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Abstract

Using an extensive expansion of pre-school childcare in Wales in 2019 this paper explores whether access to free childcare increases parental employment. Our analysis is based on two alternative identification strategies applied to rich household data from the Annual Population Survey. First, we use a regression discontinuity design to exploit eligibility cut-offs based on the child's date of birth. Second, we apply a staggered difference-in-differences approach leveraging the phased spatial rollout of the policy during its trial period. We find no evidence of an impact of free pre-school childcare on parental employment using either approach. Moreover, this is true for mothers, parents with

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relatively low education and for parents whose youngest child is eligible, where more pronounced effects might be anticipated. Our evidence therefore questions the effectiveness of the policy in increasing parental employment.

JEL classification: J13; J21; J22.

Keywords: parental employment, childcare policy, regression discontinuity design, staggered difference-in-differences.

1. Introduction

Given widespread evidence of a motherhood pay penalty, which arises at least partly due to reductions in labour supply around childbirth (for the UK, see Costa Dias *et al*., 2021), it is unsurprising that, across countries, governments provide support for childcare with the aim of enhancing maternal employment. Yet, possibly as a consequence of the range of different policy interventions and diversity of institutional settings the international evidence on its effectiveness is far from consistent, with estimates varying from significant positive employment effects (Berlinski and Galiani, 2007; Lefebvre and Merrigan, 2008; Baker *et al*., 2008; Bauernschuster and Schlotter, 2015; Bettendorf *et al*., 2015; Nollenberger and Rodriguez-Planas, 2015) to minimal impact (Lundin *et al*., 2008; Fitzpatrick, 2010; Havnes and Mogstad, 2011; DeMalach and Schlosser, 2026). In the UK context, the evidence, albeit scarce, is also mixed (see, for example, Blanden *et al*., 2016; Brewer *et al*., 2022), likely reflecting differences in approaches to evaluation and specific details of the policy interventions. We contribute new evidence to this debate by evaluating the introduction of a 2019 childcare policy which considerably extended the provision of free childcare to parents of pre-school children in Wales (Childcare Offer for Wales).[1]

The design of the policy provides an opportunity to do so using two alternative identification strategies. First, we adopt a regression discontinuity design (hereinafter, RDD) widely applied in the literature (for example, Fitzpatrick 2010; Goux and Maurin, 2010; Brewer *et al*., 2022) to exploit differences in eligibility based on the child's date of birth to assess the impact of having 30 hours free childcare per week on parental employment. Building on an alternative difference-in-differences (hereinafter, DiD) approach that leverages spatial and temporal variation in the policy, we supplement our

[1] Wales is one of the four nations that make up the UK. Childcare policy is a devolved issue, facilitating comparisons across UK nations.

RDD analysis with a staggered DiD approach based on the policy's phased trial rollout across local areas (for a similar approach see Berlinski and Galiani, 2007; Baker *et al*., 2008; Cascio, 2009; Havnes and Mogstad, 2011; Nollenberger and Rodriguez-Planas, 2015; DeMalach and Schlosser, 2026). In this case our focus is on the extension of free pre-school childcare, relative to the pre-policy minimum of 10 hours. We apply each approach to comprehensive nationally representative data, from the Annual Population Survey (hereinafter, APS), to provide the first causal estimate of the impact of the policy.[2]

International evidence highlights heterogeneity in the impact of childcare policies across parental characteristics such as gender (Bettendorf *et al*., 2015; Brewer *et al*., 2022) and education (Anderson and Levine, 1999), as well as family structure, including single parents (Gelbach 2002; Goux and Maurin, 2010) and whether eligibility relates to the youngest child (Cascio, 2009; Berlinski *et al*., 2011; Bauernschuster and Schlotter, 2015; Brewer *et al*., 2022).[3] Therefore, in addition to providing overall estimates, we explore heterogeneity on the basis of four characteristics through which we anticipate the impact might vary including parental gender, cohabitation status and education, and whether the youngest child is affected by the policy.

Our results are consistent across our two alternative empirical approaches and show no significant impact of free pre-school childcare on parental employment in aggregate, or for any of the subgroups we consider. This is despite eligibility being tied to parental employment. Our evidence therefore reinforces existing questions as to the effectiveness of subsidies to childcare as a mechanism to support parental labour supply (see, for

[2] Annual evaluations are undertaken by the Welsh Government based on surveys of providers and users (see, for example, Glyn *et al*., 2022).
[3] Fitzpatrick (2012) consider their interaction, finding positive labour supply effects for the youngest child of single mothers.

example, Fitzpatrick, 2010; Havnes and Mogstad, 2011). This is particularly important in the context of ongoing decisions about the expansion of childcare policy in the UK.[4]

In providing the first causal analysis of the Childcare Offer for Wales on parental employment our analysis contributes new evidence to the existing international literature based on quasi-experimental methods in which the impact of childcare support appears to depend on the nature of the scheme and institutional environment. Our findings suggest that the provision of 30 hours free childcare when the child is aged three has limited impact on parental employment, possibly reflecting relatively high pre-policy levels of employment. Given the similar institutional context and focus on an extension to free childcare, our analysis is most closely related to that of Brewer *et al*. (2022) for England. However, Brewer *et al*. (2022) evaluate the provision of part-time pre-school childcare (average 15 hours per week for 38 weeks) and distinguish this from full-time childcare (defined as 30-35 hours per week for 39 weeks) at the start of formal schooling, based on longstanding policies in 2011. In contrast, our focus is on full-time pre-school childcare (at age three) and a more recent (2019) *change* in childcare policy in Wales. Further, due to the trial element of the policy design in Wales, we are able to employ an additional identification strategy to the RDD approach employed by Brewer *et al*. (2022).

The remainder of this paper is structured as follows. Section 2 provides an overview of the introduction of the Childcare Offer in Wales which extended free childcare provision for those with pre-school children. Section 3 summarises existing evidence evaluating the impact of childcare policy on parental employment, particularly in the UK. Section 4 introduces the APS data and measures employed in the analysis. Section 5 sets out our

[4] The UK government expanded funded childcare in England in 2024. In contrast, in Wales the expansion of childcare was through Flying Start (see Section 2). Further expansion remains a live debate in Wales, including in relation to the 2026 Senedd Elections. See, for example: Plaid promises free childcare if it wins Senedd election - BBC News

approach and results relating to our RDD which exploits eligibility defined on the child's date of birth. Section 6 presents analysis based on a staggered DiD approach applied to the phased trial rollout of the policy across local areas. We discuss our results in Section 7 and Section 8 briefly concludes.

2. The Childcare Offer in Wales

Wales shares many features with the broader UK economic and institutional context, including a largely common taxation and welfare benefit system, minimum wage policy and equality legislation. Therefore, relative to international comparisons, Wales shares the broader UK patterns of relatively high female employment rates—albeit too a high proportion in part-time employment—alongside relatively high private childcare costs and greater reliance on informal childcare.[5,6] Before the 2017 expansion of pre-school childcare in England, childcare policy in England and Wales was relatively similar, with a minimum of 10 free hours of early years education available in Wales and an average of 15 hours in England.[7] The 2019 Childcare Offer for Wales considerably extended the provision of free childcare for parents of pre-school children. Post-reform working parents of three and four year olds in Wales became entitled to 30 hours free pre-school childcare

[5] Additional support is available for low-income parents, through for example, Universal Credit, which covers up to 85% of childcare costs.

[6] Given the largely common institutional background, there are no obvious reasons why the policy would have a different impact in Wales relative to the rest of the UK. Wales is, however, a relatively deprived nation of the UK, with lower levels of employment and average earnings than England.

[7] In 2017 free childcare for working parents of 3 and 4-year-olds was expanded in England to 30 hours per week for 38 weeks per year.

per week for up to 48 weeks a year.[8,9,10] Importantly, the provision is comparable to (if not more generous than) childcare provided by full-time schooling (which typically commences at the start of the September term after the child's fourth birthday), allowing parents to make long-term plans for engagement in the labour market. Indeed, increasing parental employment, particularly maternal employment, was a core aim of the policy (Coates and Prosser, 2017).[11] Full national roll out of the Childcare Offer was in April 2019.[12] Apart from the gradual rollout of Universal Credit (which replaced Childcare Tax Credit), there were no other national policy changes during this time.[13]

Prior to the April 2019 national implementation, the Childcare Offer was gradually rolled out across trial areas from July 2017 (see Online Appendix Figure B1).[14] No rationale was given for the selection of trial local authorities, which define the councils providing local government services. The policy was uniformly introduced across wards within fourteen Local Authorities. However, in the remaining eight local authorities a staggered rollout across wards was adopted, based on a variety of factors, including demographics, economic performance, and the availability of childcare facilities.[15] We

[8] Parents, defined as those with parental responsibility in the household, are eligible if they are resident in Wales, and are employed or self-employed, earning at least the equivalent of 16 hours a week at the National Minimum Wage, up to £100,000 gross per year. It was not until September 2022 (beyond our period of analysis) that eligibility extended to parents enrolled in publicly funded tertiary education courses and to those on long-term sick leave.

[9] The Childcare Offer operates alongside Flying Start which is targeted at children in disadvantaged areas. For children aged two and three, part-time childcare (12.5 hours, offered as 2.5 hours per day) is provided among a package of measures in Flying Start areas (which are not conditional on parental employment). For those eligible, our estimates will therefore reflect the additional impact of the Childcare Offer over Flying Start.

[10] While similar to the earlier reform in England, the Childcare Offer in Wales is relatively more generous, with 48 weeks coverage (10 weeks longer than England), anticipated to increase the impact on employment.

[11] Other aims included increasing disposable income of those already employed, reducing poverty among individuals in low-paid jobs, and promoting child development and school readiness.

[12] Full implementation of the Offer was initially planned for September 2020 but was brought forward.

[13] The gradual implementation of Universal Credit was, to our knowledge, unrelated to the trial implementation of the Childcare Offer which we exploit as part of our identification strategy.

[14] Trial areas are defined electoral wards (represented by local councillors), of which there are 762 in Wales. These are relatively small geographic areas that lie within the 22 Local Authorities within Wales. For further details see: Wales - Office for National Statistics

[15] Cardiff, Conwy, Neath Port Talbot, Newport, Rhondda Cynon Taf, Swansea and Wrexham.

utilise the trial element, which was based on the same terms and conditions as the national implementation, as an alternative complementary method to evaluate the scheme and consider the potential influence of this selection bias in the analysis which follows.[16]

While no evidence exists on the causal impact of the Childcare Offer on parental employment, economic theory suggests that a reduction in the cost of childcare increases the incentive to use formal childcare, thereby encouraging labour market participation among those out of work. For those already in work and using formal childcare, a standard model of labour supply would predict that the impact on employment will depend on the outcome of offsetting substitution and income effects associated with the change in the effective wage (that is, which exists after accounting for childcare costs). All else constant, a reduction in the cost of formal childcare reduces hours worked through the income effect, due to an increased the demand for 'leisure', while it increases labour supply through the substitution effect, as greater returns to work raise the opportunity cost of parental childcare. When modelling the likely impact of the policy, Paull and Xu (2015) found that an additional 20 hours of free childcare, with or without a parental work requirement, would not substantially impact maternal employment in Wales due to relatively low utilisation of formal childcare and the modest impact of net income on labour supply.

3. Evaluation of Childcare Policy on Parental Employment

The international literature on the parental employment effects of publicly provided or subsidised childcare is mixed, with effect sizes varying substantially across institutional contexts. Indeed, the magnitude of the employment response appears to depend on pre-

[16] While national roll out could not have been anticipated completely, the trial was designed to develop and expand the scheme consistent with a 2016 manifesto commitment of Welsh Labour, rather than to operate as a temporary arrangement. Further, since parents would only be able to utilise the scheme for a maximum of two years, the absence of a long-term commitment during the initial trial is unlikely to impact on the labour supply response.

reform levels of parental employment and childcare utilisation, as well as the extent to which new public provision crowds out existing private arrangements.

The largest employment effects are typically observed in settings where maternal employment and formal childcare coverage are low pre-policy introduction. Using difference-in-differences approaches, Lefebvre and Merrigan (2008) and Baker *et al*. (2008) compare changes in outcomes for mothers in Quebec before and after the introduction of low-cost universal childcare with changes in other Canadian provinces, estimating positive effects on maternal labour market participation. Similarly, Berlinski and Galiani (2007) and Berlinski *et al*. (2011) report positive effects in Argentina, exploiting the rollout of free public preschool and admission cut-off rules, respectively. Evidence from Germany also points in a similar direction, with Bauernschuster and Schlotter (2015) estimating that kindergarten eligibility increased the labour supply of mothers with children aged three to four by around six percentage points. Across these contexts, pre-reform maternal employment rates among mothers of young children ranged from around 40% in Argentina to 50% in Germany.

By contrast, where maternal employment and childcare attendance are already high prior to reform, expansions in provision or reductions in cost tend to have limited effects on parental employment. Lundin *et al*. (2008) examine a Swedish reform that capped childcare fees as a share of household income in a setting where maternal employment was already 80% and formal childcare use was widespread, finding no effect on maternal employment or hours worked, with increased childcare uptake concentrated among children of non-working parents. Havnes and Mogstad (2011) similarly find no positive employment response to the large-scale expansion of subsidised childcare for three- to six-year-olds in Norway in the late 1970s, despite higher subsidies being directed towards municipalities with lower childcare coverage rates, attributing this to extensive substitution

from informal to formal care among mothers already attached to the labour market, rather than any net increase in labour supply.

In the UK context, several studies have exploited the strict age eligibility criterion common to UK childcare policies to identify parental employment effects using a RDD approach. Using data from the Work and Pensions Longitudinal Study (2000-2004), Brewer and Crawford (2010) find a positive impact of eligibility for full-time primary education on the employment of lone parents, but less evidence of an impact of pre-school childcare. They further find evidence of that the impact is not immediate, but most pronounced eight to nine months post eligibility. Most closely related to our analysis, Brewer *et al*. (2022) evaluate the impact of childcare policy in England before the 2017 expansion. Applying a RDD to 2011 Census data they find no positive effect on employment or labour force participation from free part-time pre-school childcare even for mothers whose youngest child becomes eligible. In contrast, eligibility for free full-time schooling increases maternal (but not paternal) employment, and the influence becomes more pronounced over time.

Brewer *et al*. (2022) supplement their RDD approach by using the longitudinal element of the Labour Force Survey (hereinafter, LFS) to estimate a parental fixed effect DiD model where eligibility remains defined based on month of birth. Based on this approach, they find evidence of positive maternal employment effects for part-time childcare after three terms. Although their focus is on subsequent childhood educational attainment, Blanden *et al*. (2016) also apply a DiD approach to explore the impact of part-time pre-school childcare in England. Using LFS data between 2002-2007 and exploiting geographical variation in the availability of free places, they find that a 10 percentage point increase in coverage has no effect on maternal employment. Overall, therefore, despite the common institutional context and more similar levels of parental employment, estimates

of the impact of free childcare on parental employment in the UK remain mixed and inconclusive.

4. The Annual Population Survey (APS)

The UK's APS (Office for National Statistics, 2025) is a continuous household survey that provides data on key social and socioeconomic variables, combining observations from the Local LFS with those from the Quarterly LFS.[17] It is designed to enhance the LFS sample within local areas, making it the most reliable data to analyse regional and local labour markets (see, for example, Office for National Statistics, 2026). The survey has run consistently in the UK since 2004 and collects data through the year, facilitating analysis across school terms. In the UK the academic year runs from 1st September to 31st August and is split into three terms: Autumn (September to December), Spring (January to March/April), and Summer (April to July). Throughout, we restrict our analysis to individuals resident in Wales. Information is also available on local authority and ward of residence.[18]

While the precise sample for our two empirical approaches differs (see below), our key variables are defined consistently throughout the analysis. Our focus is on parents of young children, which are identified through relationships between individuals within the household. Given the policy's broad definition of a parent—which includes partners, carers, and grandparents—we follow Brewer *et al*. (2022) in defining parents as the head of household or the spouse or partner of the head, in households containing children or step-children of the head. Eligibility for the Childcare Offer begins on the first day of the first month of each school term following a child's third birthday and, with the exception

[17] The APS has a rotational panel design, combining individuals in waves 1 and 5 of the Quarterly LFS with individuals in waves 1 to 4 of the English, Welsh and Scottish Local LFS.
[18] Information on full dates of birth and wards of residence requires access to restricted versions of the APS via the Secure Data Service.

of holiday provision, extends until the child enters formal schooling—typically the September following their fourth birthday.[19] To determine childcare eligibility, we use complete information on the child's date of birth.

Dependent variables

In terms of labour market outcomes, the APS collects information based on established International Labour Organization (hereinafter, ILO) definitions. We focus on employment which is distinguished from non-employment (including both unemployment and economic inactivity) but, as part of sensitivity analysis, also explore three alternative measures based on usual hours of work in the main job excluding overtime.[20] These are: (i) usual hours of work, defined for all individuals, with non-employed individuals assigned zero hours, following Brewer *et al*. (2022); (ii) usual hours of work, conditional on employment to capture change via the intensive margin; and (iii) an indicator for working 16 or more hours per week (the minimum hours required for eligibility for the Childcare Offer), relative to those working fewer than 16 hours or non-employed.[21]

Explanatory variables

The APS also contains detailed information on parental characteristics that are well-established determinants of parental employment status. Our controls for parental characteristics include sex, age (and age squared), an indicator for having a highest qualification below A-level (low education), a cohabitation dummy indicating the presence of a spouse or partner in the household, and the number of dependent children in the family

[19] Exceptions may arise in cases where schools have multiple intakes per year (rare in Wales) or when parents defer their child's entry to Reception until later in the same year.
[20] We cannot explore labour force participation (which reflects individuals' labour supply decisions, irrespective of whether they result in employment) in addition to employment as a result of the large overlap between measures and risk of statistical disclosure.
[21] Exploring outcomes conditional on employment further restricts our sample size and so limits our ability to explore indicators of work quality.

under age 16.[22] We are further able to control for local authority of residence and time period (calendar month) fixed effects to account for spatial heterogeneity and seasonal trends as appropriate to the method adopted (see below). Throughout we restrict the sample to working-age parents, who report their employment status and have complete information on the explanatory variables outlined above.

5. Exploiting eligibility cut-off based on child's date of birth

5.1 RDD Approach

Following Fitzpatrick (2010), Goux and Maurin (2010) and Brewer *et al.* (2022), our first identification strategy is based on a sharp RDD that exploits the age-based eligibility criterion of the policy. Specifically, we compare the relevant labour market outcome (principally employment status) among parents whose children are just eligible at the start of a school term with those whose children are just outside the eligibility cutoff (and would therefore become eligible in the following term), thereby minimising unobserved differences. Household assignment to the treatment is determined simply by child date-of-birth and the approach therefore identifies the policy impact at the eligibility cutoff.[23] We estimate the impact of the Childcare Offer during its first full year of implementation (April 2019 - March 2020), thereby avoiding the complexity introduced by the COVID-19 pandemic.[24] During this time parents became eligible to access the Childcare Offer at three

[22] We also assess the sensitivity of our results to the inclusion of additional controls for white ethnicity and disability, as well as to the exclusion of proxy responses, where one household member responds on behalf of another.

[23] The scope for manipulating eligibility around the cutoff is limited. Online Appendix Figure A1 shows the density of parents with children born within a 180-day window around the cutoff. It suggests minimal discontinuity in the distribution before and after the eligibility date, which is confirmed by a McCrary (2008) density test. Our evidence is therefore consistent with the assumption that treatment is effectively as-good-as random in a neighbourhood of the cutoff.

[24] Unlike Brewer *et al*. (2022) who focus on a pre-existing policy, the introduction of the childcare policy in Wales was unlikely to have been fully anticipated or built into long-run childcare decisions, making it more likely to impact parental employment.

distinct cutoff points, corresponding to the start of the school terms: 1st April 2019, 1st September 2019 and 1st January 2020.[25]

The sample is restricted to parents around each term's cutoff.[26] Decisions on the bandwidth around the cutoff trade greater precision of the estimates from a larger sample with greater unobserved differences in parents due to children being born further apart. Following Brewer *et al*. (2022) our benchmark specification employs a 90-day bandwidth on either side of the term cutoffs.[27] Despite using the largest household survey in the UK the focus on relatively narrowly defined parental group based on eligibility for childcare means that we are constrained in terms of sample size. Pooling across the three terms yields a total sample of 210 parents, 114 of whom are eligible and 96 of whom are not yet eligible.[28] Online Appendix Table A2 provides details of parental characteristics by eligibility, with those eligible slightly older on average.[29] Average employment rates by eligibility are similar.

Within each term, the (local) average effect of the Childcare Offer can be estimated as follows:

$$Y_{it} = \alpha + \gamma Offer_{it} + f(Days_{it}) + Z_{it}\varphi + \sigma_m + \delta_a + \varepsilon_{it} \qquad (1)$$

where Y_{it}, denotes the relevant labour market outcome of parent of child *i* in time period (month) *t,* $Offer_{it}$ is a binary variable denoting eligibility and $Days_{it}$ forms the running variable capturing the difference in days between the child's date of birth and date of

[25] This has the advantage of allowing us to move beyond the September cutoff, which coincides with the start of formal schooling (albeit one year in the future) and may be associated with annual childcare and labour supply decisions.
[26] Our sample only includes 88 dual parent households, so we focus on individual labour supply decisions, whilst acknowledging the importance of joint decision-making within households.
[27] This choice automatically excludes parents in their second term of childcare eligibility and therefore neglects cumulative or longer-term effects of the policy.
[28] Online Appendix Table A1 provides further details of the sample by term and for subgroups explored as part of heterogeneity analysis.
[29] Online Appendix Table A3, which estimates the same RDD on parental characteristics confirms that eligibility is unrelated to observable parental characteristics.

eligibility. A constant is given by α and ε_{it} denotes a random error. Consistent with Brewer *et al*. (2022), we specify $f(\cdot)$ as a flexible function of the running variable. Specifically, we include both linear and quadratic terms in $Days_{it}$ to capture non-linear effects, along with their interactions with the treatment indicator $Offer_{it}$ allowing the relationships to differ depending on the child's age ($Days_{it}$) relative to the eligibility cutoff. [30] In equation (1), the coefficient γ captures the discontinuity of parental employment at the eligibility cutoff, with the interaction terms allowing for heterogeneity in the treatment effect based on the child's age, under the standard assumption there is no anticipatory behaviour or delays in response.[31] In the absence of information on childcare uptake, our estimate reflects the effect of being offered the treatment (i.e., eligibility for childcare), and thus should be interpreted as the intention-to-treat (hereinafter, ITT) effect, rather than the effect of actual utilisation. While it is typically assumed that parents are similar around the cutoff, in our most comprehensive specification we further control for observable parental characteristics (Z_{it}), calendar month (σ_m) and local authority (δ_a) fixed effects.

Given the nature of our data, which collects information throughout the year, we pool observations across the three terms and normalise the running variable so that zero corresponds to the term-specific cutoff date. Our benchmark specification constrains the impact of the policy to be common across terms, but we subsequently allow it to vary by term by interacting term of eligibility (T_{it}) with $Offer_{it}$ as follows:

[30] Our benchmark specification applies uniform kernel weighting, assigning equal weight to all observations within the bandwidth, but we explore the sensitivity of our estimates to alternative weighting schemes.
[31] As Brewer *et al*. (2022) discuss anticipatory behaviour would lead to bias in an unknown direction.

$$Y_{it} = \alpha + \gamma(Offer_{it}) + \sum_{T_{it}=2}^{T_{it}=3} \theta^T (T_{it} \times Offer_{it}) + f(Days_{it}) + Z_{it}\varphi + \sigma_T + \sigma_m + \delta_a + \varepsilon_{it} \quad (2)$$

All other terms are defined above except, variation in the impact of the policy across term cohorts, which is given by θ^T and term cohort fixed effects, given by σ_T.

5.2 RDD Estimates

We present the RDD estimates for the Childcare Offer based on the sample pooled across terms in Table 1, where we build up the model specification across columns (1)-(5).[32] The estimates confirm a positive and sizeable, but statistically insignificant, impact of the Childcare Offer, regardless of specification.[33] In other words, we find no evidence that 30 hours of free pre-school childcare leads to an increase in parental employment within the first three months of eligibility. In this respect the findings align to the pre-policy predictions of Paull and Xu (2015) and the findings of Blanden *et al*. (2016) and Brewer *et al*. (2022) relating to part-time pre-school childcare in England, despite the enhanced generosity of the scheme in Wales.

[Table 1 here]

We test the sensitivity of our benchmark estimate, based on the most comprehensive specification which includes parental characteristics, local authority and calendar month fixed effects (column (5) in Table 1), to a range of changes in sample and model specification in Online Appendix Table A4. Our findings confirm the absence of an impact of the Childcare Offer on parental employment across school terms (columns (1) and (2))

[32] We present term specific estimates and the model with term interaction effects in Online Appendix Table A4.

[33] While we might be concerned that the coefficients are imprecisely estimated due to the small sample size the coefficients on parental characteristics confirm established patterns. Mothers and parents with lower education exhibit significantly lower employment rates. Cohabiting parents are more likely to be employed, whilst an additional dependent child lowers the likelihood of employment.

and robustness to excluding proxy responses (column (3)) or trial wards (column (4)), and to including controls for additional parental characteristics within the specification (column (5)).[34]

We further test the robustness of our benchmark estimate to the precise details of the RDD approach, including the choice of bandwidth (90 versus 60 days), kernel weighting (uniform versus triangular, where the latter gives greater weight to observations near the discontinuity) and functions of $Days_{it}$ (linear versus quadratic) in Online Appendix Table A5. In all but one case (column (2)) the estimates confirm a statistically insignificant impact of the Childcare Offer on parental employment. It is only under a 90-day bandwidth with uniform weights and linear function of $Days_{it}$ that the estimate becomes significant and then only weakly at the 10% level. Overall, therefore our core finding is also robust to the choice of parameters of the RDD approach.

In a final extension of our benchmark model we explore whether the findings are robust to changes in the outcome, particularly to measures of hours worked. The estimates are presented for each of the three alterative measures of hours in Online Appendix Table A6. When focusing on all working-age individuals we similarly find no evidence of an impact of the Childcare Offer on total hours (column (1)) or the probability of working more than 16 hours (column (3)). When conditioning only on those in employment (column (2)) and focusing on adjustment at the intensive margin, there is evidence of a considerable decline in hours worked per week (-13.2), consistent with an important income effect, but the effect is only weakly significant.

[34] Significant differences between term cohorts might be predicted if children born at different times of the year have a different chance of securing childcare places due to supply-side restrictions (Brewer *et al*., 2022). In trial wards the policy was pre-existing rather than new in April 2019 which might affect anticipation effects.

Given previous evidence highlights the importance of heterogeneity in the impact of childcare policies we explore whether our findings differ by parental characteristics, in particular whether positive employment effects are identified for those who might be expected to benefit most, including mothers, parents whose youngest child is eligible and those with low education levels. We present these estimates, based on our benchmark specification but estimated for each subgroup separately, in Table 2. While the sample sizes are necessarily smaller, which precludes analysis of interactions between these characteristics, we find no evidence of significant employment effects among any of the subgroups we consider.[35] In all cases the coefficient estimates are positive but not statistically significant, consistent with the absence of an impact of the Childcare Offer even among those groups where we might expect the impact would be more pronounced.[36]

[Table 2 here]

6. Exploiting the phased geographical trial rollout

6.1 Staggered DiD Approach

To explore the robustness of our findings to the RDD approach we complement our analysis in Section 5 with a staggered DiD approach which exploits the phased geographical trial rollout of the Childcare Offer across Welsh Wards from July 2017 to April 2019 for identification.[37] The phased spatial rollout provides a unique source of variation, enabling a comparison of trends in parental employment before and after the introduction of the trial, between those who reside in wards that received the Childcare

[35] Given the small sample size for single parents, we are only able to present estimates for the larger cohabiting group.
[36] Estimates based on a corresponding model which pools across the relevant groups but includes an interaction between the group and the Offer confirm that there are no significant differences in the impact of the Offer by these characteristics (results available upon request).
[37] Precise dates were obtained from Freedom of Information requests to the Welsh Government and individual local authorities. See Online Appendix Figure B1 for details.

Offer with those in wards that received it later, with the latter group remaining eligible for the pre-existing 10-hour minimum childcare allocation.

To do this, we utilise data from an extended pre-policy introduction period, pooling wave 1 observations from APS data between January 2016 and March 2019. We focus only on parents with eligible pre-school children, that is, children aged three or four who have not yet started full-time schooling. Parents are grouped based on the timing of the rollout of the Offer in their ward of residence.[38] Parents residing in (non-trial) wards that received the Offer at the point of national rollout (April 2019) serve as the not-yet-treated (control) group for parents in all trial wards as they remain untreated across the entire sample period.[39] We are, however, forced to restrict our analysis to December 2018 given the small size of the April 2019 not-yet-treated group in the January 2019 term. Our analysis is therefore focused on four trial groups (September 2017, January 2018, April 2018, and September 2018). Our sample contains 355 observations from trial areas (58%) and 225 observations from the non-trial areas. Among the former, 280 observations are observed pre-offer, and 75 are post-offer.[40] Online Appendix Table B2 provides summary statistics for parental characteristics in the trial and non-trial areas. Average employment rates are similar between the trial and non-trial areas, at about 75%. Parental characteristics are also comparable, although the trial group is slightly older and has more dependent children on average. Within the trial area the employment rate is lower post-offer relative to pre-offer.

[38] The eight possible groups are: July 2017 (10 wards), September 2017 (26 wards), January 2018 (30 wards), April 2018 (149 wards), September 2018 (181 wards), November/December 2018 (24 wards), January 2019 (250 wards), and April 2019 (194 wards). However, we exclude the July 2017 and November 2018/December 2018 groups from the analysis since they do not coincide with the start of school terms which we use to define our staggered DiD time periods.

[39] Although we test the sensitivity of the results to using those not-yet-treated by the end of the relevant term as the control (see below).

[40] Online Appendix Table B1 provides details of the sample by the timing of trial rollout.

Abstracting from the staggered roll out, our model can be specified in terms of a standard DiD framework as follows:

$$Y_{it} = \alpha + \gamma Trial_i + \omega Post_t + \lambda(Trial_i \times Post_t) + Z_{it}\varphi + \sigma_m + \delta_a + \varepsilon_{it} \quad (3)$$

As above, Y_{it}, denotes the relevant labour market outcome of parent of child i in time period *t,* $Trial_i$ is a binary variable denoting residence in a trial ward, $Post_t$ denotes being observed post introduction of the trial within the specific ward. The interaction between $Trial_i$ and $Post_t$ therefore captures eligibility for the Offer based on being in a trial ward post-Offer. The DiD estimate, λ, is our coefficient of interest and captures differences in the outcome in trial areas pre- and post- the trial policy introduction, relative to non-trial areas. A greater increase in parental employment in trial wards than non-trial wards would be consistent with $\lambda > 0$. A constant is given by α and ε_{it} denotes a random error. As above, more comprehensive specifications of the model control for parental characteristics Z_{it}, time period (calendar month) (σ_m) and local authority fixed effects (δ_a).

Given the phased nature of the trial roll out across areas, traditional DiD estimates are potentially biased. As such, we adopt the Callaway and Sant'Anna (2021) staggered DiD methodology, which estimates the dynamic impact of potentially heterogeneous staggered treatment effects.[41,42] The approach estimates group (g) and time period (term) (t) specific ITT(g, t) as follows:

$$\text{ITT}(g,t) = \text{E}[Y_t(g) - Y_t(0)|G_i = g\ ] \quad (4)$$

[41] In additional analysis we estimate separate DiD models based on the April 2018 and September 2018 elements of the trial rollout (which had the greatest coverage), but the findings are consistent (results are available from the authors upon request).

[42] The Callaway and Sant'Anna (2021) staggered DiD methodology allows for estimation using alternative approaches, including outcome regression (OR), inverse probability weighting (IPW), and doubly robust (DR) methods. In our baseline analysis, we employ the outcome regression (OLS-based) approach, as recommended by the authors in settings with a relatively small number of treated units.

The overall ITT of the Childcare Offer on parental employment is a weighted average of the group-time-specific $ITT(g,t)$.[43] Mirroring an event-study specification, we also present aggregate dynamic estimates by term, where the term before the rollout in each group is used as the reference. This results in eight leads which allow us to explore changes in parental employment prior to/in anticipation of the policy implementation, and one lag, with post Childcare Offer estimates therefore covering up to two terms exposure to treatment, slightly longer than the RDD approach. As in the RDD analysis, we explore both the robustness of our estimates and heterogeneity in the impact by key parental characteristics.

6.2 Staggered DiD Estimates

Table 3 presents the overall ITT of the Childcare Offer on parental employment from the staggered DiD approach for different model specifications (columns (1)-(5)). While the estimates vary across specifications, the overall ITT effect (presented in the first row) is always insignificantly different from zero, that is, there is no evidence that parental employment rates increased after the introduction of the trial policy. Despite the differences in approach, the findings are therefore consistent with the estimates from the RDD. We find no evidence that parental employment responded to the 20 hours additional childcare per week associated with the rollout of the trial of the Childcare Offer.

[Table 3 here]

The associated dynamic effects for our benchmark specification (presented in column (2)), which contains parental characteristics but does not include local authority or month fixed effects given they capture elements of the roll out of the trial, are presented in Figure 1. Estimates of the policy are insignificant both in the term of implementation and

[43] Weights are proportional to the size of each treatment cohort, following Callaway and Sant'Anna (2021).

the subsequent term, and while we cannot exclude the possibility of longer-term impacts, this does not suggest a lagged response, consistent with the RDD estimates being insightful despite their short-term focus. Despite the significance of the estimate 4 terms pre-offer, the average pre-offer coefficient is not statistically significant from zero, consistent with the conditional parallel trend assumption. We also find no clear evidence of anticipation effects (aligned to Brewer *et al*., 2022).

In a similar manner to Section 5 we explore the robustness of our benchmark findings to a range of changes in the sample and model specification, particularly differences in the treatment and control groups. The coefficients in Online Appendix Table B3 confirm that the overall ITT is not significantly different from zero in specifications which exclude proxy interviews (column (1)), include the additional parental controls (column (2)), focus exclusively on the two largest treatment groups (April and September 2018) and change the control group (column (4)). More specifically, we extend the (April 2019) control group to (1) include all those not yet treated and (2) to include those treated in January 2019. We also estimate the corresponding models based on hours of work in Online Appendix Table B4 and find no significant effect on any of our three measures.[44]

We further explore heterogeneity in the staggered DiD estimates by estimating the ITT for employment on the subsamples considered above. Our estimates are presented in Table 4 and confirm the absence of an impact of the Childcare Offer on employment among those we might expect it to affect most, including mothers (column (1)), those with low education (column (3)) and where the youngest child is eligible (column (4)). As with the RDD analysis, our results therefore suggest the absence of an impact of the trial roll out of the

[44] Consistent with Section 5, there is a negative sign on the ITT for hours worked conditional on employment (column (2)) but it is not statistically significant.

Childcare Offer on parental employment even among those who we might anticipate to be most affected.

[Table 4 here]

7. Discussion

Our findings across the two alternative approaches are consistent and point to the absence of an impact on parental employment of additional free pre-school childcare. This is also true among subgroups with lower employment rates where we might expect the impact to be more pronounced. While consistent with simulations (Paull and Xu, 2015) and analysis of part-time childcare in England (Blanden *et al*., 2016; Brewer *et al*., 2022) they contrast with the positive effects identified in relation to full-time childcare in England (Brewer *et al*., 2022).[45] Despite obvious differences in the spatial and temporal focus of our study, the latter might suggest that the impact identified by Brewer *et al.* (2022) is, at least in-part, driven by the focus on the well-established timing of the transition to full-time schooling rather than the provision of additional free childcare *per se.*[46] More generally, our results suggest there needs to be caution when interpreting the findings of studies even within the same institutional context, beyond the specific policy and evaluation approach.[47]

In terms of understanding the reasons for our results, our analysis does not suggest that our focus on employment status conceals positive impacts on hours as we also find no evidence of employment gains defined at a minimum of 16 hours. Our findings based on hours of work, if anything, suggest a negative impact at the intensive margin. We therefore explore some potential alternative reasons for our findings. First, employment effects

[45] It is also consistent with evaluation evidence based on surveys of users of the Childcare Offer who suggest their hours of work had not changed (Glover *et al*., 2018; Glynn *et al*., 2022).
[46] This might include long-term planned changes in labour supply, as well as psychological effects. The age of the child at the start of full-time schooling is also at least one year older than we consider here.
[47] Female employment rates have also continued to increase, particularly for mothers, since 2011 (Roantree and Vira, 2018).

would be limited by a lack of parental awareness and/or uptake of the scheme. Evidence from the Welsh Government suggests that about half of eligible parents take up the offer (Bevan Foundation and the Joseph Rowntree Foundation, 2024). This is lower than corresponding rates in England and Scotland and would provide a potential reason for more modest parental employment gains. Similarly, the policy impact would also be more limited in the presence of childcare capacity constraints. However, we find no evidence of this in survey evidence from childcare providers, who instead suggested spare capacity (Glover *et al*., 2018).[48] The impact on employment is likely to be a function of the relative generosity of the scheme. The Childcare Offer in Wales is relatively generous in terms of both hours per week (30) and weeks per year (48) (even when compared to full-time schooling) and represents significant additionality over pre-existing childcare support (10 hours for 39 weeks). As such, we do not attribute the results to a lack of work incentives provided by the policy, although the parental employment response might be reduced by the focus on younger (pre-school) children. Instead, we suggest that it is more plausible that the policy had limited impact due to high pre-existing employment rates of parents with children at age three (for similar arguments see Bettendorf *et al*., 2015), combined with relatively high utilisation of formal or informal non-parental childcare which could be substituted with free formal childcare care (see, for example, Havnes and Mogstad, 2011).[49] That is the policy might simply act to crowd-out existing child care provision. A final explanation might lie in our focus on the relatively short-term impact of the policy. While the existing evidence suggests a possible delay in the parental labour supply

[48] One disadvantage of focusing on the trial and policy introduction is, however, the greater likelihood of capacity constraints.

[49] Data from the LFS April-June 2017 indicate that employment rates for men and women with dependent children aged 3-4 are 95.5% and 73.0% respectively (ONS, 2017). Data from the Family Resources Survey (2015/16) indicate that approximately 70% (30%) of parents with at least one 3-4 year old use formal (informal) childcare (Crocker *et al*., 2018).

response to free childcare (Brewer *et al*., 2022) such that we cannot rule out positive effects over the longer term (that is, more than two terms (about six months)), that a child becomes eligible for formal schooling within a relatively short period inevitably restricts the timing of our analysis and is consistent with the nature of the policy.

8. Conclusions

The introduction of the Childcare Offer in Wales in 2019 which substantially extended free childcare for parents of pre-school children provides an opportunity to revisit the contested impact of free childcare on parental employment. The design of the policy allows us to exploit both the age-eligibility criterion and the staggered geographical trial roll-out to evaluate its impact using two complementary quasi-experimental approaches. Using a RDD based on the eligibility cut-off, we assess whether parental employment among those just eligible increased relative to those who become eligible one school term later. Applying a staggered DiD approach we explore whether parental employment increased post policy implementation in trial wards relative to wards where the additional free childcare was not available to parents with similar aged children.

Applying these methods to comprehensive data from the APS we find no evidence that the extensive free pre-school childcare impacted parental employment using either approach. We further find this extends to mothers, those with low education and those whose youngest child is eligible who might be expected to be most affected. Instead, consistent with prior simulations (Paull and Xu, 2015) our findings suggest that parental employment remained largely unaffected by the substantial increase in free pre-school childcare. In this respect the findings are consistent with Brewer *et al*.'s (2022) analysis of part-time pre-school childcare but contrast with the positive maternal employment effects identified at the start of full-time schooling in England. More generally, by removing much of the complexity associated with international comparisons, the analysis illustrates how

differences in policy implementation in devolved contexts can provide opportunities for scrutiny of more nuanced policy differences and approaches to evaluation. In this respect, it emphasises the importance of data collection and evaluation in devolved nations. Indeed, it would be interesting to contrast our findings to future analysis of the 2017 and 2024 expansion of childcare in England.

Our evidence suggests that the Childcare Offer failed to meet its objectives in terms of increasing parental employment. This, however, needs to be considered as part of a broader assessment of the scheme given its multiple objectives. While restricted by data availability, other important potential impacts include changes in the nature of childcare, family disposable income and wellbeing, and later childhood development.

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Table 1. RDD Estimates of Impact of the Childcare Policy on Parental Employment

	(1)	(2)	(3)	(4)	(5)
Offer	0.169	0.152	0.203	0.171	0.275
	(0.268)	(0.199)	(0.202)	(0.260)	(0.255)
Days	-0.000	-0.003	-0.005	0.002	-0.001
	(0.010)	(0.008)	(0.008)	(0.010)	(0.010)
$Days^2$	0.000	-0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Offer x Days	-0.004	0.001	0.002	-0.007	-0.005
	(0.013)	(0.012)	(0.011)	(0.014)	(0.014)
Offer x $Days^2$	0.000	0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
September cohort	0.160**	0.098*	0.129	0.126	0.101
	(0.068)	(0.056)	(0.181)	(0.075)	(0.188)
January cohort	0.172*	0.147**	0.129	0.173	0.102
	(0.092)	(0.062)	(0.133)	(0.084)	(0.148)
Mother	-	-0.140***	-0.139***	-0.151**	-0.151**
		(0.048)	(0.049)	(0.056)	(0.055)
Age	-	0.054*	0.059*	0.071**	0.074**
		(0.030)	(0.033)	(0.031)	(0.034)
Age^2	-	-0.001*	-0.001	-0.001**	-0.001**
		(0.000)	(0.000)	(0.000)	(0.000)
Low education	-	-0.203***	-0.186***	-0.221***	-0.203**
		(0.063)	(0.065)	(0.068)	(0.068)
Cohabitation	-	0.317**	0.315**	0.308**	0.325**
		(0.124)	(0.114)	(0.145)	(0.128)
Number of dependent children	-	-0.058**	-0.057**	-0.066**	-0.069**
		(0.027)	(0.027)	(0.027)	(0.027)
Calendar month fixed effects	No	No	Yes	No	Yes
Local authority fixed effects	No	No	No	Yes	Yes
Adjusted R^2	0.008	0.265	0.259	0.294	0.294
N	210	210	210	210	210

Notes: (i) ITT estimates are based on RDD regressions using a 90-day either side of the cutoff and a flexible function in $Days_{it}$. (ii) Fathers, non-low education (above A-levels), non-cohabiting, the first month of each term, the Cardiff local authority and the April term cohort are the reference categories. (iii) Figures in () are standard errors, clustered at the local authority level. (iv) *p<0.10, **p<0.05, ***p<0.01.

Table 2. Heterogeneity in the RDD Estimate of Impact of the Childcare Policy on Parental Employment

	(1) Mothers	(2) Cohabitation	(3) Youngest child	(4) Low education
Offer	0.328	0.284	0.120	0.167
	(0.351)	(0.321)	(0.289)	(0.786)
Days	-0.003	-0.004	-0.000	-0.020
	(0.015)	(0.014)	(0.008)	(0.015)
$Days^2$	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Offer x Days	-0.002	0.004	-0.003	0.030
	(0.022)	(0.014)	(0.012)	(0.036)
Offer x $Days^2$	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
September cohort	0.172	0.059	0.392*	-0.327
	(0.250)	(0.209)	(0.219)	(0.534)
January cohort	0.209	0.143	0.411**	-0.384
	(0.223)	(0.161)	(0.184)	(0.481)
Mother	-	-0.154**	-0.139***	-0.341*
		(0.056)	(0.049)	(0.165)
Age	0.088**	0.068**	0.074**	0.104**
	(0.034)	(0.031)	(0.033)	(0.041)
Age^2	-0.001**	-0.001**	-0.001**	-0.001**
	(0.000)	(0.000)	(0.000)	(0.001)
Low education	-0.298**	-0.185**	-0.244***	-
	(0.117)	(0.079)	(0.075)	
Cohabitation	0.303*	-	0.245	0.250
	(0.155)		(0.177)	(0.189)
Number of dependent children	-0.143***	-0.053	-0.039	-0.068
	(0.043)	(0.032)	(0.042)	(0.069)
Calendar month fixed effects	Yes	Yes	Yes	Yes
Local authority fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.253	0.191	0.335	0.329
N	124	174	160	78

Notes: (i) ITT estimates are based on RDD regressions using a 90-day either side of the cutoff and a flexible function in $Days_{it}$. (ii) The first month of each term, the Cardiff local authority and the April term cohort are the reference categories. (iii) Figures in () are standard errors, clustered at the local authority level. (iv) *p<0.10, **p<0.05, ***p<0.01.

Table 3. Staggered DiD Estimates of the Impact of the Trial Childcare Policy on Parental Employment Rates

		(1)	(2)	(3)	(4)	(5)
Overall ITT on Treated		0.143	0.166	-0.004	0.228	0.322
		(0.134)	(0.158)	(0.697)	(0.741)	(1.550)
Dynamic effects (event study estimates)	Pre-Offer average	0.121	0.138	0.163	0.056	0.017
		(0.111)	(0.108)	(0.661)	(0.384)	(0.837)
	Post-Offer average	0.156	0.193	0.010	0.259	0.351
		(0.170)	(0.197)	(0.850)	(0.706)	(1.696)
	8 terms pre-Offer	-0.057	-0.067	0.107	-0.456	-0.465
		(0.215)	(0.215)	(1.156)	(0.672)	(0.914)
	7 terms pre-Offer	0.101	0.103	-0.178	0.092	-0.246
		(0.156)	(0.125)	(0.600)	(0.142)	(0.764)
	6 terms pre-Offer	0.202	0.220	0.233	0.228	0.364
		(0.201)	(0.211)	(2.709)	(0.984)	(3.304)
	5 terms pre-Offer	0.045	0.126	0.387	0.075	0.353
		(0.143)	(0.173)	(1.128)	(0.536)	(1.356)
	4 terms pre-Offer	0.214*	0.297**	0.200	0.267	0.003
		(0.126)	(0.145)	(1.172)	(0.178)	(1.930)
	3 terms pre-Offer	0.258**	0.203	0.153	0.259	0.165
		(0.131)	(0.124)	(0.711)	(0.334)	(1.920)
	2 terms pre-Offer	0.087	0.082	0.242	-0.072	-0.058
		(0.161)	(0.178)	(1.750)	(0.548)	(2.609)
	1 term pre-Offer	-	-	-	-	-
	Term of Offer introduction	0.124	0.122	-0.026	0.177	0.277
		(0.104)	(0.128)	(0.492)	(0.815)	(1.353)
	≥ 1 term post-Offer	0.188	0.264	0.045	0.341	0.424
		(0.282)	(0.305)	(1.278)	(0.661)	(2.126)
	Leads/Lags	-8 / ≥1	-8 / ≥1	-8 / ≥1	-8 / ≥1	-8 / ≥1
	N	580	580	580	580	580
Parental characteristics		No	Yes	Yes	Yes	Yes
Calendar month fixed effects		No	No	Yes	No	Yes
Local authority fixed effects		No	No	No	Yes	Yes

Notes: (i) Overall ITT effects of Offer eligibility on parental employment rates are estimated using Callaway and Sant'Anna (2021) staggered DiD approach, with the non-trial (April 2019) group as the not-yet-treated control. (ii) Dynamic effects reflect time-varying impacts, using the term before Offer introduction as the reference term, so that pre-Offer estimates are constructed symmetrically to post-Offer estimates and are comparable to traditional dynamic DiD estimators (Roth, 2026). (iii) Parental characteristics include sex, age and age squared, below A-level education, cohabitation status and the number of dependent children. (iv) Figures in () are standard errors, clustered at the local authority level, with inference based on a wild cluster bootstrap (1000 replications). (v) *p<0.10, **p<0.05, ***p<0.01.

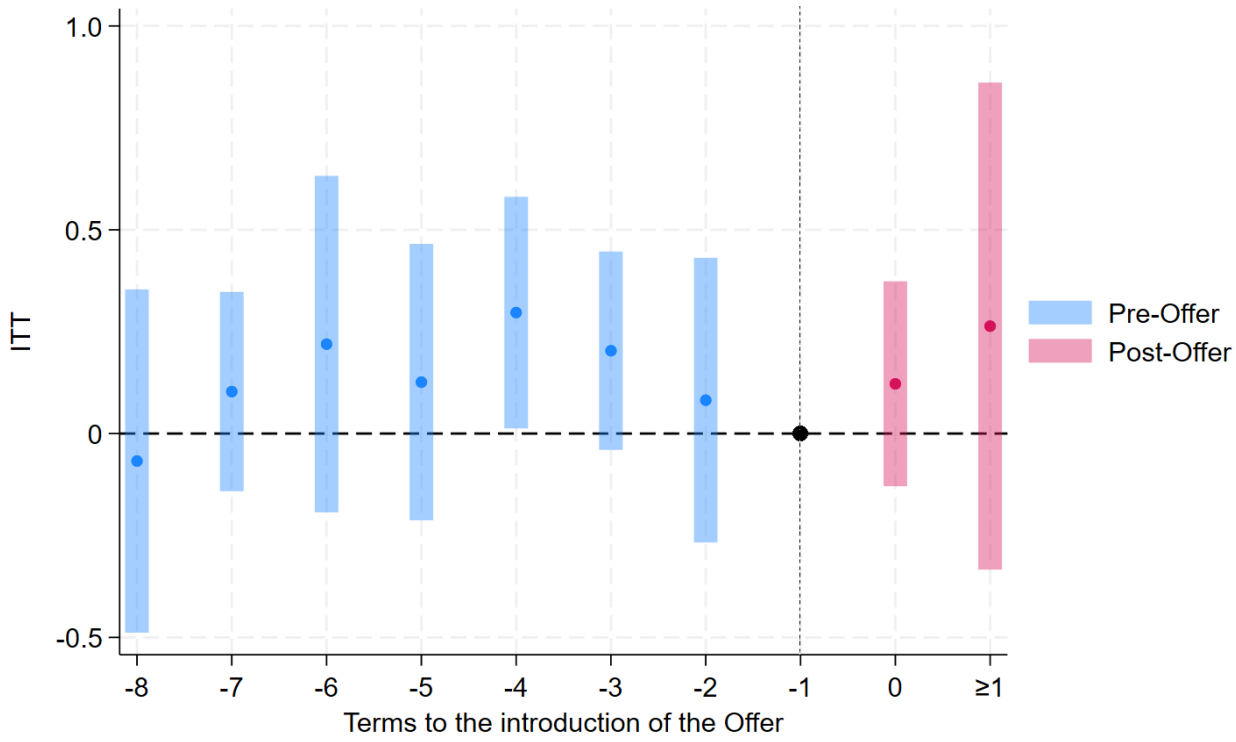
Table 4. Heterogeneity in Staggered DiD Estimates of the Impact of the Trial Childcare Policy on Parental Employment Rates

		(1)	(2)	(3)	(4)
		Mothers	Cohabitation	Low education	Youngest child
Overall ITT on Treated		0.269	0.107	-1.285	1.073
		(0.211)	(0.083)	(0.995)	(0.815)
Dynamic effects (event study estimates)	Pre-Offer average	0.329*	0.061	-0.879	0.116*
		(0.190)	(0.066)	(0.587)	(0.063)
	Post-Offer average	0.282	0.126	-1.116	0.935*
		(0.245)	(0.096)	(0.680)	(0.517)
	8 terms pre-Offer	0.497	0.064	-0.863	0.200**
		(0.484)	(0.308)	(0.587)	(0.080)
	7 terms pre-Offer	0.333**	0.008	-0.557	0.251***
		(0.147)	(0.090)	(0.511)	(0.094)
	6 terms pre-Offer	0.280	0.225*	-0.839	0.248
		(0.276)	(0.119)	(0.780)	(0.200)
	5 terms pre-Offer	0.116	0.237*	-0.685	0.323**
		(0.438)	(0.122)	(0.632)	(0.151)
	4 terms pre-Offer	0.569**	-0.023	-0.945	-0.023
		(0.225)	(0.115)	(0.700)	(0.169)
	3 terms pre-Offer	0.263*	0.024	-0.844	-0.131
		(0.157)	(0.047)	(0.651)	(0.118)
	2 terms pre-Offer	0.244	-0.107	-1.418***	-0.057
		(0.248)	(0.067)	(0.508)	(0.076)
	1 term pre-Offer				
		-	-	-	-
	Term of Offer	0.247	0.084	-1.489	1.198
	introduction	(0.99)	(0.073)	(1.326)	(1.046)
	≥ 1 term post-Offer	0.317	0.168	-0.743*	0.673***
		(0.370)	(0.134)	(0.427)	(0.168)
	Leads/Lags	-8 / ≥1	-8 / ≥1	-8 / ≥1	-8 / ≥1
	N	326	504	233	385
Parental characteristics		Yes	Yes	Yes	Yes

Calendar month fixed effects	No	No	No	No
Local authority fixed effects	No	No	No	No

Notes: Overall ITT effects of Offer eligibility on parental employment rates are estimated using Callaway and Sant'Anna (2021) staggered DiD approach, with the non-trial (April 2019) group as the not-yet-treated control. (ii) Dynamic effects reflect time-varying impacts, using the term before Offer introduction as the reference term, so that pre-Offer estimates are constructed symmetrically to post-Offer estimates and are comparable to traditional dynamic DiD estimators (Roth, 2026). (iii) Parental characteristics include (as appropriate to the specification) sex, age and age squared, below A level education, cohabitation status and the number of dependent children. (iv) Figures in () are standard errors, clustered at the local authority level, with inference based on a wild cluster bootstrap (1000 replications). (v) *p<0.10, **p<0.05, ***p<0.01.

Figure 1. Staggered DiD Event Study Estimates of the Impact of the Trial Childcare Offer on Parental Employment Rates



Notes: This graph plots the estimates of the ITT effect of Offer eligibility on parental employment rates by event period (defined as terms to the introduction of the Offer) and their 95% intervals, derived using the Callaway and Sant'Anna (2021) DiD estimator with an event-study specification. (ii) ITT estimates represent the average effect of the Offer for eligible parents in each event period relative to the term the Offer was introduced, regardless of whether the Offer was actually accessed. (iii) Dynamic effects show the time-varying impacts of Offer eligibility relative to the term before the Offer's introduction. Term 0 indicates the term the Offer was introduced. (iv) The model controls for parental characteristics including sex, age and age squared, below A level education, cohabitation status and the number of dependent children. (vi) Corresponding estimates can be found in column 2, Table 3.

Online Appendix: Does Free Pre-school Childcare Increase Parental Employment?

Appendix A. RDD Design

Table A1. Details of RDD sample

				N				
Term	APS months	Eligibility	Birth dates	All	Mothers	Youngest child	Cohabitation	Low education
Pooled	April-March 2020	Not yet eligible	-	96	59	74	76	37
		Eligible	-	114	65	68	98	41
April 2019	April-June 2019	Not yet eligible	1st Apr 16 – 29th Jun 16	23	-	-	-	-
		Eligible	1st Jan 16 – 31st Mar 16	33				
September 2019	September-December 2019	Not yet eligible	1st Sep 16 – 29th Nov 16	36	-	-	-	-
		Eligible	2nd Jun 16 – 31st Aug 16	47				
January 2020	January-March 2020	Not yet eligible	1st Jan 17 – 31st Mar 17	37	-	-	-	-
		Eligible	2nd Oct 16 – 31st Dec 16	34				

Notes: '-' indicates where cells are suppressed due to small number of observations to prevent statistical disclosure.

Table A2. Observable Parental Characteristics by RDD Eligibility

	Not yet eligible	Eligible
Employment (%)	75.00	74.56
Mother (%)	61.46	57.02
Age (years)	33.21	36.78***
Youngest child (%)	77.08	75.44
Below A-level education (%)	38.54	35.96
Cohabitation (%)	79.17	85.96
Number of dependent children	1.90	1.78
N	96	114

Notes: (i) Variable means relate to the estimation sample and are rounded to two decimal places. (ii) *p<0.10, **p<0.05, ***p<0.01 denote a statistically significant difference between those not yet eligible and eligible.

Table A3. Impact of Eligibility on Observable Characteristics

	(1) Age	(2) Low education	(3) Cohabitation	(4) Number of dependent children
Offer	2.833	-0.024	-0.302	0.457
	(3.890)	(0.355)	(0.301)	(0.546)
Days	0.181	-0.005	0.015	-0.012
	(0.136)	(0.015)	(0.010)	(0.022)
$Days^2$	0.002	-0.000	0.000	-0.000
	(0.002)	(0.000)	(0.000)	(0.000)
Offer x Days	-0.195	0.007	-0.015	0.007
	(0.216)	(0.018)	(0.011)	(0.027)
Offer x $Days^2$	-0.002	0.000	-0.000	0.000
	(0.002)	(0.000)	(0.000)	(0.000)
September cohort	0.445	-0.039	0.035	-0.548
	(3.125)	(0.181)	(0.192)	(0.420)
January cohort	-0.382	0.101	0.136	-0.326
	(2.358)	(0.222)	(0.185)	(0.422)
Calendar month fixed effects	Yes	Yes	Yes	Yes
Local authority fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.123	-0.052	0.111	0.026
N	210	210	210	210

Notes: (i) ITT estimates are based on RDD regressions using a 90-day bandwidth either side of the cutoff and a flexible function in $Days_{it}$. (ii) The outcome is the parental characteristic indicated in the column title. (iii) The first month of each term, the Cardiff local authority and the April term cohort are the reference categories. (iv) Figures in () are standard errors, clustered at the local authority level. (iv) *p<0.10, **p<0.05, ***p<0.01.

Table A4. Robustness of RDD Estimates of the Impact of the Childcare Policy on Parental Employment, Model Specification

	April	(1) September	January	(2) Term interactions	(3) Excluding proxy responses	(4) Excluding trial wards	(5) Extra parental controls
Offer	-0.367	0.255	0.198	0.345	0.335	-0.029	0.275
	(2.631)	(0.537)	(0.251)	(0.261)	(0.310)	(0.051)	(0.263)
Days	0.029	-0.027	0.008	-0.001	0.002	0.004	-0.001
	(0.055)	(0.027)	(0.012)	(0.010)	(0.013)	(0.033)	(0.010)
$Days^2$	0.000	-0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Offer x Days	-0.010	0.041	-0.007	-0.005	-0.009	-0.012	-0.007
	(0.057)	(0.031)	(0.017)	(0.013)	(0.018)	(0.077)	(0.013)
Offer x $Days^2$	-0.001	0.000	-0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
September cohort	-	-	-	0.163	0.036	-0.147	0.119
				(0.226)	(0.209)	(0.308)	(0.173)
September cohort x Offer	-	-	-	-0.123	-	-	-
				(0.146)			
January cohort	-	-	-	0.111	0.022	0.287	0.134
				(0.210)	(0.203)	(0.235)	(0.134)
January cohort x Offer	-	-	-	-0.018	-	-	-
				(0.179)			
Mother	-0.064	-0.271***	-0.011	-0.150**	-0.137	-0.115	-0.132**
	(0.190)	(0.090)	(0.100)	(0.055)	(0.088)	(0.203)	(0.055)
Age	0.061	0.103*	0.010	0.074**	0.082**	-0.004	0.086***
	(0.039)	(0.050)	(0.074)	(0.034)	(0.039)	(0.063)	(0.029)
Age^2	-0.001**	-0.001*	0.000	-0.001**	-0.001**	0.000	-0.001***
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)
Low education	0.047	-0.203*	-0.225	-0.204***	-0.327***	-0.007	-0.167**
	(0.386)	(0.100)	(0.133)	(0.069)	(0.113)	(0.115)	(0.070)
Cohabitation	0.106	0.225	0.422	0.318**	0.271**	0.244	0.264*
	(0.767)	(0.345)	(0.246)	(0.135)	(0.130)	(0.214)	(0.128)

Number of dependent children	-0.045 (0.261)	0.010 (0.074)	-0.117 (0.098)	-0.071** (0.027)	-0.081 (0.049)	-0.043 (0.065)	-0.075*** (0.025)
Disabled	-	-	-	-	-	-	-0.181* (0.091)
White ethnicity	-	-	-	-	-	-	0.042 (0.097)
Calendar month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local authority fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.058	0.254	0.470	0.289	0.315	0.240	0.327
N	56	83	71	210	135	53	208

Notes: (i) ITT estimates are based on RDD regressions using a 90-day either side of the cutoff and a flexible function in $Days_{it}$. (ii) The first month of each term, the Cardiff local authority and the April term cohort (where appropriate) are the reference categories. (iii) Figures in () are standard errors, clustered at the local authority level. (iv) *p<0.10, **p<0.05, ***p<0.01. (v) Non-trial wards are defined as wards where parents first became eligible for the Offer from April 2019.

Table A5. Robustness of RDD Estimates of the Impact of the Childcare Policy on Parental Employment, RDD Approach

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Offer	0.275	0.230*	0.263	0.230	0.328	0.275	0.311	0.175
	(0.255)	(0.124)	(0.291)	(0.174)	(0.395)	(0.290)	(0.302)	(0.266)
Days	-0.001	-0.003**	-0.004	-0.002	-0.016	-0.002	-0.022	-0.002
	(0.010)	(0.001)	(0.011)	(0.002)	(0.018)	(0.006)	(0.014)	(0.006)
Days^2	0.000	-	-0.000	-	-0.000	-	-0.000*	-
	(0.000)		(0.000)		(0.000)		(0.000)	
Offer x Days	-0.005	0.002	0.002	0.001	0.021	-0.002	0.028	0.000
	(0.014)	(0.002)	(0.015)	(0.004)	(0.024)	(0.007)	(0.018)	(0.008)
Offer x Days^2	0.000	-	0.000	-	0.000	-	0.000	-
	(0.000)		(0.000)		(0.000)		(0.000)	
September cohort	0.101	0.112	0.113	0.117	0.078	0.113	0.042	0.085
	(0.188)	(0.188)	(0.220)	(0.209)	(0.234)	(0.238)	(0.218)	(0.246)
January cohort	0.102	0.103	0.117	0.119	0.139	0.132	0.132	0.091
	(0.148)	(0.144)	(0.155)	(0.146)	(0.222)	(0.212)	(0.125)	(0.151)
Mother	-0.151**	-0.148**	-0.201***	-0.201***	-0.161***	-0.161***	-0.223***	-0.221***
	(0.055)	(0.054)	(0.049)	(0.049)	(0.054)	(0.053)	(0.058)	(0.056)
Age	0.074**	0.073**	0.068*	0.068*	0.064*	0.066*	0.070	0.069
	(0.034)	(0.034)	(0.038)	(0.037)	(0.034)	(0.035)	(0.044)	(0.043)
Age^2	-0.001**	-0.001**	-0.001*	-0.001*	-0.001*	-0.001*	-0.001	-0.001
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.00)	(0.001)	(0.001)
Low education	-0.203***	-0.204***	-0.277***	-0.278***	-0.278***	-0.274***	-0.311***	-0.331***
	(0.068)	(0.068)	(0.080)	(0.083)	(0.086)	(0.091)	(0.098)	(0.105)
Cohabitation	0.325**	0.331**	0.293**	0.289*	0.262	0.251	0.314*	0.282
	(0.128)	(0.127)	(0.154)	(0.149)	(0.156)	(0.161)	(0.177)	(0.187)
Number of dependent children	-0.069**	-0.068**	-0.061	-0.060	-0.040	-0.033	-0.067	-0.054
	(0.027)	(0.027)	(0.038)	(0.036)	(0.065)	(0.064)	(0.074)	(0.069)
Bandwidth	90 days				60 days			
Weights	Uniform		Triangular		Uniform		Triangular	
Function of $Days_{it}$	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear
Calendar month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Local authority fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.294	0.299	0.312	0.320	0.269	0.273	0.318	0.320
N	210	210	210	210	146	146	146	146

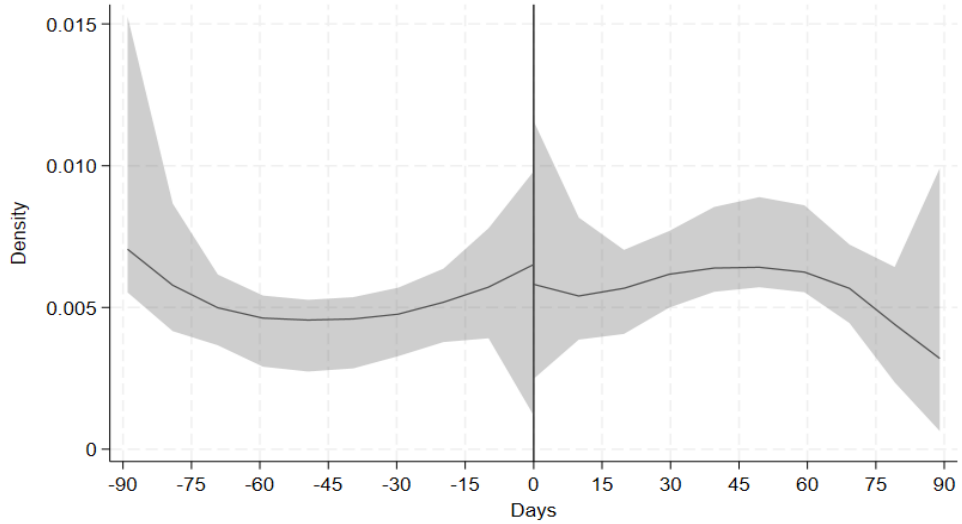
Notes: (i) ITT estimates are based on RDD regressions using a 90-day either side of the cutoff and a flexible function in $Days_{it}$or otherwise specified. (ii) The first month of each term, the Cardiff local authority and the April term cohort are the reference categories. (iii) Figures in () are standard errors, clustered at the local authority level. (iv) *p<0.10, **p<0.05, ***p<0.01.

Table A6. RDD Estimates of Impact of the Childcare Policy on Parental Working Hours

	Hours worked (all) (1)	Hours worked (in employment) (2)	Working 16 or more hours (all) (3)
Offer	-0.637	-13.173*	0.178
	(12.295)	(7.603)	(0.293)
Days	0.181	0.258	0.007
	(0.418)	(0.302)	(0.011)
Days^2	0.003	0.002	0.000
	(0.004)	(0.004)	(0.000)
Offer x Days	-0.171	-0.072	-0.011
	(0.644)	(0.401)	(0.016)
Offer x Days^2	-0.003	-0.005	-0.000
	(0.005)	(0.004)	(0.000)
September cohort	6.492	7.571	0.090
	(5.254)	(4.870)	(0.206)
January cohort	4.141	0.814	0.074
	(5.254)	(5.008)	(0.142)
Mother	-14.919***	-11.431***	-0.207***
	(2.287)	(1.721)	(0.062)
Age	3.453***	2.084	0.088**
	(0.961)	(1.709)	(0.036)
Age^2	-0.045***	-0.025	-0.001**
	(0.013)	(0.021)	(0.000)
Low education	-6.926**	-0.134	-0.196**
	(3.068)	(2.393)	(0.082)
Cohabitation	9.356**	3.070	0.299*
	(4.126)	(7.100)	(0.159)
Number of dependent children	-1.672	-0.039	-0.054*
	(1.106)	(1.332)	(0.028)
Calendar month fixed effects	Yes	Yes	Yes
Local authority fixed effects	Yes	Yes	Yes
Adjusted R^2	0.349	0.206	0.278
N	197	144	197

Notes: (i) ITT estimates are based on RDD regressions using a 90-day either side of the cutoff and a flexible function in $Days_{it}$. (ii) The first month of the term, the Cardiff local authority and the April term cohort are the reference categories. (iii) Figures in () are standard errors, clustered at the local authority level. (iv) *p<0.10, **p<0.05, ***p<0.01.

Figure A1. Distribution of Parents around the Childcare Offer RDD Eligibility Cutoff



Notes: McCrary (2008) density test in the running variable. Density estimation uses bins of 15 days to ensure no statistical disclosure. The local polynomial density estimates are plotted with 95% confidence intervals (shown as the shaded regions).

Appendix B. Staggered DiD Design

Table B1. Staggered DiD Sample Size by the Timing of Trial Rollout

	Trial (treated) group					Non-trial (not-yet treated) group
	All	September 2017	January 2018	April 2018	September 2018	
All	355	17	33	110	195	225
Pre-policy	280	-	-	77	174	-
Post-policy	75	-	-	33	21	-

Notes: (i) The pre-policy period is defined relative to the rollout of the Offer for each trial group. (ii) The non-trial group is formed of parents residing in wards that received the Offer in April 2019. (iii) '-' indicates cells are suppressed due to small number of observations to prevent statistical disclosure.

Table B2. Observable Parental Characteristics by Staggered DiD Group

	Trial group			Non-trial group
	All	Pre-policy	Post-policy	All
Employment (%)	74.65	75.36	72.00	74.22
Mother (%)	56.06	55.36	58.67	56.44
Age (years)	34.34	34.12	35.15	35.44*
Below A-level education (%)	41.13	41.07	41.33	38.67
Cohabitation (%)	86.76	89.29	77.33	87.11
Number of dependent children	2.07	2.03	2.23	2.23**
N	355	280	75	225

Notes: (i) The pre-policy period is defined relative to the rollout of the Offer for each trial group. (ii) The non-trial group is formed of parents residing in wards that received the Offer in April 2019. The trial group pools from parents residing in wards that received the Offer in September 2017, January 2018, April 2018 and September 2018. (iii) Variable means are constructed on the basis of the estimation sample and rounded to two decimal places. (iv) $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$ denote a statistically significant difference between the trial and non-trial groups.

Table B3. Robustness of Staggered DiD estimates of the Impact of the Trial Childcare Policy on Parental Employment Rates

		(1)	(2)	(3)	(4) Control groups	
		Excluding proxy responses	Enhanced parental controls	Apr-18 and Sep-18 treatment groups only	Not yet treated	Jan-19 and April-19
Overall ITT on Treated		0.001	0.172	0.068	0.209	0.154
		(0.217)	(0.288)	(0.176)	(0.181)	(0.140)
Dynamic effects (event study estimates)	Pre-Offer average	0.143	0.153	0.088	0.114	0.099
		(0.146)	(0.162)	(0.117)	(0.105)	(0.096)
	Post-Offer average	0.024	0.197	0.056	0.234	0.164
		(0.284)	(0.181)	(0.227)	(0.215)	(0.183)
	8 terms pre-Offer	0.302	-0.063	-0.067	-0.067	-0.117
		(0.618)	(0.304)	(0.215)	(0.215)	(0.169)
	7 terms pre-Offer	-0.059	0.119	0.103	0.121	0.124
		(0.194)	(0.182)	(0.125)	(0.125)	(0.098)
	6 terms pre-Offer	0.249	0.207	0.171	0.169	0.113
		(0.276)	(0.203)	(0.218)	(0.205)	(0.201)
	5 terms pre-Offer	-0.313	0.086	0.005	0.104	0.075
		(0.289)	(0.166)	(0.149)	(0.161)	(0.146)
	4 terms pre-Offer	0.419**	0.280	0.295**	0.253*	0.211*
		(0.189)	(0.218)	(0.144)	(0.141)	(0.110)
	3 terms pre-Offer	0.117	0.301	0.122	0.170	0.185
		(0.112)	(0.189)	(0.154)	(0.118)	(0.113)
	2 terms pre-Offer	0.287	0.143	-0.011	0.048	0.102
		(0.253)	(0.289)	(0.158)	(0.178)	(0.197)
	1 term pre-Offer	-	-	-	-	-
	Term of Offer introduction	-0.030	0.131	0.079	0.170	0.138
		(0.167)	(0.313)	(0.147)	(0.158)	(0.103)
	≥ 1 term post-Offer	0.078	0.263	0.033	0.298	0.190
		(0.460)	(0.351)	(0.356)	(0.323)	(0.311)
	Leads/Lags	-8 / ≥1	-8 / ≥1	-8 / ≥1	-8 / ≥1	-8 / ≥1
	N	388	575	530	580	875
Parental characteristics		Yes	Yes	Yes	Yes	Yes
Calendar month fixed effects		No	No	No	No	No
Local authority fixed effects		No	No	No	No	No

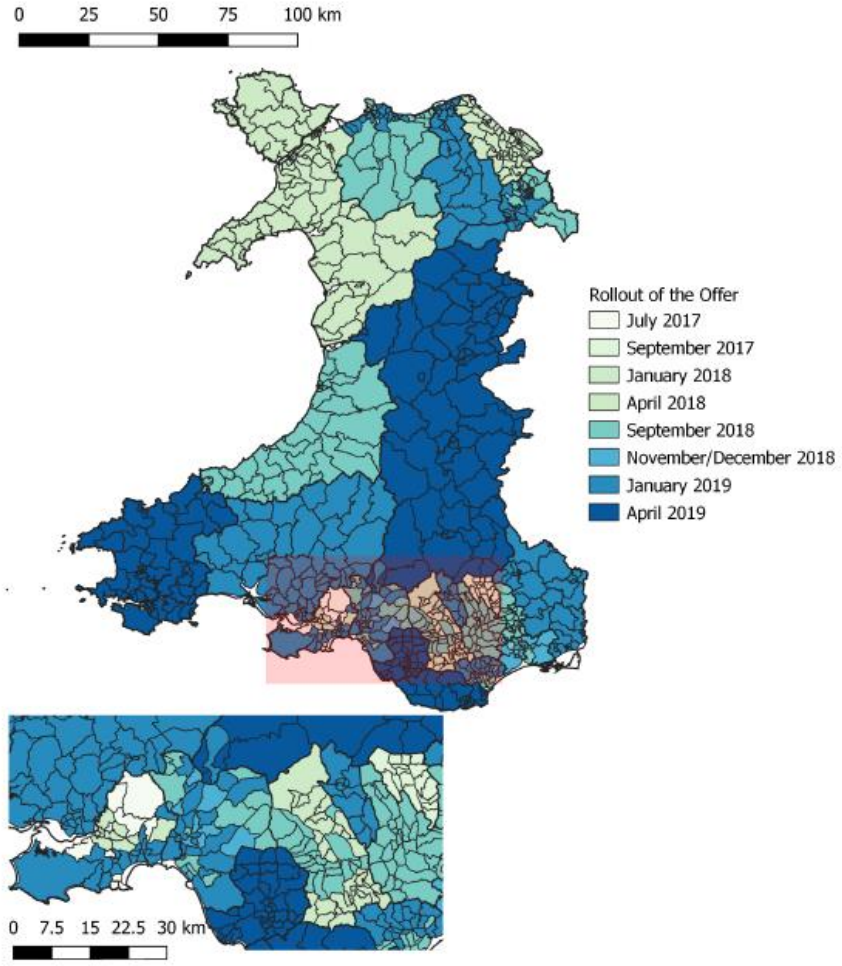
Notes: Overall ITT effects of Offer eligibility on parental employment rates are estimated using Callaway and Sant'Anna (2021) staggered DiD approach, with the non-trial (April 2019) group as the not-yet-treated control (except in column (4)). (ii) Dynamic effects reflect time-varying impacts, using the term before Offer introduction as the reference term, so that pre-Offer estimates are constructed symmetrically to post-Offer estimates and are comparable to traditional dynamic DiD estimators (Roth, 2026). (iii) Parental characteristics include sex, age and age squared, below A level education, cohabitation status and the number of dependent children. Column (2) includes additional controls for white ethnicity and disability. (iv) Figures in () are standard errors, clustered at the local authority level, with inference based on a wild cluster bootstrap (1000 replications). (v) *p<0.10, **p<0.05, ***p<0.01.

Table B4. Staggered DiD estimates of the Impact of the Trial Childcare Policy on Parental Working Hours

		Hours worked (all) (1)	Hours worked (in employment) (2)	Working 16 or more hours (all) (3)
Overall ITT on Treated		4.032	-3.730	0.103
		(6.230)	(4.161)	(0.173)
Dynamic effects (event study estimates)	Pre-Offer average	0.633	-6.957	0.066
		(4.694)	(5.747)	(0.136)
	Post-Offer average	5.023	-5.976	0.152
		(7.443)	(3.703)	(0.221)
	8 terms pre-Offer	-9.971	-12.533	-0.090
		(9.678)	(35.948)	(0.278)
	7 terms pre-Offer	-2.152	-14.456	-0.003
		(5.059)	(9.036)	(0.112)
	6 terms pre-Offer	1.823	-13.326	0.097
		(7.643)	(4.113)	(0.223)
	5 terms pre-Offer	1.527	-2.435	0.026
		(4.644)	(4.72)	(0.209)
	4 terms pre-Offer	6.221	-4.474	0.292*
		(4.448)	(3.262)	(0.150)
	3 terms pre-Offer	1.918	-6.772	0.095
		(4.680)	(3.780)	(0.162)
	2 terms pre-Offer	5.066	5.298	0.042
		(7.606)	(4.751)	(0.177)
	1 term pre-Offer	-	-	-
	Term of Offer introduction	2.421	-1.365	0.024
		(5.490)	(4.915)	(0.137)
	≥ 1 term post-Offer	7.626	-10.587*	0.281
		(11.075)	(5.666)	(0.348)
	Leads/Lags	-8 / ≥1	-8 / ≥1	-8 / ≥1
	N	567	419	567
Parental characteristics		Yes	Yes	Yes
Calendar month fixed effects		No	No	No
Local authority fixed effects		No	No	No

Notes: Overall ITT effects of Offer eligibility on parental employment rates are estimated using Callaway and Sant'Anna (2021) staggered DiD approach, with the non-trial (April 2019) group as the not-yet-treated control. (ii) Dynamic effects reflect time-varying impacts, using the term before Offer introduction as the reference term, so that pre-Offer estimates are constructed symmetrically to post-Offer estimates and are comparable to traditional dynamic DiD estimators (Roth, 2026). (iii) Parental characteristics include sex, age and age squared, below A level education, cohabitation status and the number of dependent children. (iv) Figures in () are standard errors, clustered at the local authority level, with inference based on a wild cluster bootstrap (1000 replications). (v) *p<0.10, **p<0.05, ***p<0.01.

Figure B1. The Phased Trial Rollout of the Childcare Offer for Wales across Wards, July 2017 - April 2019



Notes: Data obtained from Freedom of Information requests to the Welsh Government and individual Local Authorities. Full details available upon request.