacts have also been seen as relevant to the question of whether formal childcare for pre-school children affects parental labour supply. Brewer & Paull (2004) note that "the sudden provision of free childcare during school hours should encourage mothers to enter work or extend their working hours...yet reality may not be so simple: suitable childcare to cover the remaining hours may not always be available or may create too many complexities in differing and irregular arrangements. In addition, a child starting school brings a new involvement in school life for the parent as well as the child, potentially generating new responsibilities for the mother outside the formal labour market" (p10). Despite these competing effects, the appeal of examining the impact of children beginning full- or part-time education on parental labour supply is that school (or pre-school) admissions policies can provide convincing natural experiments that can tell us about causal impacts; furthermore, as entitlement to public education tends to be universal, there are also no selection problems of the kind that might arise by studying a means-tested programme (Gelbach, 2002).

While a number of studies make use of geographical or time variation in the introduction or expansion of pre-school or kindergarten programmes (see, for example, Schlosser, 2006, for Israel, and Cascio, 2009, for the US), the principle behind many of these studies is to make use of date-of-birth cut-offs that exist when schools or school districts determine when a child can or should begin full-time education. These administrative rules mean that a child's birthday (or month or quarter of birth) or age can be used as an instrument for attending full-time education (as in Gelbach, 2002, as we explain below), or that researchers can use a regression discontinuity design (as in Berlinski et al, 2009, Fitzpatrick, 2010, and this study) exploiting the fact that admissions rules mean that children born a day apart are eligible to start school up to a year apart.

As we discuss in more detail in Section 3, our study builds on and develops the findings in Gelbach (2002), Berlinski et al (2009) and Fitzpatrick (2010). We examine the impact of eligibility for publicly-funded, free, full-time primary education (relative to publicly funded, free, part-time nursery education) on the labour supply of lone parents in England whose youngest child turns four between 2000 and 2004[5]. We make use of birth date cut-offs in eligibility rules: in the areas of England we focus on, most children are eligible to start school in the academic year which starts after they turn four. So, for example, a child born on 31 August 2006 will be eligible to start school on 1 September 2010 (aged 4 years and 1 day), but a child born just one day later on 1 September 2006 will be eligible to start school a whole year later on 1 September 2011 (aged 5 years). We also examine the impact of eligibility for publicly-funded, free, part-time nursery education, to which children in England become entitled from the beginning of the term after they turn three.

We examine the impact of this eligibility on lone parents (predominantly mothers) receiving welfare benefits who have no younger children using administrative data on welfare receipt and employment spells. Clearly we cannot claim that our results are informative about the likely response of all mothers with three or four year olds in England. But lone mothers with no younger children are the group that Cascio (2009) and Fitzpatrick (2009) identify as being the most responsive to starting (pre-)kindergarten in the US. And there are several advantages of using administrative (rather than survey) data: accurate measures of the children's date of birth and home address (which we use to determine the relevant school admissions policy), continuous (not point-in-time) measures of outcomes (whether receiving welfare and/or in employment), and a 100% sample. Indeed, the key advantage of our study compared

[5] Full-time primary education is broadly equivalent to kindergarten, and the part-time nursery education is broadly equivalent to pre-K.

to previous ones is that we can estimate the precise timing (relative to the date on which children become eligible) of the impact of full-time primary education (relative to part-time nursery education), or the impact of part-time nursery education, on parental labour supply, including testing for anticipation effects, something that to our knowledge no other studies have been able to do.

The disadvantage of our study compared to previous ones is that we have no direct measure of school or nursery enrolment, which means that our estimates are of the intention-to-treat: we estimate the causal impact of being entitled to publicly-funded, free, full-time education (or publicly-funded, free, part-time nursery education), and not the causal impact of attending school or nursery, or being induced to attend school or nursery, as in Gelbach (2002) or Berlinski et al (2009). However, we argue that our estimates are close to the causal impact of attending full-time primary education (relative to part-time nursery education), or of attending part-time nursery education, because very few lone parents on welfare will be using private schools or private childcare facilities. Moreover, it is not at all common for parents in England to hold their children back a year, as can happen in other countries: the majority of children will therefore be in full- or part-time education of any kind if and only if they are entitled to publicly-funded, free, full-time primary education or free, part-time nursery education.

Using a regression discontinuity approach, we find that, amongst those receiving welfare when their youngest child is aged approximately three and a half, eligibility for full-time primary education (relative to part-time nursery education) increases the proportion of lone parents off welfare and in employment by a small but significant margin that peaks (at around 2 percentage points, or 10-15 per cent) eight to nine months after the child becomes eligible (aged approximately 4 years and 9 months). Moreover, we find that these effects do not start to emerge until some four to six months after eligibility, which suggests that lone parents may not start looking for work until their child enters school. This indicates that some previous estimates (notably those of Gelbach, 2002, and Fitzpatrick, 2010) may under-estimate the effect of (pre-)kindergarten entry on mothers' labour supply, as they are only able to consider outcomes seven and five months respectively after the children have started school.

Amongst those receiving welfare when their youngest child is aged approximately two and a half, however, eligibility for part-time nursery education appears to make little difference to the proportion of lone parents off welfare or in employment. This may be because the entitlement offers only a small (2½) number of hours per day, with few jobs likely to fit into these hours alone, or because take-up is

low amongst those who are eligible, or because of the relatively short horizon over which to observe any potential impacts.

We make use of a number of placebo tests to verify our results. In particular, we compare outcomes amongst parents of children who are either too young or too old to be affected by the school or nursery entry discontinuity (including those turning two, six and ten). We also compare outcomes amongst parents of children turning four who are born either side of a discontinuity which has no bearing on school entry. As hoped, we find virtually no difference in welfare receipt or employment outcomes for any of these groups, which provides reassurance that our main finding – of a small but significant effect of entitlement to full-time primary education (relative to part-time nursery education) on welfare receipt and employment outcomes amongst low income lone parents – is valid.

This paper now proceeds as follows. Section 2 provides background information about the provision of childcare and public education in England, as well as some information about employment rates and childcare use amongst lone parents. Section 3 outlines our identification strategy and modelling approach, and describes the datasets that we use and how we select our sample. Section 4 describes our results and compares them to those from previous studies. Section 5 concludes.

2 Institutional background

2.1 Provision of full-time public education in England

The academic year in England runs from 1 September to 31 August, and is split into three terms (September to December; January to April; May to August). Parents are statutorily obliged to send their child to school (or provide alternative appropriate means of education such as home schooling) from the school term that begins after the child's fifth birthday, earlier than in most OECD countries. This is known as the statutory school age. However, the age from which children are entitled to attend publicly-provided, free-to-access primary education is not determined by the national government; instead, it is up to each local authority[6] – and in some cases (as we discuss below), individual schools – to decide the age at which to admit pupils.

For the overwhelming majority of children, an entitlement to attend publicly-provided, free-to-access primary education is in place before the statutory school age. While parents are free not to take

[6] There are around 150 local authorities in England.

advantage of this entitlement while their children are below the statutory school age, anecdotal evidence suggests that it is difficult for parents to request alternative entry dates, and parents who do not use their entitlement at the earliest possible date may face a more restricted choice of schools, as there are legally-binding limits on class sizes for children in primary schools. Moreover, in contrast to the US – where it is common practice for parents of children who are young in their school year to hold them back (known as academic "red-shirting"[7]) – parents in England appear to regard it as advantageous for their child to start school as early as possible.

The most common admissions policy adopted by local authorities in England (covering around 50% of school entrants) entitles all children to start school in the September after they turn four (we refer to this as Policy 1). So, for example, all children born between 1 September 2005 and 31 August 2006 will be eligible to start school on 1 September 2010. It is clear that such a policy gives rise to considerable variation in the age at which children start school: two children born only one day apart – on 31 August 2006 and 1 September 2006 – will be eligible start school one year apart (the former on 1 September 2010, the latter on 1 September 2011).

The second most common admissions policy (covering around 15% of school entrants) entitles children born between 1 September and 28/29 February to start school in the September after they turn four, and children born between 1 March and 31 August to start school in the January after they turn four (we refer to this as Policy 2). In this case, a child born on 31 August 2006 would be eligible to start school in January 2011, while a child born just one day later on 1 September 2006 would be eligible to start school in September 2011, a gap of eight months. A further discontinuity also exists under this policy: a child born on 28 February 2006 would be eligible to start school in September 2010, while a child born just one day later on 1 March 2006 would be eligible to start school in January 2011, a gap of four months.

Of course, not all children in England will attend public (state) schools (although the vast majority of children in our sample are likely to do so). Moreover, some state schools (non-community schools,

[7] http://nces.ed.gov/pubs2006/2006064.pdf suggests that around 6% of children start kindergarten a year late. These children are more likely to be male, white and from families with higher educational qualifications than those who start kindergarten on time.

catering for around 30% of primary school children in England[8]) operate their own admissions policy (independent of the local authority policy). We discuss below what this means for our empirical work.

2.2 Provision of part-time nursery education in England

Local authorities in England are obliged to offer free-to-access nursery education for all three and four year olds (usually in 2.5 hour sessions, five days a week, over the course of the school year). This entitlement has existed for all four year-olds since 2001, and was phased in for three year olds over the period covered by our study, with 63 of the most deprived local authorities (covering approximately 86% of our sample) given funding to provide free part-time nursery places for three year olds from 2000.[9] Parents could either make use of publicly-provided, free-to-access options, which were generally over-subscribed, or, if they did not want to use (or could not access) these places, they were entitled to a voucher of a (supposedly) equivalent value which they could use at centre-based childcare facilities run by private sector or not-for-profit organisations. This scheme is not limited to working families and is not subject to an income test; its primary aim is to promote child development and ensure that all children are ready to start formal schooling at the age of five, rather than to facilitate work amongst parents. Almost all four year old children either benefitted from a voucher, or were enrolled in a publicly-funded and free-to-access nursery or reception class during the period covered by our study, with the equivalent proportion of three year-olds rising from 59% to 93% between 2001 and 2004.[10]

2.3 Provision of and government support for childcare in England

To identify the relevant counter-factual for our analysis, it is important to understand the policy regime faced by those pre-school children who are not entitled to full-time primary or part-time nursery education. During the period covered by our data, there were two main options for formal childcare for

[8] Source: authors' calculations based on school census data for all children attending state primary schools in England. Such schools exist within each local authority.

[9] These local authorities are: Durham, Gateshead, Middlesbrough, Newcastle Upon Tyne, North Tyneside, South Tyneside, Stockton on Tees, Sunderland, Blackburn with Darwen, Blackpool, Bolton, Halton, Knowsley, Liverpool. Manchester, Oldham, Rochdale, Salford, Sefton, St Helens, Tameside, Wirral, Barnsley, Bradford, Doncaster, Kingston Upon Hull, Kirklees, Leeds, North East Lincolnshire, Rotherham, Sheffield, Wakefield, Leicester, Nottingham, Birmingham, Coventry, Stoke on Trent, Telford and Wrekin, Walsall, Wolverhampton, Camden, Hackney, Hammersmith and Fulham, Haringey, Islington, Lambeth, Lewisham, Newham, Southwark, Tower Hamlets, Wandsworth, Westminster, Barking and Dagenham, Brent, Ealing, Greenwich, Hounslow, Waltham Forest, Brighton and Hove, Cornwall and Isles of Scilly, and Plymouth. Information derived from: http://www.dcsf.gov.uk/rsgateway/DB/SBU/b000430/bulletintext2003final.pdf.

[10] Source: http://www.dcsf.gov.uk/rsgateway/DB/SFR/s000604/SFR43-2005.pdf.

pre-school children: 1) centre-based care, provided by private sector companies, not-for-profit organisations, and (rarely) directly by local authorities, and; 2) self-employed childminders, who provide non-relative care outside of the family home (almost always in the childminder's home), and who must be registered with a statutory authority to care for children under the age of 8. Many families, of course, use informal care from relatives and friends as well as or instead of formal childcare.

Aside from the provision of free part-time nursery education places (discussed above), there are two other programmes for supporting formal childcare in England.[11] First, working families may be able to claim the childcare tax credit (from 2003, the childcare element of the working tax credit) which rebated up to 70% (from 2005, 80%) of spending on formal, registered, childcare (subject to a generous cap) for working families who passed an income test.[12] Second, from April 2005, employers could pay their employees childcare vouchers of up to £50 a week free of income tax and payroll taxes: these could only be used to pay registered, formal, childcarers; care by relatives or friends was not eligible.

2.4 Employment rates and childcare use amongst lone parents in England

To put our sample into context, 27 per cent of families whose youngest child turned four between April 2001 and March 2005 were headed by a lone parent. Amongst lone parents with children of this age, 43 per cent were in work and 54 per cent were receiving welfare benefits.[13,14] This employment rate is low by international standards and may be partly explained by the absence of work requirements for the vast majority of lone parents on welfare.[15]

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[11] The first two programmes are UK-wide; the third exists only in England, but there are similar programmes in Wales, Scotland and Northern Ireland as well.

[12] Entitlements to these childcare subsidies were added to a family's entitlement to the working families' tax credit (from 2003, the working tax credit), and then withdrawn in the same way as those credits. This means that characterising how much a particular family could receive in support for childcare depends upon family pre-tax income, the number of children and the amount spent on childcare: for further details, see http://www.ifs.org.uk/budgets/gb2005/05chap9.pdf.

[13] Based on our own analysis of the Family Resources Survey.

[14] By "welfare benefits" we mean either Income Support, Jobseeker's Allowance or Incapacity Benefit. Incapacity Benefit is designed for claimants who are too ill to work. The main difference between Income Support and Jobseeker's Allowance is that recipients of Income Support were not required to undertake any work-related activities while they had children aged 16 or under, but recipients of Jobseekers Allowance – the key benefit for the unemployed – were. Given that lone parents could choose which to claim, and would usually be entitled to identical amounts of each, only a tiny fraction of those on welfare were receiving Jobseeker's Allowance.

[15] This began to change in 2008, and, by 2011, lone parents whose children are all aged 7 or over will be subject to similar work-search requirements as other unemployed or disabled people. The government has also announced that lone parents will have to take steps to prepare for work when their youngest children are aged 3; at the time

Figure 1 shows how the employment rate amongst lone parents in the UK varies by age of youngest child. There is a substantial difference in the employment rate between those whose youngest child is 2 and those whose youngest child is 4, but little discernible change around the age at which children are entitled to start school.

Unsurprisingly, what does change dramatically as children age is the type of childcare used by working families: Figure 2 shows that there is a 30 percentage point difference in the use of childcare between children aged 2 with working lone parents and children aged 6 with working lone parents, almost all of which comes from a fall in the use of formal childcare.

3 Methods and data

3.1 Relationship to previous studies

Our study builds on and develops the findings in Gelbach (2002), Berlinski et al (2009) and Fitzpatrick (2010), so we start by briefly outlining the methods and findings of those studies.

Gelbach (2002) sought to estimate the impact of public school enrolment (in kindergarten) on maternal labour supply, using US Census data from 1980. A straight-forward ordinary least squares (OLS) regression of measures of labour supply on enrolment in school would give biased results, as some parents used private schools, and others delayed their child's entry to public schools. Gelbach argued that quarter-of-birth acts as a valid instrument for whether a five-year-old child is enrolled in public school, and he then uses it in regressions of various measures of labour supply on enrolment in public school, finding that public school enrolment increases labour supply for married mothers, and lone mothers with no younger children. But, as Fitzpatrick (2010) argues, the issue with Gelbach's results is that maternal labour supply is measured on the same calendar date for all mothers, and so the children are all of different ages: this means that quarter of birth will be an invalid instrument if a mother's labour supply is related to her children's age independently of the impact of being enrolled in public school.

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of writing, this was due to begin in certain parts of the country in late 2010 (the co-called 'Progression to Work pathfinders'). Department for Work and Pensions, 'Realising potential: developing personalised conditionality and support: a discussion paper on next steps in implementing the Gregg Review', 2009, http://www.dwp.gov.uk/docs/gregg-review-discussion-paper-jan09.pdf.

Fitzpatrick (2010), in an approach very similar to our own, studies a related situation by exploiting date-of-birth cut-offs in eligibility for publicly-subsidised prekindergarten (pre-K). She argues that administrative rules create a sharp regression discontinuity, with children born on or before 1 September being different from those born on or after 2 September in that only the former are eligible for pre-K.[16] Using information on the exact date-of-birth of children of mothers in the US Census, she is able to use the regression discontinuity to estimate the causal impact of universal pre-K on pre-school enrolment and maternal labour supply. The approach adopted by Berlinski et al (2009) is very similar, exploiting the discontinuity caused by administrative cut-offs as an instrument in a regression relating pre-school attendance to maternal labour supply in Argentina.

The main difference between Gelbach and the subsequent two studies is that, with access to the children's exact date-of-birth, Fitzpatrick (2010) and Berlinski et al (2009) can relax the assumption in Gelbach (2002) that a child's age is not directly related to maternal labour supply.[17] A disadvantage common to Gelbach (2002) and Fitzpatrick (2010) is that they record labour supply outcomes at only two particular points in time (and on the same calendar date for each mother): in a reference week before the Census interview (April 1980 in Gelbach, and February 2000 in Fitzpatrick), and in the calendar year before the interview (1979 in Gelbach, and 1999 in Fitzpatrick). The latter outcomes cover a period where the children are in kindergarten/pre-K for only around one third of the time (4 out of 12 months, assuming a 1 September start date), meaning any estimated impact understates the true impact of full participation in kindergarten or pre-K. The former outcomes are measured approximately 7 months after the child started in kindergarten (or approximately 5 months after starting pre-K). This is not invalid, but it gives only a glimpse of the overall impact on maternal labour supply during the entire year in which some children are in kindergarten/pre-K and some otherwise-identical children are not, and gives no indication as to whether any impacts occur immediately after school entry or with a lag, and whether they persist.

[16] She also uses states which did not offer universal pre-K as additional "controls", so that the impact of pre-K is estimated as the difference in the regression discontinuity estimates of an effect in the states running the program, and those which were not.

[17] Fitzpatrick (2010) reports that a replication of Gelbach's study using more recent US Census data that also uses precise date-of-birth of the children – and which can therefore relax the assumption that a child's age is not directly related to maternal labour supply – finds that public school enrolment affects labour supply only among lone mothers with no younger children: see Fitzpatrick (2009).

Our aim is to build on the findings of these previous studies by exploiting the discontinuities in eligibility to start full-time primary education described above in order to identify the causal impact of eligibility for full-time primary education (relative to eligibility for part-time nursery education) on the labour supply of low income lone parents. One advantage of our study compared to previous ones is that we are able to estimate the precise timing of any impact of eligibility for full-time primary education by comparing outcomes up to a year after children first become entitled. However, we are unable to observe school enrolment in our data. This means that we are in essence identifying the intention-to-treat effect of living in an area in which the local authority policy allows children to start school at a certain age. Furthermore, it should be remembered that the relevant counterfactual for our population of interest is likely to be 2.5 hours of free nursery education per day throughout the majority of this period (as low-income lone parents are unlikely to be purchasing additional hours of formal childcare). Thus, our estimates should be regarded as the causal impact of living in an area in which the local authority policy offers children approximately 4 additional hours of free childcare per day during term time.

3.2 The regression discontinuity model

We adopt a regression discontinuity (RD) design (Imbens & Lemieux, 2008). In a model in which there is a sharp cut-off based on age, the principle is that, if the underlying relationship between the outcome and age is smooth (continuous), then individuals observed just above and just below the cut-off should have very similar observed and unobserved characteristics; within this group, then, the allocation of treatment status is almost as good as random (Hahn et al, 2001; Lee, 2008). Of course, age is not strictly a continuous variable, and so identification relies on a parametric specification of the underlying relationship between age and the outcome (Card & Lee, 2008).

Administrative date-of-birth cut-offs should lead to sharp discontinuities (Hahn et al, 2001), but, in this case, the discontinuity is fuzzy, mostly because of the 30 per cent of children who attend state schools that are responsible for their own admissions policies.[18] However, the discontinuity is not that fuzzy: Figures 3a to 3c, based on school census data of the population of students who joined public (state) schools in England between academic years 2001-02 and 2004-05, show that a very high proportion of all children (88 per cent, on average) started school at the expected time, presumably because many

[18] Our data do not tell us which school children attended, so there is no way of dropping children who attend such schools from our sample.

non-community schools find it expedient to synchronise their admissions policies with that of the relevant local authority. Expected start date is also a strong and significant predictor of actual start date in a simple regression model.[19]

To justify the use of the RD approach, it is customary to present a number of graphical analyses. First, as parents have some influence over the date of birth of their children, and given that it is widely known that 1 September represents a discontinuity in schools admissions policies, the distribution of births around the cut-off may not be continuous (McCrary, 2008). Figure 4 therefore shows the number of children in our sample born on each day in a 120 day window around our main cut-off. [McCrary (2008) suggest how to test formerly for discontinuities in the density of the "running variable" (date-of-birth), but we simply present graphical evidence on whether the density appears smooth around the cut-off.] This figure suggests that while there are slightly fewer births just before than just after the 1 September cut-off – which would be consistent with parents timing conception or birth so that their children start school later (as the oldest in their academic year)[20] – this difference is small relative to the variation in birth rates in the sample as a whole.[21]

Second, if some families are more likely to time conception or birth than others, then there may be differences in observed or unobserved characteristics amongst families with children born either side of the cut-off. Appendix A illustrates graphically how some of the key observable characteristics vary by date of birth, while Table 1 presents differences in the mean values of these characteristics between families with children born up to 60 days before and 60 days after the 1 September cut-off. None of the Figures exhibit any obvious discontinuity on 1 September[22], and Table 1 suggests that there are no significant differences in average characteristics, except in terms of the age of the parent (presumably reflecting that children born before 1 September are on average two months older than children born after 1 September when they are sampled, on 31 March)[23] and the number of children. We account for

[19] Unfortunately, we cannot use the data from the school census to estimate a two-sample 2SLS estimator as the school census does not identify which children are in lone parent families.
[20] Such children would start school later, but would be the oldest in their academic year. Gans and Leigh (2009) show striking evidence that parents have some ability to time births even given the date of conception.
[21] Section 4.5 presents a variant of our analysis which omits children born up to a week either side of the cut-off.
[22] If we estimate equations analogous to (3) in Section 3.3 using these characteristics as outcome variables (and no other regressors), then none of the estimated impacts are significantly different from zero.[0]
[23] Omitting controls for "age of parent" had no discernible impact on the estimates.

these small differences in observable characteristics by including individual and local area controls in our regressions.[24]

3.3 Econometric specification

To operationalise this RD approach, ideally, we would estimate a model relating parents' labour supply to child's enrolment in school:

$$Y_{ijcm} = \alpha S_{ijcm} + \beta_1 X_{1ijc} + \beta_2 X_{2ijcm} + \mu_j + \delta_c + \varepsilon_{ijcm} \quad [1]$$

where Yijcm is some labour supply outcome for parent i in local authority j in cohort c in month m; Sijcm is an indicator for whether the parents' youngest child is in school in month m; X1ijc is a vector of individual characteristics that do not vary over time, such as gender, ethnicity and employment and welfare history at the point of selection into our sample; X2ijcm is a vector of characteristics that are allowed to vary over time, such as the local unemployment rate; μj is a set of local authority dummies; δc is a set of cohort dummies; and εijcm is an error term.

However, Sijcm may be endogenous (if, for example, parents choose to hold their children back, or choose to access the private sector). The challenge we face, as did Gelbach (2002), Fitzpatrick (2010) and Berlinski et al (2009), is thus to find an instrument which induces exogenous variation in Sijcm but has no direct effect on Yijcm. As outlined above, our approach is to make use of several birth-date discontinuities induced by the English education system, whereby children of approximately the same age start school several (up to 12) months apart. The main discontinuity we exploit compares children born either side of the academic year cut-off of 1 September. Let Aijc define a child's day of birth relative to 1 September, such that it equals 0 on 1 September, 1 on 31 August, -1 on 2 September, and so on. A potential instrument for school enrolment is the indicator variable:

$$Z_{ijc} = \mathbf{1}\{A_{ijc} > 0\} \quad [2]$$

[24] The full set of controls in our models includes: gender, ethnicity, age, number of children, age of youngest child relative to cut-off, employment and welfare histories, an indicator for the amount of Income Support received (as a proxy for income), past participation in a particular voluntary welfare-to-work programme (the New Deal for Lone Parents), an indicator of disability, the proportion of formal childcare places available in the lone parents' local area, their local area deprivation score, their local unemployment rate, the proportion of lone parents in their local area who are professionals (as a proxy for their own socio-economic status), and the proportion of workless lone parents in their area with particular education levels (as a proxy for their own education level). We also include a set of local authority dummies and a set of cohort dummies.

where Zijc = 1 if the child is born on or before 31 August and 0 otherwise.

Berlinski et al (2009) use such an instrument to estimate a two-stage least squares estimate of α. But Sijcm is not observed in our data, and so we estimate a reduced-form version of [1]:

$$Y_{ijcm} = \alpha_0 Z_{ijc} + \alpha_1 A_{ijc} + \alpha_2 A_{ijc} Z_{ijc} + \beta_1 X_{1ijc} + \beta_2 X_{2ijcm} + \mu_j + \delta_c + \varepsilon_{ijcm} \quad [3]$$

This estimates α0, the impact of eligibility for full-time primary education (relative to part-time nursery education) on parental labour supply.[25] Following Berlinksi et al (2009) and Fitzpatrick (2010), we control linearly for child's age in days, allowing it to have differential effects either side of the cut-off. Following Card & Lee (2008), we cluster standard errors by day of birth (as is also done by Fitzpatrick (2010) and Berlinski et al (2009)).

3.4 Data

We use a 100% sample of administrative records, held by the UK Department for Work and Pensions, which record information about welfare receipt and time spent in paid employment for all individuals who received a welfare payment or participated in a welfare-to-work programme in Great Britain between June 1999 and March 2007.[26,27] This data allows us to construct detailed employment and

[25] Note that we run separate regressions for each monthly outcome. Our main findings still hold up if we use seemingly unrelated regressions to take account of the fact that the errors may be correlated over time. We have also experimented with an individual random effects model, although this was less successful (as we have few time varying characteristics), and in any case we might be concerned about the correlation between the individual effects and our characteristics of interest.

[26] Technically, the data records spells where the individual was potentially liable to pay income tax, either through their earnings or receipt of a taxable state benefit, so may miss periods of part-time, low paid work (for example, an individual working 16 hours per week at the minimum wage in the UK would not be liable for income tax). This would suggest that we might underestimate the proportion of individuals in employment using this data. However, there is actually strong evidence that the data *overestimates* the number of people in employment, due to inaccuracies in recording employment start and end dates. If the tax year during which the job started (ended) is known, but the day is not, then the start (end) dates are coded as those of the start (end) of the tax year, 6 (5) April; if the end date is entirely unknown, then the job may be regarded as ongoing. Both issues lead to a significant overstatement of the number of individuals in employment. While we have cleaned the data to the best of our ability (for example, by using known welfare claim start dates to infer the end date of an apparently ongoing job), it is not possible to entirely correct for this issue. As we are primarily concerned with *differences* in employment rates, however, this type of measurement error will matter only to the extent that it varies systematically between parents of children born in different months. A priori, there is no reason to expect that it should. See Brewer et al (2009) for further discussion, including details of the data cleaning process and an example of the use of this data.

[27] The data is not publicly available, and this project was done under a contract with the data owners, the Department for Work and Pensions.

welfare histories, as well as monthly outcomes. Being administrative data – collected for the purposes of administering welfare payments or paying income tax – it contains relatively limited information on personal characteristics, but does record information such as age, gender and ethnicity.

We supplement this data with a more detailed record of the personal circumstances of individuals who claimed a particular means-tested welfare payment – Income Support – over the same period. Income Support is available to individuals who are working no more than 15 hours a week, have sufficiently low levels of financial capital, and a sufficiently low weekly income.[28] Moreover, the level of Income Support available to a particular individual depends on their personal circumstances – including whether or not they have a partner, how many children they have and the age of their youngest child – with all changes in circumstances recorded in this file. Importantly, this means that we have access to full date of birth of youngest child for all lone parents claiming Income Support.[29]

This file also provides us with the home postcode of the claimant over time. We use home postcode for two purposes: firstly, to map in very local area information about the number of formal childcare places on offer, and the local unemployment rate, both of which we might expect to affect the likelihood of a lone parent finding a job and leaving welfare.[30] Secondly, and more importantly, we use home postcode to map in local authority admissions policy information. This data was collected retrospectively by the authors for a previous study, and contains details of school admissions policies in England from 1989 to 2008: see Crawford, Dearden & Meghir (2010) for more details and another use of this data.

As we only observe information about admissions policies in England, we restrict our attention to welfare recipients who are resident in England. We further restrict ourselves to claimants of Income Support as these are the only individuals for whom we observe date of birth of youngest child, and to lone parents, as past studies have suggested that they are more responsive than mothers with partners.

[28] Many of the income-related programmes in the UK welfare system use hours rules to enforce a dichotomy between welfare benefits which are aimed at families where no adult is in work, and tax credits which are aimed at families where at least one adult is in paid work. Non-working lone parents would be entitled to other cash payments, principally child benefit, (from 2003) child tax credit and support for rental costs and local taxes (through housing benefit and council tax benefit). Some examples of the financial incentives to work faced by lone parents can be seen in Brewer, Saez & Shephard (2010) or http://www.jrf.org.uk/sites/files/jrf/2110-lone-parents-minijobs.pdf.

[29] We do not observe the date of birth of older children.

[30] Of course, there is no a priori reason to suppose that lone parents of children born in different months systematically locate in areas with different characteristics, but we include such information to improve the precision of our estimates.

To ensure that we have a reasonably lengthy period of employment and welfare history prior to sample selection, as well as a relatively long period over which to assess outcomes, we select individuals whose youngest child turns four between 30 November 2000 and 29 November 2004, and who were on Income Support on 1 March in the year in which their youngest child turned four: this allows us to observe, for all cohorts, 18 months of employment and welfare history, and three years of monthly employment and welfare outcomes. [31,32] This gives us a total of 214,305 individuals.

As a result of our regression discontinuity approach to identification, we restrict attention to individuals born immediately either side of each of our cut-offs of interest (primarily 1 September and 1 March). Table 2 provides some indication of the sample sizes we use when analysing particular admissions policies, using individuals born up to 14, 30, 60 (our main specification) or 90 days either side of each cut-off.

4 Results

4.1 Policy 1 areas

Our identification of the effect of eligibility for full-time primary education (relative to part-time nursery education) on parents' labour supply in Policy 1 areas arises from a comparison of children born either side of a 1 September cut-off, where the older children (those born in July and August in our main specification) are eligible to start school 12 months earlier than the younger children (those born in September and October in our main specification).

Figure 5 plots the proportion of lone parents off welfare, monthly, from 6 months before the older children are eligible to start school to 18 months after the younger children are eligible to start school, separately for parents of children born either side of the 1 September cut-off. If there were no effect of eligibility for full-time primary education (relative to part-time nursery education) on off-flow rates from

[31] For individuals born close to our main cut-off (1 September), this means we sample lone parents who are on welfare when their youngest child is approximately three years and six months old.

[32] When considering the outcomes of parents of children turning two, six and ten as part of our placebo tests, we choose our sample in exactly the same way: for example, we select individuals whose youngest child turns two between 30 November 2000 and 29 November 2004 and who were on Income support on 1 March of the year in which their youngest child turned two. When analysing the effect of eligibility for part-time nursery education on lone parents' labour supply, we select individuals whose youngest child turns three between 1 November 2000 and 31 October 2004 and who were on Income Support on 1 March of the year in which their youngest child turned three. Table 3 provides sample sizes for each of these groups.

welfare (and no difference in characteristics between the two groups), then we would expect minimal differences between the lines. On the other hand, if eligibility for full-time school did have an effect (and there were no differences in other characteristics), then we would expect the lines to diverge during the period in which the older children are eligible for full-time education and the younger ones are not, before coming together again when the younger children become eligible for full-time education. (If there were anticipation effects, then we might expect the lines to diverge before the older become eligible.) Figure 5 suggests that the welfare receipt rates amongst lone parents of older and younger children are almost identical until about three months after the older children become eligible to start school, at which point there is a (small) divergence, which begins to tail off around six months after the younger children become eligible. This suggests that there are no (or at least no differential) anticipation effects, and would be consistent with, for example, parents not starting to look for work until their youngest child starts school, giving rise to a lag before the rates of welfare receipt diverge.[33]

Table 4 then shows estimates of $\alpha 0$ from equation (3) based on the same sample, and Figure 6 plots these estimates graphically along with the 95% confidence interval.[34] These results suggest that eligibility for full-time primary education (relative to part-time nursery education) has a small but significant effect on lone parents' welfare receipt from around six months after their youngest child becomes eligible, peaking at 1.9 percentage points (around 10 per cent) nine months after the child first becomes eligible, before falling away to approximately zero after 14 months: this is unsurprising, as all

[33] It would also be consistent with a scenario in which (some) children attend school part-time (rather than full-time) for the first few months while they "settle in". Authors' calculations based on the Millennium Cohort Study (which follows a cohort of children expected to start school in 2005-06) suggest that around 40% of children in England start school part-time rather than full-time, although it is not clear for how long they do so, or the extent to which this varies by area.

[34] We do not show the other coefficients, mostly because there are dozens of regressions each with dozens of regressors. Choosing the outcome with the largest estimated impact (nine months after eligibility for welfare receipt), the other coefficients suggest that lone parents with more than one child, those of Chinese or mixed ethnic origin (relative to whites), those who have spent a smaller proportion of the past three years in work and off benefit, those who receive relatively more Income Support, those who have never participated in the New Deal for Lone Parents (NDLP), those who are also claiming Incapacity Benefit, those who have been disabled in the past three years, and those who live in areas with high unemployment rates are less likely to be off benefit; lone parents of Black ethnic origin are more likely to be off benefit than whites. A full set of coefficient estimates is available from the authors on request.

children are now eligible for full-time education (and suggests that there are no experience effects, at least in terms of welfare receipt).[35]

Figures 7 and 8 and Table 5 repeat this analysis for employment outcomes.[36] Figure 7 highlights the likely over-estimation of the proportion of lone parents in employment discussed in Section 3: around 13 per cent of lone parents were apparently in employment when sampled, when we know that they were also claiming Income Support, and therefore highly likely to be out of work.[37] It also shows that the effect of eligibility for full-time primary education (relative to part-time nursery education) on employment outcomes appears to kick in just after the lone parents' youngest child starts school. Moreover, this effect is larger – and lasts for longer – than the effect on welfare receipt discussed above. Table 5 and Figure 8 show that the effect of eligibility for full-time primary education (relative to part-time nursery education) on the likelihood of a lone parent being in paid employment peaks at 2.4 percentage points (around 13 per cent) eight months after eligibility, before falling away to approximately zero a further five months later (as the younger children also become eligible). This might suggest that Gelbach (2002) and Fitzpatrick (2010) may have slightly under-estimated the effect of attending (pre-)kindergarten on mothers' labour supply by observing outcomes relatively soon (five and seven months, respectively) after school entry.

4.2 Policy 2 areas

There are two distinct cut-offs in Policy 2 areas. First, as in Policy 1 areas, there is a discontinuity around 1 September. In Policy 2 areas, however, the older children are eligible to start school eight months earlier than the younger children (in January rather than September of the previous year). Second, there is a discontinuity around 1 March, where the older children (those born in January and February in our

[35] Of course, after month 12, our estimates cannot be interpreted as the impact of eligibility for full-time education as all children are now eligible: instead, they estimate the difference between being entitled for *M*+12 and *M* months).

[36] Choosing the outcome with the largest estimated impact (eight months after eligibility for employment), the other coefficients suggest that older, male lone parents, those with four or more children, those of Asian or other ethnic origin (relative to whites), those who have spent a smaller proportion of the past three years in work, those who receive relatively more Income Support, those who have never been on NDLP, those who have been disabled in the past three years, and those who live in areas with a high claimant count are less likely to be in employment; lone parents of black ethnic origin are more likely to be in employment than whites. A full set of coefficient estimates is available from the authors on request.

[37] While it is possible for lone parents to be in work and still entitled to Income Support, after a small earnings disregard (£20 a week), there is a 100% withdrawal rate. Jobs paying less than £20 a week are highly unlikely to be recorded in the WPLS as they are far below the income tax personal allowance,

main specification) are eligible to start school four months earlier than the younger children (those born in March and April in our main specification) – in September rather than the following January. We explore the effects of eligibility for full-time education relative to part-time nursery education on parental labour supply using each of these discontinuities in turn.

Tables 6 and 7 present estimates of the effect of youngest child being born just before rather than just after the 1 September and 1 March cut-offs in Policy 2 areas respectively on the proportion of lone parents off welfare and in employment. For these groups, we find no consistent or significant effects of eligibility for full-time primary education (relative to part-time nursery education) on either welfare receipt or employment rates. While it is disappointing that these results do not clearly support our findings from the Policy 1 areas, both the shorter period over which to observe any effects, and the considerably smaller sample sizes, may provide at least a partial explanation.

4.3 Effect of eligibility for part-time nursery education

Children in England are eligible to take up a free part-time nursery place from the beginning of the term after they turn three. This means that we can estimate the effect of eligibility for part-time nursery education on the labour supply of low income lone parents by comparing children born either side of a 1 September, 1 January or 1 April discontinuity. (We make these comparisons separately, as they do not all give rise to the same treatment length: for example, those born just before 1 September will be eligible for a free part-time nursery place four months before those born just after 1 September, while those born just before 1 January will be eligible for a free part-time nursery place three months before those born just after 1 January.)

Tables B1 to B6 in Appendix B present estimates of the effect on lone parents' employment and welfare receipt of their youngest child being born either side of each of these three discontinuities in each of Policy 1 and 2 areas. These results provide scant evidence of an effect of eligibility for part-time nursery education on lone parents' labour supply: there appears to be a small and insignificant effect on welfare receipt for those whose youngest child is born either side of the 1 September discontinuity in Policy 1 areas, but very little evidence elsewhere. This may be because the offer is only for a small number of hours per day (few jobs are likely to fit into these hours alone), or because take-up is low amongst those who are eligible, or because of the relatively short horizon over which to observe any potential impacts.

4.4 Subgroup analysis

In this section, we assess whether the effects of youngest child's eligibility for full-time primary education (relative to part-time nursery education) on the labour supply of low income lone parents varies according to the characteristics of the lone parent. In particular, we consider whether it differs by previous participation in a particular voluntary welfare-to-work programme, the New Deal for Lone Parents (NDLP). Results for the 1 September discontinuity in Policy 1 areas can be found in Tables C1 and C2 of Appendix C; results for Policy 2 areas are available from the authors on request.

These results suggest that lone parents who have previously been on NDLP are more responsive to the childcare subsidy provided by entitlement to full-time primary education (relative to part-time nursery education) than lone parents who have never previously been on NDLP. For example, nine months after eligibility begins, lone parents whose youngest child is born immediately before the 1 September discontinuity in Policy 1 areas are around 4.3 percentage points more likely to be off welfare than lone parents whose youngest child is born immediately after the 1 September discontinuity if they have ever been on NDLP, compared to only 1.6 percentage points if they have not. Similarly, eight months after eligibility, the effect on employment outcomes amongst lone parents who have ever been on NDLP is 5.7 percentage points, compared to 2.0 percentage points amongst those who have not. In both cases, the estimates are significantly different from each other.

Brewer et al (2009) similarly found that the impact of a wage supplement was greater for lone parents who had previously participated in NDLP than those who had not. They suggested that this greater response could be because lone parents on NDLP were more likely to hear about the wage supplement, or that, since NDLP is a voluntary programme, those who sign up are lone parents with a greater underlying propensity to work and a greater responsiveness to financial incentives to work. In this case, though, it is not very likely that a lone parent could be unaware that their child was entitled to full-time education, and so the higher impact amongst lone parents participating in NDLP seems likely to be a selection effect.

4.5 Specification checks

We carry out specification checks on our choice of sample window, our choice of age specification and our decision to include children born up to a week either side of the discontinuity in our analysis. We discuss the results of each of these specification tests in turn.

To ensure that our choice of sample window does not affect our results, we have run the same analysis on welfare receipt and employment outcomes using children born up to 14, 30 and 90 days either side of the discontinuity (results for the 1 September discontinuity in Policy 1 areas are shown in Tables D1 to D3 of Appendix D; results for Policy 2 areas are available from the authors on request). These figures suggest that our finding of a small but significant effect of eligibility for full-time primary education (relative to part-time nursery education) on parental labour supply is not materially affected by our choice of sample window.

Similarly, to ensure that our choice of age specification does not affect our results, we have run the same analysis using age squared (allowing it to have different slopes either side of the discontinuity). Results for the 1 September discontinuity in Policy 1 areas are shown in Table D4 in Appendix D; results for Policy 2 areas are available from the authors on request. These figures suggest that our findings are not materially affected by our choice of age specification; while the standard errors increase (a common finding), the basic pattern remains the same, particularly for welfare receipt.

As discussed in Section 3, slightly more children are born immediately before than immediately after the 1 September discontinuity. To ensure that this discrepancy is not biasing our results, we run the same analysis omitting children born up to a week either side of the discontinuity. Results for Policy 1 areas are shown in Table D5 in Appendix D; results for Policy 2 areas are available from the authors on request. These figures suggest that our findings are not unduly biased by the inclusion of children born up to a week either side of the discontinuity.

4.6 Placebo tests

To check the validity of our results, we ran a series of placebo tests. First, we compared outcomes amongst parents of children who are either too young or too old to be affected by the school entry discontinuity: in particular, we sampled children about to turn two, six and ten (rather than about to turn four, as in our main specification). Those turning two are too young to be eligible for either full-time primary education or part-time nursery education, and those turning six or ten are already in full-time primary education, so there are no discontinuities caused by school admissions policies; we would

therefore expect to see no difference in outcomes for these individuals.[38] Second, we compared outcomes amongst parents of children whose youngest child is turning four, but who are born either side of a discontinuity which has no bearing on school entry. For example, we examine the effects of being born either side of 1 March cut-off in Policy 1 areas (where it has no bearing on school entry).

Tables E1, E2 and E3 in Appendix E compare outcomes amongst parents whose youngest child is turning two, six or ten respectively, and who are born up to 60 days either side of the 1 September cut-off in Policy 1 areas, while Table E4 compares outcomes amongst parents whose youngest child is turning four and is born either immediately before or immediately after 1 March cut-off in Policy 1 areas. (Results for Policy 2 areas are available from the authors on request.) We find virtually no difference in welfare receipt or employment outcomes for any of these groups.[39] As such, these results provide reassurance that our main finding of a small but significant effect of entitlement to full-time primary education (relative to part-time nursery education) on welfare receipt and employment outcomes amongst low income lone parents is valid.

4.7 Discussion

How do our findings compare to those of the handful of other studies that have sought to estimate the impact of children attending full-time education on parental (largely maternal) employment?[40]

Gelbach (2002) studied the impact of children aged 4 to 5 starting full-time education in 1980 in the US on their mothers' labour supply. He found that free public schooling increased measures of labour supply amongst lone mothers with no younger children by between 6 and 24 percent, and reduced welfare receipt by 10 percent, but that there was no statistically significant impact on lone mothers with a younger child.[41] The estimated impact on the outcomes closest to ours suggest that employment in the week before the Census rose by 5 percentage points (equivalent to a 10 percent rise in the number of lone mothers in work) and that the fraction who received welfare in the previous year fell by 4

[38] Entitlement to benefits and tax credits for children is constant for children aged 1 to 16.

[39] The significant differences in employment rates towards the end of the period of interest amongst lone parents whose youngest child is turning two are likely to reflect differences in eligibility for full-time primary education, as these children will be turning four around this time.

[40] As we mentioned above, there are many papers which attempt to estimate the link between the price of formal childcare (usually for pre-school children) and maternal employment: see Blau & Currie (2004) for a review, and Brewer & Paull (2004) for a discussion of what these mean for the UK.

[41] The impact on married women was between 6 and 15 percent, and varied little with the presence of younger children.

percentage points (also a 10 percent rise).[42] Fitzpatrick (2009) repeats this analysis using the 2000 Census and finds significant impacts on maternal labour supply only for lone mothers with no younger children.

Fitzpatrick (2010) studies the impact of universal entitlement to free pre-K on various measures of mothers' labour supply in Georgia and Oklahoma.[43] She finds that entitlement to pre-K increases pre-school enrolment by 7.2 percentage points, or 12-14 percent, but that it has little discernable impact on maternal employment; for both findings, there are no statistically significant differences between lone mothers and married mothers.[44] Berlinksi et al (2009) study a similar set-up in Argentina, again exploiting date-of-birth cut-offs which mean that children aged around 5 and born 1 day apart will be entitled to free pre-primary school a year apart.45 They find that entitlement to pre-primary education increases enrolment by just under 30 percentage points, and that it increases the proportion of mothers in work by just under 4 percentage points (the mean employment rate of the sample is 37%): this means that, for every 100 children who start pre-primary school because of this reform, around 13 (the ratio of these two estimates, or 0.038/0.299) mothers move into work, although this is not significantly different from zero.

We are not aware of any studies that have attempted to estimate the impact of starting school using UK data. The study coming closest is Brewer & Paull (2006), which examined the impact of a child turning 5 on maternal labour supply and a wide range of employment characteristics. They used longitudinal data on children born in the 1990s and early 2000s to examine changes in maternal labour supply and a range of work characteristics as children aged. They find that the maternal employment rate rises by 3.4 percentage points (4.7 percentage points for lone mothers) over a 15 month period during which

[42] The fractions of lone mothers whose youngest child is aged 5 who are in work or on welfare in the US in 1980 are very similar to those of the same group in the UK in the mid-2000s.

[43] Pre-K or pre-kindergarten is part-time (2.5 hours/day) or full-time (6 hours/day) during the school year and is intended for four year-olds. A UK equivalent would be somewhere between the entitlement to nursery places for three and four year olds and reception classes in infant schools.

[44] She suggests two reason for the smaller impact found in her study compared to Gelbach's: first, that mothers' employment decisions have become less responsive to financial incentives as the maternal employment rate has risen (she uses data from 1999/2000 rather than 1979/1980, as in Gelbach's paper); second, that government support for all forms of childcare has increased, meaning that there is less difference in the childcare available to children who were and were not eligible to free pre-K in 2000 than there was between that available to children who were and were not eligible to free public schooling in 1980.

[45] Berlinski et al (2009) report that these are generally part-time sessions (3.5 hours a day), available 5 days a week in the school year, and generally within existing primary schools. A UK equivalent would be nursery classes in state-run infant or primary schools.

children start school, almost all of which is due to families whose last child is starting school (their Table 5.7). On the other hand, they also show that the maternal (and paternal) work dynamics around the time of school entry seem little different to those experienced by parents of slightly younger or slightly older children: in other words, the rise in maternal employment over this period could simply be a general response to the aging of the youngest child, rather than a specific response to the start of school.

It is interesting to compare these impacts reported in this paper to those of welfare programmes affecting lone parents or changes to the rules of welfare benefits for lone parents in the UK. A partial summary of the estimated impact of welfare reforms affecting lone parents is given in Cebulla et al (2008), and Brewer et al (2009) estimated the impact of a temporary wage supplement programme for lone parents (known as "In Work Credit").[46] We find that, amongst those receiving welfare when their youngest children is aged approximately three and a half, eligibility for full-time primary education (relative to part-time nursery education) increases the proportion of lone parents off welfare and in employment by a small but significant margin that peaks at around 2 percentage points (or 10-15 per cent) eight to nine months after the child becomes eligible (aged approximately 4 years and 9 months). Brewer et al report that, after 12 months of being potentially eligible for a wage supplement, an additional 1.6 ppts of potentially eligible lone parents had left welfare, and after 24 months, the figure was 2.0 ppts. Cebulla et al calculated the impact of the New Deal for Lone Parents - a voluntary programme featuring personalised support and advice – on all lone parents on welfare benefits to be 1.7 percentage points after nine months and 1.4 percentage points after two years. They also reported that, after 12 months, the impact of Work Focussed Interviews (WFIs) was 0.8 per cent for lone parents with youngest children aged over 13 and 2.0 per cent for lone parents with youngest children aged 9–12. [47] Eligibility for full-time primary education seems, therefore, to have an impact broadly comparable to these three welfare-to-work programmes, but at vastly greater cost, not least because the full-time

[46] Cebulla et al discussed extensively the difficulties involved in making direct comparisons, given the different approaches taken by the original evaluations.

[47] Cebulla et al discussed extensively the difficulties involved in making direct comparisons, given the different approaches taken by the original evaluations, but these results are as comparable as they can be given the published data. Importantly, all express the intention to treat: they are the estimated impact on those lone parents who were exposed to the treatment, where the treatments are: a child's eligibility to full-time education; eligibility for a wage supplement were they to enter work; eligibility to volunteer for the NDLP; and a requirement to attend an annual Work Focused Interview. However, the estimated impacts of the three programmes are all for slightly different populations: the IWC estimates are for all lone parents whose welfare claim reaches 12 months, the NDLP estimate is for all lone parents on IS in Great Britain, and the WFI estimates are for the stock of lone parents on IS with children of various ages.

school places were provided to all (or almost all) of the lone parents on welfare, but only a small minority of lone parents on welfare actually moved into work and took up the wage supplement or volunteered to participate in NDLP.

6 Conclusions

In this paper, we have argued that we can identify the causal impact of youngest child's eligibility for full-time primary education (relative to part-time nursery education) on the labour supply of low income lone parents by comparing outcomes of lone parents whose youngest child is born either side of cut-offs which determine eligibility to start school. Using a regression discontinuity approach, we find that, amongst those receiving welfare when their youngest children is aged approximately three and a half, eligibility for full-time primary education (relative to part-time nursery education) increases the proportion of lone parents off welfare and in employment by a small but significant margin that peaks (at around 2 percentage points, or 10-15 per cent) eight to nine months after the child becomes eligible (aged approximately 4 years and 9 months). Moreover, we find that these effects do not start to emerge until some four to six months after school entry, which suggests that lone parents may not start looking for work until their youngest child is eligible for school. This indicates that some previous estimates (notably those of Gelbach, 2002, and Fitzpatrick, 2010) may slightly under-estimate the effect of (pre-)kindergarten entry on mothers' labour supply by considering outcomes measured seven and five months respectively after the children start school.

It is worth noting, however, that this effect is small in comparison to the proportion of lone parents with children around school entry age who are leaving welfare and entering employment over time anyway. This is perhaps surprising, given that we are focusing on what might be regarded as a relatively responsive group (and a group for whom other authors – for example, Cascio (2009) and Fitzpatrick (2009) – have found the largest effects), although there may be relatively few jobs that fall entirely within school hours. As we were conducting this research, the new (from May 2010) UK government announced a plan to remove entitlement for income support from lone parents whose youngest child was aged 5 or over (compared with the previous government's plan to do this for lone parents whose youngest child was aged 7 or over); this will mean that non-working lone parents who are not eligible for disability benefits will have to claim Jobseekers Allowance, a welfare benefit with job-search

requirements.[48] Our findings are clearly pertinent to this decision: they certainly suggest that eligibility for full-time education (corresponding to a relatively large childcare subsidy) does not precipitate a large increase in labour market activity. On the other hand, proponents of the move to require lone parents to seek work as a condition of receiving welfare benefits when their children are of school age might well say that the relatively small impacts found in our study mostly result from the lack of obligation to look for work that existed at the time our data was collected. Furthermore, our results suggest that the expansion of public education programmes to younger disadvantaged children – such as the policy of extending free nursery education to disadvantaged two year olds – may only encourage a small number of low income lone parents to return to work.

[48] See para 1.101 of HC 61 (2010-11).

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TABLES

Table 1 Selected characteristics of lone parents whose youngest child is born up to 60 days either side of the relevant cut-off

	Parents of older children	Parents of younger children	Difference
	1 September cut-off in Policy 1 areas		
Male	0.029	0.027	0.002
Number of children	2.01	1.985	0.025*
Age	29.84	29.483	0.357**
Non-white	0.125	0.125	0.000
Work history	0.156	0.158	-0.002
Welfare history	0.104	0.103	0.002
Disabled	0.057	0.057	0.000
Local employment rate	0.656	0.656	0.000
	23,181	23,992	
	1 September cut-off in Policy 2 areas		
Male	0.028	0.025	0.003
Number of children	2.033	2.012	0.021
Age	30.574	30.268	0.306**
Non-white	0.234	0.25	-0.016*
Work history	0.149	0.146	0.003
Welfare history	0.093	0.098	-0.005
Disabled	0.052	0.054	-0.001
Local employment rate	0.632	0.63	0.002
	8,745	9,051	
	1 March cut-off in Policy 2 areas		
Male	0.035	0.033	0.002
Number of children	2.037	2.04	-0.003
Age	31.288	31.139	0.149
Non-white	0.238	0.226	0.012
Work history	0.148	0.146	0.002
Welfare history	0.092	0.096	-0.004
Disabled	0.055	0.053	0.002
Local employment rate	0.63	0.632	-0.002
	8,267	7,998	

Source: authors' calculations based on administrative data.

Table 2 Sample size by policy area, cut-off date and window

	Policy 1 areas	Policy 2 areas	
	1 September cut-off	1 September cut-off	1 March cut-off
14 day window	11,060	4,088	3,766
30 day window	23,857	8,883	8,120

60 day window	47,173	17,796	16,265
90 day window	70,368	26,425	24,309

Source: authors' calculations based on administrative data.

Table 3 Sample size by age and cut-off date for Policy 1 and 2 areas using 60 day window

	1 September cut-off	1 January cut-off	1 April cut-off
Youngest child turning two	91,537	N/A	N/A
Youngest child turning three	65,534	44,221	57,803
Youngest child turning six	48,752	N/A	N/A
Youngest child turning ten	30,335	N/A	N/A

Source: authors' calculations based on administrative data. Note: we restrict our sample of parents whose youngest child is turning three to the 63 local authorities that received early funding to provide free part-time nursery places.

Table 4 Effect on proportion of lone parents off benefit of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas

Month relative to cut-off	Approx. age	Treatment effect	Month relative to cut-off	Approx. age	Treatment effect	Month relative to cut-off	Approx. age	Treatment effect
-6	3y, 6m	0.00007 [0.002]	6	4y, 6m	0.017** [0.006]	18	5y, 6m	0.005 [0.007]
-5	3y, 7m	0.005 [0.003]	7	4y, 7m	0.016** [0.006]	19	5y, 7m	-0.001 [0.007]
-4	3y, 8m	0.0003 [0.004]	8	4y, 8m	0.018** [0.006]	20	5y, 8m	0.0004 [0.007]
-3	3y, 9m	0.0003 [0.004]	9	4y, 9m	0.019** [0.005]	21	5y, 9m	-0.001 [0.007]
-2	3y, 10m	-0.0008 [0.004]	10	4y, 10m	0.018** [0.006]	22	5y, 10m	-0.003 [0.008]
-1	3y, 11m	-0.003 [0.004]	11	4y, 11m	0.016** [0.006]	23	5y, 11m	-0.001 [0.007]
0	4y, 0m	-0.007 [0.004]	12	5y, 0m	0.015* [0.006]	24	6y, 0m	0.001 [0.008]
1	4y, 1m	-0.005 [0.005]	13	5y, 1m	0.012* [0.006]	25	6y, 1m	-0.004 [0.008]
2	4y, 2m	0.005 [0.005]	14	5y, 2m	0.009 [0.007]	26	6y, 2m	-0.005 [0.008]
3	4y, 3m	0.006 [0.005]	15	5y, 3m	0.010 [0.006]	27	6y, 3m	-0.003 [0.008]
4	4y, 4m	0.008 [0.006]	16	5y, 4m	0.004 [0.007]	28	6y, 4m	-0.003 [0.007]
5	4y, 5m	0.010 [0.006]	17	5y, 5m	0.005 [0.007]	29	6y, 5m	-0.00007 [0.007]

Estimates from a series of linear probability models which also include controls for: gender, ethnicity, age, number of children, age of youngest child relative to cut-off, day of week of birth of youngest child, whether youngest child born on a bank holiday, employment and welfare histories, an indicator for the

amount of Income Support received (as a proxy for income), past participation in a particular voluntary welfare-to-work programme (the New Deal for Lone Parents), an indicator of disability, the proportion of formal childcare places available in the lone parents' local area, their local area deprivation score, their local unemployment rate, the proportion of lone parents in their local area who are professionals (as a proxy for their own socio-economic status), and the proportion of workless lone parents in their area with particular education levels (as a proxy for their own education level). We also include a set of local authority dummies and a set of cohort dummies. Full details of all coefficient estimates are available from the authors on request. Standard errors are robust, clustered by day of birth and shown in parentheses. These estimates are plotted in Figure 6.

Table 5 Effect on proportion of lone parents in employment of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas

Month relative to cut-off	Approx. age	Treatment effect	Month relative to cut-off	Approx. age	Treatment effect	Month relative to cut-off	Approx. age	Treatment effect
-6	3y, 6m	0.012** [0.004]	6	4y, 6m	0.022** [0.005]	18	5y, 6m	0.0008 [0.007]
-5	3y, 7m	0.009* [0.004]	7	4y, 7m	0.023** [0.006]	19	5y, 7m	0.004 [0.007]
-4	3y, 8m	0.006 [0.004]	8	4y, 8m	0.024** [0.006]	20	5y, 8m	0.007 [0.007]
-3	3y, 9m	0.009 [0.005]	9	4y, 9m	0.021** [0.006]	21	5y, 9m	0.008 [0.007]
-2	3y, 10m	0.007 [0.005]	10	4y, 10m	0.018** [0.007]	22	5y, 10m	0.004 [0.007]
-1	3y, 11m	0.007 [0.006]	11	4y, 11m	0.019** [0.007]	23	5y, 11m	0.001 [0.007]
0	4y, 0m	0.009 [0.005]	12	5y, 0m	0.016* [0.007]	24	6y, 0m	0.001 [0.007]
1	4y, 1m	0.011* [0.005]	13	5y, 1m	0.008 [0.006]	25	6y, 1m	0.002 [0.007]
2	4y, 2m	0.013* [0.005]	14	5y, 2m	0.010 [0.006]	26	6y, 2m	0.001 [0.007]
3	4y, 3m	0.011 [0.006]	15	5y, 3m	0.008 [0.006]	27	6y, 3m	0.003 [0.008]
4	4y, 4m	0.017** [0.005]	16	5y, 4m	0.006 [0.006]	28	6y, 4m	0.0004 [0.008]
5	4y, 5m	0.018** [0.005]	17	5y, 5m	0.0009 [0.006]	29	6y, 5m	0.0006 [0.008]

See notes to Table 4. These estimates are plotted in Figure 8.

Table 6 Effect of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 2 areas

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	0.005 [0.003]	-0.0006 [0.005]	6	-0.012 [0.011]	0.009 [0.010]	18	-0.023 [0.012]	0.005 [0.010]
-5	0.003 [0.005]	-0.003 [0.007]	7	-0.014 [0.010]	0.012 [0.010]	19	-0.019 [0.013]	0.006 [0.010]
-4	0.003 [0.006]	-0.007 [0.008]	8	-0.010 [0.011]	0.009 [0.011]	20	-0.024 [0.013]	0.001 [0.011]
-3	-0.002 [0.006]	-0.007 [0.008]	9	-0.013 [0.012]	0.015 [0.011]	21	-0.021 [0.013]	-0.003 [0.010]
-2	-0.004 [0.007]	-0.008 [0.008]	10	-0.018 [0.012]	0.012 [0.010]	22	-0.018 [0.013]	-0.011 [0.010]
-1	-0.005 [0.008]	-0.010 [0.008]	11	-0.017 [0.011]	0.014 [0.010]	23	-0.021 [0.013]	-0.008 [0.010]
0	-0.008 [0.009]	-0.005 [0.009]	12	-0.012 [0.011]	0.010 [0.010]	24	-0.021 [0.014]	-0.005 [0.010]
1	-0.006 [0.009]	-0.007 [0.009]	13	-0.015 [0.011]	0.014 [0.010]	25	-0.026 [0.014]	-0.008 [0.011]
2	-0.009 [0.009]	-0.009 [0.009]	14	-0.018 [0.011]	0.010 [0.010]	26	-0.023 [0.014]	0.0007 [0.010]
3	-0.012 [0.009]	-0.010 [0.009]	15	-0.015 [0.011]	0.009 [0.010]	27	-0.028 [0.014]	0.0005 [0.011]
4	-0.014 [0.009]	0.0004 [0.009]	16	-0.013 [0.011]	0.004 [0.010]	28	-0.022 [0.014]	-0.003 [0.011]
5	-0.008 [0.010]	0.005 [0.010]	17	-0.022 [0.011]	0.003 [0.010]	29	-0.028 [0.015]	0.0008 [0.011]

See notes to Table 4.

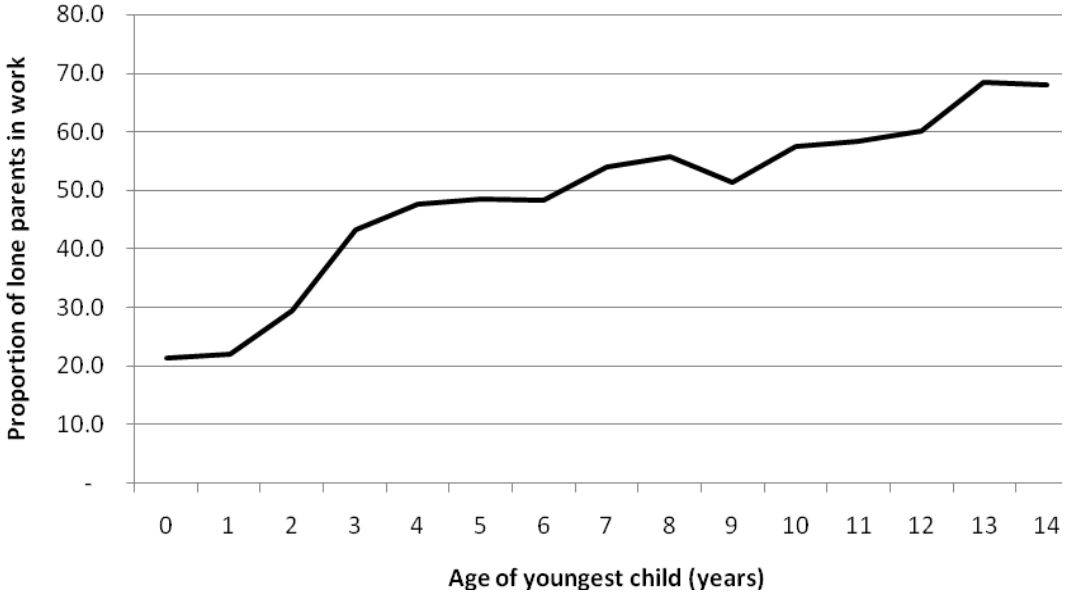
Table 7 Effect of youngest child being born immediately before (rather than immediately after) 1 March cut-off in Policy 2 areas

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	-0.002 [0.003]	0.009 [0.005]	6	-0.010 [0.008]	0.0002 [0.010]	18	-0.004 [0.009]	-0.004 [0.010]
-5	0.002 [0.003]	0.010 [0.007]	7	-0.011 [0.007]	-0.002 [0.011]	19	-0.002 [0.009]	0.006 [0.010]
-4	-0.003 [0.004]	0.013 [0.008]	8	-0.012 [0.007]	-0.009 [0.011]	20	-0.004 [0.009]	0.007 [0.010]
-3	0.0006 [0.005]	0.010 [0.008]	9	-0.010 [0.007]	-0.008 [0.011]	21	0.0006 [0.009]	0.009 [0.010]
-2	0.001 [0.005]	0.004 [0.008]	10	-0.010 [0.008]	-0.011 [0.011]	22	0.001 [0.009]	0.014 [0.011]
-1	-0.001 [0.005]	0.005 [0.008]	11	-0.006 [0.008]	-0.006 [0.011]	23	-0.0006 [0.009]	0.015 [0.012]
0	-0.0001 [0.006]	0.007 [0.008]	12	-0.009 [0.008]	-0.003 [0.011]	24	0.003 [0.010]	0.014 [0.012]
1	-0.00002 [0.005]	0.009 [0.009]	13	-0.012 [0.008]	-0.008 [0.011]	25	0.010 [0.009]	0.014 [0.011]
2	-0.004 [0.006]	0.014 [0.010]	14	-0.010 [0.008]	-0.007 [0.011]	26	0.010 [0.009]	0.011 [0.011]
3	-0.009 [0.006]	0.009 [0.011]	15	-0.010 [0.009]	-0.009 [0.011]	27	0.008 [0.009]	0.010 [0.011]
4	-0.010 [0.006]	-0.003 [0.010]	16	-0.009 [0.009]	-0.005 [0.011]	28	0.010 [0.010]	0.010 [0.011]
5	-0.008 [0.007]	-0.007 [0.009]	17	-0.008 [0.008]	-0.004 [0.010]	29	0.014 [0.010]	0.018 [0.011]

See notes to Table 4.

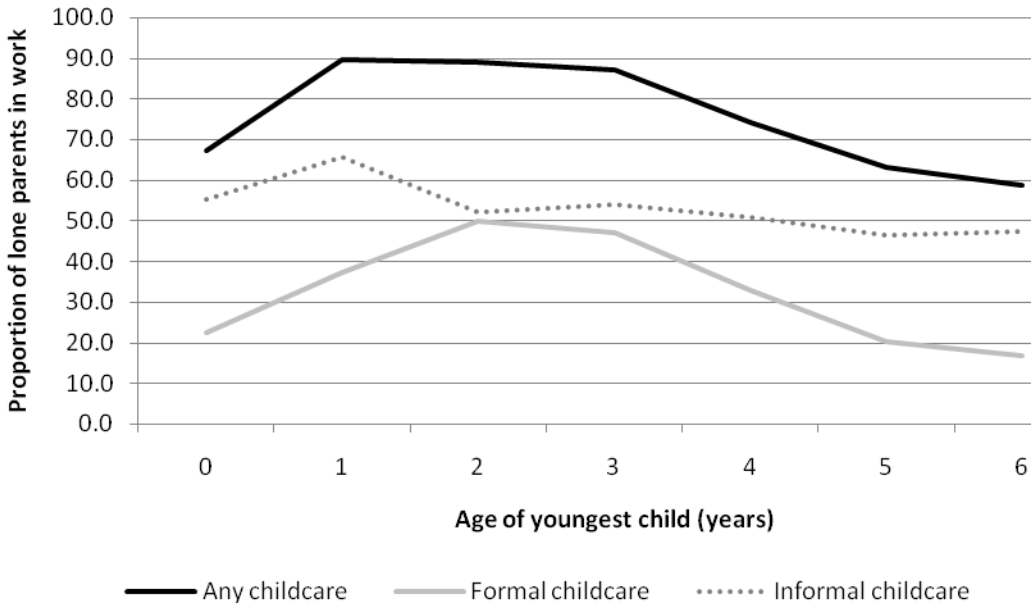
FIGURES

Figure 1 Employment rate amongst lone parents by age of youngest child, 2001-02 to 2003-04



Source: authors' calculations based on Family Resources Survey, April 2001 to March 2004.

Figure 2 Childcare usage amongst children of working lone parents by age of youngest child, 2001-02 to 2003-04



Source: authors' calculations based on Family Resources Survey, April 2001 to March 2004.

Figure 3a Actual school start dates of children who we expect to start school in September in Policy 1 areas in England between 2001-02 and 2004-05

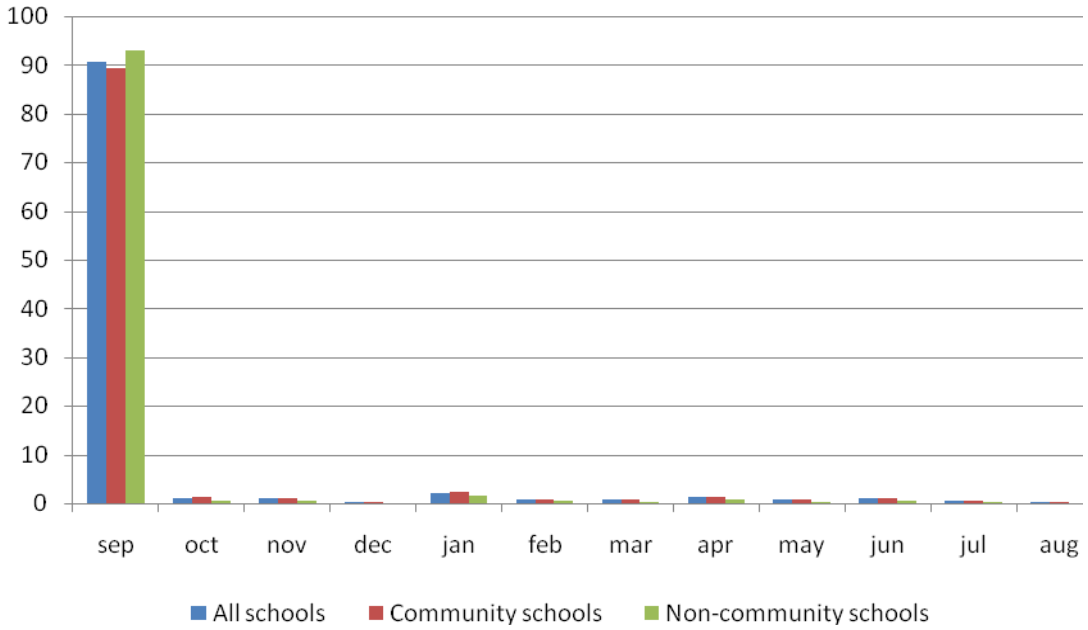
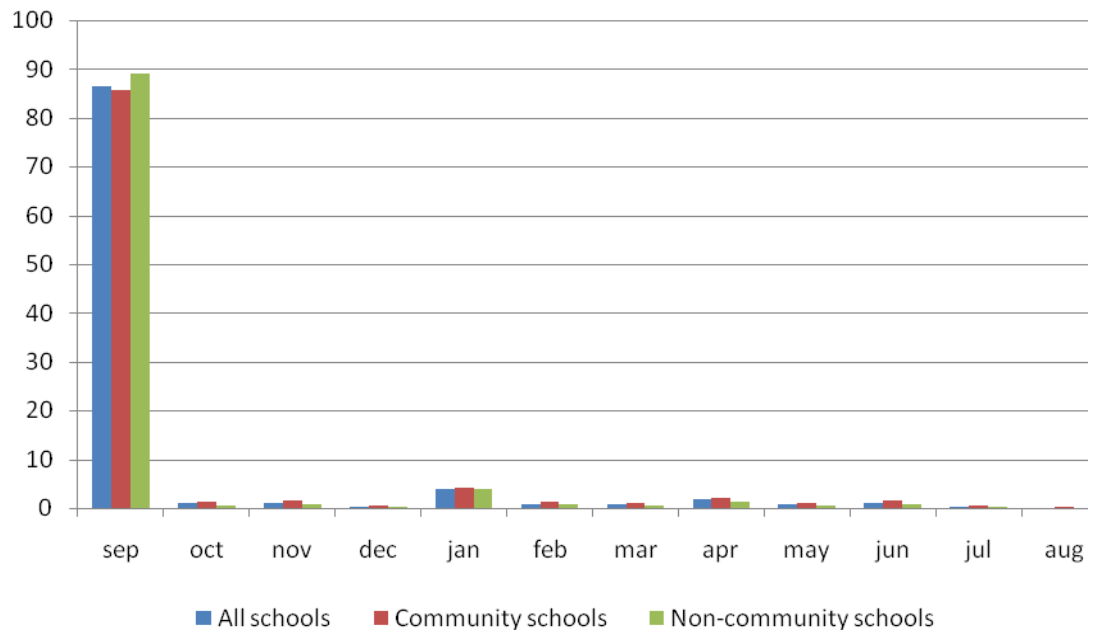
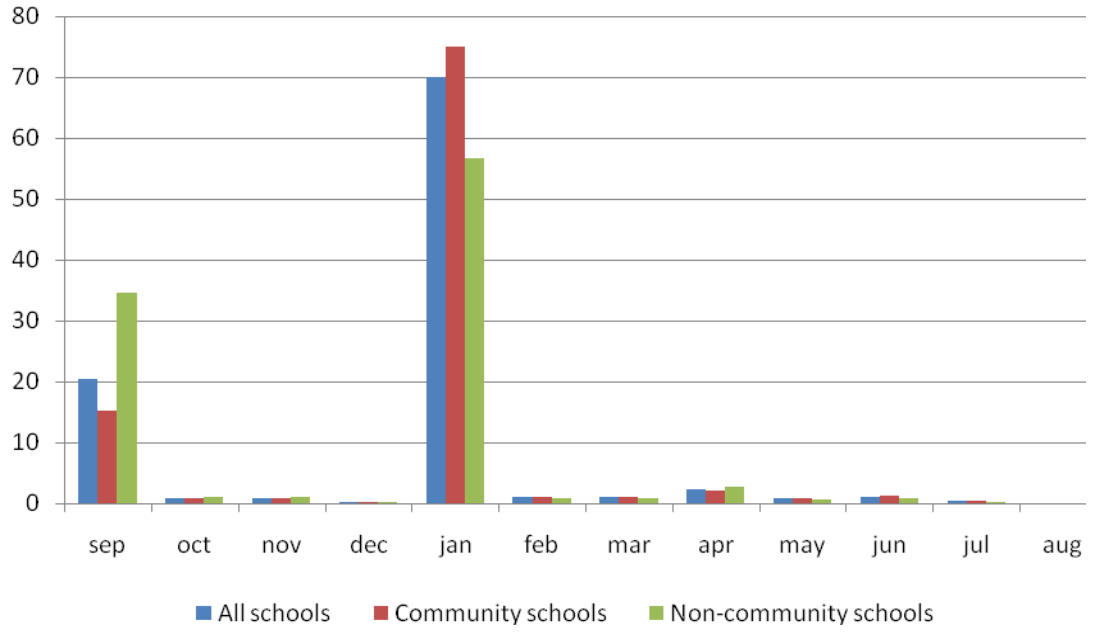


Figure 3b Actual school start dates of children who we expect to start school in September in Policy 2 areas in England between 2001-02 and 2004-05



Notes to Figures 1-3: authors' calculations based on school census data from 2001-02 to 2004-05. Community schools (64% of the sample) are required to follow the local authority admissions policy, while non-community schools (36%) are not.

Figure 3c Actual school start dates of children who we expect to start school in January in Policy 2 areas in England between 2001-02 and 2004-05



See notes above.

Figure 4 Density of birthdates in our main sample

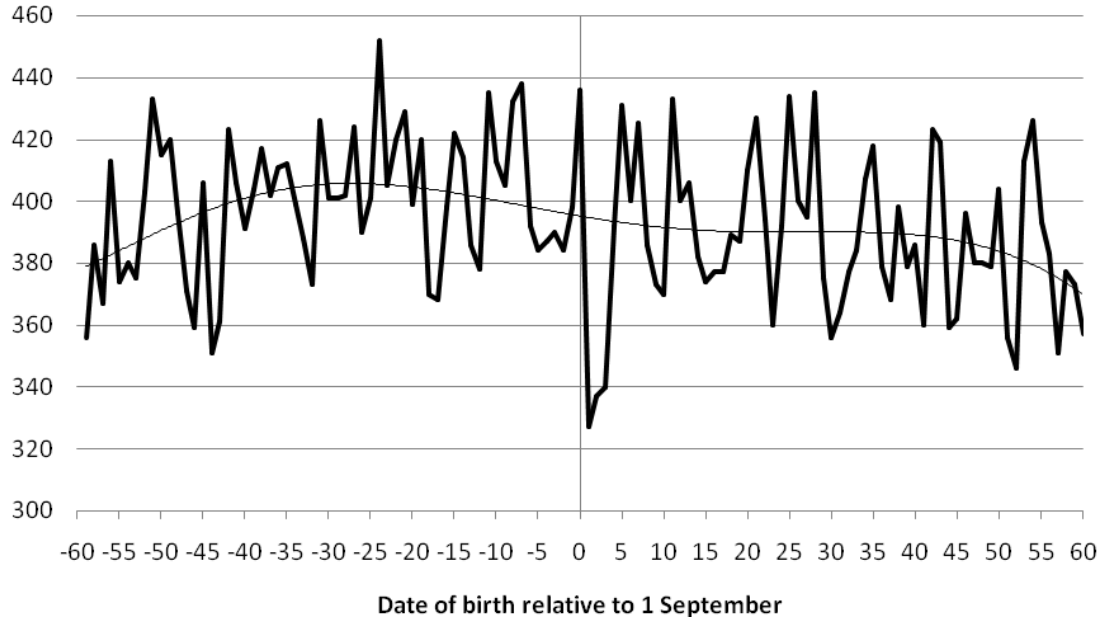
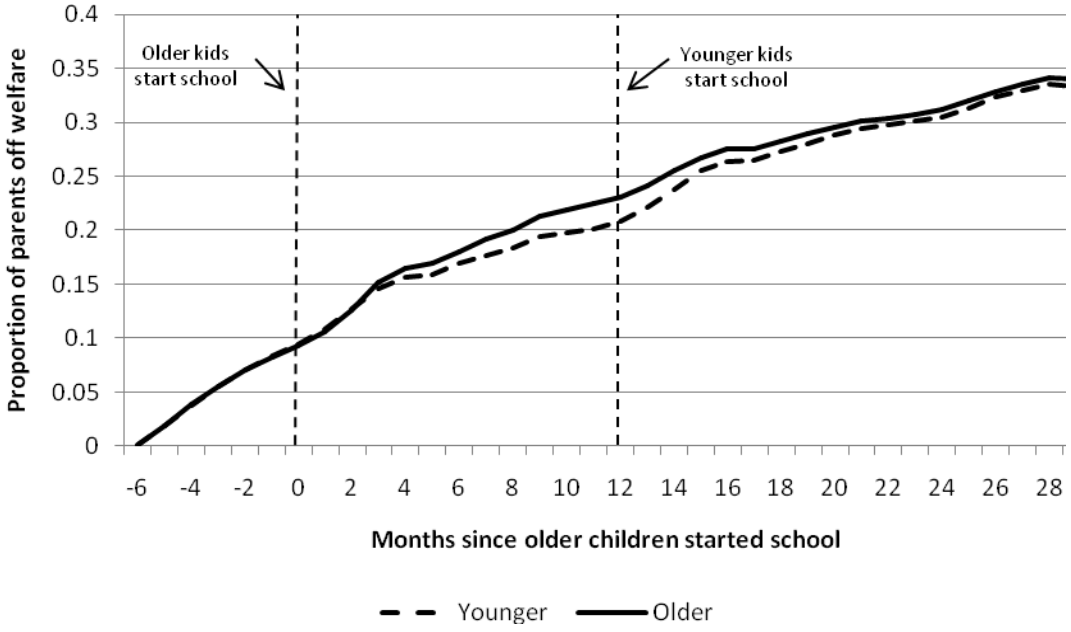


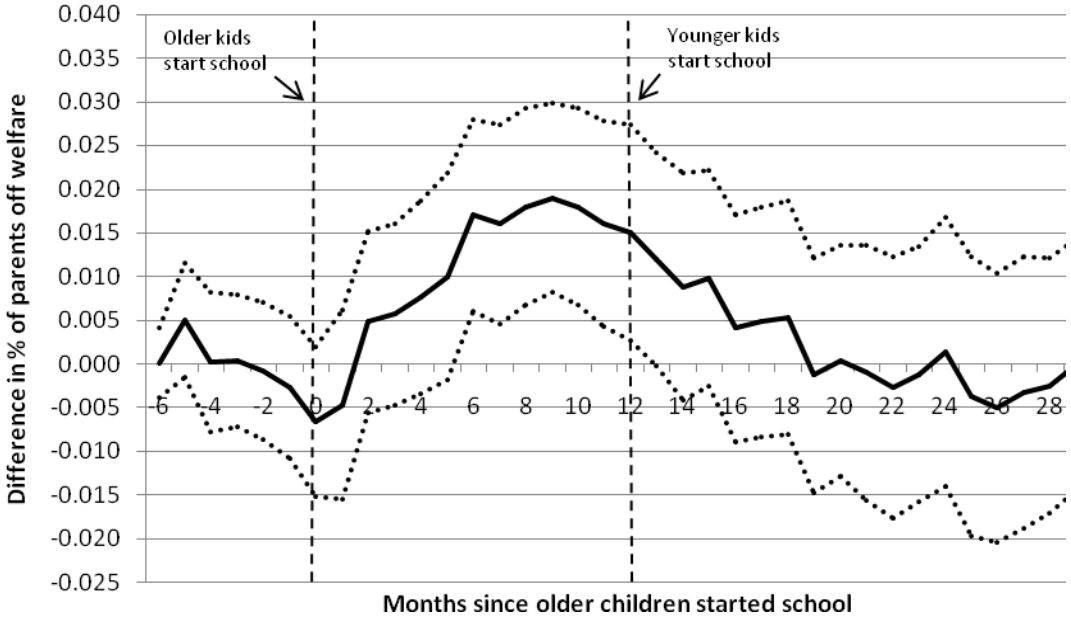
Figure shows number of children in our main sample born on each day relative to 1 September (1=born on 31 August). Figure also shows a sixth-order polynomial.

Figure 5 Proportion off welfare: comparing parents of children born up to 60 days before and after the 1 September cut-off in Policy 1 areas



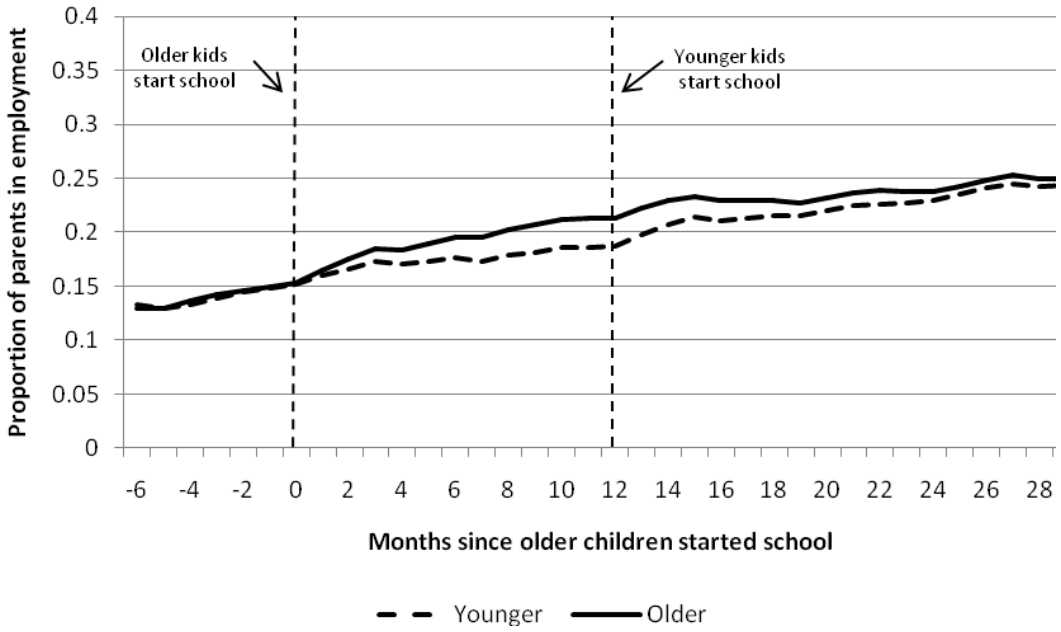
Source: authors' calculations based on administrative data.

Figure 6 Effect on proportion of lone parents off welfare of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas



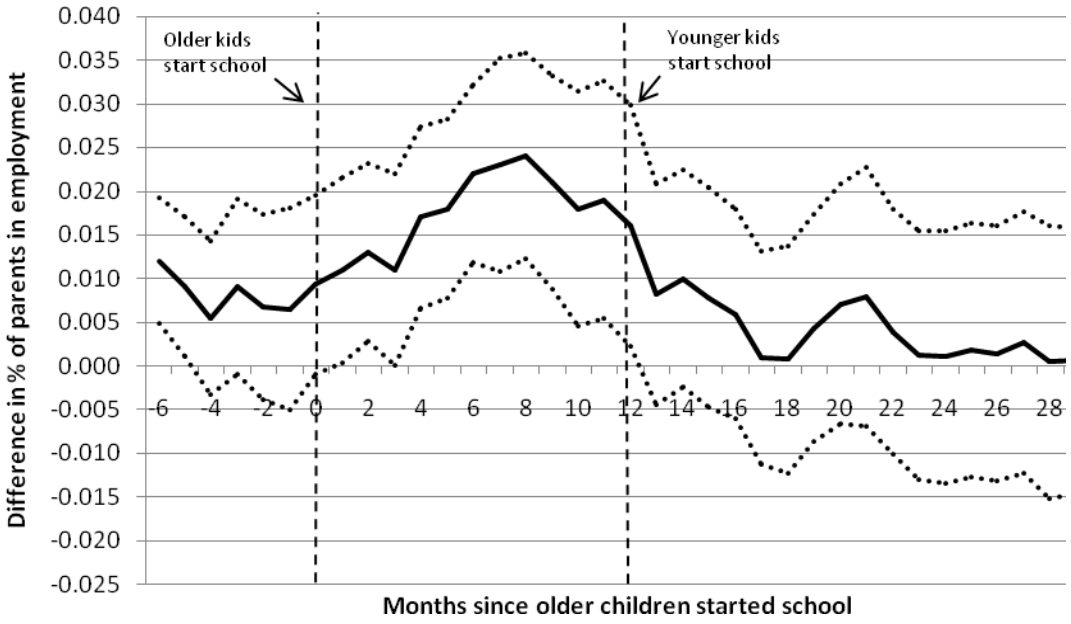
Source: estimates from a series of linear probability models which also include a full set of controls (see Table 4 for details).

Figure 7 Proportion in employment: comparing parents of children born up to 60 days before and after the 1 September cut-off in Policy 1 areas



Source: authors' calculations based on administrative data.

Figure 8 Effect on proportion of lone parents in employment of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas



Source: estimates from a series of linear probability models which also include a full set of controls (see Table 5 for details).

APPENDIX A RELATIONSHIP BETWEEN YOUNGEST CHILD'S DATE OF BIRTH AND OTHER CHARACTERISTICS AMONGST MAIN SAMPLE OF LONE PARENTS

Figure A1 Age of lone parent in our main sample

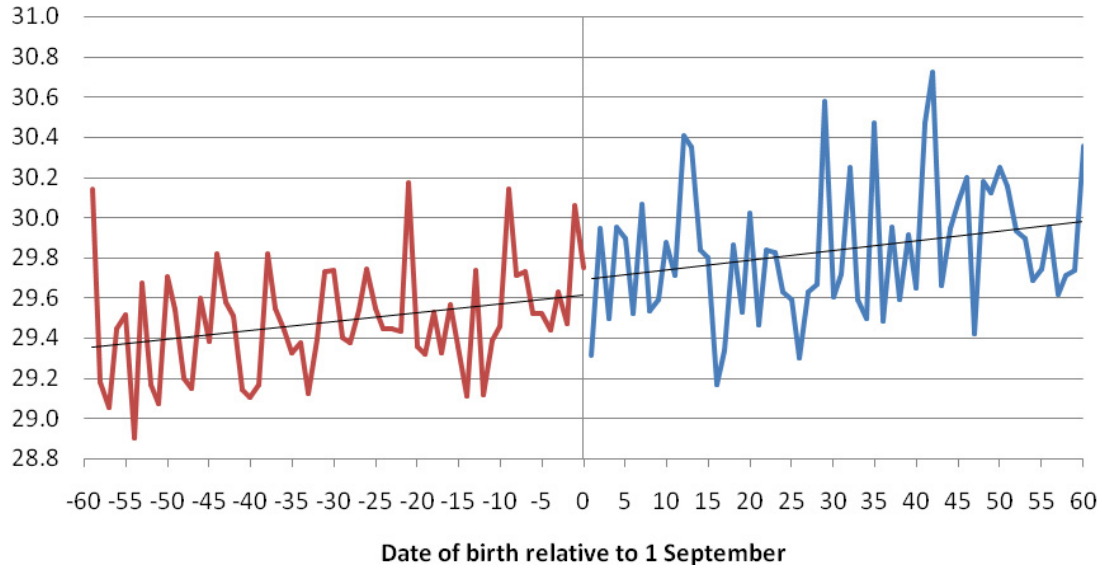


Figure shows age of lone parent when sampled for children in our main sample born on each day relative to 1 September (1=31 August). Figure also shows a linear trend estimated separately either side of 1 September cut-off.

Figure A2 Proportion of previous 18 months not on welfare in our main sample

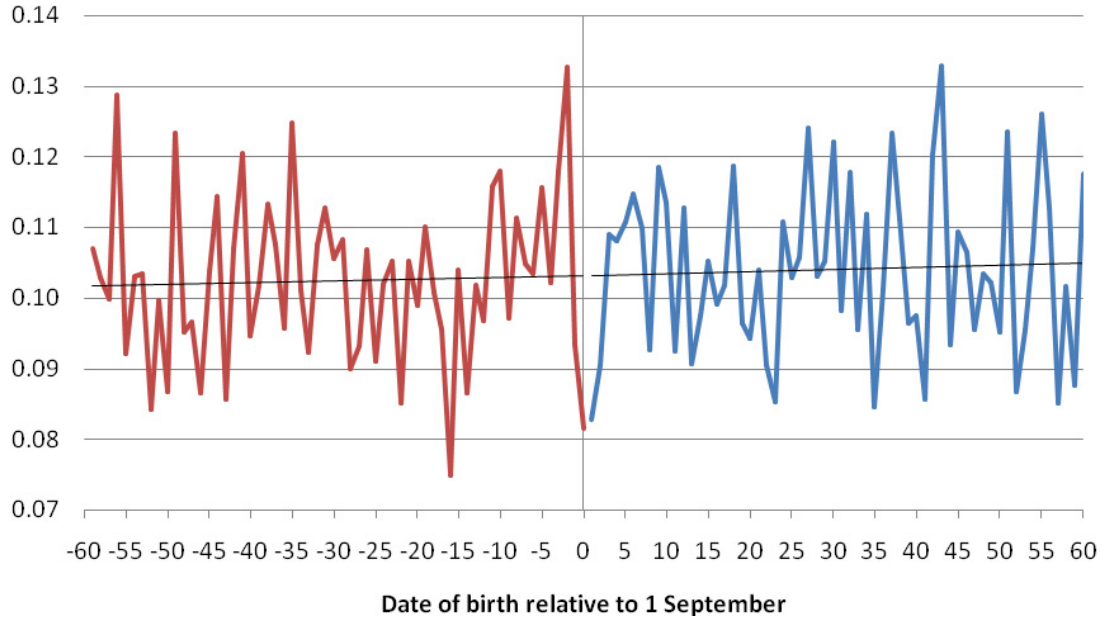


Figure shows proportion of 18 months before sampling that lone spent not on welfare for children in our main sample born on each day relative to 1 September (1=31 August). Figure also shows a linear trend estimated separately either side of 1 September cut-off.

Figure A3 Whether ever received a disability benefit in previous 18 months in our main sample

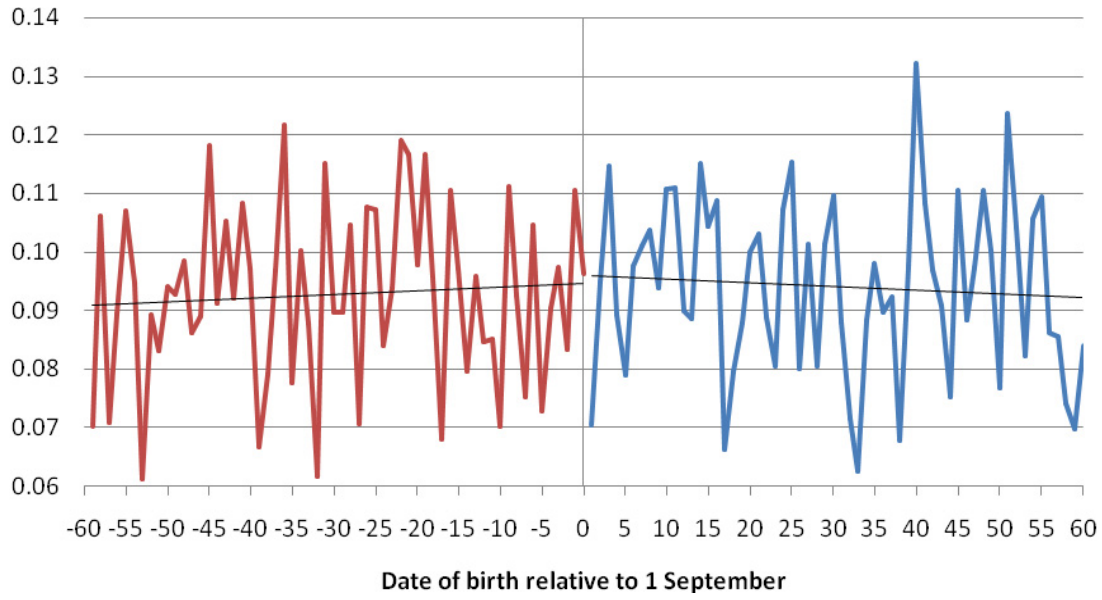


Figure shows whether lone parent ever received a disability benefit in 18 months before sampling for children in our main sample born on each day relative to 1 September. Figure also shows a linear trend estimated separately either side of 1 September cut-off (1=31 August).

Figure A4 Whether male in our main sample

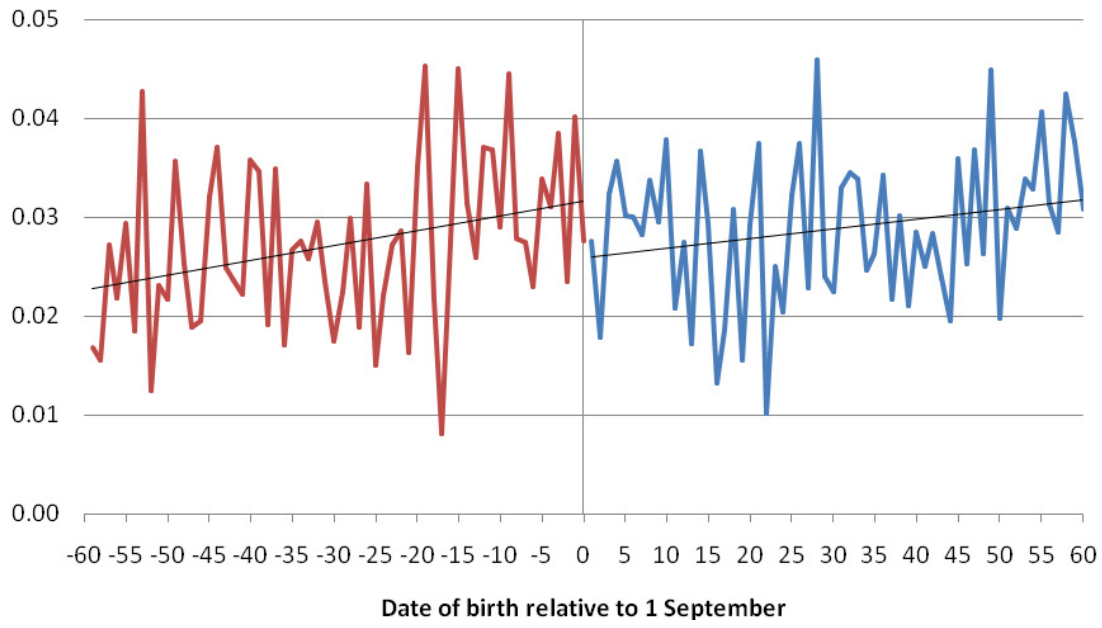


Figure shows proportion of lone parents who are male for children in our main sample born on each day relative to 1 September (1=31 August). Figure also shows a linear trend estimated separately either side of 1 September cut-off.

Figure A5 Whether non-white

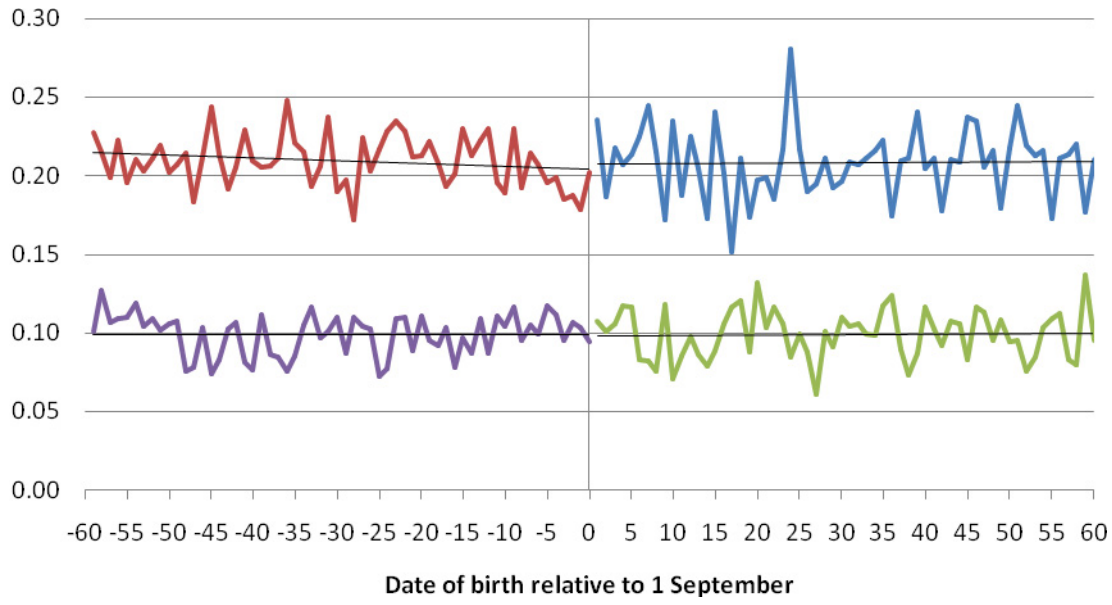


Figure shows proportion of lone parents with specified ethnicity for children in our main sample born on each day relative to 1 September (1=31 August). Top line is fraction whose ethnicity is not known; bottom line is fraction (of whole sample) with non-white ethnicity. Figure also shows a linear trend estimated separately either side of 1 September cut-off .

Figure A6 Work history in our main sample

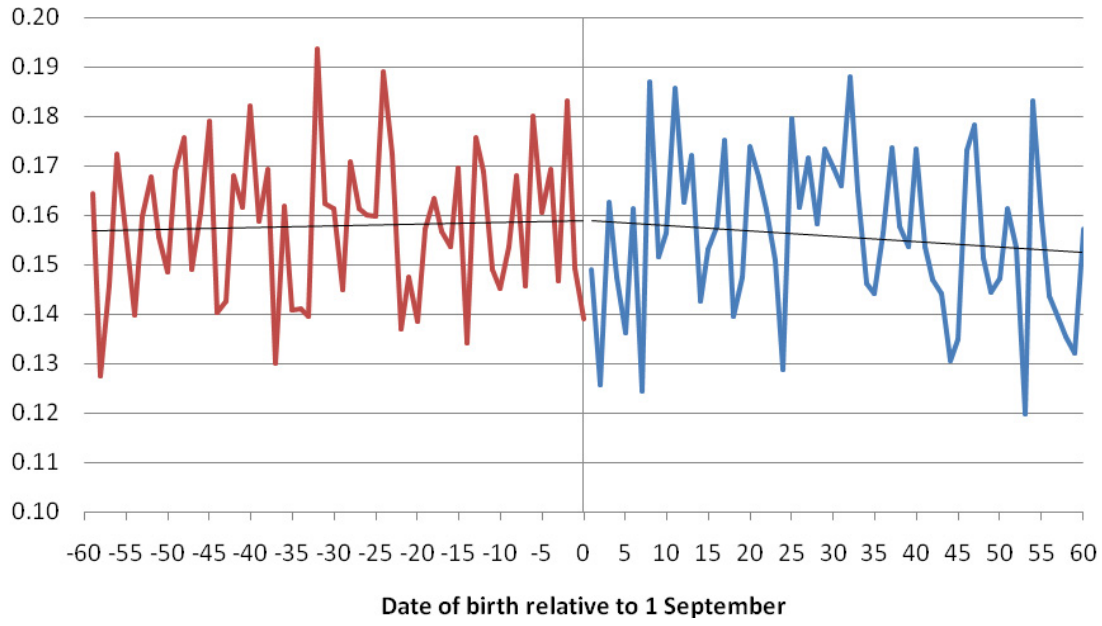


Figure shows proportion of 18 months before sampling that lone spent in work for children in our main sample born on each day relative to 1 September (1=31 August). Figure also shows a linear trend estimated separately either side of 1 September cut-off.

Figure A7 Local employment rate in our main sample

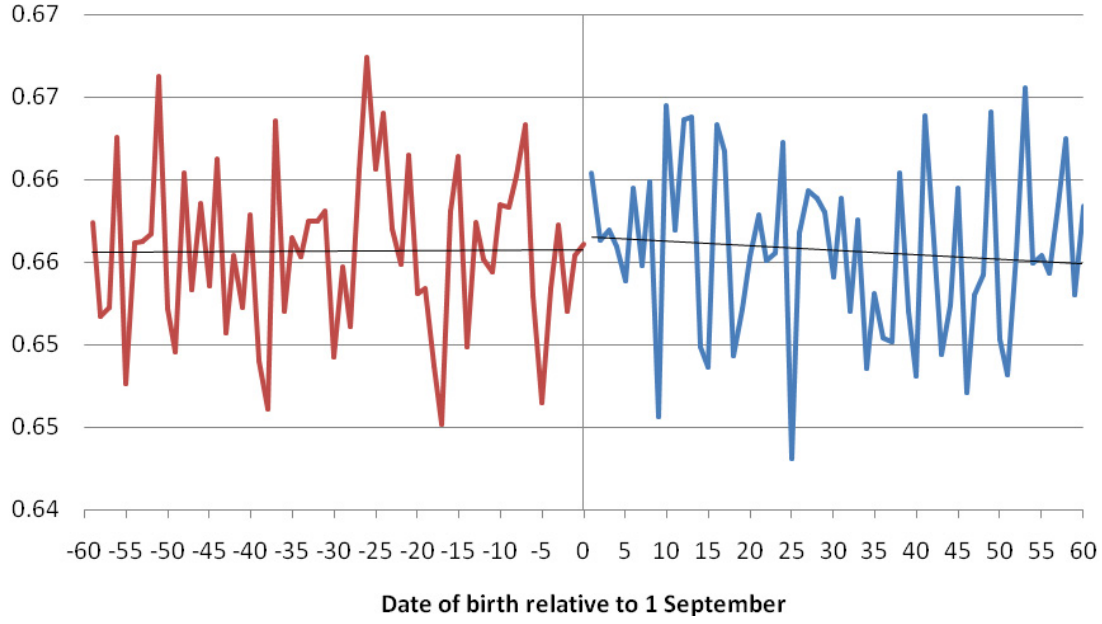


Figure also shows a linear trend estimated separately either side of 1 September cut-off (1=31 August).

APPENDIX B EFFECT OF ELIGIBILITY FOR PART-TIME NURSERY EDUCATION

Table B1 Effect of youngest child being born before (rather than after) 1 September cut-off determining eligibility for part-time nursery education in Policy 1 areas

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	0.0004 [0.002]	-0.006 [0.003]	6	0.005 [0.006]	0.007 [0.006]	18	0.012 [0.007]	0.010 [0.007]
-5	0.0005 [0.003]	0.0005 [0.004]	7	0.004 [0.006]	0.005 [0.005]	19	0.012 [0.007]	0.015* [0.006]
-4	-0.003 [0.004]	-0.0008 [0.005]	8	0.005 [0.006]	0.003 [0.005]	20	0.014 [0.007]	0.017** [0.006]
-3	-0.002 [0.004]	0.0003 [0.005]	9	0.0008 [0.006]	-0.001 [0.005]	21	0.016* [0.007]	0.014* [0.006]
-2	-0.002 [0.004]	0.002 [0.005]	10	-0.001 [0.006]	-0.003 [0.005]	22	0.015* [0.006]	0.013* [0.006]
-1	0.002 [0.005]	0.0007 [0.005]	11	0.0004 [0.006]	-0.004 [0.006]	23	0.015* [0.007]	0.015* [0.006]
0	0.005 [0.005]	0.004 [0.005]	12	0.0003 [0.006]	0.002 [0.006]	24	0.012 [0.007]	0.012* [0.006]
1	0.007 [0.005]	0.001 [0.006]	13	-0.0008 [0.007]	0.004 [0.006]	25	0.008 [0.008]	0.011 [0.006]
2	0.010 [0.005]	0.002 [0.005]	14	0.004 [0.007]	0.002 [0.006]	26	0.005 [0.008]	0.012* [0.006]
3	0.009 [0.006]	0.0001 [0.006]	15	0.007 [0.007]	0.003 [0.006]	27	0.006 [0.008]	0.010 [0.005]
4	0.010 [0.006]	0.003 [0.006]	16	0.007 [0.007]	0.007 [0.006]	28	0.004 [0.007]	0.008 [0.005]
5	0.007 [0.006]	0.004 [0.006]	17	0.008 [0.007]	0.007 [0.007]	29	0.0009 [0.007]	0.004 [0.005]

Estimates from a series of linear probability models, restricted to the 63 local authorities which received early funding for free part-time nursery places, and which also include controls for: admissions policy area, gender, ethnicity, age, number of children, age of youngest child relative to cut-off, day of week of birth of youngest child, whether youngest child born on a bank holiday, employment and welfare histories, an indicator for the amount of Income Support received (as a proxy for income), past participation in a particular voluntary welfare-to-work programme (the New Deal for Lone Parents), an indicator of disability, the proportion of formal childcare places available in the lone parents' local area, their local area deprivation score, their local unemployment rate, the proportion of lone parents in their local area who are professionals (as a proxy for their own socio-economic status), and the proportion of workless lone parents in their area with particular education levels (as a proxy for their own education level). We also include a set of local authority dummies and a set of cohort dummies. Full details of all

coefficient estimates are available from the authors on request. Standard errors are robust, clustered by day of birth and shown in parentheses.

Table B2 Effect of youngest child being born before (rather than after) 1 September cut-off determining eligibility for part-time nursery education in Policy 2 areas

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	-0.004 [0.002]	-0.005 [0.004]	6	-0.008 [0.007]	0.004 [0.007]	18	-0.003 [0.008]	0.0015 [0.008]
-5	-0.006 [0.004]	0.001 [0.005]	7	-0.007 [0.008]	0.005 [0.007]	19	-0.002 [0.008]	0.003 [0.008]
-4	-0.008 [0.004]	-0.001 [0.006]	8	-0.008 [0.007]	0.002 [0.007]	20	-0.0002 [0.008]	0.005 [0.008]
-3	-0.010* [0.005]	-0.004 [0.006]	9	-0.009 [0.007]	-0.001 [0.007]	21	-0.002 [0.008]	0.0008 [0.008]
-2	-0.010 [0.005]	-0.002 [0.006]	10	-0.009 [0.007]	-0.0010 [0.007]	22	-0.005 [0.008]	0.0002 [0.008]
-1	-0.009 [0.006]	-0.003 [0.006]	11	-0.008 [0.007]	-0.006 [0.008]	23	-0.007 [0.008]	0.0010 [0.008]
0	-0.008 [0.006]	0.001 [0.006]	12	-0.005 [0.008]	0.00003 [0.008]	24	-0.010 [0.008]	-0.002 [0.008]
1	-0.007 [0.006]	-0.0006 [0.007]	13	-0.009 [0.007]	-0.003 [0.008]	25	-0.014 [0.009]	-0.004 [0.008]
2	-0.009 [0.007]	-0.0001 [0.007]	14	-0.009 [0.007]	-0.007 [0.008]	26	-0.015 [0.009]	-0.004 [0.007]
3	-0.006 [0.007]	-0.0010 [0.007]	15	-0.010 [0.007]	-0.006 [0.008]	27	-0.011 [0.009]	-0.007 [0.008]
4	-0.005 [0.007]	0.0002 [0.008]	16	-0.006 [0.008]	-0.002 [0.008]	28	-0.012 [0.009]	-0.007 [0.008]
5	-0.011 [0.007]	0.002 [0.008]	17	-0.007 [0.008]	-0.003 [0.008]	29	-0.015 [0.009]	-0.007 [0.007]

See notes to Table B1.

Table B3 Effect of youngest child being born before (rather than after) 1 January cut-off determining eligibility for part-time nursery education in Policy 1 areas

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	-0.001 [0.002]	-0.003 [0.003]	6	-0.004 [0.009]	-0.015* [0.006]	18	0.006 [0.010]	0.012 [0.008]
-5	-0.003 [0.003]	-0.003 [0.004]	7	-0.0008 [0.008]	-0.005 [0.007]	19	0.005 [0.010]	0.014 [0.008]
-4	-0.006 [0.004]	-0.005 [0.005]	8	-0.002 [0.007]	-0.0009 [0.007]	20	0.003 [0.009]	0.015 [0.008]
-3	-0.006 [0.005]	-0.009 [0.006]	9	-0.003 [0.008]	0.003 [0.006]	21	0.008 [0.008]	0.014 [0.008]
-2	-0.008 [0.005]	-0.011* [0.006]	10	-0.004 [0.008]	0.0009 [0.006]	22	0.006 [0.009]	0.012 [0.007]
-1	-0.006 [0.005]	-0.011 [0.006]	11	-0.003 [0.008]	0.0007 [0.007]	23	0.007 [0.009]	0.012 [0.008]
0	-0.007 [0.005]	-0.013* [0.006]	12	-0.009 [0.009]	0.007 [0.006]	24	0.009 [0.009]	0.010 [0.008]
1	-0.011 [0.007]	-0.016** [0.006]	13	-0.010 [0.010]	0.006 [0.007]	25	0.009 [0.008]	0.011 [0.008]
2	-0.004 [0.008]	-0.017** [0.006]	14	-0.002 [0.010]	0.005 [0.007]	26	0.012 [0.009]	0.009 [0.008]
3	-0.007 [0.007]	-0.019** [0.006]	15	-0.004 [0.010]	0.003 [0.007]	27	0.012 [0.010]	0.010 [0.008]
4	-0.009 [0.008]	-0.022** [0.006]	16	-0.003 [0.010]	0.009 [0.008]	28	0.012 [0.010]	0.008 [0.008]
5	-0.006 [0.008]	-0.017** [0.006]	17	-0.003 [0.010]	0.013 [0.008]	29	0.010 [0.009]	0.006 [0.008]

See notes to Table B1.

Table B4 Effect of youngest child being born before (rather than after) 1 January cut-off determining eligibility for part-time nursery education in Policy 2 areas

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	0.001 [0.003]	0.005 [0.005]	6	-0.010 [0.010]	-0.008 [0.007]	18	-0.004 [0.012]	0.012 [0.009]
-5	0.0009 [0.004]	0.010 [0.007]	7	-0.011 [0.010]	-0.001 [0.009]	19	0.001 [0.012]	0.015 [0.010]
-4	0.0008 [0.006]	0.007 [0.007]	8	-0.009 [0.010]	0.005 [0.008]	20	-0.004 [0.012]	0.013 [0.010]
-3	0.002 [0.007]	0.006 [0.008]	9	-0.009 [0.010]	0.006 [0.008]	21	-0.005 [0.012]	0.015 [0.011]
-2	0.0007 [0.007]	0.004 [0.008]	10	-0.009 [0.010]	0.005 [0.008]	22	-0.005 [0.012]	0.013 [0.011]
-1	-0.0004 [0.008]	0.003 [0.008]	11	-0.013 [0.010]	0.006 [0.008]	23	-0.0006 [0.011]	0.012 [0.011]
0	-0.0009 [0.007]	0.0006 [0.008]	12	-0.011 [0.011]	0.004 [0.008]	24	-0.004 [0.012]	0.009 [0.011]
1	-0.004 [0.008]	-0.004 [0.008]	13	-0.014 [0.012]	0.007 [0.009]	25	-0.004 [0.011]	0.012 [0.010]
2	0.001 [0.008]	-0.004 [0.008]	14	-0.009 [0.012]	0.006 [0.008]	26	-0.003 [0.012]	0.007 [0.010]
3	-0.003 [0.008]	-0.009 [0.007]	15	-0.011 [0.012]	0.006 [0.009]	27	-0.003 [0.013]	0.007 [0.010]
4	-0.009 [0.008]	-0.011 [0.007]	16	-0.008 [0.012]	0.012 [0.008]	28	-0.0004 [0.012]	0.009 [0.010]
5	-0.009 [0.009]	-0.009 [0.007]	17	-0.006 [0.012]	0.014 [0.009]	29	-0.0004 [0.012]	0.007 [0.009]

See notes to Table B1.

Table B5 Effect of youngest child being born before (rather than after) 1 April cut-off determining eligibility for part-time nursery education in Policy 1 areas

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	0.004 [0.003]	0.004 [0.003]	6	-0.005 [0.006]	0.003 [0.005]	18	-0.004 [0.008]	-0.004 [0.006]
-5	0.003 [0.003]	0.008 [0.004]	7	-0.008 [0.006]	-0.006 [0.005]	19	-0.006 [0.007]	-0.003 [0.006]
-4	0.0004 [0.004]	0.008* [0.004]	8	-0.005 [0.005]	-0.006 [0.006]	20	-0.007 [0.007]	-0.005 [0.006]
-3	-0.0004 [0.004]	0.007 [0.004]	9	-0.005 [0.006]	-0.006 [0.005]	21	-0.009 [0.007]	-0.001 [0.006]
-2	0.0004 [0.004]	0.008 [0.004]	10	-0.003 [0.006]	-0.004 [0.006]	22	-0.010 [0.007]	-0.002 [0.006]
-1	-0.0004 [0.004]	0.006 [0.004]	11	-0.005 [0.006]	-0.006 [0.006]	23	-0.010 [0.007]	0.0004 [0.006]
0	0.001 [0.004]	0.007 [0.005]	12	-0.003 [0.006]	-0.008 [0.006]	24	-0.010 [0.007]	0.002 [0.006]
1	-0.002 [0.004]	0.006 [0.005]	13	-0.007 [0.007]	-0.008 [0.006]	25	-0.008 [0.007]	0.0007 [0.006]
2	-0.003 [0.005]	0.007 [0.005]	14	-0.004 [0.007]	-0.006 [0.006]	26	-0.008 [0.007]	0.0008 [0.007]
3	-0.005 [0.005]	0.006 [0.005]	15	-0.007 [0.008]	-0.008 [0.006]	27	-0.008 [0.007]	0.001 [0.007]
4	-0.003 [0.005]	0.008 [0.005]	16	-0.006 [0.008]	-0.006 [0.006]	28	-0.006 [0.007]	0.003 [0.007]
5	-0.005 [0.005]	0.006 [0.005]	17	-0.008 [0.007]	-0.004 [0.006]	29	-0.007 [0.008]	0.001 [0.007]

See notes to Table B1.

Table B6 Effect of youngest child being born before (rather than after) 1 April cut-off determining eligibility for part-time nursery education in Policy 2 areas

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	0.003 [0.003]	0.002 [0.004]	6	0.008 [0.008]	0.006 [0.006]	18	0.012 [0.009]	0.001 [0.007]
-5	0.003 [0.004]	0.003 [0.005]	7	0.007 [0.008]	0.002 [0.006]	19	0.008 [0.009]	0.010 [0.008]
-4	0.001 [0.004]	0.006 [0.005]	8	0.010 [0.008]	-0.002 [0.007]	20	0.008 [0.010]	0.006 [0.007]
-3	-0.002 [0.005]	0.004 [0.005]	9	0.008 [0.008]	-0.004 [0.007]	21	0.004 [0.009]	0.004 [0.007]
-2	-0.003 [0.005]	0.005 [0.005]	10	0.004 [0.009]	-0.003 [0.007]	22	0.0003 [0.010]	0.001 [0.008]
-1	-0.001 [0.006]	0.004 [0.006]	11	0.001 [0.009]	-0.001 [0.007]	23	-0.005 [0.010]	-0.001 [0.008]
0	0.003 [0.006]	0.003 [0.006]	12	0.004 [0.008]	0.003 [0.006]	24	-0.003 [0.010]	0.003 [0.008]
1	0.003 [0.006]	0.006 [0.006]	13	0.003 [0.009]	-0.0001 [0.007]	25	-0.00002 [0.010]	-0.001 [0.008]
2	0.007 [0.007]	0.006 [0.006]	14	0.007 [0.009]	0.005 [0.006]	26	-0.0002 [0.010]	-0.003 [0.008]
3	0.004 [0.007]	0.007 [0.006]	15	0.005 [0.009]	-0.0007 [0.006]	27	-0.004 [0.010]	-0.001 [0.008]
4	0.010 [0.007]	0.006 [0.006]	16	0.008 [0.009]	-0.002 [0.007]	28	-0.003 [0.010]	-0.004 [0.008]
5	0.010 [0.007]	0.005 [0.006]	17	0.004 [0.009]	0.002 [0.007]	29	-0.005 [0.010]	-0.006 [0.008]

See notes to Table B1.

APPENDIX C SUBGROUP ANALYSIS

Table C1 Effect on welfare receipt of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas, by NDLP participation

Month relative to cut-off	Ever been on NDLP	Never been on NDLP	Month relative to cut-off	Ever been on NDLP	Never been on NDLP	Month relative to cut-off	Ever been on NDLP	Never been on NDLP
-6	-0.00007	0.00009	6	0.046**	0.013*	18	0.020	0.003
	[0.005]	[0.002]		[0.013]	[0.006]		[0.015]	[0.007]
-5	0.011	0.004	7	0.045**	0.012*	19	0.008	-0.002
	[0.007]	[0.003]		[0.012]	[0.006]		[0.014]	[0.007]
-4	0.005	-0.0003	8	0.042**	0.016**	20	0.012	-0.001
	[0.009]	[0.004]		[0.012]	[0.006]		[0.014]	[0.007]
-3	0.010	-0.0009	9	0.043**	0.016**	21	0.016	-0.003
	[0.009]	[0.004]		[0.011]	[0.006]		[0.015]	[0.007]
-2	0.008	-0.002	10	0.038**	0.015*	22	0.015	-0.005
	[0.009]	[0.004]		[0.011]	[0.006]		[0.016]	[0.008]
-1	0.008	-0.004	11	0.032**	0.014*	23	0.010	-0.003
	[0.010]	[0.004]		[0.011]	[0.006]		[0.016]	[0.007]
0	0.007	-0.008	12	0.028*	0.013	24	0.012	0.0001
	[0.010]	[0.004]		[0.012]	[0.007]		[0.016]	[0.008]
1	0.010	-0.007	13	0.024	0.011	25	0.008	-0.005
	[0.011]	[0.006]		[0.012]	[0.006]		[0.016]	[0.008]
2	0.032**	0.001	14	0.020	0.007	26	0.006	-0.006
	[0.011]	[0.005]		[0.013]	[0.007]		[0.015]	[0.008]
3	0.033**	0.002	15	0.026	0.008	27	0.011	-0.005
	[0.012]	[0.005]		[0.014]	[0.006]		[0.014]	[0.008]
4	0.028*	0.005	16	0.021	0.002	28	0.013	-0.005
	[0.012]	[0.006]		[0.014]	[0.007]		[0.014]	[0.008]
5	0.039**	0.006	17	0.024	0.002	29	0.018	-0.002
	[0.013]	[0.006]		[0.014]	[0.007]		[0.014]	[0.007]

Estimates from a series of linear probability models which also include controls for: gender, ethnicity, age, number of children, age of youngest child relative to cut-off, day of week of birth of youngest child, whether youngest child born on a bank holiday, employment and welfare histories, an indicator for the amount of Income Support received (as a proxy for income), past participation in a particular voluntary welfare-to-work programme (the New Deal for Lone Parents), an indicator of disability, the proportion of formal childcare places available in the lone parents' local area, their local area deprivation score, their local unemployment rate, the proportion of lone parents in their local area who are professionals (as a proxy for their own socio-economic status), and the proportion of workless lone parents in their area with particular education levels (as a proxy for their own education level). We also include a set of local authority dummies and a set of cohort dummies. Full details of all coefficient estimates are available from the authors on request. Standard errors are robust, clustered by day of birth and shown in parentheses.

Table C2 Effect on employment of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas, by NDLP participation

Month relative to cut-off	Ever been on NDLP	Never been on NDLP	Month relative to cut-off	Ever been on NDLP	Never been on NDLP	Month relative to cut-off	Ever been on NDLP	Never been on NDLP
-6	0.016*	0.011**	6	0.054**	0.018**	18	0.024	-0.002
	[0.007]	[0.004]		[0.012]	[0.005]		[0.013]	[0.007]
-5	0.023*	0.007	7	0.056**	0.018**	19	0.018	0.003
	[0.009]	[0.004]		[0.013]	[0.006]		[0.012]	[0.007]
-4	0.020	0.004	8	0.057**	0.020**	20	0.024	0.005
	[0.010]	[0.005]		[0.013]	[0.006]		[0.012]	[0.007]
-3	0.026*	0.007	9	0.058**	0.017**	21	0.023	0.006
	[0.011]	[0.005]		[0.013]	[0.006]		[0.013]	[0.008]
-2	0.029*	0.004	10	0.052**	0.013*	22	0.019	0.002
	[0.011]	[0.005]		[0.013]	[0.007]		[0.013]	[0.007]
-1	0.027*	0.004	11	0.053**	0.015*	23	0.013	-0.0003
	[0.011]	[0.006]		[0.013]	[0.007]		[0.013]	[0.007]
0	0.032**	0.006	12	0.053**	0.011	24	0.0076	0.0002
	[0.012]	[0.005]		[0.013]	[0.007]		[0.013]	[0.007]
1	0.034**	0.008	13	0.038**	0.0044	25	0.011	0.0006
	[0.011]	[0.005]		[0.013]	[0.007]		[0.013]	[0.007]
2	0.044**	0.009	14	0.035*	0.007	26	0.013	-0.00008
	[0.011]	[0.005]		[0.014]	[0.006]		[0.013]	[0.008]
3	0.036**	0.008	15	0.039**	0.004	27	0.021	0.0004
	[0.012]	[0.006]		[0.013]	[0.006]		[0.013]	[0.008]
4	0.047**	0.013*	16	0.040**	0.002	28	0.018	-0.002
	[0.012]	[0.005]		[0.013]	[0.006]		[0.014]	[0.008]
5	0.047**	0.015**	17	0.030*	-0.003	29	0.02	-0.002
	[0.012]	[0.005]		[0.013]	[0.006]		[0.013]	[0.008]

See notes to Table C1.

APPENDIX D SPECIFICATION TESTS

Table D1 Effect of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas, using 28 day window (14 days either side of cut-off)

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	0.003 [0.003]	0.011 [0.007]	6	0.027 [0.013]	0.022 [0.011]	18	0.016 [0.016]	-0.015 [0.013]
-5	0.008 [0.007]	0.013 [0.007]	7	0.027 [0.015]	0.012 [0.014]	19	0.010 [0.015]	-0.014 [0.013]
-4	0.011 [0.009]	0.006 [0.007]	8	0.028 [0.016]	0.021 [0.015]	20	0.003 [0.016]	-0.020 [0.014]
-3	0.004 [0.009]	0.014 [0.009]	9	0.032* [0.014]	0.019 [0.016]	21	0.008 [0.017]	-0.024 [0.013]
-2	0.014 [0.008]	0.011 [0.009]	10	0.033* [0.013]	0.009 [0.017]	22	0.005 [0.018]	-0.020 [0.012]
-1	0.018* [0.007]	0.004 [0.010]	11	0.031 [0.015]	0.008 [0.016]	23	0.004 [0.018]	-0.026* [0.012]
0	0.011 [0.007]	0.005 [0.010]	12	0.028 [0.016]	0.003 [0.016]	24	0.005 [0.018]	-0.026 [0.014]
1	0.012 [0.010]	0.008 [0.012]	13	0.029 [0.015]	0.005 [0.014]	25	0.0005 [0.019]	-0.021 [0.013]
2	0.024 [0.012]	0.015 [0.012]	14	0.026 [0.014]	0.0009 [0.013]	26	-0.008 [0.018]	-0.022 [0.013]
3	0.017 [0.011]	0.012 [0.012]	15	0.023 [0.014]	-0.0003 [0.014]	27	-0.008 [0.017]	-0.027* [0.013]
4	0.021 [0.012]	0.016 [0.011]	16	0.017 [0.014]	-0.007 [0.010]	28	-0.008 [0.015]	-0.029 [0.015]
5	0.020 [0.014]	0.017 [0.011]	17	0.017 [0.014]	-0.016 [0.011]	29	-0.012 [0.014]	-0.027 [0.014]

Estimates from a series of linear probability models which also include controls for: gender, ethnicity, age, number of children, age of youngest child relative to cut-off, day of week of birth of youngest child, whether youngest child born on a bank holiday, employment and welfare histories, an indicator for the amount of Income Support received (as a proxy for income), past participation in a particular voluntary welfare-to-work programme (the New Deal for Lone Parents), an indicator of disability, the proportion of formal childcare places available in the lone parents' local area, their local area deprivation score, their local unemployment rate, the proportion of lone parents in their local area who are professionals (as a proxy for their own socio-economic status), and the proportion of workless lone parents in their area with particular education levels (as a proxy for their own education level). We also include a set of local authority dummies and a set of cohort dummies. Full details of all coefficient estimates are available from the authors on request. Standard errors are robust, clustered by day of birth and shown in parentheses.

Table D2 Effect of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas, using 60 day window (30 days either side of cut-off)

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	0.002	0.012*	6	0.015	0.020**	18	0.009	-0.002
	[0.002]	[0.005]		[0.008]	[0.007]		[0.010]	[0.010]
-5	0.006	0.010	7	0.015	0.017	19	0.004	0.003
	[0.005]	[0.005]		[0.008]	[0.009]		[0.010]	[0.010]
-4	0.006	0.004	8	0.019*	0.023*	20	-0.0004	0.008
	[0.006]	[0.006]		[0.008]	[0.009]		[0.010]	[0.011]
-3	0.004	0.008	9	0.024**	0.023*	21	0.001	0.006
	[0.006]	[0.007]		[0.007]	[0.010]		[0.011]	[0.011]
-2	0.005	0.008	10	0.026**	0.018	22	-0.0005	0.004
	[0.005]	[0.008]		[0.008]	[0.010]		[0.012]	[0.010]
-1	0.0008	0.004	11	0.024**	0.016	23	0.003	0.002
	[0.006]	[0.008]		[0.008]	[0.010]		[0.011]	[0.010]
0	-0.003	0.010	12	0.021*	0.016	24	0.006	0.0002
	[0.006]	[0.007]		[0.009]	[0.011]		[0.012]	[0.011]
1	-0.004	0.011	13	0.021*	0.010	25	0.003	0.003
	[0.007]	[0.008]		[0.009]	[0.010]		[0.012]	[0.011]
2	0.004	0.014	14	0.016	0.007	26	0.0011	-0.0008
	[0.008]	[0.007]		[0.009]	[0.010]		[0.011]	[0.011]
3	0.004	0.012	15	0.013	0.006	27	0.002	-0.003
	[0.008]	[0.008]		[0.009]	[0.010]		[0.011]	[0.011]
4	0.006	0.018*	16	0.009	0.004	28	0.002	-0.003
	[0.008]	[0.007]		[0.009]	[0.008]		[0.010]	[0.012]
5	0.007	0.021**	17	0.010	-0.0007	29	0.002	-0.008
	[0.009]	[0.007]		[0.009]	[0.009]		[0.010]	[0.011]

See notes to Table D1.

Table D3 Effect of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas, using 180 day window (90 days either side of cut-off)

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	0.0002	0.008**	6	0.019**	0.027**	18	0.008	0.009
	[0.002]	[0.003]		[0.005]	[0.004]		[0.005]	[0.006]
-5	0.004	0.010**	7	0.018**	0.027**	19	0.004	0.009
	[0.003]	[0.003]		[0.005]	[0.005]		[0.005]	[0.006]
-4	0.0008	0.009*	8	0.021**	0.029**	20	0.005	0.010
	[0.003]	[0.004]		[0.005]	[0.005]		[0.005]	[0.006]
-3	0.0009	0.008*	9	0.023**	0.027**	21	0.004	0.011
	[0.003]	[0.004]		[0.005]	[0.005]		[0.006]	[0.007]
-2	0.002	0.008	10	0.022**	0.026**	22	0.002	0.006
	[0.003]	[0.004]		[0.005]	[0.006]		[0.006]	[0.006]
-1	0.0009	0.009	11	0.020**	0.026**	23	0.005	0.005
	[0.004]	[0.005]		[0.005]	[0.006]		[0.006]	[0.006]
0	-0.004	0.009*	12	0.019**	0.024**	24	0.005	0.004
	[0.004]	[0.004]		[0.005]	[0.006]		[0.006]	[0.006]
1	0.00009	0.014**	13	0.018**	0.019**	25	0.002	0.004
	[0.005]	[0.005]		[0.005]	[0.006]		[0.006]	[0.006]
2	0.007	0.017**	14	0.011*	0.019**	26	0.001	0.003
	[0.004]	[0.004]		[0.005]	[0.006]		[0.006]	[0.006]
3	0.010*	0.016**	15	0.014**	0.018**	27	0.001	0.004
	[0.005]	[0.005]		[0.005]	[0.006]		[0.006]	[0.007]
4	0.012*	0.022**	16	0.010	0.016**	28	0.002	0.0005
	[0.005]	[0.004]		[0.005]	[0.006]		[0.006]	[0.007]
5	0.013**	0.024**	17	0.007	0.010	29	0.003	-0.0003
	[0.005]	[0.004]		[0.005]	[0.006]		[0.006]	[0.007]

See notes to Table D1.

Table D4 Effect of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas, using 120 day window and a quadratic in age

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	-0.0004 [0.003]	0.010 [0.005]	6	0.013 [0.009]	0.021** [0.008]	18	0.007 [0.010]	-0.001 [0.009]
-5	0.006 [0.005]	0.007 [0.006]	7	0.015 [0.010]	0.017 [0.010]	19	0.003 [0.010]	0.003 [0.009]
-4	0.006 [0.006]	0.002 [0.007]	8	0.017 [0.009]	0.023* [0.010]	20	0.001 [0.010]	0.008 [0.010]
-3	0.001 [0.006]	0.007 [0.008]	9	0.025** [0.008]	0.025* [0.010]	21	0.001 [0.012]	0.005 [0.010]
-2	0.001 [0.006]	0.008 [0.008]	10	0.026** [0.008]	0.019 [0.011]	22	-0.0006 [0.012]	0.003 [0.009]
-1	-0.0005 [0.006]	0.004 [0.008]	11	0.026** [0.009]	0.018 [0.010]	23	0.002 [0.012]	0.0007 [0.010]
0	-0.007 [0.006]	0.010 [0.007]	12	0.019 [0.010]	0.016 [0.010]	24	0.005 [0.012]	-0.0007 [0.010]
1	-0.006 [0.008]	0.010 [0.008]	13	0.019 [0.010]	0.009 [0.009]	25	0.005 [0.012]	0.005 [0.010]
2	0.003 [0.009]	0.013 [0.008]	14	0.017 [0.009]	0.007 [0.009]	26	0.0003 [0.012]	0.003 [0.010]
3	0.003 [0.008]	0.009 [0.009]	15	0.014 [0.009]	0.005 [0.009]	27	0.004 [0.012]	0.0002 [0.010]
4	0.003 [0.009]	0.017* [0.008]	16	0.010 [0.009]	0.002 [0.008]	28	0.003 [0.011]	-0.002 [0.011]
5	0.004 [0.009]	0.021* [0.008]	17	0.010 [0.009]	-0.002 [0.008]	29	0.004 [0.010]	-0.006 [0.011]

See notes to Table D1.

Table D5 Effect of youngest child being born before (rather than after) 1 September cut-off in Policy 1 areas, omitting children born up to a week either side of the discontinuity

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	-0.0009 [0.003]	0.010* [0.005]	6	0.018** [0.007]	0.020** [0.007]	18	0.0005 [0.008]	0.003 [0.009]
-5	0.005 [0.004]	0.006 [0.005]	7	0.015* [0.007]	0.025** [0.007]	19	-0.007 [0.008]	0.005 [0.009]
-4	-0.002 [0.005]	0.005 [0.005]	8	0.019** [0.006]	0.024** [0.007]	20	-0.002 [0.008]	0.011 [0.009]
-3	0.0003 [0.005]	0.005 [0.006]	9	0.017* [0.007]	0.019** [0.007]	21	-0.005 [0.008]	0.014 [0.010]
-2	-0.005 [0.005]	0.002 [0.007]	10	0.014* [0.007]	0.017* [0.008]	22	-0.007 [0.009]	0.007 [0.010]
-1	-0.008 [0.005]	0.006 [0.008]	11	0.012 [0.007]	0.019* [0.008]	23	-0.004 [0.008]	0.007 [0.010]
0	-0.010 [0.006]	0.008 [0.007]	12	0.011 [0.007]	0.013 [0.009]	24	-0.002 [0.009]	0.008 [0.010]
1	-0.007 [0.007]	0.009 [0.007]	13	0.007 [0.007]	0.006 [0.008]	25	-0.007 [0.009]	0.007 [0.010]
2	0.002 [0.006]	0.010 [0.006]	14	0.003 [0.009]	0.010 [0.008]	26	-0.007 [0.009]	0.007 [0.010]
3	0.006 [0.007]	0.008 [0.006]	15	0.006 [0.008]	0.007 [0.008]	27	-0.003 [0.009]	0.010 [0.010]
4	0.007 [0.007]	0.015* [0.007]	16	-0.00003 [0.009]	0.008 [0.008]	28	-0.004 [0.009]	0.007 [0.010]
5	0.011 [0.007]	0.016* [0.007]	17	-0.0005 [0.009]	0.003 [0.008]	29	-0.0005 [0.009]	0.009 [0.010]

See notes to Table D1.

APPENDIX E PLACEBO TESTS

Table E1 Effect of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas, for sample of children turning two

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	-0.002 [0.002]	0.002 [0.002]	6	-0.0008 [0.006]	0.003 [0.004]	18	-0.003 [0.006]	0.010* [0.004]
-5	-0.003 [0.003]	0.005 [0.003]	7	-0.002 [0.006]	0.005 [0.005]	19	-0.003 [0.006]	0.005 [0.004]
-4	-0.005 [0.003]	0.002 [0.003]	8	-0.006 [0.006]	0.004 [0.005]	20	-0.0002 [0.006]	0.007 [0.004]
-3	-0.009* [0.004]	0.003 [0.003]	9	-0.002 [0.006]	0.005 [0.005]	21	-0.005 [0.006]	0.005 [0.004]
-2	-0.007 [0.004]	0.002 [0.003]	10	-0.001 [0.006]	0.004 [0.005]	22	-0.002 [0.006]	0.007 [0.004]
-1	-0.009* [0.005]	-0.001 [0.003]	11	0.00007 [0.006]	0.006 [0.004]	23	0.0007 [0.006]	0.006 [0.005]
0	-0.006 [0.004]	-0.0006 [0.004]	12	0.002 [0.006]	0.008 [0.005]	24	0.0006 [0.006]	0.011* [0.005]
1	-0.008 [0.004]	0.002 [0.004]	13	0.002 [0.006]	0.006 [0.005]	25	0.0006 [0.006]	0.012* [0.005]
2	-0.003 [0.005]	0.003 [0.005]	14	0.0001 [0.006]	0.006 [0.005]	26	0.003 [0.006]	0.011* [0.005]
3	-0.005 [0.005]	0.002 [0.004]	15	0.001 [0.006]	0.005 [0.005]	27	0.003 [0.006]	0.013* [0.005]
4	-0.004 [0.005]	0.005 [0.004]	16	0.00002 [0.006]	0.005 [0.005]	28	0.0003 [0.006]	0.012* [0.005]
5	-0.005 [0.006]	0.006 [0.004]	17	-0.002 [0.006]	0.008 [0.005]	29	0.002 [0.006]	0.014** [0.005]

Estimates from a series of linear probability models, which also include controls for: gender, ethnicity, age, number of children, age of youngest child relative to cut-off, day of week of birth of youngest child, whether youngest child born on a bank holiday, employment and welfare histories, an indicator for the amount of Income Support received (as a proxy for income), past participation in a particular voluntary welfare-to-work programme (the New Deal for Lone Parents), an indicator of disability, the proportion of formal childcare places available in the lone parents' local area, their local area deprivation score, their local unemployment rate, the proportion of lone parents in their local area who are professionals (as a proxy for their own socio-economic status), and the proportion of workless lone parents in their area with particular education levels (as a proxy for their own education level). We also include a set of local authority dummies and a set of cohort dummies. Full details of all coefficient estimates are available from the authors on request. Standard errors are robust, clustered by day of birth and shown in parentheses.

Table E2 Effect of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas, for sample of children turning six

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	-0.0008 [0.002]	0.002 [0.004]	6	0.003 [0.007]	0.0003 [0.006]	18	-0.0007 [0.008]	-0.007 [0.006]
-5	-0.002 [0.003]	0.006 [0.005]	7	0.003 [0.007]	0.002 [0.006]	19	0.0009 [0.008]	-0.008 [0.007]
-4	-0.0007 [0.004]	0.005 [0.005]	8	0.001 [0.007]	-0.0007 [0.007]	20	0.004 [0.009]	-0.008 [0.007]
-3	-0.002 [0.004]	0.004 [0.005]	9	-0.002 [0.008]	-0.003 [0.007]	21	0.003 [0.009]	-0.007 [0.007]
-2	0.001 [0.005]	0.002 [0.005]	10	0.0001 [0.008]	-0.006 [0.007]	22	0.003 [0.009]	-0.006 [0.007]
-1	-0.0002 [0.005]	0.0004 [0.005]	11	-0.0003 [0.007]	-0.010 [0.006]	23	0.003 [0.009]	-0.003 [0.008]
0	0.0004 [0.006]	-0.002 [0.005]	12	-0.004 [0.007]	-0.008 [0.007]	24	-0.005 [0.009]	-0.006 [0.008]
1	0.003 [0.006]	0.0004 [0.005]	13	-0.007 [0.007]	-0.009 [0.006]	25	-0.005 [0.009]	-0.002 [0.008]
2	0.005 [0.007]	0.0003 [0.006]	14	-0.008 [0.008]	-0.009 [0.007]	26	-0.005 [0.009]	-0.005 [0.008]
3	0.003 [0.007]	0.004 [0.006]	15	-0.010 [0.008]	-0.007 [0.006]	27	-0.005 [0.009]	-0.003 [0.008]
4	0.004 [0.007]	0.002 [0.006]	16	-0.006 [0.008]	-0.006 [0.006]	28	-0.007 [0.009]	-0.003 [0.008]
5	0.002 [0.007]	0.0007 [0.006]	17	-0.007 [0.008]	-0.008 [0.006]	29	-0.005 [0.009]	-0.0009 [0.008]

See notes to Table E1.

Table E3 Effect of youngest child being born immediately before (rather than immediately after) 1 September cut-off in Policy 1 areas, for sample of children turning ten

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	0.005 [0.003]	-0.0009 [0.005]	6	-0.002 [0.010]	-0.014 [0.008]	18	-0.011 [0.010]	-0.014 [0.010]
-5	-0.00009 [0.004]	-0.006 [0.006]	7	-0.002 [0.010]	-0.014 [0.009]	19	-0.010 [0.010]	-0.005 [0.009]
-4	-0.0004 [0.006]	-0.008 [0.007]	8	-0.003 [0.010]	-0.017 [0.009]	20	-0.008 [0.011]	-0.007 [0.009]
-3	0.002 [0.006]	-0.009 [0.007]	9	-0.003 [0.010]	-0.015 [0.009]	21	-0.003 [0.011]	-0.004 [0.009]
-2	-0.0005 [0.007]	-0.010 [0.007]	10	-0.004 [0.011]	-0.018* [0.009]	22	0.002 [0.011]	-0.004 [0.009]
-1	0.004 [0.007]	-0.010 [0.007]	11	-0.009 [0.011]	-0.021* [0.009]	23	-0.001 [0.011]	-0.006 [0.009]
0	0.004 [0.007]	-0.008 [0.007]	12	-0.008 [0.011]	-0.021* [0.009]	24	-0.002 [0.011]	-0.009 [0.009]
1	0.006 [0.007]	-0.010 [0.008]	13	-0.007 [0.011]	-0.017 [0.009]	25	-0.001 [0.011]	-0.004 [0.009]
2	0.002 [0.008]	-0.012 [0.008]	14	-0.003 [0.010]	-0.021* [0.009]	26	-0.002 [0.011]	-0.001 [0.009]
3	-0.001 [0.009]	-0.011 [0.008]	15	-0.005 [0.010]	-0.017 [0.009]	27	0.0004 [0.011]	0.001 [0.009]
4	-0.004 [0.009]	-0.012 [0.008]	16	-0.004 [0.010]	-0.015 [0.009]	28	0.002 [0.011]	-0.003 [0.009]
5	-0.004 [0.009]	-0.014 [0.008]	17	-0.011 [0.010]	-0.016 [0.009]	29	0.004 [0.011]	-0.004 [0.009]

See notes to Table E1.

Table E4 Effect of youngest child being born immediately before (rather than immediately after) 1 March cut-off in Policy 1 areas (which has no bearing on eligibility)

Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work	Month relative to cut-off	Effect on % off benefit	Effect on % in work
-6	-0.002 [0.003]	-0.0008 [0.004]	6	-0.010 [0.008]	0.003 [0.006]	18	-0.004 [0.009]	-0.005 [0.008]
-5	0.002 [0.003]	-0.0004 [0.004]	7	-0.011 [0.007]	0.002 [0.007]	19	-0.002 [0.009]	-0.005 [0.009]
-4	-0.003 [0.004]	-0.001 [0.005]	8	-0.012 [0.007]	0.003 [0.007]	20	-0.004 [0.009]	-0.007 [0.008]
-3	0.0006 [0.005]	0.003 [0.005]	9	-0.010 [0.007]	0.001 [0.007]	21	0.0006 [0.009]	-0.001 [0.008]
-2	0.0010 [0.005]	0.005 [0.005]	10	-0.010 [0.008]	0.00003 [0.007]	22	0.0014 [0.009]	-0.001 [0.008]
-1	-0.001 [0.005]	0.003 [0.005]	11	-0.006 [0.008]	-0.002 [0.007]	23	-0.0006 [0.009]	-0.002 [0.008]
0	-0.0001 [0.006]	0.003 [0.006]	12	-0.009 [0.008]	-0.006 [0.007]	24	0.003 [0.010]	0.001 [0.008]
1	-0.00002 [0.005]	0.006 [0.005]	13	-0.012 [0.008]	-0.001 [0.008]	25	0.010 [0.009]	0.002 [0.008]
2	-0.004 [0.006]	0.005 [0.006]	14	-0.010 [0.008]	-0.004 [0.008]	26	0.010 [0.009]	0.002 [0.008]
3	-0.009 [0.006]	0.002 [0.006]	15	-0.010 [0.009]	-0.003 [0.008]	27	0.008 [0.009]	0.002 [0.008]
4	-0.010 [0.006]	0.00009 [0.006]	16	-0.009 [0.009]	-0.007 [0.007]	28	0.010 [0.010]	0.003 [0.008]
5	-0.008 [0.007]	0.0009 [0.006]	17	-0.008 [0.008]	-0.006 [0.008]	29	0.014 [0.010]	-0.001 [0.008]

See notes to Table E1.