Does reducing child benefits mean parents work more?

A mixed methods study of the labor market effects of the UK's 'two child limit'

Child benefits can play an important role in supporting families during a life-stage of increased household needs. However, they may also have negative effects on parental work incentives, potentially limiting their impact on child poverty. We examine the employment effects of a substantial benefit cut affecting larger families in the UK. The 'two-child limit' restricted means-tested child benefits to two children only, affecting new births from April 2017. Using difference-in-difference models, we find no positive impact on employment rates, with some models even pointing to negative effects. Among coupled mothers who are already working we do find small increases in working hours. Qualitative research with affected families helps make sense of these limited effects, indicating inelastic labor market responses due to strong commitment to unpaid care, challenges of caregiving responsibilities, and gaps in suitable childcare. We further find that hardship linked to the policy may make labor market engagement harder for some parents.

JEL: J08, J22, I38, H31, J13

Acknowledgements: We are grateful to two anonymous reviewers for invaluable comments. We also thank the Nuffield Foundation for the funding that made this research possible. The project has been funded by the Nuffield Foundation, but the views expressed are those of the authors and not necessarily the Foundation. Visit www.nuffieldfoundation.org. This work contains statistical data from the UK Office for National Statistics (ONS) which is Crown Copyright. The use of the ONS statistical data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates. Analysis of the data was carried out in the ONS Secure Research Service.

INTRODUCTION

Child benefits can play an important role in providing financial support to families during a life-stage of increased household needs and greater constraints to labor market participation (Bäckman and Ferrarini 2010; Van Lancker and Van Mechelen 2015). Such support is common throughout European and other industrialised countries, sometimes paid universally to all children but more often as a targeted transfer for children in lower-income families (Bradshaw 2010).

However, child benefit receipt has the potential to reduce parental work incentives, in turn reducing the effectiveness of the transfers as a tool to reduce child poverty (Corinth et al. 2021). If benefits do depress parental employment, there are also wider implications for economic growth and, where maternal employment is concerned, gender equality. Support targeted towards children which is intended to improve family well-being may therefore end up having counter-productive effects. Studies examining changes in child benefit policies have identified mixed evidence regarding employment effects. For example, González (2013) finds that the introduction of a universal childbirth grant in Spain led to eligible mothers staying out of the labor market longer after childbirth, while a generous new family allowance in Poland is found to have decreased labor force participation and employment, especially among the lowest-educated mothers (Magda et al. 2020). Mazar and Reingewertz (2023) find a sizeable positive impact on female labor supply of the sharp reduction in child allowances in Israel in the early 2000s. In contrast, the findings from analysis of 'family caps' in welfare in the US have been inconclusive, pointing variously to positive, null and negative impacts on maternal employment (Kaushal and Kaester 2001; Kim 2012; Janmohamed 2022), while recent studies looking at the 2021 temporary increase in the US Child Tax Credit and extension to families

with zero earnings have found no discernible labor supply effects (Ananat et al. 2023; Enriquez et al. 2023).

This paper contributes to this debate by analysing a cut in financial support for children in larger families in the UK: the 2017 introduction of the 'two-child limit', which restricts per child means-tested support to the first two children in the family only (with some exemptions), affecting new births from 6 April 2017. This policy affects those in receipt of Universal Credit (UC) (or its predecessor, Child Tax Credit), the main form of means-tested support for working-age families in the UK. Families receive UC if they are not in paid work, work few hours, or have relatively low earnings. At the time of introduction, the per-child element was 232 GBP per month, so the imposition of a two-child limit effectively meant a loss of this amount for each additional child. As Universal Credit is gradually withdrawn using a fixed taper as earnings rise, the reduction in the maximum eligible amount affects families right up to the point at which earnings are sufficient that they no longer qualify for UC. This means that for most families any impact on employment would operate via an income effect (i.e. a hit to family income which parents might choose to make up for with more paid work) rather than a substitution effect (a change in the return to an extra hour of work). For some dual-earner families a substitution effect might also operate: the two-child limit means they would phase out from means-testing sooner and therefore would effectively keep a greater share of any extra pay.

The government's stated intention in introducing the two-child limit was that families in receipt of benefits should 'face the same financial choices about having children as those supporting themselves in work' (HM Treasury 2015). The implication is that families should not have more than two children if they cannot support them through wages alone (a fertility

disincentive) and that if they do so, they should increase work effort to raise hours or earnings (an employment incentive). Any suggestion that the goal is to discourage fertility among low-income families has remained oblique in government discourse since the policy was introduced, but the employment goal has been repeatedly emphasised (HC Debs 2022). For example, in a parliamentary committee a senior government official referred to 'the dynamic impact [of the two child limit] of more people looking for work, more people finding work' (Work and Pensions Committee 2019, 8). In practice, research has to date found only a negligible impact of the policy on fertility (Reader et al. 2022). Our focus in this paper is on the potential employment impact. We explore whether cutting benefits led to increases in employment rates or in hours worked for the target group: families who (despite the policy) have three or more children.

The paper takes a mixed-methods approach. We conduct quantitative analysis of a large scale dataset in which we treat the introduction of the two-child limit as a natural experiment, exploiting variation by family size, parents' education level (as a proxy for low income) and child's date of birth to isolate the causal effects of the policy on employment using a difference-in-difference design. We then complement this analysis with evidence from qualitative longitudinal research with families affected by the two-child limit, with whom we conducted repeat interviews as they lived with and responded to the policy. This enables us to understand the policy's effects on individuals' attitudes to paid work and the barriers to work as they perceive them.

Our quantitative analysis identifies no evidence that the two-child limit has increased employment rates at what economists call the extensive margin; i.e. it has not led to more non-working parents moving into work. Some of our models even point to reductions in

employment rates and increased inactivity. For coupled mothers who are already working, we do find a small increase in hours worked among those already working (known as the intensive margin). Our qualitative longitudinal research suggests three explanations for such limited effects. First, parents in our sample appear to be relatively 'sticky' in their employment preferences, with strong preferences to care for their own children, particularly in the first five years. Second, parents who would like to work face significant barriers to entering the labor market, notably their own or their child's health, childcare costs and practical logistics. Third, the policy increases financial strain and harms mental health, which may be pushing some parents further away from the labor market.

Our contribution is twofold. First, we contribute to the literature on the labor market implications of child benefits, with a focus on the impact of a policy which by design a) operates primarily via an income effect and b) affects larger families in particular. There is less evidence in support of income than substitution effects in the literature on benefit changes and employment responses. There is also limited existing evidence on how family size may also affect such responses, although wider research has found that other demographic characteristics, including single parenthood and the age of children, are important determinants of labor elasticities (Saez 2002; Michelmore and Pilkauskas 2021). Our paper provides evidence that reducing benefit income may not raise employment for demographic groups that face considerable wider barriers to work; the result may instead be increased poverty and hardship, with potentially counterproductive effects. This is an important finding with resonance for debates about the most effective child poverty reduction strategies in the UK and beyond.

Second, we combine quasi-experimental quantitative techniques with qualitative longitudinal research, a unique and powerful combination within mixed methods approaches. Since the 1990s there have been growing calls for more mixed methods research as an attempt to combine the strengths, and mitigate against the weaknesses, of exclusively quantitative and qualitative research (Edin and Pirog 2014; Hendren et al. 2018). Despite this, mixed methods papers remain relatively rare in public policy research (Hendren et al. 2018). Part of our contribution is to bring quantitative and qualitative methods into dialogue with each other and show how they can inform one another. While the qualitative research findings cannot be considered as causal in the way that our quantitative analysis can, our interview data help us dig beneath the quantitative results and understand them. In sum, our paper deepens our understanding of how changes in child benefits may affect family outcomes while also illustrating the strengths of multi-method research in policy evaluation (Edin and Pirog 2014).

The paper is structured as follows. We begin by providing more detail on the two-child limit and its impact on a family's budget constraint, showing why in theory it might be expected to increase parental employment. We then outline our data and methods for the quantitative and qualitative analysis. After presenting our quantitative results, we go onto explore potential causal mechanisms through an analysis of our qualitative data. A concluding section reflects on our findings and their policy implications.

POLICY BACKGROUND: THE TWO-CHILD LIMIT

The main source of support for low-income working-age families in the UK is Universal Credit (UC), which is gradually replacing a range of individual means-tested benefits including out-of-work support for the unemployed and disabled (Jobseekers' Allowance and Employment and Support Allowance), in-work subsidies (Working Tax Credit), and allowances for the extra costs of children (Child Tax Credit) and housing (Housing Benefit). Building on the tax credit

system it replaces, UC is intended to create a seamless system of support as families move into work, and hence is available both to those not working and to those in work but on a low income. The maximum amount available is calculated as a standard allowance, which depends on whether the claimant is single or a couple, and their ages, plus additional elements for children and housing. Those with no earnings receive the maximum amount provided they fulfil work search requirements; in US parlance there is no 'phase in' stage. The total amount is then 'phased out' as income from earnings rise, using a taper rate of 63p withdrawn for every £1 in earnings beyond a monthly work allowance (55p for every £1 since December 2021). There is also a savings limit: any savings above 6,000 GBP reduce the entitlement and families with savings of more than 16,000 GBP are not eligible to claim. Because the maximum amount is tailored to family circumstances and the taper is uniform, there are wide differences in what individual families can earn before they are no longer eligible for any support: for example, a couple with two children and relatively low eligible housing costs could earn 42,000 GBP between them before ceasing to qualify, while a single parent with two children and high housing costs could earn up to 50,000 GBP (median annual earnings for full-time employees in 2022 were 33,000 GBP; ONS 2022). Government statistics show that in May 2022 3.9 million children lived in households in receipt of UC with a further 2.7 million children in households receiving the legacy Child Tax Credit; close to 45% of all UK children in all.[1]

[1] Universal Credit is gradually being rolled out to replace CTC and other benefits. Our data includes some families still on the old 'legacy' benefits. While Child Tax Credit rules are slightly different to UC, with some families slightly better off and some worse off, these differences are not of substantial interest for our purposes; the child element is the same under both systems and the two-child limit is applied in exactly the same way. UC statistics available here: https://stat-xplore.dwp.gov.uk/webapi/jsf/tableView/tableView.xhtml. Child Tax Credit statistics from DWP (2022). UK child population from IFS (2023).

Until 2017, the element for each child was 2,780 GBP per year (paid monthly as 232 GBP per child), with an additional 545 GBP per year for the first child only (the 'family element'). In 2015, the UK government announced the decision to restrict the child element to the first two children in a family only. Initially the intention was for the two-child policy to affect all new benefit claims, regardless of the child's birth date, but this was later revised so that it only affected children born after the introduction of the policy on 6 April 2017. Thus a family applying for UC with a third child born on 5 April 2017 can claim support for all three children, while a family with a third child born one day later can only claim for two. There are exemptions in place covering multiple births, children adopted from local authority care, and children conceived as a result of rape or coercion; around 5% of otherwise eligible households receive such an exemption, mostly for multiple births.[2]

By 2022, the two-child limit affected 1.3 million children living in 359,000 households, 59 percent of whom were engaged in some paid work (DWP and HMRC 2022). Just under half (47%) were single parent households (ibid). At 2021-22 benefit rates, the reduced entitlement was worth 2,845 GBP per year per affected child. A separate payment through Child Benefit, distinct from UC and available to the majority of families, was unaffected and continues to be payable for all children regardless of birth date.[3] This is small by comparison to the UC child element: 728 GBP/year per child in 2021-22 compared to 2,845 GBP for the child element of UC. Two other changes to the benefit system are worth noting as they also took place in April 2017 and therefore need to be considered in our analysis: the 'family element' (545 GBP annually) was scrapped, affecting families having a first child from that point; and greater

[2] A claim for exemption on the basis of coercion must be signed by a doctor or social worker. The recipient must not/no longer be living with the perpetrator. Data on exemptions from DWP and HMRC (2022).

[3] Child Benefit is in principle a universal cash transfer, but since 2013 it has been taxed back fully from earners of 60,000 GBP or more a year through the High-Income Child Benefit Charge. Close to 90% of families continue to be eligible.

conditionality was introduced for lone parents, who now needed to be fully available for work when their youngest child reached three rather than five years old.

In Figure 1 we use EUROMOD's tax-benefit microsimulation model to illustrate the difference in budget constraint for three families with three children depending on whether their youngest child was born before or after the April 2017 cut-off. For lone parents and one-earner couples the effect is to shift the budget constraint downwards. Under standard economic theory this is a negative income effect (a hit to total income) which may be expected to increase labor supply as parents will work more hours to make up some or all of the difference (Ashenfelter and Heckman 1974). For some two-earner couples, there is also a substitution effect operating over a portion of the income schedule: the cut in maximum benefit entitlement means the family phases out of means-testing at an earlier point, which means they get to keep a greater share of earned income beyond that point (their effective marginal tax rate on each extra pound earned is reduced).

[FIGURE 1]

Studies examining labor market responses to benefit changes have identified mixed responses, with stronger evidence for substitution than for income effects. There is extensive evidence that the US Earned Income Tax Credit (EITC) induced significant increases in the employment rate of single parents via positive substitution effects (the new benefit meant a lower effective marginal tax rate on earned income) but little evidence that it reduced hours worked for this group, despite a predicted negative income effect resulting from the boost in incomes for those working low hours (see Eissa and Hoynes 2006 for a review). For second earners, the EITC seems to have had small negative effects on both participation and hours worked, potentially reflecting both income and substitution effects given many such families will be in the 'phase

out' part of the income schedule (ibid.). Examining a generous new family allowance in Poland, the Family 500+ programme, Magda et al. (2020) find sizeable falls in labor force participation and employment, especially among the lowest-educated mothers, but Gromadzki (2021) finds much smaller effects when focusing only on the fully unconditional part of the Polish transfer (which went only to second and subsequent children). This again points to a stronger role for substitution than income effects.

Some studies are able to test more clearly for the presence of income effects by examining universal or near-universal transfers. González (2013) finds that the introduction of a universal childbirth grant in Spain led to eligible mothers staying out of the labor market longer after childbirth. Schirle (2015) identifies significant negative income effects of the Canadian Universal Child Care Benefit on the labor supply of married individuals, especially lower-educated mothers. Consistent with these findings from studies of benefit expansion, Mazar and Reingewertz (2023) find a sizeable positive impact on female labor supply of the sharp reduction in child allowances in Israel in the early 2000s; this is especially relevant to our study not only as it is a relatively rare example of a study looking at a cut rather than increase in benefits, but also because the change most affected larger families (in this case those with four or more children). In contrast to these studies, however, recent research examining the 2021 temporary increase in the US Child Tax Credit and extension to families with zero earnings have found no discernible labor supply effects (Ananat et al. 2023; Enriquez et al. 2023).

The policy that is arguably most similar to the two-child limit is the US 'family cap' which was introduced across 24 states in the 1990s and which denied additional financial support to any child conceived to a mother already receiving cash assistance through public welfare. There has been much more extensive analysis of the fertility than the employment impacts of

the family cap, but a small number of studies consider employment. Examining the late 1990s, Kaushal and Kaestner (2001) find an association between state family cap provisions and increased employment among unmarried women with children, though only about half the estimates are statistically significant; while Kim (2012) finds mothers were significantly more likely to enter employment if in a state operating both a family cap and time limits on welfare provision. But looking at a longer sweep of data, 1989-2010, Janmohamed (2022) finds evidence that family caps reduced rather than increased the probability of employment. She points to reduced affordability of complements to employment such as childcare as a possible mechanism. An alternative possible mechanism pushing in the direction of lower employment could be an impact on mental health. Existing literature provides strong evidence of a causal relationship between low-income and the probability of mothers reporting mental ill health (Cooper and Stewart 2021; Evans and Garthwaite 2014; Lindahl 2005). Poor mental health has in turn been found to have a negative effect on labor market outcomes (Frijters et al. 2014; García-Gómez et al. 2010). Reduced income through the two-child limit could therefore have the effect of pushing parents further away from the labor market.

Clearly, the employment impact of any benefit change is likely to vary depending on the responsiveness of the specific group affected (Saez 2002; Moffitt 2003). There are two reasons to think that larger families may be less responsive (or more 'sticky') than parents in general: a selection effect and a direct effect. First, individuals with lower preferences for paid employment are likely to select into larger families: individuals with preferences for more children are likely, on average, to prioritise paid labor market activity less than individuals with preferences for fewer children. Second, the process of having a larger family increases the costs of childcare and the returns to one parent staying at home to look after children. Other things equal, a family with more children is less likely to be responsive to work incentives because

the costs of entering work are larger. Relatedly, all of those affected by the two-child limit to date have at least one very young child (aged six or under). This may also affect affected households' labor market elasticities (Michelmore and Pilkauskas 2021). Kaushal and Kaestner (2001) found the largest positive effects of US family caps on employment among unmarried women with children were for those with no children under six. Therefore, although both an income effect (for all affected families) and a substitution effect (for second earners) should be in operation, it is unclear a priori how larger families in the UK will have responded to the reduction in state support represented by the two-child limit. This is the focus of our paper.

DATA AND METHODS

QUANTITATIVE DATA AND METHODS

Research design and data

We start by using a quasi-experimental approach to identify whether the two-child limit resulted in increases in labor supply at the extensive or intensive margin. Treating the introduction of the two-child limit as a natural experiment, we use the UK's Annual Population Survey (APS) to investigate the impact of the policy at the population level. The APS is a version of the UK's Labour Force Survey (LFS) (similar to the US's Current Population Survey) with a boosted sample size of approximately 80,000 responding households and 320,000 individuals each year, making it the largest household survey in the UK. We use quarterly data for 2013-2019 (ONS Social Survey Division 2023 and earlier editions).

The APS contains individual-level data on employment, gross and net earnings and other demographic characteristics (including gender, ethnicity, marital status, age, tenure, region). We merge these person-level files to their respective household-level data, giving us access to

data on net (family) earnings, combined (family) occupation status, and the number and age of dependent children aged under 19 in the family. We use the APS person calibration weights to derive population-level estimates and to correct for non-response.

We restrict our sample to adult respondents aged 19-45 living in a family with at least one dependent child.[4] We exclude data from 2020 due to the Covid-19 pandemic, which appears to have had differential impacts by family size and income and could thereby introduce bias into our estimates (Reader and Andersen 2022). This leaves us with a total sample of 348,809 adult respondents interviewed between 2013 and 2019, including 211,343 in our main 'lower-income' category (lower educated), although the final sample size varies by analysis.

To isolate the effect of the policy, we use a difference-in-difference design, exploiting the fact that families are affected by the two-child limit if they a) are in receipt of means-tested benefits; b) have three or more children; and c) have at least one child born on or after the arbitrary date of 6 April 2017. The last point creates a sudden and plausibly exogenous increase in the probability of being affected. While in principle, families could have reacted to the policy by choosing not to have an additional child, analysis to date has identified only a negligible fertility impact (Reader et al. 2022). Our main models are double-difference models which restrict the sample to lower-income families - those likely to be receiving the relevant benefits - and compare differences in employment outcomes for larger families with a youngest child born before or after April 2017, with differences for smaller families with a youngest child born before or after this date. The assumption is that any differences in the change in outcomes

[4] Dependent children are defined here as those aged under 19 living in the family; this is the closest match to the definition employed by the two-child limit, which counts all children aged 16 or under and those aged 16-19 who are in full-time education or training (the latter has been compulsory for all young people since 2015).

between the two groups can be attributed to the two-child limit. As a robustness check, we also estimate triple differences models, which introduces a further comparison with higher income larger families, controlling for the possibility that changes in employment outcomes have taken place over time for larger families for reasons unrelated to benefit changes (e.g. changing social norms).

Our basic empirical specification is the following, with the sample restricted to lower-income families using two alternative proxy measures (see below):

$$Employment\ outcome_{it} = \beta_0 + \beta_1 Bigfam_i + \beta_2 Post_t + \beta_3 (Bigfam_i * Post_t) + X_{it} + u_{it} \quad (1)$$

where $Bigfam_i$ is a dummy variable equal to one if the respondent lives in a family with three or more children and is zero if they live in a family with 1-2 children (our sample excludes those without children); and $Post_t$ is a dummy variable equal to one if the respondent lives in a family in which a child was born during or after April 2017. Since the data is a repeated cross-section, this is only coded as one when the respondent is interviewed in the post period, after April 2017, with a child born after that date. Our coefficient of interest is β_3, which identifies the differential effect of having a third or subsequent child born during or after April 2017. X_{it} is a vector of controls for characteristics likely to be associated with our labour market outcomes. In the full model these include a dummy for single parenthood; age of the respondent (5 categories); respondent's gender; age of the youngest child in the family (6 categories); respondent's reported ethnicity (4 categories); a dummy for urban residence; and a dummy capturing the presence of other adult relatives resident in the household. We also include year fixed effects.

For the triple difference models we widen the sample to include all parents, and add a further interaction between being a lower-income larger family with a child born after the cut-off:

$$Employment_{it} = \beta_0 + \beta_1 Bigfam_i + \beta_2 Lowinc_i + \beta_3 Post_t + \beta_4(Bigfam_i * Lowinc_i) + \beta_5(Lowinc_i * Post_t) + \beta_6(Bigfam_i * Post_t) + \beta_7(Bigfam_i * Lowinc_i * Post_t) + X_{it} + u_{it} \quad (2)$$

where $Lowinc_i$ is a dummy variable equal to one if the respondent lives in a 'lower-income' family, i.e. potentially affected by the policy. Here our coefficient of interest is β_7, which identifies the differential effect of having a third or subsequent child born during or after April 2017 if the respondent is from a lower rather than higher income family.

In addition to the full models, we run everything separately by gender and single/couple parenthood. We also try restricting the sample in ways that avoid the potential impact of simultaneous benefit changes: we drop families with a single child to avoid any impact of the abolition of the 'family element'; and we restrict to parents with a child under three to avoid any impact of the heightened conditionality requirements on lone parents.

Measures

We use four labor market outcome measures: employed, unemployed and inactive (each binary) and the number of working hours. Measurement follows International Labour Organisation (ILO) definitions. A respondent is 'employed' if they are either an employee, self-employed or participating in a government employment and training programme. Those who are not in work fall into two categories: 'unemployed' (without a job but actively seeking

work) and economically ‘inactive’ (without a job and not actively seeking work) (ONS 2018a). Appendix Figure A1 gives a full breakdown of the categories of economic activity available in the APS. For those who are in employment we also look at working hours, which captures the total weekly working hours across all jobs.

To identify those potentially affected by the two-child limit policy we need a measure of low income. The APS includes measures of earnings and benefit receipt, though not a direct and comprehensive measure of income. However, as these indicators carry risks of endogeneity our preferred approach is to use the individual’s education level as a proxy indicator for low income, a popular approach in the EITC literature (see, e.g., Eissa and Liebman 1996; Schanzenbach and Strain 2020). We classify individuals as low-income if their highest education level is below university degree or equivalent. This choice (rather than ‘good secondary education’, which is perhaps closer to the US ‘high school degree’) is justified by the much stronger negative correlation between the university degree variable and benefit receipt, as shown in Appendix Figure A2. As a robustness check we use an alternative proxy: whether the highest occupation in the family is a low-income occupation, classified as those in routine or semi-routine jobs, those who have never worked, the long-term unemployed and those not classified (see Appendix Figure A3 for a visual justification of this cut-off).

Identifying larger family status and those affected by the policy is straightforward as we have data on number of children and (using the secure version of the APS data) child’s birth month and year. Blended families are not distinguished in the analysis; i.e. all children in the household are counted.

For control variables, we include a binary variable for sex of respondent; lone-parent/couple status as reported by the respondent; age of respondent in five bins (19-25, 26-30, 31-35, 36-40, 41-45); number of children in the family (0,1,2,3,4,5+); age of the youngest child in the family (0-1, 2-3, 4-6, 7-9, 10-13, 14-18); ethnicity (grouped as White, Black, South Asian and other); whether the respondent lives in an urban area; and whether there are other adult household members besides a partner or older child (e.g. a child's aunt/uncle or grandparent). All of these variables are self-reported by the respondent.

We also look at two indicators of mental health as potential explanatory mechanisms. These are responses to yes/no questions on health conditions: 'Do you have depression, bad nerves or anxiety?' and 'Do you have mental illness or suffer from phobias, panics or other nervous disorders?'

Table 1 displays summary statistics for the full sample, by family size and parental education, and including some additional variables such as earnings and benefit receipt for reference. Our core group of interest – larger families with low levels of parental education – is shown in column 5. Table A1 in the Appendix shows statistics for the low education group split by family size and by whether the youngest child was born before or after the cut-off. While there are clear differences by family size – notably, parents with larger families have lower levels of employment, higher levels of inactivity, are more likely to be in receipt of state support and are less likely to be of White ethnicity (Table 1) – no substantial changes are observed in the differences between smaller and larger families before and after the cut-off (Table A1). This supports our assumption that compositional change is not a concern for the analysis, tested more formally using parallel trends analysis of covariates (Table A2). A key identifying assumption of the difference-in-differences strategy is that the treatment group would have

evolved in a similar way to the control group in the absence of the reform. Figure 2 provides a visual indication of prior trends in the outcome variables for the sample as a whole. In Appendix Tables A3 and A4 we explore the assumption of parallel trends in the outcome variables formally by replacing the treatment dummy with dummies for each quarter of interview. Results are shown separately for lone and coupled mothers, as we might expect differential trends, and as our main results focus on these two groups. While there are occasional significant coefficients before and after the reform, these are noisy and inconsistent, providing reassurance for our identification strategy (Cunningham 2021). In the controlled models shown below, we include year dummies to control for general trends (these dummies in practice make negligible difference to results).

[TABLE 1]

QUALITATIVE DATA AND METHODS

A mixed-methods design using qualitative longitudinal research

Qualitative methods offer a unique opportunity to generate hypotheses, probe causal mechanisms, and understand the 'meaning' of policies to people (Edin and Pirog 2014). In this case, we combine quantitative and qualitative methods for the purpose of 'complementarity' (Greene et al. 1989). The quasi-experimental quantitative analysis provides a causal picture at the population level; the qualitative longitudinal research zooms in on a small number of affected families, enabling us to see the policy from the parents' perspective. It is worth flagging that the time periods covered by the quantitative and qualitative analysis in this study do not directly map onto each other. Our quantitative analysis focuses on the years up to 2019 to allow us to compare families with children born either side of the policy's introduction, while excluding 2020 to avoid the complications of the pandemic. Our qualitative interviews

took place in 2021 and 2022 with the pandemic as a backdrop, and the lockdowns and their impact on the wider economy did sometimes feature in people's experiences of employment. However, the finely grained detail of everyday lives which characterises qualitative inquiry enables us to disentangle these effects from our participants' wider relationships with the paid labour market in a way that is not possible with survey data. As demonstrated in this paper, despite the difference in timing, we were able to bring the interview evidence into direct conversation with our quantitative findings, providing better insight into the broad trends uncovered there.

We were helped in this regard by the longitudinal element of our research design. Repeated interviews with the same individual over time has several advantages. It strengthens the researcher-participant relationship and generates dynamic insight into how policy changes are experienced. It also enabled us to mine for more detail at subsequent interview waves, pulling out themes emerging in earlier interview waves and exploring them more comprehensively and across the sample. In terms of sequencing, our quantitative and qualitative analysis were conducted concurrently, with regular discussions among the team. This meant that we were able to respond to our provisional quantitative findings by exploring labor market attachment and engagement more closely in our second round of qualitative interviews. We then returned to the quantitative data in light of emerging qualitative findings which suggested a potential role for mental health as a mechanism.

Sample and analytical approach

We restricted our qualitative sample to two parts of the country: London (Tower Hamlets, Hackney and Greenwich) and Yorkshire in the North of England (Bradford, Leeds and York). The areas were chosen to ensure demographic diversity. To recruit participants, we developed partnerships with local authorities and voluntary sector organisations who helped us contact those who were likely to be affected by the two-child limit. We developed a sampling frame to ensure diversity in terms of ethnicity, family size and shape, and employment status. All participants have three or more children and are affected by the two-child limit (i.e., they had a third or subsequent child born on or after 6th April 2017). A breakdown of our initial sample is included in Table 2. This paper draws on data from the first two waves of interviews. Thirty-three participants took part in the first round of interviews (2021) and twenty-four of these participants took part in the second round (2022). There was an interval of nine months between the two rounds of interviews. One third of the initial interviewees (eleven participants) lived in households where at least one parent was in paid employment at the time of interview. The participants who did not take part in a second interview, either because we could not retrace them or because they opted not to participate again, were mainly single mothers who were not in paid work. All participants were sent a £20 gift card as a 'thank you' for participating in each interview. We also periodically sent update postcards and small gifts such as holiday chocolate for the children as a way of keeping in touch and expressing appreciation for participants' time on the project.

[TABLE 2]

Our interviews were semi-structured in nature and we developed a flexible topic guide for each interview wave. These guides included general questions for all participants as well as specific questions for individuals where we wanted to follow up or probe more deeply into an issue raised at an earlier wave. The first interview guide focused on the impacts of the policies on the participants' household finances, health, children, relationships, and motivation and effort

to find paid work, and on strategies parents used to get by on a low income. Participants were also asked about the policy's influence on fertility decision-making. The second interview guide covered changes in family life, paid work and benefit receipt since the first interview, changes in the impacts of the policies particularly in light of the cost-of-living crisis, the impacts of the policies on mental health and the participants' housing situations. When asking about work-care preferences, to help ensure participants felt able to say if they had a preference for unpaid care, they were asked if they would like to enter paid work at the current time or would prefer to continue to look after their children. All team members conducted some of the interviews, including those who had focused on quantitative research in the past. Team members more familiar with qualitative interviewing methods provided training and guidance to the rest of the team. We periodically reviewed transcripts by different team members together to increase consistency in the way the interviews were conducted.

Due to the Covid-19 pandemic, most interviews were conducted via telephone (some using an interpreter). Towards the end of the first wave, Covid restrictions eased so two interviews took place in person. At the second wave, three interviews were conducted in person. Interviews lasted on average 50 minutes (ranging from 15 to 80 minutes). All were recorded and transcribed verbatim. Participants are anonymised, with the participant either choosing a pseudonym for themselves or asking us to assign one for them. Transcripts were coded using NVivo, following a coding frame. The coding frame contained salient codes identified in the transcripts as well as codes that were derived from the research questions and the interview guide. Two members of the research team coded the transcripts, and a selection were double-coded to check for consistency. We analysed the data thematically. The initial analysis was cross-sectional (synchronic analysis), which entailed identifying and interpreting key themes in the data (such as 'barriers to employment'). We then used longitudinal analytical approaches

to explore the data in relation to changes (or absence thereof) over time (diachronic analysis). An ethics of care and reciprocity governed our qualitative research, which received formal ethical approval from the University of [anon]. This included signposting individuals for further support where this was appropriate.

QUANTITATIVE RESULTS: DID THE TWO-CHILD LIMIT INCREASE EMPLOYMENT AMONG AFFECTED FAMILIES?

We begin by charting trends in the four main employment outcome variables, split by family size and parental education status (Figure 2). There is no noticeable differential change for the treatment group (larger family with low education) in any of the outcomes after the announcement of the policy in July 2015, nor after the introduction of the policy in April 2017, although the trends in conditional working hours are unstable. However this is clearly just a starting point: these figures do not control for any other characteristics, and significant effects can remain invisible to the eye.

[FIGURE 2]

To explore effects formally we use our difference-in-difference models to leverage the date of birth restriction in the policy design. Table 3 shows the results for double difference models for the lower-educated sample (no higher education). The first row includes the whole sample, and we then split by gender and lone/couple parenthood, with results shown with and without the raft of controls.

[TABLE 3]

Strikingly, the results show no significant positive effects on employment activity for the sample as a whole or for any of the sub-groups, either at the 'intensive margin' (working hours for those already in employment) or at the 'extensive margin' (employment, unemployment or inactivity). Instead we see some *negative* employment effects. In the sample as a whole, we observe significant reductions in the likelihood of being in paid employment and corresponding

increases in the likelihood of being inactive. We can read this result as a 5-6 percentage point reduction in paid employment (and increase in inactivity) associated with the loss of the GBP 2780 child element. The sub-group analysis indicates that this result is driven by changes in employment activity for coupled mothers in particular. For lone parents, most of the results are not significant, though the sign of the coefficients on employment and inactivity also point towards less employment rather than more. Further, we see a reduction in conditional working hours for lone mothers who are already in work (a drop of 2.7 hours linked to the loss of the child element) though this is only barely significant.

We conduct four robustness checks on our results. We restrict the sample in two ways to ensure that other contemporaneous policy changes are not influencing our results. First, we exclude families having their first child, restricting the comparison group to families with two children. This removes families who were affected by the abolition of the 'family element' in April 2017. Results (shown in Appendix Table A5) are largely very similar to those for the full sample, though less significant, probably reflecting the smaller sample size. One new thing emerges, however: we see an *increase* in conditional working hours for coupled mothers: the loss of the child element is associated with an additional 2.6 hours work per week for coupled mothers who are already working. This is interesting in being the first result to run in the 'expected' way – the two-child limit resulting in more work. The finding is consistent with the fact that this is precisely the group of parents most likely to be affected by a substitution and not just an income effect: the loss of the third child element means they will phase out earlier from Universal Credit and face lower effective marginal tax rates for increased hours (as was illustrated in Figure 1).

Second, we restrict the sample to individuals with a child under three at the time of interview, to avoid results being affected by the increased conditionality facing families with a child aged three and over. Most results run in the same direction as for the full sample but lose significance (see Table A6). Notably, however, these results also show a positive effect on conditional working hours for coupled mothers, this time of 2.2 hours per week.

Third, we rerun all the analysis using household SES instead of education as an alternative proxy for low-income (Appendix Table A7). Results here are almost entirely null although negative statistically significant effects show up for employment for lone mothers, with corresponding increases in inactivity.

Finally, we run some triple differences models, making use of the wider sample including higher educated parents (Table 4). This makes sure that what we observe is not driven or obscured by wider changes for larger compared to smaller families over time: we are now taking the difference between smaller and larger lower-income families before and after the cut-off and comparing to a similar difference for smaller and larger higher-income families. While this is potentially a stronger identification strategy, it relies on the 'higher income' group not being exposed to the policy. As 63% of lone mothers with a university degree in our data report receipt of relevant benefits (compared to 11% for coupled mothers with a university degree), this is likely to be a weaker strategy for lone parents than couples. Table 4 shows that none of the triple-difference results for lone mothers are significant. For coupled mothers, results for inactivity and employment run in the same direction as the double-differences models but are smaller and not significant. However, we do see a significant increase in conditional working hours, reinforcing the findings from some of the double-difference models.

[TABLE 4]

In sum, several of our models find positive effects of the two-child limit on conditional working hours for coupled mothers, the group most likely to be affected by a substitution as well as an income effect. But with this exception, we only find null or negative employment effects associated with the policy: if anything, the two-child limit appears in general to have reduced rather than increased engagement with the labour market, with indications of reductions in employment and increases in inactivity for both lone and coupled mothers. In the next section we use our qualitative evidence base to explore this finding.

WHY SUCH LIMITED EFFECTS? INSIGHTS FROM QUALITATIVE LONGITUDINAL RESEARCH

We begin by noting that nearly all the participants in our qualitative longitudinal research (28 out of 33) had carried out paid work in the past; many had stopped work after the birth of their first or a subsequent child. At the first wave of interviews, seven of 33 participants were in paid work, including two single mothers, three coupled mothers and two coupled fathers. A further four participants, all coupled mothers, were not working but had a partner who was in paid work. These work histories pre-dated being affected by the policy. The following year, at wave two, two of the 24 people we spoke to had moved into paid work, both single mothers. There had been no further increases in hours among the participants.

While this limited change is broadly consistent with our quantitative analysis, our small qualitative sample is not able to corroborate or challenge our quantitative results. The contribution of the interviews lies in providing insight into families' experiences and decision-making processes. Three key relevant themes emerge from our analysis of the qualitative data: the participants' current preferences regarding unpaid care and paid work; the barriers the participants' faced to paid work; and the direct effects of the two-child limit.

Parental preferences regarding unpaid care and paid work

While the majority of participants had previous work histories and all intended to work in the future, most did not want to undertake paid work at the current time, despite the financial hit of the two-child limit. A number had made an active choice to prioritise unpaid care while their children were young. As Melissa and Kimberley explained:

> I'm enjoying it for the moment, I'm enjoying the last few years that I've got, cos then obviously when she's in school that'll be it then, if you know what I mean.
>
> (Melissa, single mother, four children)

> I'd rather watch me kids grow up and then once she's in full-time school then, you know, just doing summat round school hours.
>
> (Kimberley, single mother, four children)

Parents often expressed a preference to care for their children themselves and were aware that their childhoods would pass quickly. As Kimberley's comment shows, even once her youngest child is in full-time school, she plans to continue to prioritise her unpaid care by fitting her paid work around school hours.

Of the 33 participants interviewed, on being asked whether the two-child limit had made any difference to whether they wanted to find paid work, only three said that it had made a difference, and a further three gave a mixed response. The others replied that the two-child limit had not made a difference. This was either because of a commitment to their caring role or because structural barriers meant paid work was not an option (discussed more below). Yalina's response articulates the lack of difference the two-child limit has made to her paid work choices:

Interviewer: Has having the two-child limit made a difference as to whether or not you want to find paid work?

Yalina: Not really, cos I think my first priority is my kids, being with my kids.

(Yalina, coupled mother, three children)

The choice to prioritise unpaid care despite the two-child limit demonstrates the strength of parental preferences regarding unpaid care and paid work, referred to by Duncan and Edwards (1999) as 'gendered moral rationalities'. Even though parents face a reduction in income due to the two-child limit, some still prefer to care for their children than enter paid work, particularly in the early years.

Barriers to paid work

A number of participants did want to enter paid work but cited a range of barriers, chiefly concerning childcare and health conditions. Childcare costs and childcare logistics were significant issues, as Aadya explained:

I did want to [enter paid work]… It just gets very difficult to try and manage three different pick-ups and then putting my youngest, which would make things easy if I could get him into like a full day kind of setting, and then the amount that they charge in the nurseries it's like, I was quite shocked, to be honest, and taken back how much they asked. I can't remember exact amount right now, but when I kind of totted it up I thought it's just really gonna take a chunk out of what I'm going to be earning, a massive chunk, on top of the fact that I have to think of somebody coming collecting them because three-thirty is quite a long time because most jobs finish at five; and for that reason I have kind of been forced to be at a little bit of a standstill at the moment.

(Aadya, coupled mother, three children)

While claimants in receipt of means-tested benefits (Universal Credit) can receive 85 percent of their childcare costs, the system has limitations. First, it does not completely cover childcare costs and therefore, as previous research has also found, childcare can remain unaffordable

(Wood 2021). Parents in larger families who need childcare for three or more children face additional difficulties with childcare costs as there is no increment for a third child (under Universal Credit, the government pays a maximum of 646 GBP per month for one child and 1108 GBP for two or more children). Second, parents have to pay childcare costs upfront themselves and only receive subsidies in arrears via Universal Credit. Paying for childcare can therefore lead to debt, or be impossible altogether if households face credit constraints (Andersen 2023, McDonough 2019).

Aadya's comments also illustrate the difficulties of arranging childcare for multiple children, highlighting the logistical challenges of finding after-school childcare for older children and combining multiple drop-off and pick-up times with paid work.

The cost of childcare also posed a difficulty for participants who were already in paid work, as Asma, who works three days a week doing office work in a hospital, told us:

> Like I say, financial-wise we could have had a bit more extra income coming in, but we can't at the moment cos somebody needs to look after the child and, as I said it like before, if I put her in nursery it's gonna cost us, all our wages are just gonna go on childcare.
>
> (Asma, coupled mother, five children)

Aside from the cost, for some participants the availability of suitable childcare was a barrier, especially where children had significant additional needs. Bushra was highly motivated to work but her youngest child was tube-fed:

> Every childminder that I've tried, they're all scared about his tube. They get, something happens like they think they can get in trouble… Otherwise I'm very active, I could get a receptionist job or what I studied as a facilitator. I'm a speaker as well, motivational speaker… But then three childminders have rejected me because of his condition, so then I kind of like, OK, slide it away, look after your child, that's it.

(Bushra, single mother, seven children)

Similarly, Riya, a coupled mother working 14 hours a week in health care, with a husband working as a taxi-driver, told us she could not increase her hours:

> If you're someone like me you can't work more, it's not about benefits, it's about the fact that I literally need to be home when my kids are home… I can't expect somebody to look after, even a babysitter, to look after two disabled children, it's just not possible.
>
> (Riya, coupled mother, three children)

Challenges related to childcare therefore seem to contribute towards the absence of employment effects at both the intensive and extensive margin.

Many participants faced health challenges of their own. Susie, who had started claiming benefits after her husband had committed suicide, said:

> My depression doesn't help me, because some days I can be OK and some days I just don't want to talk to anybody.
>
> (Susie, single mother, five children)

Laura became affected by the two-child limit a few years after being diagnosed with a serious health condition which required her to quit her job. At the time of the first interview, her youngest child was also being assessed for multiple health conditions:

> I mean there's two things; one is am I gonna be healthy enough to work, and also like this kind of, things are so uncertain for my youngest at the moment, you know, they're testing him for multiple conditions and I just don't know if I'm ever gonna be able to not be caring for him full-time. So I'd love to [go] back to work, yeah so he's got suspected autism but they also think that he might have [health condition] which is like a genetic condition, which I kind of don't really understand yet. I can't, you know, what, what his needs are gonna be as he gets older but right now he's, he needs full-time care.

(Laura, single mother, three children)

At the second round of interviews, Laura's youngest child had been diagnosed with autism. Her essential caring responsibilities were recognised when she became eligible for Carer's Allowance. While the extra money was a huge help, Laura continued to struggle financially. The two-child limit makes no exemption for health conditions of either adults or children, so Laura was still missing out on nearly 3000 GBP a year in child benefits, despite being physically unable to work.

Counter-productive effects of the two-child limit

Our quantitative analysis found some evidence of *negative* employment effects: moves out of employment and into inactivity among families affected by the two-child limit. In our qualitative interviews we found three main reasons that capping child benefits might actually push people further away from the labor market.

First, some participants found that the income shock of the two-child limit made it harder to afford the financial costs involved in entering paid work (e.g., childcare, interview clothes and transport to work). For some, it became harder to afford training or further education. Amanda, who at the first interview had recently obtained a degree in graphic design, had to sell the equipment she had bought to establish her own business as she did not have enough income from her benefit payments to cover her family's basic needs:

> I'd started buying knitting machines, which I had to sell to basically feed the kids…I started buying equipment to make the graphic studio in the house and then when I started getting broke I sold back my Apple Mac. So all the things I tried to make myself financially secure with had to go.
>
> (Amanda, single mother, four children)

Second, it was clear that managing life on a lower income involved considerable effort and energy. While none of our participants drew a direct connection between the two-child limit and this additional labor, several talked about the time and thought involved in managing a very tight budget. For example, Rachel told us about the way she shops for her family:

> So most of our shopping is done at Aldi and Lidl, but yeah, then I'll nip into Asda and just top up. I'll go and have a look at Tesco's cos they do the Clubcard thing so sometimes their deals are quite good and that, and Iceland, love Iceland for freezer stuff and their frozen veg… and you get a bit more at Iceland, they sort of buy three get a fourth for, for nothing kinda thing. So, so yeah, I kind of shop all over.
>
> (Rachel, coupled mother, eight children)

Perhaps the most significant theme to emerge, however, linked to the perceived impact of the two-child limit on parents' mental health. This came through very clearly in the second interview wave, in which we asked specifically whether participants felt the policy had affected their mental health. In response, participants set out how the absence of sufficient income (a direct result of the removal of funds for third and subsequent children) left them facing additional mental health struggles, which often manifested themselves in feelings of depression, low mood, stress and anxiety. For example, Susie explained:

> I'm always stressing about money, how am I supposed to pay this, how am I supposed to pay that? So I'm always stressed about that and I always wake up in the middle of the night with all that and it takes me ages to go to sleep on a night-time thinking about that.
>
> (Susie, single mother, five children)

Among our participants, there were strong indications that these mental health impacts had implications for the ability to find or sustain employment. Daneen talked explicitly about the

worry caused by the two-child limit, and how this in turn negatively impacted her ability to think about paid work:

> They're telling me now to live with three [children] off that money; so it causes me worry. Like even before I get paid like I know what's going out and I know what I'm gonna be left with and I know it's gonna be a struggle again… it makes you lose everything, your motivation, your ambition, you know, your mental health; how can I even think about working when I'm constantly feeling ill, I feel sick and I feel like I haven't been able to do anything that I wanted to?
>
> (Daneen, single mother, three children)

Similarly, Jessica explained:

> I couldn't even pay my gas, electric, council tax, rent, there wouldn't be enough money a month to even pay them, and that's without food and clothes for the kids; so I've just had to make the decision of I need to feed my children, I can't pay my council tax and my bills; and that's the decision that I've had to make until hopefully I'll manage to get back to work. But, you know, that's kind of been dragged out of it because the more stressed and the worse my mental health gets the longer I'm gonna be off work for.
>
> (Jessica, single mother, four children)

The APS data we use to examine employment does contain questions on self-reported health, though the approach taken has limitations. Participants are asked whether health problems affect the kind of paid work they can do, and if they reply yes they are asked which problems they have, from a list that includes 'depression, bad nerves or anxiety' and 'mental illness, phobia, panics or other disorders' (ONS 2018a). With mental health emerging from our qualitative data as a possible mechanism for negative effects of the two-child limit on engagement with paid work, we returned to the APS to look at these variables. Visually, the data provides some indication of an increase in mental health difficulties among larger low-educated families around the introduction of the policy (Appendix Figure A4), but the pre-trends are not parallel, and our difference-in-difference estimates remain statistically

insignificant, both overall and for specific groups including lone parents (Appendix Table A8). In fact there is a negative effect (i.e. improved mental health) for lone fathers, though the sample size here is very small. The weaknesses in the APS variables may be the explanation for the difference between our quantitative and qualitative findings in relation to mental health, in particular the restrictions imposed by the gatekeeper question ('Do health problems affect the kind of paid work you can do?'). This is an area where further quantitative work would be very valuable.

Exceptional cases of moves into paid work

There were two participants in our sample, Fiona and Meera, who moved into paid work between the first and second round of interviews. Both were single mothers who had stopped working shortly before the first round of interviews. Fiona had lost her job during the Covid-19 pandemic while Meera was taking a short break from work on account of mental ill health. Fiona explained that while she would have got a job regardless of the two-child limit, the policy meant that she took a full-time job when she would have preferred to work part-time while her children were still young:

> Well I'd have, I'd have had a job anyway but I think it definitely like, wouldn't say forces yer but pushes yer or encourages yer. I'd, I'd have probably have got a part-time job but because of the two child limit it wouldn't have been enough so I did need to get a full-time job… because she's, me youngest is only three I'd have probably have preferred part-time and around school hours and stuff for other two, cos all my children are at primary age.
> (Fiona, single mother, three children)

When asked if the two-child limit was a motivating factor in obtaining paid work, Meera replied that she got her job for financial reasons. In her first interview, she explained she was struggling financially following her divorce and was in a lot of debt. Her job was twenty-five hours a

week. Meera told us that she struggled to leave her young daughter when she went to work and found balancing paid work and unpaid care tiring:

> It does feel challenging sometimes, I am finding it tiring.
> (Meera, single mother, three children).

Fiona and Meera's cases illustrate that for some lone parents, the loss of support through the two-child limit does provide a financial motivation to enter work, or to work longer hours, though neither of these effects show up in our quantitative data. It may be relevant that for both these mothers childcare posed fewer challenges than for most of our participants. Fiona had considerable support from her father with childcare and she lived next door to her older children's school. Meera had three children, two of whom were seventeen and were fairly independent. She and her ex-husband shared the care of her youngest child, age three, equitably. It is likely that Fiona and Meera's recent history of paid work and their lesser childcare difficulties made work seem a more manageable response to the two-child limit.

DISCUSSION AND CONCLUSION

A core justification given for the introduction of the UK's two-child limit was to incentivise families to increase their income from work, rather than relying on government support to meet the additional costs of an extra child. In practice, the policy operates primarily via a sizeable income effect – it sharply reduces the budget constraint of families where adults are not in paid work or have low earnings. Existing evidence is ambiguous about the impact of such effects in practice. In the case of the two-child limit there is particular doubt about whether the tighter budget constraint would boost employment given plausibly sticky labor market preferences of the target group (parents of three or more children), both because of selection into having a larger family and because of the practicalities of the additional care constraints.

Examining a large representative dataset, we find very limited evidence that the two-child limit has increased levels of paid work among parents. The policy does not appear to have increased employment for any group at the extensive margin; that is, it did not increase the rate of employment or reduce economic inactivity, and in fact there is some evidence that it has done the opposite, for both lone and coupled mothers, although these effects only show up in some of our models. We do find evidence of a small increase in paid hours for coupled mothers who were already working (likely to be second earners). This is the group shown by our microsimulation modelling to be most likely to experience substitution as well as income effects, making our findings consistent with the operation of (some limited) substitution effects but no income effects. Our quantitative findings are consistent with Janmohamed's (2022) work, which links reductions in employment to the US family cap (the policy perhaps most similar to the two-child limit), and with research into the 2021 extension of the US Child Tax Credit which has found no discernible labor supply effects (Ananat et al. 2023; Enriquez et al. 2023). However, other studies of the US family cap have suggested positive employment effects (Kaushal and Kaestner 2001), though less so for families with young children.

Our qualitative longitudinal research with parents in larger families sheds light on some of the causal mechanisms that may explain our results: multiple barriers to paid work including health challenges and the cost and availability of childcare; counter-productive effects of the two-child limit policy including impacts on mental health and the additional labor involved in making ends meet; and, among some parents, a strong preference to provide care for their children, even if it meant other sacrifices.

Underpinning the policy rhetoric behind the two-child limit is a focus on the assumed deficit of the individual benefit claimant, and a suggestion that they just need to do more, and to work harder, to become self-sufficient through paid work (see, for example, an April 2024 speech on 'welfare' by then prime minister Rishi Sunak (2024)). Our findings expose the ways in which this picture clashes with lived reality, contributing to a wider literature on the stubborn and persistent barriers many parents face to increasing their engagement in the labour market (e.g. Patrick 2014; Wright 2023). Barriers include a lack of available childcare (Jitendra 2024) and an absence of suitably flexible jobs that allow parents to combine their parenting work with formal labour market participation (Cain 2016; Andersen 2023). In addition, parents in low-income households are at higher risk of illness and disability (Marmot et al. 2020), and we know that the labour market continues to discriminate and erect barriers against the full and inclusive participation of disabled people (Work and Pensions Committee 2021).

Our qualitative work also highlighted the negative consequences for mental health of leaving families without enough to live on. There is a well-developed literature on the relationship between poverty and mental ill-health, with poor mental health not only a driver of increased risk of poverty but also a direct outcome of exposure to material hardship (Schrecker and Bamber 2015; Money and Mental Health Policy Institute 2019; Kaufman et al. 2022). There is also growing evidence that the social security system can itself cause mental ill-health, with a particular role here for conditionality, sanctions and inadequate benefit payments (Andersen and Reeves 2022; Bambra et al. 2021; Patrick and Pybus 2022). On top of the difficulties created by the level of financial support itself, the impact of the two-child limit on mental health may be exacerbated through the message that is communicated to parents – the provision of an amount that is explicitly inadequate. As Daneen put it, "They're telling me now to live with three [children] off that money; so it causes me worry." Despite the strength of the mental

health findings coming through our qualitative data, we were unable to identify effects in our quantitative analysis, but the variables we had available are not ideal for this purpose. Further quantitative research in this area is important.

The two-child limit may also move some families further from the labor market by absorbing their time and energy in the work of 'getting by'; work that is often repetitive, time-intensive, and emotionally arduous and draining. As Lister (2020) argues, this is a fundamental aspect of the everyday agency displayed by people experiencing poverty (see also Wright 2023), and is work that is made more demanding by welfare reforms, which are ironically themselves defended as part of a push to encourage households to transition from 'welfare' and into 'work' (Patrick 2014).

Finally, our work speaks to increasing calls within social policy and related disciplines to shift away from a focus on paid employment as the marker of a dutiful citizen, and instead to do more to recognise both the importance and also the right to choose to prioritise care work, especially at key points in the life cycle (see, for example Tronto 2015; Wright 2023; Care Collective 2020). The parents we spoke to for this study routinely sought to realise an ethics of care in their own parenting; trying to put their parenting work first, especially when children were very young. The policy design of the two-child limit and wider underpinning narrative seeks to undermine this, privileging a narrow focus on supporting transitions into the formal labour market. But many of the parents we spoke to were not amenable to this pressure, and this may help explain their relative 'inelasticity' to the income changes to which they were exposed. These parents felt they were doing the right thing for their families by choosing to stay home, despite the hardship this created.

Our analysis inevitably has a number of limitations. First, there is no administrative data available in the UK on labor market activity and benefits receipt. While the APS is the largest household survey in the UK and benefits from a large sample size, it is possible that sampling error and non-response may dull our estimation of effects. Second, we had to exclude data from 2020 and 2021 from our quantitative analysis due to the confounding effects of the Covid-19 pandemic, which disproportionately affected the labor market activity of larger families (Reader and Andersen 2022). This naturally restricts the length of our 'post' period and means that our estimates are limited to the short run. It also means our quantitative and qualitative data relate to different time periods, though we do not think this prevents us from drawing out casual understanding from the qualitative data to helps us understand the patterns in the quantitative analysis. Third, our qualitative sample is relatively small and geographically focused. Nevertheless, it does include areas with very different labor market profiles (e.g. London and Bradford). Fourth, it is possible that employment effects may increase over time, as the average age of children affected by the two-child limit grows older – although if the policy is maintained, it will continue to affect families with young children as well as older ones.

Finally, we should be clear that the design of the two-child limit means that we are not looking at the impact of a direct cut in benefits for a given family – unlike, for example, Mazar and Reingewertz's (2023) study of benefit cuts in Israel, where child benefits were cut for all larger families. The UK policy was rolled out gradually for new babies, meaning families did not receive the support for their third or subsequent child that they would have received had the child been born earlier – they experienced the change as an absence of potential support (though one they are made very aware of), rather than as an actual reduction in income. A direct cut may plausibly be experienced in a different way. Nonetheless, the policy as designed was

expected by the UK government to have an impact on employment decisions. Further, to have long-term effects any policy must have an impact on those potentially as well as immediately affected.

Our findings have three main policy implications. First, and most directly, as the two-child limit has not yielded substantial positive employment effects, its primary effect has been to deprive larger families of 3000 GBP per year per child at a time when child poverty among larger families was already increasing sharply (Stewart et al. 2023). Indeed, estimates from the Child Poverty Action Group suggest that the two-child limit is pushing 50,000 children into poverty every year (CPAG, 2022). Wider research has found that the policy also had negligible fertility effects (Reader et al. 2022). Clearly if welfare reforms aimed at achieving behavioral change fail to do so, they simply increase poverty and create significant harm to affected families. Our findings underline the need for caution in assuming that benefit cuts will induce increased employment via income effects when the targeted group has sticky labor market preferences. Instead, they may just remove crucial resources and increase child poverty.

Second, and related, in illuminating the reasons for lack of responsiveness to the policy, our qualitative evidence shows up the gap between policymaker expectations about individuals' decision-making and the lived reality for those affected. We argue that those designing and predicting the effects of welfare reforms need to have a better understanding of the everyday lives of those using the welfare system.

Finally, our results have implications for the role of public policy in recognising individuals' contribution to society and to the economy. The only way to make up for the shortfall created by the two-child limit is to increase labor supply, but this fails to recognise the contribution of

unpaid care work to society and to the economy. UK national statistics indicate that in 2016, the value of unpaid household work to the economy equated to 64 percent of GDP (ONS 2018b). An alternative policy direction would be to embed an 'ethics of care' into public policy, in which social structures are designed to recognise care work and to make it possible (McDowell 2004; Tronto 2015, Patrick 2017). We hope that our mixed methods study, the first to examine the employment effects of this unique UK policy, can contribute to ongoing debates about how best to support parents through this life-stage and to reduce child poverty.

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Table 1: Summary statistics for the quantitative sample, 2013-2019

	Smaller families (1-2 children)			Larger families (3+ children)		
	All	**Low education**	**High education**	**All**	**Low education**	**High education**
Employment	0.793	0.737	0.875	0.649	0.577	0.804
Unemployment	0.040	0.051	0.023	0.048	0.059	0.025
Economic inactivity	0.167	0.212	0.101	0.303	0.364	0.171
Conditional working hours	34.5	34.2	34.9	33.3	33.0	34.0
Unconditional working hours	27.3	25.0	30.5	21.4	18.8	27.1
Net annual family earnings (GBP)	31,873	25,222	39,885	27,588	21,460	38,283
Receives 2CL-relevant benefits	0.208	0.276	0.111	0.382	0.461	0.212
Number of children in family	1.50	1.48	1.53	3.38	3.45	3.23
Age of youngest child in family	6.0	6.7	5.1	4.2	4.1	4.3
Age	34.1	35.8	36.1	35.8	34.7	38.4
Single parent	0.168	0.216	0.099	0.141	0.173	0.073
Female	0.566	0.555	0.583	0.579	0.578	0.582
White	0.827	0.853	0.789	0.725	0.720	0.736
Good secondary education or above	0.843	0.734	1.000	0.745	0.629	1.000
Higher education or above	0.408	0.000	1.000	0.313	0.000	1.000
Lower NSSEC	0.175	0.058	0.257	0.272	0.085	0.358
Urban	0.756	0.767	0.741	0.765	0.793	0.705
Other adult relative in household	0.023	0.025	0.020	0.026	0.028	0.022
Depression, bad nerves, anxiety	0.059	0.073	0.040	0.070	0.083	0.042
Mental illness, phobias, panics	0.022	0.030	0.011	0.026	0.032	0.013
Observations	285,374	168, 174	117,200	63,435	43,169	20,266

Notes: Data from the Annual Population Survey. The table details mean values of each variable by family size and education, where 'high education' means having at least a university degree or equivalent. A respondent is in employment if they meet the ILO definition of being an employee, self-employed, or in a government employment or training programme. Conditional working hours refer to the mean number of hours of paid work among those in work; unconditional working hours to mean hours of paid work among the population as a whole. ILO

unemployment measure used. ‘Good secondary education’ is when the respondent’s highest qualification is a good (A*-C) grade at the General Certificate of Secondary Education (GCSE), the UK’s main examinations at age 16, or higher (including GCE, A-level or equivalent, higher education, degree or equivalent). Mental health indicators cover ‘depression, bad nerves or anxiety’, and ‘mental illness, phobias, panics and other nervous disorders’. Person-household weightings are utilised to correct for non-response.

Table 2: Characteristics of the participants in the qualitative sample

Characteristic	Number of participants
Number of children	
3	16
4	7
5	6
6	1
7	1
8	2
Age of youngest child in years	
0	5
1	8
2	11
3	6
4	3
Gender	
Female	30
Male	3
Relationship status	
Single	22
Partnered	11
Ethnicity	
Black African	7
Black Caribbean	1
Pakistani	6
Bangladeshi	5
Black Caribbean and White	1
White	13

Table 3 Difference-in-difference estimates by gender and single/couple status (core coefficient only) – for low education sample

		(1)		(2)		(3)		(4)	
		Working hours		In paid employment		Unemployed		Inactive	
Full sample	β_3	-0.580	-0.580	-0.061***	-0.055***	-0.002	-0.004	0.063***	0.060***
	s.e.	(0.532)	(0.532)	(0.015)	(0.013)	(0.005)	(0.005)	(0.015)	(0.013)
	N	145410	145356	211343	211238	211343	211238	211343	211238
Lone mothers	β_3	-1.734	-2.717*	0.022	-0.048	-0.013	-0.002	-0.009	0.050
	s.e.	(1.256)	(1.251)	(0.035)	(0.034)	(0.016)	(0.017)	(0.036)	(0.036)
	N	20914	20899	39219	39193	39219	39193	39219	39193
Lone fathers	β_3	17.740*	13.296	-0.121	-0.249	0.166	0.206	-0.045	0.043
	s.e.	(8.140)	(7.502)	(0.268)	(0.257)	(0.284)	(0.277)	(0.236)	(0.219)
	N	2901	2901	5529	5526	5529	5526	5529	5526
Coupled mothers	β_3	1.463	0.854	-0.050*	-0.076***	-0.013*	-0.011	0.063**	0.087***
	s.e.	(0.910)	(0.915)	(0.023)	(0.022)	(0.006)	(0.006)	(0.023)	(0.022)
	N	52285	52270	83916	83878	83916	83878	83916	83878
Coupled fathers	β_3	-1.178*	-0.757	-0.050**	-0.030	0.004	0.002	0.047**	0.028
	s.e.	(0.550)	(0.527)	(0.017)	(0.017)	(0.009)	(0.009)	(0.015)	(0.015)
	N	69310	69286	82679	82641	82679	82641	82679	82641
Controls		No	Yes	No	Yes	No	Yes	No	Yes

Notes: Data from the Annual Population Survey. The table shows estimates of β_3 from Equation (1) estimated with an OLS linear probability model separately by gender and lone parenthood/coupled status. Controls are a dummy variable equal to one if the respondent is a single parent

and zero otherwise, age of the respondent (5 categories), age of the youngest child in the family (6 categories), education (5 categories), ethnicity (4 categories), urban status, whether other adult relatives share the home and year dummies. Gender is also controlled for in the full sample model. Working hours are conditional on being in paid employment. Person-household weightings are utilised to correct for non-response. Standard errors in parentheses. * $p<0.05$ ** $p<0.01$ *** $p<0.001$.

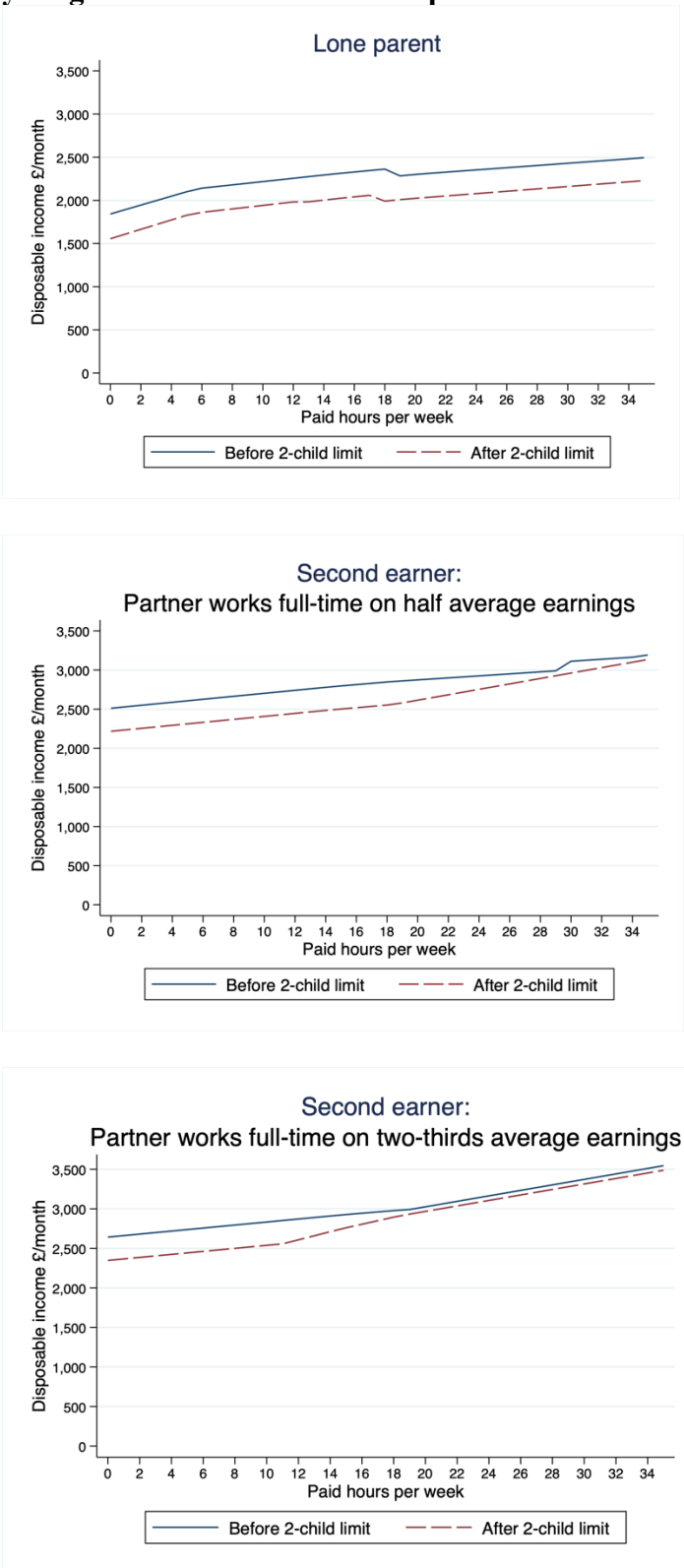
Table 4 Triple Difference estimates by gender and single/couple status (core coefficient only)

		-1		-2		-3		-4	
		Working hours		In paid employment		Unemployed		Inactive	
Full sample	β_7	0.763	0.367	-0.022	-0.037	-0.001	-0.002	0.023	0.039
	s.e.	-0.763	-0.629	-0.022	-0.02	-0.007	-0.007	-0.022	-0.02
	N	262785	262671	348809	348598	348809	348598	348809	348598
Lone mothers	β_7	0.031	-0.003	-0.007	-0.024	-0.01	-0.002	0.017	0.025
	s.e.	-2.696	-2.711	-0.096	-0.096	-0.048	-0.048	-0.093	-0.093
	N	30540	30518	51545	51505	51545	51505	51545	51505
Lone fathers	β_7	7.216	2.047	-0.220	-0.512	0.188	0.232	0.032	0.280
	s.e.	(9.049)	(9.127)	(0.275)	(0.270)	(0.288)	(0.283)	(0.241)	(0.236)
	N	3854	3852	6781	6775	6781	6775	6781	6775
Coupled mothers	β_7	3.534**	3.332**	-0.005	-0.041	-0.011	-0.01	0.017	0.051
	s.e.	-1.174	-1.177	-0.035	-0.034	-0.009	-0.009	-0.035	-0.034
	N	110173	110134	155580	155495	155580	155495	155580	155495
Coupled fathers	β_7	-0.880	-0.874	-0.028	-0.019	0.008	0.006	0.020	0.013
	s.e.	(0.770)	(0.753)	(0.022)	(0.021)	(0.011)	(0.011)	(0.020)	(0.019)
	N	118218	118167	134903	134823	134903	134823	134903	134823
Controls		No	Yes	No	Yes	No	Yes	No	Yes

Notes: Data from the Annual Population Survey. The table shows estimates of β_7 from Equation (1) estimated with an OLS linear probability model separately by gender and lone parenthood/coupled status. Low income is proxied by a lack of higher education. Controls are a dummy

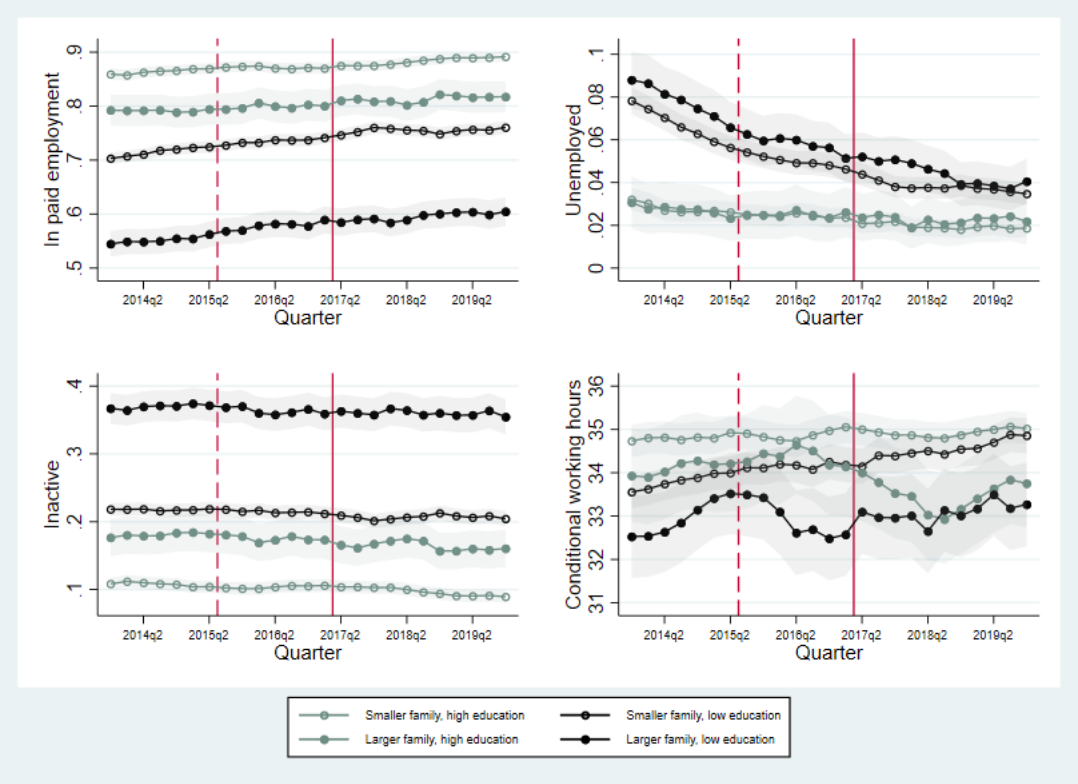
variable equal to one if the respondent is a single parent and zero otherwise, age of the respondent (5 categories), age of the youngest child in the family (6 categories), education (5 categories), ethnicity (4 categories), urban status, whether other adult relatives share the home and year dummies. Gender is also controlled for in the full sample model. Working hours are conditional on being in paid employment. Person-household weightings are utilised to correct for non-response. Standard errors in parentheses. * $p<0.05$ ** $p<0.01$ *** $p<0.001$.

Figure 1 Effect of the two-child limit on the budget constraint for three-child families; youngest born before or after April 6 2017



Note: 2021 data. The lone parent and the second earner both earn £12.50/hour (minimum wage £8.91). All three families have low eligible housing costs of 250 GBP/month. Source: Results presented here are based on UKMOD version B1.08. UKMOD is maintained, developed and managed by the Centre for Microsimulation and Policy Analysis (CeMPA) at the University of Essex (Richiardi et al, 2021). The results and their interpretation are the authors' sole responsibility.

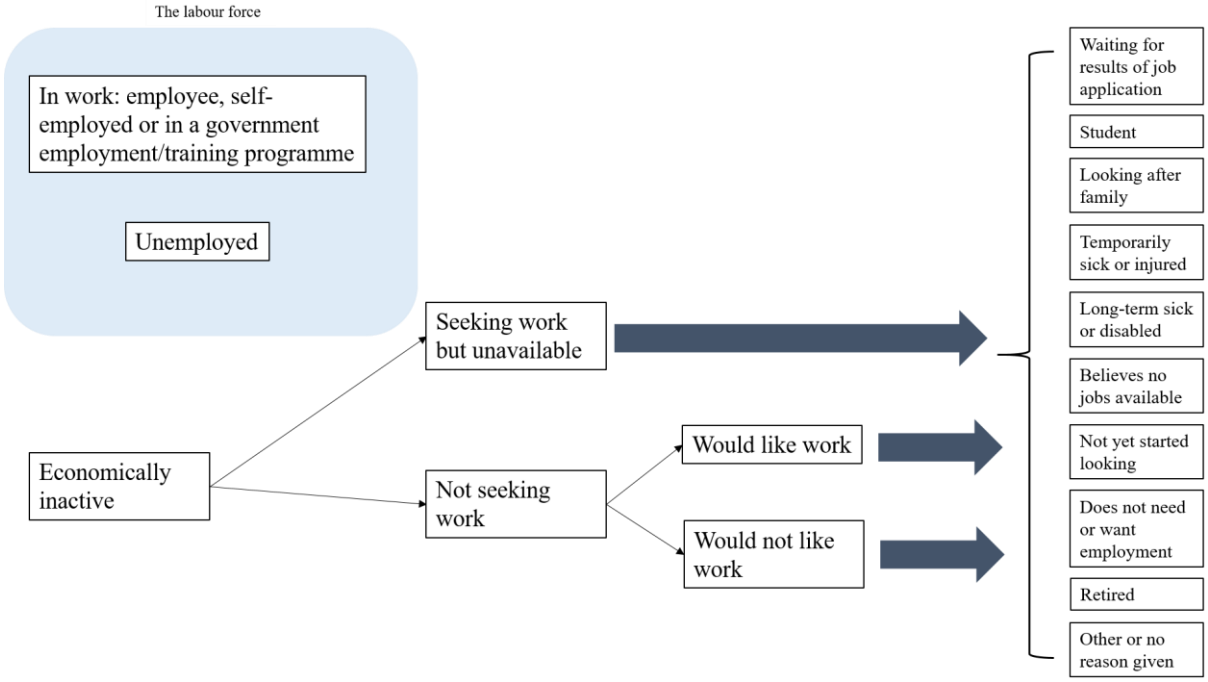
Figure 2: Employment outcomes by family size and education, 2013-2019



Notes: Data from the Annual Population Survey. The figure charts the proportion of adults aged 16-45 in paid employment as a four-quarter rolling average, by family size and education. Shaded areas denote 95 percent confidence intervals. Person-household weightings are utilised to correct for non-response. The solid vertical line indicates the introduction of the two-child limit in April 2017; the dashed vertical line indicates the announcement of the two-child limit in July 2015.

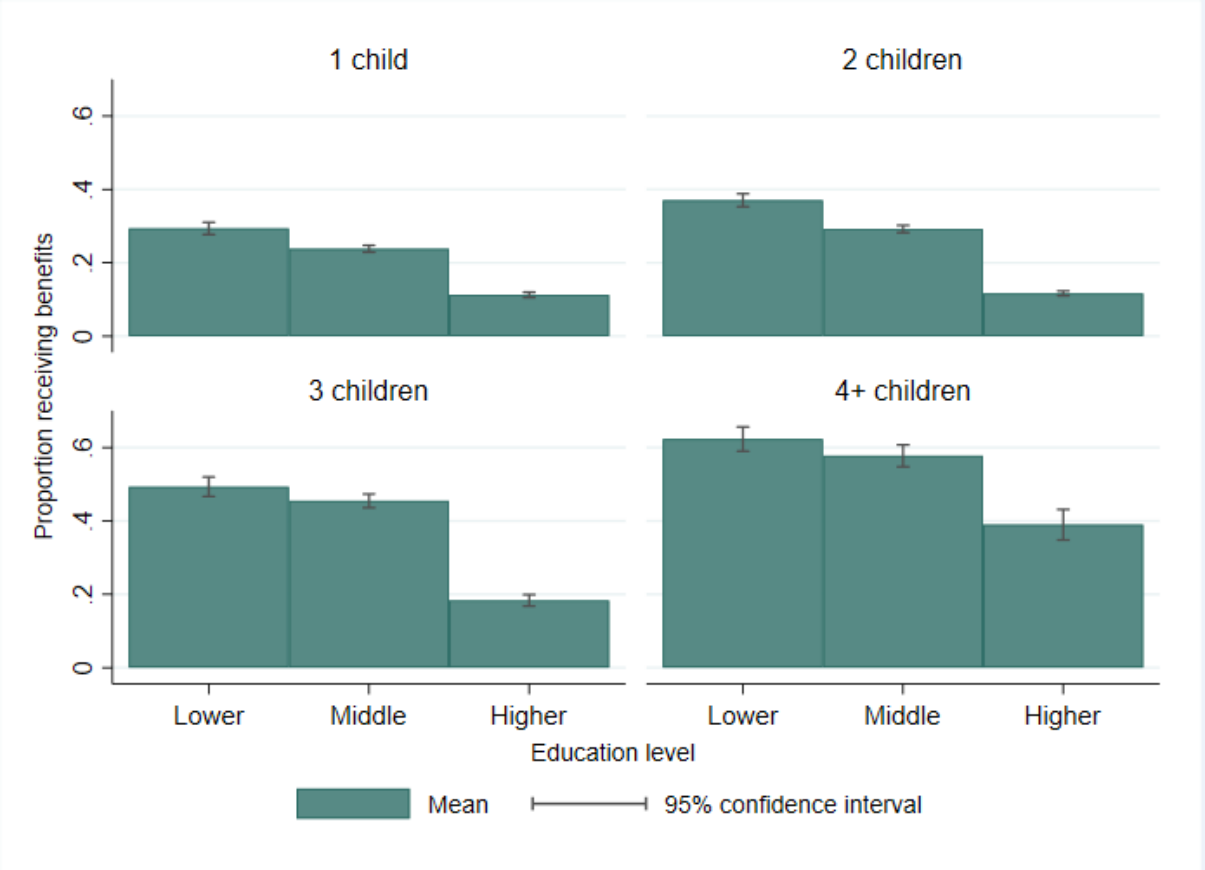
Appendix: Supplementary figures and tables

Figure A1: Employment categories in the Annual Population Survey



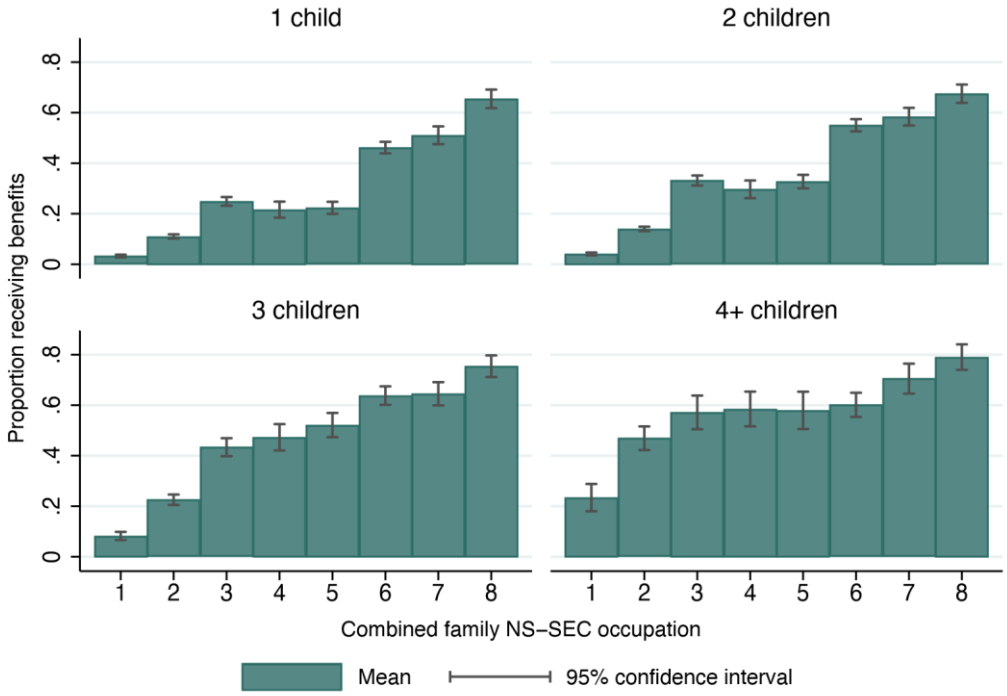
Notes: The figure describes the key employment categories in the Annual Population Survey, including the reasons given for economic inactivity.

Figure A2: Education level and the probability of benefits receipt, 2017



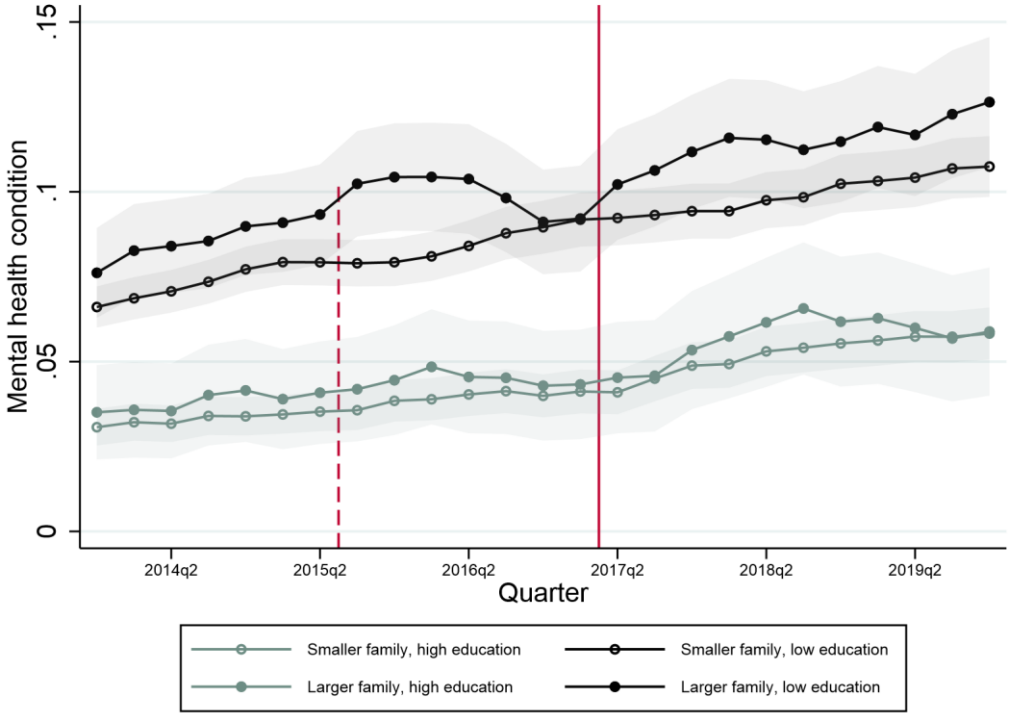
Notes: Data from Annual Population Survey. The bar chart shows the proportion of adult respondents aged 16-45 who are in receipt of means-tested benefits, by education level and by the number of children in the family. The sample is adult respondents aged 19-45 in 2017 (N=42,463). Education categories are as follows: 'Low' is a maximum of good (A*-C) grades at the General Certificate of Secondary Education (GCSE), the UK's main examinations at age 16. 'Medium' is A-level or equivalent vocational qualifications (usually taken at age 18) but not a university degree. 'High' means at least a university degree or equivalent. Person-household weightings are utilised to correct for non-response. The number of children refers to the number of dependent children under-19 reported in the family unit. Error bars indicate 95 percent confidence intervals.

Figure A3: NS-SEC occupation and the probability of benefits receipt, 2017



Notes: Data from Annual Population Survey. The bar chart shows the proportion of adult respondents aged 16-45 who are in receipt of means-tested benefits, by combined family NSSEC occupation and by the number of children in the family. The sample is adult respondents aged 19-45 in 2017 (N=42,463). NS-SEC occupation categories are as follows: 1=Large employers, higher management and higher professional; 2=Lower management and professional; 3=Intermediate; 4=Small employers and own-account; 5=Lower supervisory and technical; 6=Semi-routine; 7=Routine; 8=Never worked, long-term unemployed, or not classified. Person-household weightings are utilised to correct for non-response. The number of children refers to the number of dependent children under-19 reported in the family unit. Error bars indicate 95 percent confidence intervals.

Figure A4: Percentage reporting a mental health condition, by family size and education, 2013-2019



Notes: Data from the Annual Population Survey. The figure charts the proportion of adults aged 16-45 reporting mental health problems (depression, bad nerves or anxiety, and other mental illness, phobias or panic attacks) by quarter of interview, family size and education. Person-household weightings are utilised to correct for non-response. The solid vertical line indicates the introduction of the two-child limit in April 2017; the dashed vertical line indicates the announcement of the two-child limit in July 2015.

Table A1: Descriptive statistics split for families with youngest child born before/after two-child limit cut-off – Lower education only

	Smaller families (1-2 children)			Larger families (3+ children)		
	All	Before cut-off	After cut-off	All	Before cut-off	After cut-off
Employment	0.737	0.714	0.749	0.577	0.550	0.591
Unemployment	0.051	0.069	0.042	0.059	0.079	0.049
Economic inactivity	0.212	0.218	0.209	0.364	0.371	0.360
Conditional working hours	34.220	33.768	34.450	32.977	32.897	33.015
Unconditional working hours	25.026	23.901	25.629	18.750	17.790	19.245
Net annual family earnings (GBP)	25,222	23,348	26,226	21,460	20,366	22,020
Receives 2CL-relevant benefits	0.276	0.259	0.285	0.461	0.386	0.499
Number of children in family	1.48	1.48	1.48	3.45	3.44	3.45
Age of youngest child in family	6.7	6.7	6.7	4.1	4.1	4.2
Age	32.7	32.7	32.7	34.7	34.5	34.7
Single parent	0.216	0.228	0.210	0.173	0.177	0.171
Female	0.555	0.565	0.549	0.578	0.579	0.577
White	0.853	0.861	0.849	0.720	0.731	0.715
Good secondary education or above	0.734	0.733	0.735	0.629	0.624	0.631
Higher education or above	0.000	0.000	0.000	0.000	0.000	0.000
Lower NSSEC	0.257	0.273	0.248	0.358	0.376	0.349
Urban	0.767	0.769	0.767	0.793	0.793	0.793
Other adult relative in household	0.025	0.029	0.023	0.028	0.033	0.026
Depression, bad nerves, anxiety	0.073	0.060	0.080	0.083	0.069	0.090
Mental illness, phobias, panics	0.030	0.026	0.031	0.032	0.027	0.035
Observations	168,174	63, 632	104,542	43,169	15,703	27,466

Notes: Data from the Annual Population Survey.

Table A2: Parallel covariate trends test (lower educated sample)

	Lone parent	White	Age youngest child	Number of children	Urban
Bigfam*2013 Q2	0.009	0.003	-0.037	-0.072	-0.005
	(0.018)	(0.019)	(0.186)	(0.031)	(0.018)
Bigfam*2013 Q3	0.004	-0.001	-0.371	-0.022	-0.014
	(0.017)	(0.020)	(0.180)	(0.033)	(0.018)
Bigfam*2013 Q4	-0.013	0.019	-0.122	-0.041	-0.031
	(0.017)	(0.019)	(0.182)	(0.032)	(0.018)
Bigfam*2014 Q1	-0.005	-0.011	-0.094	-0.004	-0.013
	(0.017)	(0.019)	(0.182)	(0.035)	(0.018)
Bigfam*2014 Q2	0.016	-0.010	-0.127	0.036	-0.022
	(0.018)	(0.020)	(0.184)	(0.034)	(0.018)
Bigfam*2014 Q3	0.002	0.004	-0.502	-0.017	-0.029
	(0.017)	(0.020)	(0.179)	(0.034)	(0.018)
Bigfam*2014 Q4	-0.012	0.028	0.168	-0.051	-0.053
	(0.017)	(0.020)	(0.186)	(0.033)	(0.019)
Bigfam*2015 Q1	0.011	-0.040	-0.096	0.018	0.008
	(0.018)	(0.020)	(0.183)	(0.035)	(0.018)
Bigfam*2015 Q2	-0.001	-0.004	-0.081	0.017	-0.030
	(0.018)	(0.020)	(0.183)	(0.034)	(0.018)
Bigfam*2015 Q3	0.014	-0.020	-0.138	-0.015	-0.017
	(0.018)	(0.020)	(0.186)	(0.035)	(0.018)
Bigfam*2015 Q4	0.006	0.014	-0.108	-0.026	-0.036
	(0.018)	(0.020)	(0.188)	(0.034)	(0.019)
Bigfam*2016 Q1	0.012	0.011	0.156	-0.037	-0.029
	(0.018)	(0.020)	(0.186)	(0.036)	(0.019)

Bigfam*2016 Q2	-0.014	-0.024	-0.279	0.017	-0.013
	(0.017)	(0.020)	(0.186)	(0.036)	(0.019)
Bigfam*2016 Q3	0.014	-0.018	0.010	-0.034	-0.005
	(0.018)	(0.021)	(0.191)	(0.033)	(0.019)
Bigfam*2016 Q4	0.013	-0.001	-0.054	0.041	-0.008
	(0.017)	(0.020)	(0.189)	(0.037)	(0.018)
Bigfam*2017 Q1	-0.017	0.002	-0.075	-0.012	-0.026
	(0.018)	(0.020)	(0.187)	(0.036)	(0.019)
Bigfam*2017 Q2	0.010	-0.005	-0.197	-0.052	-0.010
	(0.018)	(0.021)	(0.185)	(0.034)	(0.019)
Bigfam*2017 Q3	0.029	0.009	-0.165	0.008	-0.048
	(0.018)	(0.020)	(0.186)	(0.035)	(0.019)
Bigfam*2017 Q4	0.024	0.006	0.245	0.039	-0.023
	(0.017)	(0.020)	(0.195)	(0.037)	(0.018)
Bigfam*2018 Q1	0.003	0.018	0.123	-0.035	0.006
	(0.019)	(0.021)	(0.201)	(0.038)	(0.019)
Bigfam*2018 Q2	0.024	-0.026	0.027	-0.031	0.017
	(0.018)	(0.022)	(0.192)	(0.035)	(0.018)
Bigfam*2018 Q3	0.021	0.007	0.277	-0.059	-0.032
	(0.018)	(0.021)	(0.193)	(0.034)	(0.019)
Bigfam*2018 Q4	0.008	-0.022	0.351	0.017	-0.017
	(0.019)	(0.022)	(0.195)	(0.040)	(0.020)
Bigfam*2019 Q1	0.026	-0.027	0.089	0.006	0.028
	(0.018)	(0.022)	(0.193)	(0.041)	(0.019)
Bigfam*2019 Q2	0.014	-0.020	-0.144	-0.011	-0.008
	(0.019)	(0.022)	(0.193)	(0.036)	(0.019)
Bigfam*2019 Q3	0.028	0.011	-0.045	-0.147	0.001
	(0.018)	(0.022)	(0.193)	(0.035)	(0.019)
Bigfam*2019 Q4	0.021	-0.009	0.020	0.019	-0.012
	(0.019)	(0.022)	(0.194)	(0.042)	(0.019)
N	211,238	211,238	211,238	211,238	211,238

Controls	No	No	No	No	No

Notes: Data from the Annual Population Survey. The table reports estimates of β_3 from Equation (1), where the Post variable is replaced with a dummy variable for each quarter of birth and regressions are run with each covariate in turn as the outcome variable. The reference category is 2013Q1. An OLS linear probability model is used. 'Bigfam' is a dummy variable equal to one if the respondent lives in a family with three or more children and zero otherwise. Person-household weightings are utilised to correct for non-response. Standard errors in parentheses. * $p<0.05$ ** $p<0.01$ *** $p<0.001$.

Table A3: Parallel trends test: outcome variables (lower educated coupled mothers)

	(1) Working hours		(2) In paid employment		(3) Unemployed		(4) Inactive	
Bigfam*2013 Q2	-0.724	-0.621	-0.003	0.014	0.032*	0.031*	-0.029	-0.045
	(1.206)	(1.200)	(0.033)	(0.032)	(0.015)	(0.015)	(0.033)	(0.032)
Bigfam*2013 Q3	-0.162	-0.100	-0.016	-0.001	0.024	0.023	-0.008	-0.022
	(1.184)	(1.183)	(0.033)	(0.031)	(0.015)	(0.015)	(0.033)	(0.032)
Bigfam*2013 Q4	1.474	1.515	0.014	0.017	0.037*	0.036*	-0.051	-0.053
	(1.204)	(1.198)	(0.033)	(0.032)	(0.016)	(0.016)	(0.033)	(0.032)
Bigfam*2014 Q1	0.780	0.878	-0.029	-0.022	0.022	0.021	0.007	0.001
	(1.190)	(1.184)	(0.033)	(0.031)	(0.014)	(0.014)	(0.033)	(0.031)
Bigfam*2014 Q2	2.195	2.274	-0.009	0.001	0.022	0.021	-0.013	-0.022
	(1.179)	(1.174)	(0.033)	(0.031)	(0.014)	(0.014)	(0.033)	(0.032)
Bigfam*2014 Q3	0.000	0.080	-0.058	-0.039	0.035*	0.033*	0.023	0.006
	(1.276)	(1.270)	(0.033)	(0.032)	(0.015)	(0.015)	(0.033)	(0.032)
Bigfam*2014 Q4	2.067	2.089	-0.006	-0.003	0.033*	0.033*	-0.027	-0.031
	(1.201)	(1.199)	(0.034)	(0.032)	(0.014)	(0.014)	(0.034)	(0.032)
Bigfam*2015 Q1	1.509	1.573	-0.040	-0.014	0.023	0.021	0.017	-0.007
	(1.225)	(1.223)	(0.033)	(0.032)	(0.015)	(0.015)	(0.034)	(0.032)
Bigfam*2015 Q2	-0.038	0.030	0.005	0.019	0.016	0.014	-0.020	-0.033
	(1.249)	(1.243)	(0.033)	(0.032)	(0.014)	(0.014)	(0.033)	(0.032)
Bigfam*2015 Q3	-1.064	-0.997	-0.021	-0.006	0.021	0.019	0.000	-0.013
	(1.350)	(1.346)	(0.034)	(0.033)	(0.015)	(0.015)	(0.034)	(0.033)
Bigfam*2015 Q4	0.812	0.830	-0.022	-0.007	0.024	0.022	-0.002	-0.015
	(1.230)	(1.224)	(0.034)	(0.033)	(0.013)	(0.013)	(0.034)	(0.033)
Bigfam*2016 Q1	0.071	0.093	0.030	0.041	0.022	0.020	-0.051	-0.062
	(1.267)	(1.262)	(0.035)	(0.033)	(0.014)	(0.014)	(0.035)	(0.033)
Bigfam*2016 Q2	0.500	0.550	0.007	0.020	0.020	0.019	-0.027	-0.039
	(1.302)	(1.300)	(0.034)	(0.032)	(0.014)	(0.014)	(0.034)	(0.032)
Bigfam*2016 Q3	0.922	0.965	-0.032	-0.015	0.029*	0.028	0.002	-0.012
	(1.265)	(1.260)	(0.035)	(0.033)	(0.015)	(0.015)	(0.035)	(0.034)

Bigfam*2016 Q4	-0.406	-0.358	-0.050	-0.031	0.023	0.021	0.027	0.010
	(1.274)	(1.268)	(0.034)	(0.032)	(0.014)	(0.014)	(0.034)	(0.032)
Bigfam*2017 Q1	0.236	0.277	0.019	0.023	0.038*	0.037*	-0.057	-0.060
	(1.264)	(1.263)	(0.035)	(0.033)	(0.015)	(0.015)	(0.035)	(0.033)
Bigfam*2017 Q2	2.741*	2.748*	-0.051	-0.035	0.039**	0.038**	0.012	-0.003
	(1.206)	(1.202)	(0.035)	(0.033)	(0.014)	(0.014)	(0.035)	(0.033)
Bigfam*2017 Q3	-0.955	-0.901	-0.043	-0.038	0.026	0.026	0.017	0.012
	(1.240)	(1.240)	(0.034)	(0.033)	(0.014)	(0.014)	(0.034)	(0.034)
Bigfam*2017 Q4	0.360	0.358	-0.079*	-0.063*	0.042**	0.040**	0.037	0.023
	(1.230)	(1.226)	(0.034)	(0.032)	(0.015)	(0.015)	(0.034)	(0.032)
Bigfam*2018 Q1	0.363	0.404	-0.007	-0.006	0.020	0.020	-0.013	-0.014
	(1.234)	(1.227)	(0.036)	(0.035)	(0.015)	(0.015)	(0.036)	(0.035)
Bigfam*2018 Q2	0.675	0.729	0.017	0.044	0.019	0.015	-0.036	-0.059
	(1.304)	(1.305)	(0.037)	(0.035)	(0.013)	(0.013)	(0.037)	(0.035)
Bigfam*2018 Q3	2.459	2.466	-0.000	0.010	0.025	0.023	-0.025	-0.033
	(1.280)	(1.274)	(0.035)	(0.033)	(0.015)	(0.015)	(0.035)	(0.034)
Bigfam*2018 Q4	1.654	1.775	-0.017	0.015	0.014	0.011	0.003	-0.027
	(1.291)	(1.280)	(0.036)	(0.034)	(0.013)	(0.013)	(0.036)	(0.034)
Bigfam*2019 Q1	-0.182	-0.142	-0.049	-0.025	0.032*	0.029	0.018	-0.004
	(1.202)	(1.196)	(0.036)	(0.034)	(0.015)	(0.015)	(0.036)	(0.034)
Bigfam*2019 Q2	1.332	1.363	-0.015	0.002	0.023	0.022	-0.007	-0.024
	(1.264)	(1.261)	(0.036)	(0.034)	(0.014)	(0.014)	(0.036)	(0.034)
Bigfam*2019 Q3	0.164	0.201	0.009	0.023	0.013	0.012	-0.022	-0.035
	(1.300)	(1.291)	(0.036)	(0.035)	(0.013)	(0.013)	(0.036)	(0.035)
Bigfam*2019 Q4	1.318	1.351	0.022	0.039	0.020	0.018	-0.042	-0.056
	(1.261)	(1.253)	(0.036)	(0.034)	(0.013)	(0.013)	(0.036)	(0.034)
N	52285	52270	83916	83878	83916	83878	83916	83878
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Data from the Annual Population Survey. The table reports estimates of β_3 from Equation (1), where the Post variable is replaced with a dummy variable for each quarter of birth. The reference category is 2013Q1. An OLS linear probability model is used. 'Bigfam' is a dummy variable equal to one if the respondent lives in a family with three or more children and zero otherwise. Controls are included for age of the respondent (5 categories), number of children in the family, age of the youngest child in the family (6 categories), education (2 categories), ethnicity (4 categories), urban status, whether there is another adult in the household. Working hours are conditional on being in paid employment. Person-household weightings are utilised to correct for non-response. Standard errors in parentheses. * $p<0.05$ ** $p<0.01$ *** $p<0.001$.

Table A4: Parallel trends test: outcome variables (lower educated single mothers)

	(1) Working hours		(2) In paid employment		(3) Unemployed		(4) Inactive	
Bigfam*2013 Q2	3.517	3.123	-0.014	-0.024	0.102**	0.107**	-0.088	-0.083
	(1.967)	(1.980)	(0.050)	(0.049)	(0.039)	(0.039)	(0.053)	(0.052)
Bigfam*2013 Q3	0.293	0.307	0.078	0.086	-0.007	-0.007	-0.071	-0.079
	(1.838)	(1.825)	(0.050)	(0.049)	(0.034)	(0.034)	(0.052)	(0.051)
Bigfam*2013 Q4	-0.204	-0.529	-0.034	-0.029	0.044	0.047	-0.010	-0.018
	(1.758)	(1.761)	(0.050)	(0.048)	(0.035)	(0.035)	(0.053)	(0.051)
Bigfam*2014 Q1	1.983	1.855	0.030	0.025	-0.018	-0.017	-0.011	-0.008
	(1.863)	(1.855)	(0.051)	(0.049)	(0.032)	(0.032)	(0.053)	(0.051)
Bigfam*2014 Q2	1.050	0.804	0.056	0.059	0.039	0.042	-0.095	-0.101*
	(1.890)	(1.902)	(0.050)	(0.049)	(0.035)	(0.035)	(0.053)	(0.051)
Bigfam*2014 Q3	0.897	0.851	0.003	0.002	0.014	0.014	-0.017	-0.016
	(2.024)	(2.018)	(0.051)	(0.050)	(0.033)	(0.033)	(0.053)	(0.051)
Bigfam*2014 Q4	1.181	0.867	0.021	0.025	-0.002	0.001	-0.019	-0.026
	(1.908)	(1.900)	(0.052)	(0.051)	(0.034)	(0.034)	(0.054)	(0.053)
Bigfam*2015 Q1	1.908	1.935	-0.009	-0.014	0.034	0.036	-0.025	-0.022
	(1.759)	(1.752)	(0.052)	(0.051)	(0.034)	(0.034)	(0.055)	(0.053)
Bigfam*2015 Q2	-0.034	-0.232	0.072	0.077	0.031	0.033	-0.103	-0.110*
	(1.762)	(1.748)	(0.053)	(0.051)	(0.033)	(0.033)	(0.055)	(0.053)
Bigfam*2015 Q3	-0.335	-0.385	-0.047	-0.043	0.068	0.069	-0.021	-0.026
	(1.946)	(1.923)	(0.052)	(0.050)	(0.038)	(0.038)	(0.055)	(0.054)
Bigfam*2015 Q4	2.645	2.469	0.013	0.029	0.020	0.022	-0.033	-0.051
	(1.922)	(1.920)	(0.054)	(0.053)	(0.036)	(0.036)	(0.056)	(0.056)
Bigfam*2016 Q1	-1.487	-1.138	0.027	0.037	0.087*	0.086*	-0.113*	-0.123*
	(1.746)	(1.739)	(0.053)	(0.052)	(0.037)	(0.038)	(0.055)	(0.054)
Bigfam*2016 Q2	-0.177	0.063	0.017	0.013	0.054	0.057	-0.071	-0.070
	(1.786)	(1.772)	(0.053)	(0.052)	(0.035)	(0.035)	(0.055)	(0.053)
Bigfam*2016 Q3	2.731	2.531	0.009	-0.007	0.048	0.052	-0.057	-0.045
	(2.027)	(1.999)	(0.054)	(0.053)	(0.038)	(0.038)	(0.056)	(0.055)

Bigfam*2016 Q4	-0.339	-0.559	-0.002	0.003	0.026	0.031	-0.024	-0.034
	(1.866)	(1.874)	(0.055)	(0.053)	(0.036)	(0.036)	(0.058)	(0.057)
Bigfam*2017 Q1	0.540	0.434	0.081	0.070	0.022	0.026	-0.103	-0.097
	(1.803)	(1.804)	(0.055)	(0.053)	(0.032)	(0.032)	(0.056)	(0.054)
Bigfam*2017 Q2	3.419	3.411	0.045	0.040	0.054	0.058	-0.099	-0.098
	(1.935)	(1.923)	(0.054)	(0.053)	(0.034)	(0.034)	(0.055)	(0.054)
Bigfam*2017 Q3	-0.748	-0.653	0.072	0.080	0.002	0.005	-0.074	-0.085
	(1.959)	(1.941)	(0.056)	(0.056)	(0.031)	(0.031)	(0.057)	(0.056)
Bigfam*2017 Q4	-1.026	-1.380	-0.014	-0.016	0.041	0.047	-0.026	-0.031
	(1.717)	(1.705)	(0.053)	(0.052)	(0.035)	(0.035)	(0.055)	(0.054)
Bigfam*2018 Q1	-0.238	-0.329	0.123*	0.118*	0.043	0.047	-0.166**	-0.165**
	(1.754)	(1.756)	(0.056)	(0.055)	(0.036)	(0.036)	(0.057)	(0.056)
Bigfam*2018 Q2	1.964	1.692	0.092	0.085	0.019	0.024	-0.111*	-0.109*
	(1.814)	(1.817)	(0.055)	(0.054)	(0.033)	(0.033)	(0.056)	(0.055)
Bigfam*2018 Q3	0.229	-0.064	0.059	0.056	0.006	0.009	-0.065	-0.065
	(1.964)	(1.958)	(0.055)	(0.055)	(0.033)	(0.033)	(0.056)	(0.055)
Bigfam*2018 Q4	-0.060	-0.100	0.091	0.075	-0.003	0.002	-0.088	-0.078
	(1.895)	(1.878)	(0.056)	(0.055)	(0.031)	(0.031)	(0.058)	(0.056)
Bigfam*2019 Q1	0.899	0.558	0.051	0.049	0.023	0.027	-0.074	-0.076
	(1.873)	(1.861)	(0.056)	(0.053)	(0.031)	(0.031)	(0.057)	(0.055)
Bigfam*2019 Q2	1.315	1.046	0.003	0.013	0.035	0.035	-0.037	-0.049
	(1.889)	(1.874)	(0.057)	(0.055)	(0.032)	(0.032)	(0.059)	(0.057)
Bigfam*2019 Q3	-1.162	-1.575	0.070	0.050	0.009	0.015	-0.079	-0.065
	(1.812)	(1.817)	(0.056)	(0.055)	(0.030)	(0.030)	(0.057)	(0.057)
Bigfam*2019 Q4	0.068	-0.138	0.019	0.014	0.045	0.049	-0.063	-0.063
	(1.958)	(1.972)	(0.058)	(0.058)	(0.036)	(0.036)	(0.060)	(0.059)
N	20914	20899	39219	39193	39219	39193	39219	39193
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Data from the Annual Population Survey. The table reports estimates of β_3 from Equation (1), where the Post variable is replaced with a dummy variable for each quarter of birth. An OLS linear probability model is used. 'Bigfam' is a dummy variable equal to one if the respondent lives in a family with three or more children and zero otherwise. 'Post' is a dummy variable equal to one if the respondent lives in a family in which a child was born during or after April 2017. Controls are included for age of the respondent (5 categories), number of children in the family, age of the youngest child in the family (6 categories), education (2 categories), ethnicity (4 categories), urban status, whether there is another adult in the household. Working hours are conditional on being in paid employment. Person-household weightings are utilised to correct for non-response. Standard errors in parentheses. * $p<0.05$ ** $p<0.01$ *** $p<0.001$.

Table A5: Robustness check - Core coefficients with sample restricted to parents with at least two children

		(1)		(2)		(3)		(4)	
		Working hours		In paid employment		Unemployed		Inactive	
Overall	β_3	0.336	0.224	-0.022	-0.034*	-0.009	-0.008	0.031	0.042**
	s.e.	-0.595	-0.483	-0.017	-0.014	-0.006	-0.006	-0.016	-0.014
	N	84392	84357	126146	126080	126146	126080	126146	126080
Lone mothers	β_3	-1.998	-2.632	0.054	-0.008	-0.006	0.001	-0.049	0.007
	s.e.	-1.58	-1.584	-0.04	-0.039	-0.017	-0.018	-0.041	-0.041
	N	9851	9843	20269	20253	20269	20253	20269	20253
Coupled mothers	β_3	3.085**	2.639**	-0.02	-0.055*	-0.013	-0.01	0.033	0.065**
	s.e.	-0.968	-0.972	-0.026	-0.024	-0.007	-0.007	-0.026	-0.024
	N	30966	30955	53449	53422	53449	53422	53449	53422
Coupled fathers	β_3	-0.487	-0.349	-0.010	-0.012	-0.009	-0.006	0.019	0.017
	s.e.	(0.617)	(0.589)	(0.019)	(0.018)	(0.010)	(0.010)	(0.016)	(0.016)
	N	42671	42655	50607	50584	50607	50584	50607	50584
Controls		No	Yes	No	Yes	No	Yes	No	Yes

Notes: Data from the Annual Population Survey. The table shows estimates of β_3 from Equation (1) estimated with an OLS linear probability model separately by gender and lone parenthood/coupled status. Controls are a dummy variable equal to one if the respondent is a single parent and zero otherwise, age of the respondent (5 categories), age of the youngest child in the family (6 categories), education (5 categories), ethnicity (4 categories), urban status, whether other adult relatives share the home and year dummies. Gender is also controlled for in the full sample model. Working hours are conditional on being in paid employment. Person-household weightings are utilised to correct for non-response. Standard errors in parentheses. * p<0.05 ** p<0.01 *** p<0.001. Results for lone fathers are not shown because of small sample size.

Table A6: Robustness check - Core coefficients with sample restricted to parents with a child under three

		(1)		(2)		(3)		(4)	
		Working hours		In paid employment		Unemployed		Inactive	
Overall	β_3	-0.173	0.115	-0.033*	-0.016	-0.004	-0.007	0.037*	0.022
	s.e.	-0.549	-0.458	-0.015	-0.013	-0.006	-0.006	-0.015	-0.013
	N	54562	42474	82921	64514	82921	64514	82921	64514
Lone mothers	β_3	-2.513	-2.233	-0.044	-0.034	0.009	0.001	0.035	0.033
	s.e.	-1.3	-1.308	-0.036	-0.036	-0.017	-0.017	-0.037	-0.038
	N	4904	3520	13586	10119	13586	10119	13586	10119
Coupled mothers	β_3	1.562	2.176*	-0.017	-0.013	-0.011	-0.009	0.028	0.023
	s.e.	-0.935	-0.943	-0.023	-0.023	-0.006	-0.006	-0.023	-0.023
	N	18202	14090	32921	25664	32921	25664	32921	25664
Coupled fathers	β_3	-0.269	-0.250	-0.013	-0.013	-0.006	-0.010	0.020	0.023
	s.e.	(0.567)	(0.552)	(0.017)	(0.017)	(0.010)	(0.010)	(0.015)	(0.015)
	N	31221	24722	35952	28440	35952	28440	35952	28440
Controls		No	Yes	No	Yes	No	Yes	No	Yes

Notes: Data from the Annual Population Survey. The table shows estimates of β_7 from Equation (1) estimated with an OLS linear probability model and where 'lowinc' is a dummy variable equal to one if a respondent has a degree and zero otherwise. Controls are a one/zero dummy for single parenthood, age of the respondent (5 categories), age of the youngest child in the family (6 categories), education (5 categories), and ethnicity (4 categories). Working hours are conditional on being in paid employment. Person-household weightings are utilised to correct for non-response. * p<0.05 ** p<0.01 *** p<0.001. Results for lone fathers are not shown because of small sample size.

Table A7: Robustness check - Core coefficients NSSEC used to proxy low income families

		(1)		(2)		(3)		(4)	
		Working hours		In paid employment		Unemployed		Inactive	
Overall	β_3	-0.975	-1.374	-0.043	-0.059**	-0.008	-0.010	0.052*	0.069**
	s.e.	1.094	0.875	0.025	0.023	0.011	0.011	0.025	0.023
	N	32786	32773	68676	68620	68676	68620	68676	68620
Lone mothers	β_3	-0.786	-1.459	-0.035	-0.093*	-0.005	0.001	0.04	0.092*
	s.e.	-1.479	-1.481	-0.037	-0.036	-0.019	-0.02	-0.039	-0.039
	N	11509	11504	27461	27440	27461	27440	27461	27440
Coupled mothers	β_3	-0.48	-1.128	-0.026	-0.046	-0.014	-0.01	0.04	0.056
	s.e.	-1.668	-1.695	-0.037	-0.037	-0.011	-0.012	-0.038	-0.038
	N	8281	8279	20545	20525	20545	20525	20545	20525
Coupled fathers	β_3	-1.302	-0.838	-0.04	-0.029	-0.026	-0.02	0.066	0.049
	s.e.	-1.284	-1.187	-0.042	-0.041	-0.025	-0.025	-0.038	-0.037
	N	11979	11973	17915	17901	17915	17901	17915	17901
Controls		No	Yes	No	Yes	No	Yes	No	Yes

Notes: Data from the Annual Population Survey. The table shows estimates of β_7 from Equation (1) estimated with an OLS linear probability model and where 'lowinc' is a dummy variable equal to one if a respondent's combined (highest) family occupation is NS-SEC 6-8, and zero if it is NSSEC 1-5. Controls are a one/zero dummy for single parenthood, age of the respondent (5 categories), age of the youngest child in the family (6 categories), education (5 categories), and ethnicity (4 categories). Working hours are conditional on being in paid employment. Person-household weightings are utilised to correct for non-response. * p<0.05 ** p<0.01 *** p<0.001.

Table A8: Double difference estimates of the mental health effects of the two-child limit

		(1)		(2)		(3)	
		Any mental health condition		Depression, bad nerves or anxiety		Other mental illness, phobias or panic attacks	
Full sample	β_3	0.018	0.007	0.016	0.007	0.002	-0.003
	s.e.	(-0.009)	(-0.009)	(-0.009)	(-0.009)	(-0.005)	(-0.005)
	N	211343	211238	211343	211238	211343	211238
Lone mothers	β_3	-0.029	-0.051	-0.037	-0.057	0.013	0.003
	s.e.	(0.031)	(0.031)	(0.030)	(0.030)	(0.020)	(0.020)
	N	39219	39193	39219	39193	39219	39193
Lone fathers	β_3	-0.331*	-0.388*	-0.229	-0.277	-0.086	-0.120
	s.e.	(0.148)	(0.152)	(0.138)	(0.143)	(0.090)	(0.088)
	N	5529	5526	5529	5526	5529	5526
Coupled mothers	β_3	0.018	0.016	0.021	0.019	-0.007	-0.009
	s.e.	(0.015)	(0.015)	(0.014)	(0.014)	(0.008)	(0.008)
	N	83916	83878	83916	83878	83916	83878
Coupled fathers	β_3	0.015	0.014	0.013	0.013	-0.001	-0.001
	s.e.	(0.012)	(0.011)	(0.010)	(0.010)	(0.007)	(0.007)
	N	82679	82641	82679	82641	82679	82641
Controls		No	Yes	No	Yes	No	Yes

Notes: Data from the Annual Population Survey. The table shows estimates of β_3 from Equation (1) estimated with an OLS linear probability model with mental health dummy variables as outcomes. Controls as in the employment outcomes models. Mental health conditions include depression, bad nerves or anxiety (column 2), and other mental illness, phobias or panic attacks (column 3). The first column includes both categories. Person-household weightings are utilised to correct for non-response. Standard errors in parentheses. * $p<0.05$ ** $p<0.01$ *** $p<0.001$.