Does cutting child benefits reduce fertility in larger families? Evidence from the UK's two-child limit*

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Last updated: September 27, 2022

Abstract

We study the fertility effects of restricting child-related social assistance to the first two children in the family. As of April 2017, all third and subsequent children born to low-income families in the UK were made ineligible for approximately 3000 GBP of means-tested child benefits per year. We leverage administrative births microdata to estimate the impact of the two-child limit on higher-order births with a triple differences approach. We find some suggestive evidence of a decline in higher-order fertility among low-income families. However, effects are not statistically significant and compared to previous research in the UK and elsewhere, the implied elasticities are small.

Keywords: fertility, family size, social assistance, welfare reform *JEL*: J13, J18, H31, H53

*We acknowledge funding from the Nuffield Foundation from grant FR-23208. We thank Mike Brewer, Kitty Stewart, Kate Andersen, our Nuffield advisory board, CASE Researchers' Workshop attendees, and the Social Policy Quantitative Reading Group for helpful comments on previous drafts.

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1 Introduction

How do financial incentives and constraints affect household fertility decisions? The basic economic model of fertility, as developed by Becker (1960), treats children as analogous to consumer durables, with associated costs and benefits. The implication is that government policies, in particular the treatment of children in the tax and benefit system, will change these costs and benefits and therefore affect fertility decisions. However, even in this simple model, the magnitude of these effects will depend both on individual and household preferences and on other variables affecting family incomes; they are therefore likely to depend on the specific economic and social context. The complexity of the factors involved, and the relative rarity of substantial changes to financial incentives that can be used to cleanly identify the impacts, mean that empirical estimates remain limited.

In this paper we use administrative and survey data to examine the impact of a major change to the treatment of children in the UK benefit system: the introduction of the "two-child limit" for cash benefits to low-income families. This change meant that means-tested child benefits – typically of the order of 2,845 GBP per year per child – were no longer payable for children born on or after 6 April 2017 to households that already had two or more children. While a handful of countries restrict means-tested child benefits to the third or fourth child, this policy was the first attempt internationally to cap child benefits at the second child (Patrick 2022). The two-child limit therefore represents a rare but significant policy experiment, which enables us to identify how financial incentives impact on fertility decision-making.

The impact of the two-child limit on families' fertility decisions is of considerable interest, both from a policy and a research perspective. In contrast to other changes to the UK benefit system for low-income families, this change was justified, at least in part, by the argument that benefit payments to low-income families with children incentivised higher levels of fertility, and that removing them would have the opposite effect (Treasury & DWP 2015). Implicitly, at least, the objective of the policy was not just to reduce expenditure but also to reduce fertility among larger families on a low-income. It was argued that families on a low-income should have to make fertility decisions based on what they can afford.

By contrast, opponents of the change made both normative arguments – that it was inappropriate for the state to attempt to reduce fertility among low-income households by cutting benefits, and that if a certain level of support per child was appropriate, it should apply to all children – and positive ones, that the policy change was unlikely to have a large impact on fertility decisions. The main impact of the change, opponents argued, would therefore be to increase poverty among larger families. Establishing to what extent these narratives are consistent with the empirical evidence is useful in informing both evaluation of the policy and wider debate on the future of the welfare system. It is also relevant to wider debates about trends in fertility in advanced economies: the change came as overall fertility rates in the UK were falling steadily, with the UK's total fertility rate falling from 1.9 in 2011 to 1.65 in 2019.

The nature of the two-child limit, and the manner of its introduction, makes it well suited to causal identification of the impact of financial incentives in the welfare system on fertility. It was a large change, introduced at a single point in time, and it affected some groups, while leaving others (those with fewer than two children, or not receiving benefits) entirely unaffected. In 2022, 359,000 households (including 1.3 million children) were affected by the policy (Department of Work and Pensions and Her Majesty's Revenue and Customs 2022). The policy only applied to third or subsequent children who were born on or after 6 April 2017, creating a plausibly exogenous source of treatment variation both by number of children and by child's date of birth. We leverage this variation to isolate the causal impact of the two-child limit on fertility using a difference-in-difference-in-differences (or triple differences) strategy, exploiting variation over time, over socio-economic status, and over birth order.

Our results suggest that the impact of the policy change on fertility was relatively small. The period before and after the policy change saw steady falls in the overall number of births in the UK. However, at the point of change, we do not find evidence of a significant reduction in the relative number of births among those affected. Our preferred triple-difference estimate is of a decline of approximately 0.24 percentage points (approximately 3.2 percent) in the probability of low-income occupation women with two or more children having a 'higher-order' (third or subsequent) child after the policy. Of the births that took place, we also see a slight moderation of the pre-existing trend of increasing relative higher-order fertility rates among low-income groups. However, the effect size is again small. These results are corroborated by a regression discontinuity design in the date of birth of the child.

These results are relatively surprising, and out of line with the findings of most previous research in the UK and elsewhere; they challenge standard economic models of how financial incentives affect household decision-making. They also undermines the implicit policy rationale for the change: rather than causing a major reduction in the number of children born into low-income families, the main impact will have been to increase the depth and incidence of child poverty.

The paper is structured as follows. We briefly describe the increase, and subsequent reduction, in the generosity of the welfare system in the UK over the last two decades, and the context for the introduction of the two-child limit. We then review the evidence on the impact of the structure and generosity of the benefit system on fertility. Our principal contribution is our empirical analysis, based on a version of a triple differences strategy which uses comprehensive administrative data on 3 million births in England and Wales both before and after the policy change. To conclude, we discuss possible interpretations and implications of our analysis, from both a research and policy perspective.

2 Background

Since the establishment of the modern welfare state after the Second World War, the UK has supported families with children through the tax and benefit system through a combination of universal (Family Allowance and Child Benefit) and means-tested support. The latter was initially for families with essentially no income, but was extended to low-income working families with the introduction of Family Income Supplement in the 1970s. This provision was made significantly more generous in the 2000s with the introduction of Working Families Tax Credit, which was modelled on the US Earned Income Tax Credit.

The objective of this expansion of benefits in the 2000s was both to reduce poverty among low-income families and to increase employment. Though there is no evidence to suggest that increased fertility was an objective of these policies, in practice Brewer, Ratcliffe & Smith (2012) found that those most likely to have been affected by the changes in financial incentives saw a differential increase in fertility, with an estimated rise of 1.2 to 3.6 percentage points in the annual probability of those most affected having a child, equating to an increase of approximately 10,000 to 35,000 in annual births. This was a period in which overall fertility rates in the UK were rising, with an increase in annual births of over 100,000 a year in the 2000s. In ONS (2009)'s descriptive analysis of these trends, they attributed about a third of the rise to the greater affordability of children for low-income households (including improved childcare provision as well as higher benefit levels), though this was not a causal estimate.

After the global financial crisis, a Conservative-led Coalition government enacted significant cuts to welfare benefits as part of its wider programme of fiscal consolidation. The initial programme of cuts included changes to benefits for sick and disabled people, capping the amount of benefits that could be received by individual households, reductions in the uprating of benefits, and the so-called 'bedroom tax' that restricted housing benefit payments for those with 'extra' bedrooms. This programme did not focus explicitly on larger families, though overall larger families were more adversely affected, especially by the benefit cap (Stewart, Reeves & Patrick 2021, Gaffney 2015, Reed & Portes 2015).

While presented primarily as an economic necessity – the prevailing levels of benefits were described as unaffordable, given the impact of the recession on government borrowing – a parallel narrative also suggested that the benefit system had undesirable moral hazard effects. The Prime Minister, David Cameron, argued in 2011:

> "The benefit system has created a benefit culture. It doesn't just allow people to act irresponsibly, but often actively encourages them to do so." (UK Government 2021).

In conjunction with some sections of the press, the government sought to justify benefit cuts by arguing that very significant payments were being made to people who were either making dubious or fraudulent claims or who were (legally) "milking the system" (Gaffney & Portes 2013). Frequently, the latter referred to larger families or so-called "benefits broods"; such a case, and the wider background, is described at length in Jensen & Tyler (2015).

2.1 The two-child limit

After winning the 2015 General Election, in July a majority Conservative government announced a further programme of benefit cuts, including the introduction of a "two-child limit". While the existing system of tax credits (which was in the process of being replaced with a new, integrated benefit, Universal Credit) paid an equal amount for each child, in future payments would only be made in respect of the first two children for each household. That is, for children born on or after 6 April 2017, no child element would be paid if the household already had two or more children. Households are exempt from the two-child limit if their third child is a multiple birth, an adopted child, in a non-parental caring arrangement, or if they are the result of non-consensual conception (see Treasury & DWP (2015) for details of the policy's scope and impact).

The government described the objective of the two-child limit as being "to ensure that families in receipt of benefits faced the same financial choices about having children as those supporting themselves solely in work" (Treasury 2015). Implicit in this was the assumption that incentives within the benefits system influenced fertility. This view was made explicit in published Impact Assessments, which (based in part on Brewer, Ratcliffe & Smith (2012)) suggested that the policy change would result in reduced fertility, although no quantified estimates were made:

> "In practice people may respond to the incentives that this policy provides and may have fewer children...Given that families are aware of the policy they may make the choice not to have (further) children." (Treasury & DWP 2015)

Yet in response to a Work and Pensions Select Committee report on the two-child limit, the government appeared to deny that curbing fertility was a key policy aim:

> "This policy does not attempt to limit the number of children people have. Claimants are able to have as many children as they choose, in the knowledge of the support available." (Work and Pensions Select Committee 2019).

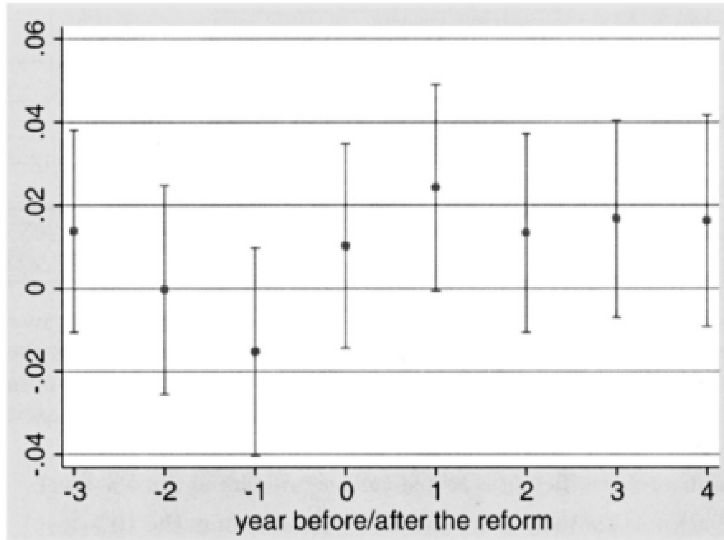
There were thereby some inconsistencies in how the government defended the policy, with ambiguity over how far fertility changes were themselves an explicit policy objective, perhaps reflecting the political sensitivity around the intervention. But ultimately, irrespective of whether the reduction of fertility was intended or not, it is plausible that it may have achieved this in practice, given existing evidence on the link between welfare and fertility.

2.2 Existing evidence on welfare and fertility

As noted above, Brewer, Ratcliffe & Smith (2012) examined the impact of increases in the generosity of the UK benefit system in 1999. They used a difference-in-differences approach, exploiting the fact that the reforms were targeted at low-income households, and that the changes were likely to differentially affect the fertility of women in couples and single women because of the opportunity cost effects of the welfare-to-work element. Using data from the Family Resources Survey, they found no increase in births among single women, but an increase in births among coupled women on relatively low incomes, although, given the relatively small number of births observed in the data, there is considerable uncertainty around this estimate.

The key results are shown graphically in their Figure 2, reproduced below: there is an observable upward shift coinciding with the increase in the generosity of the system. It seems reasonable to conclude that there was indeed an impact, but the confidence intervals are large relative to its size and since the analysis uses annual data, there is scope for other exogenous changes to affect the estimates. So while the central estimate is that the impact was an increase in the probability of having a child of approximately 2 percentage points per year in the affected groups (an increase in fertility rates of about 15 percent), a very high degree of uncertainty must be attached to this.

Fig. 2 Birth differentials between treatment and comparison groups



Source: Brewer, Ratcliffe & Smith (2012).

Meanwhile, Laroque & Salanié (2014) take a structural approach and, using data from France, find a substantial response of fertility to financial incentives in the French tax and benefit system. The response differs according to birth order, with first and (especially) third births particularly responsive. They use their estimates to simulate the impacts of an unconditional child credit of 150 Euros a month per child; they estimate that this would raise birth rates among the most affected group (i.e., those with two children) by 4.8 percentage points. These studies suggest quite large positive "elasticities" of fertility with respect to benefits. A number of other empirical studies in developed countries suggest positive fertility effects from the expansion of benefits (González & Trommlerová 2021). Evidence of negative fertility effects from benefit cuts or withdrawals is thinner, but there are some indications of this (Cohen, Dehejia & Romanov 2013, González & Trommlerová 2021).

These results are sensitive to institutional context. Research in the US is less suggestive of large impacts. Following the introduction of Temporary Assistance for Needy Families (TANF) programme in the 1990s, a number of states introduced measures to deny additional social assistance to any children born after a benefit claim was made. Research on these family caps has broadly found no effect on fertility (Joyce, Kaestner, Korenman & Henshaw 2004, Camasso & Jagannathan 2016, Kearney 2004, Dyer & Fairlie 2004). However, even leaving aside the very different social and economic context of the US, it is not clear that the two-child limit will have the same fertility effects as family caps. While the policies are similar in their aims, they are rather different in their application: for family caps, the determinant of whether you are capped is the timing of pregnancy relative to your benefits claim; for the two-child limit, it is the size of your family. The interaction with other aspects of the welfare system (notably work requirements and childcare) is also very different.

Fertility responses are also heterogeneous across the income distribution. The existence and magnitude of effects, therefore, depends on the specific targeting of the policy in question. Milligan (2005), using a triple differences approach to examine the introduction and subsequent withdrawal of a universal C$8,000 cash payment to families with newborn children in Quebec, found large fertility effects, with an extra C$1,000 leading to an increase of about 17 percent in fertility. However, the effects were heterogeneous in income, with considerably larger fertility responses among better-off families. While this may seem counterintuitive, it is in fact consistent with the canonical Becker model. Where families can choose to spend on both the quantity and the "quality" of children (where "quality" is defined as per-child spending), the impact of a simple per-child benefit is ambiguous. While the substitution effect (having children becomes less costly relative to working) unambiguously increases fertility, the income effect might either increase or decrease fertility, depending on whether the income elasticity of fertility is negative or positive (just as a cut in income taxes can either increase or decrease hours worked, depending on the income elasticity of demand for leisure). So while intuitively one would expect a cut in child benefits to reduce fertility, this need not be the case for all families (Milligan 2005, Cohen, Dehejia & Romanov 2013, Riphahn & Wiynck 2017).

In particular, if low-income families face a particularly sharp "quality-quantity" trade off, they may respond to the higher incomes that result from a per-child benefit by reducing the number of children. That is, the increased income resulting from a per-child benefit for poorer families with one or two children may mean that they choose not to have a subsequent child.

However, for the policy change we examine – the removal of benefits for third and subsequent children – the income effect is less relevant. There is no income effect for the first two children, so the choice to have a third child should be driven solely by the substitution effect, and the impact of the policy change should be unequivocally negative.[1]

Our prior, therefore, based on previous research, and the nature of the policy change, is that the two-child limit is likely to have reduced fertility among affected households. There is no reason to expect the fertility elasticities with respect to financial incentives to be the same across time and place, but if the impacts were of similar magnitudes to those found in the papers we focus on in the discussion above, we would expect to see very large changes among affected households. The financial impact of the two-child limit is almost twice as large in PPP terms as that of the hypothetical benefit increase modelled by Laroque & Salanié (2014) (OECD 2021), implying an impact of more than 7 percentage points, and even larger compared to Milligan (2005). Similarly, the implied impact would be considerably larger than that suggested by the Brewer, Ratcliffe & Smith (2012) estimate: the two-child limit costs affected households by up to 55 GBP per week, considerably larger (in real terms) than the differential impacts of the 1999 changes. With this in mind, we turn to our empirical analysis.

3 Data

Our main data source is administrative birth registrations microdata from 2015-2019 in England and Wales.[2] This provides individual-level data for all births (both live and stillbirths) in England and Wales over this period, with variables on the date of birth of the child, the number of previous live births born to the mother, multiple birth status, stillbirth status, maternal and paternal age, country of birth and postcode of residence of the mother. In addition, for a random 10 percent

[1] In principle, the removal of benefits for the third child could, via the aforementioned income effect, lead to some families choosing to have a fourth child. However, this is highly unlikely to be relevant over the time period we examine.

[2] This work was produced using statistical data from the Office for National Statistics. 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.

sample of live births, the data includes socio-economic status (SES), as defined by the National Statistics Socio-economic Classification (NS-SEC)'s measure of occupation (Office for National Statistics 2016). We create a measure of combined occupational status by selecting the highest-ranked occupation of the mother or father, where available. While we use mother's occupation as a robustness check, family occupation is our preferred measure as it best captures the likelihood of household entitlement to means-tested benefits (to see the relationship between family occupation, the number of children and benefits receipt, see Figure 1 in Appendix B). Among women aged 16-45 with two children, for example, 65 percent of those in routine occupations receive one or more of these benefits, relative to just 6 percent of those in higher professional and managerial occupations.

In the births data we also construct geographical variables to proxy the probability that a household will be affected by the two-child limit. We match each child's postcode of residence to its Lower Super Output Area (LSOA) – a small geographical area of approximately 1500 residents – and in turn match its LSOA to an Index of Income Deprivation score[3], a government measure which uses administrative benefits data to capture the proportion of the population in an area who are receiving means-tested benefits (Ministry of Housing, Communities and Local Government 2020).[4] We also match the residence of the child to published data at local authority level on the number of households who are affected by the two-child limit in 2021 (Department of Work and Pensions and Her Majesty's Revenue and Customs 2022). In combination with published data from the Annual Population Survey on the number of households by local authority as of 2019, we construct an local incidence variable that measures the percentage of total households in a local authority who are affected by the two-child limit (Office for National Statistics 2021).

Our second data source is the Annual Population Survey (APS). The APS is the largest household survey in the UK, with its annual sample including approximately 320,000 individual respondents

[3]We use the joint Income Deprivation Index from 2019 which covers both England and Wales.

[4]These benefits are very similar to, and in fact exhaustively cover, the benefits through which the two-child limit is implemented. This includes those in receipt of Income Support, income-based Jobseeker's Allowance, income-based Employment and Support Allowance, Pension Credit (Guarantee); Universal Credit where no adult is classed within the 'Working – no requirements' conditionality group; those not already covered in receipt of Working Tax Credit or Child Tax Credit with an equivalised income (excluding housing benefit) below 60 per cent of the national median before housing costs; and asylum seekers in receipt of subsistence support, accommodation support, or both (Ministry of Housing, Communities and Local Government 2020).

in 122,000 households. The APS contains data at the individual level on the sex, age and NS-SEC occupation of the respondent, which benefits they receive (if any), the number of dependent children under 16 in the family unit, and the type of family unit (e.g. single parent or couple family). The family unit is the relevant unit of observation for benefits receipt in the APS. We match respondents by family unit and construct a combined occupation measure that mirrors that of the births data, selecting the highest-ranked occupation of members of the family. We restrict our APS sample to women of approximate childbearing age (16-45) in England and Wales, the latter of which to be consistent with the geographical coverage of the administrative births data.

The administrative births data enable us to accurately capture the number of births at population level, with very low measurement and sampling error. However, the unit of observation in the births data is the child rather than the woman or couple deciding whether to have a child (our main population of interest). Women who do not have a child are, by definition, not captured in the births data. Additionally, the births data does not contain individual-level data on benefits receipt. We therefore use the APS for two purposes: first, to calculate the total number of women in different population subgroups (by receipt of benefits and number of children), so that we can compute average fertility rates for those groups; and second, to quantify the relationship between occupation and benefits receipt, so that we can use occupation as a robust proxy for benefits receipt in our analysis. We are unfortunately not able to link the two datasets together; instead, we draw on them separately to estimate the relevant numerators and denominators for the probability of having a child, first by benefits receipt and family size, and then by occupation and family size, in order to conduct a causal triple differences analysis.

We focus on the years 2015-2019 in our analysis. This gives an approximately equal amount of time either side of the introduction of the policy. The recording of benefits receipt in the APS changed considerably in 2015, so it is difficult to construct a consistent series prior to that. We do not use data beyond the first quarter of 2020 on account of the possible impact of Covid on fertility, which may introduce systematic bias into our results by affecting low-income and larger families more (Reader & Andersen 2022).

In Table 1 we present descriptive statistics for the administrative births data for the universe of all births in England and Wales from 2015 to 2019. 15.7 percent of births have single parents and 3 percent are multiple births (i.e., twins or triplets).[5] Mean maternal and paternal age is 30.6 and 33.4 years respectively. The mean income deprivation score is 14.9, which means that on average 14.9 percent of the population in a given neighbourhood receive means-tested benefits. In Table 2 we include a breakdown of the number of women in our population of interest – women aged 16-45 in England and Wales – using weighted annual population estimates from the Annual Population Survey. On average across our chosen time period, there are approximately 10.3 million women in our population of interest, 993,833 of whom have two or more children and are in receipt of the benefits affected by the two-child limit.[6] Approximately 1.4 million women have two or more children and are in what we define below as low-income (or high benefits receipt) occupations.

It is worth noting three key features of the aggregate data. First, the number of births by year has been decreasing throughout the period covered by the data; second, the proportion of births that are 'higher-order' (that is, third or subsequent live-born children to the same mother) has also fallen. Figure 1 charts the number of total births (left axis) alongside the number of higher-order births (right axis) in England and Wales by year from 2013 to 2019. Higher-order births started to fall in 2015, as did total births in 2016. Finally, fertility is higher among low-income groups (see Appendix B Table 1).

4 Method

Our objective is to identify the impact of the two-child limit on fertility of those 'treated' by the policy – that is, women (and/or couples) who, if they were to have a child after the implementation date of the policy, were no longer eligible for child-related social assistance for that child. Since the policy applied only to those in receipt of benefits with two or more existing children and was rolled

[5] Single parents defined as those who either register the birth on their own or report that the parents live at different addresses.

[6] These benefits are Universal Credit, tax credits, housing benefit, Jobseeker's Allowance (JSA), and income support.

out over time, a difference-in-differences setup is a natural choice for an identification strategy. The policy also features exogenous variation in treatment status by child's date of birth, enabling us to estimate instantaneous effects local to the cut-off using a regression discontinuity design in Section 5.3.1. As a further robustness check, in Appendix A we undertake an alternative approach with the administrative births data in which we focus on the births that did take place, estimating differential changes in the probability of a child, having been born, being a higher-order (3+) birth to a low-income family after the reform.

Estimating a difference-in-differences approach requires us to define treatment and control groups, and to establish that the parallel trends assumption is satisfied (that is, that in the absence of treatment, the treatment and control group would have evolved similarly). The obvious candidate for the treatment group is low-income women (single parents or members of a couple household) with at least two children. It is less clear what the control group is. One candidate is low-income women with no children or one child, who, even after the policy change, could have a child and still receive an additional child-related benefit payment. But the parallel trends assumption seems implausible: there are numerous factors other than the policy change (other changes to the benefits system, as well as wider social and demographic trends) that would be likely to change the desired number of children, and hence relative birth rates to women with different numbers of children.

An alternative candidate for the control group is high-income women with at least two children. Broader social trends on family size would arguably affect both these treatment and control groups in similar ways. However, again, it is plausible that trends in fertility may differ between low-income and high-income women.

Our preferred approach is therefore a difference-in-difference-in-differences (or triple differences) approach, which creates four groups, only one of which is treated, and which combines the two differences above. That is, the impact of the policy change is estimated by looking first at the change in fertility between low-income women with larger families and high-income women with larger families; and then at the change in fertility between low-income women with smaller families and high-income women with smaller families, and comparing these two changes.[7]

In principle, this should abstract both from differential changes in fertility between those on a low income and those not, and differential changes in fertility between larger and smaller families. This triple difference approach (which is possible because of the nature of the policy change, affecting only women with larger families on benefits, rather than just those on benefits or just those with large families) enables us to be more confident that the parallel trends assumption is satisfied, and should be more robust than a simple difference-in-differences strategy (Milligan 2005).

We implement the triple differences by leveraging the administrative births and APS data to estimate differential changes in the probability of low income women having a higher-order (3+) child after the reform. There are two main methodological choices here: how to define being on a low-income, and how to calculate the probability of having a child with the data available.

4.1 Defining low income

One of the three elements of variation in the triple differences is whether a birth is to a low-income family (and therefore a family likely to be receiving means-tested benefits) or not. Deciding how to measure this involves a trade-off between accuracy and bias. One obvious option is to use self-reported benefits receipt[8], since it should directly capture whether a family falls within the scope of the benefits system and therefore the two-child limit. This is the approach we take in Section 5.1.

However, benefits receipt may generate bias in our estimates over time for two reasons. First, benefits receipt is not exogenous either to having an additional child or to the policy itself. Most obviously, single women with no children are likely to be in work, and unlikely to qualify for means-

[7]Note that it does not matter which difference is taken first; that is, we could equally compare fertility rates for low-income women with no or one child with those with two or more children; do the same for high-income women; and then compare the two differences. As the algebraic presentation below makes clear, the result is identical.

[8]These benefits are the main means-tested benefits that are affected by the two-child limit: Universal Credit, tax credits, income support, and jobseekers' allowance. We do not include child benefit because it not affected by the two-child limit.

tested benefits; but most single women with children do qualify, and in general the likelihood of claiming benefits rises with the number of children.[9] Moreover, the policy change itself impacts benefit entitlement, so some women with two children not on benefits who go on to have a third child would, prior to the policy change, qualify for benefits, but no longer do so. This subgroup should in principle be in the treatment group (since they are affected by the policy change) but the analysis above will not identify them as such.

Second, the characteristics of women on benefits has changed over the period in question due to the decline in the number of women claiming benefits over this period (DWP 2021, HMRC 2021). While this fall was primarily exogenous – it was not driven by the two-child limit, but rather by other reductions in benefit generosity, and rising employment – it was sufficiently large to affect the demographic composition of the different groups. While we have no reason to believe that this would bias our results, particularly as the triple differences exploits variation by birth order as well as benefits receipt, it is difficult to rule out the possibility entirely. This also makes interpretation harder, since the impact on families' decision making presumably depends on whether they expect to be affected by the policy change, which may in turn be determined by benefit history and expectations as well as current benefit status.

In Section 5.2 we therefore use a more exogenous, and less time-variant, measure of low-income: whether parents are working in a low-income occupation or not. This is plausibly a fuzzy but valid instrument for the probability of being on benefits, as indicated in Figure 1 of Appendix B. Family occupation is strongly correlated with benefits receipt, but it is much less likely to change from year to year and (particularly at the family level) is less likely to be endogenous to the number of children or to the policy change. It may therefore be better correlated with a woman's perception of the likelihood of being affected by the policy than benefits receipt at a point in time. Measuring treatment by reference to this more persistent indicator may give a better measure of "effective" treatment than benefits receipt at a point in time (just as SES may in some contexts be a better indicator of, say, living standards than contemporaneous income, if income is highly variable).

[9] This is likely to explain at least in part the much higher 'risk' shown in the third column of Table 3; the denominator is an underestimate of the true size of the treatment group here.

Based on the observed probabilities in Figure 1 of Appendix B, we group observations into those with a family occupation with a higher probability of benefits receipt (NS-SEC 3, 4, 5, 6, and 7) and those with a lower probability of benefits receipt (those in NS-SEC 1 and 2).

4.2 Estimating the probability of having a child

Our relevant outcome of interest is the probability of a woman or couple having a child. To estimate these probabilities, we need to separately identify the numerators and denominators for each group at each time point.

The denominator is relatively simple: it is the number of women who could potentially have children, grouped by whether they already have two or more children or not, and by whether they are on a low income (as defined by benefits receipt or family occupation). To construct the denominators we use the Annual Population Survey to construct annual weighted population estimates of the number of women aged 16-45 in the following categories: low-income women with 0-1 child; high-income women with 0-1 child; low-income women with two or more children; and high-income women with two or more children. This provides our estimate of the number of women in the four distinct groups (one treated; three non-treated) defined above.

We now need to estimate the numerator: the number of births in each group during each time period. Here we use the administrative births microdata to estimate the total number of births by year, family occupation, and the number of previous live births to the mother. This is preferable to relying on the Annual Population Survey alone; the administrative data (with family occupation) captures 10 percent of all births directly, much more than the APS and with greater reliability. In Section 5.1, where we use benefits receipt as a measure of treatment status, we need an estimate of the number of births by benefits receipt: this is complicated by the fact that the administrative births data does not provide any information on benefits eligibility. For this part of our analysis, we therefore use family occupation and the number of previous live births to estimate the probability that a given birth is to a family on benefits using the APS.[10] We use these probabilities to estimate

[10]Note that the APS does not include an exact variable for the number of previous live births to the

the relevant numerators: the number of births by benefits receipt, higher-order (3+) status, and year. In Section 5.2, where we use low-income family occupation as a measure of treatment status, we simply count the number of births by family occupation, higher-order (3+) status, and year, to construct the relevant numerators.

For each group, we combine these numerators and denominators to calculate quarterly probabilities that a woman has a child. Since the births and APS data cannot be combined, we calculate standard errors manually.[11] We apply the triple differences approach to these probabilities to estimate whether fertility decreases differentially after the two-child limit (from 2017 Q2) among women on benefits with two or more existing children.

Algebraically, we present the probability of having a child as follows:

$$P_{it} = \beta_0 + \beta_1 B_{it} + \beta_2 L_{it} + \beta_3 (B_{it} \times L_{it}) + \epsilon_{it}, \tag{1}$$

where P_{it} is the probability of individual i having a child in time period t, B_{it} is a dummy variable equal to one if the individual is on a low income (as defined either by benefits receipt or by low-income occupation), L_{it} is a dummy variable equal to one if the individual has two or more children, and ϵ_{it} is the error term. Our coefficient of interest is β_3: it represents the marginal impact of being on a low income and having two or more children on the probability of having a child.

Four groups can then be defined by whether they are low-income and whether they have two or more children. The probability of having a child for each of these groups can be expressed as:

$$A : P_{it}|(B_{it}(1) \times L_{it}(1)) = \beta_0 + \beta_1 B_{it} + \beta_2 L_{it} + \beta_3 (B_{it} \times L_{it}) \tag{2}$$

mother, but the number of dependent children under 16 in the family is a relatively good proxy for this.

[11] We calculate standard errors manually by extracting the standard error from the proportion of women/births in each category and scaling this up to a population estimate. As recommended by Lindberg (2000), we back out the probability SEs according the following formula: (Probability of having a child SE/Probability of having a child) = (Births SE/Births) + (Women SE/Women). In subtracting the various differences in the triple differences framework, we base the standard errors on the number of women in the group according to the formula $\sqrt{((p * (1 - p)/n))}$.

$$B : P_{it}|(B_{it}(1) \times L_{it}(0)) = \beta_{0it} + \beta_1 B_{it} \tag{3}$$

$$C : P_{it}|(B_{it}(0) \times L_{it}(1)) = \beta_{0it} + \beta_2 L_{it} \tag{4}$$

$$D : P_{it}|(B_{it}(0) \times L_{it}(0)) = \beta_{0it} \tag{5}$$

In a standard difference-in-differences, the parallel trends assumption, required to identify the impact of the policy change at time t, is that while β_0, the constant/base probability, can vary (the "trend"), the subgroup specific parameters β_1, β_2 and β_3, remain constant (so the trends are parallel for different groups) except when affected by the policy change.

Our triple difference analysis allows us to relax that assumption partially. In particular, we assume that β_0, β_1 and β_2 – the base probability, but also the marginal effect of either being on benefits or having two or more children – vary over time. For the parallel trends assumption to be valid here, all we need is that β_3 – the additional probability of having a child that results from the *interaction* of being on benefits and of already having had two or more children – remains constant except when affected by the policy change. As shown in Figure 2 and Figure 3 in Appendix B, there are no significant trends in β_3 prior to the policy change.

We denote the change in β_3 from time period *Post* to period *Pre* as the following:

$$\beta_3{}' = \beta_{3post} - \beta_{3pre} \tag{6}$$

The difference in the probability of having a child between the *Post* and *Pre* period is therefore the following for each group:

$$A : \beta_0{}' + \beta_1{}' + \beta_2{}' + \beta_3{}' \tag{7}$$

$$B : \beta_0{}' + \beta_1{}' \tag{8}$$

$$C : \beta_0{}' + \beta_2{}' \tag{9}$$

$$D : \beta_0{}' \tag{10}$$

We then have that $\beta_3{}'$ – the average impact of the change in benefits entitlement for a third or subsequent child, or the average treatment effect on the treated (ATT) – can be estimated by calculating $(A - B) - (C - D)$.

5 Results

We document our results in three parts. First, in Section 5.1, we define treatment status by benefits receipt and estimate differential changes in the probability of having a child for those on benefits with two or more existing children. This has the benefit of enabling us to accurately track fertility trends for those actually treated by the policy, albeit with potential bias and endogeneity. In Section 5.2, we adopt more of an intention-to-treat approach and use the fuzzier proxy of low-income occupation to estimate differential changes in the probability of having a child for those in low-income occupations. In Section 5.3, we report results from two main robustness checks: a regression discontinuity design in the date of birth of the child, and a test for anticipation or lagged fertility effects.

5.1 Differential changes in the probability of having a child, by benefits receipt and family size

Figure 2 charts the quarterly probability of having a child by benefits receipt, family size from 2015 to 2019, with simple DDD estimates shown in Table 3. The quarterly data is preferable for the identification of effects since it enables us to utilise the April 2017 cut-off and be more precise in our identification of a *Pre* and *Post* period. However, in Online Appendix B we produce the graphs on an annual basis (Figure 4 and Figure 5), which smoothes out the data more clearly. While there is a fall in the probability of having a higher-order child among women on benefits in the post period, it is of the same magnitude as the fall in the probability of having a lower-order child among women on benefits. The mechanical effect of the triple difference is to produce a slight positive effect. It is implausible to suggest that the policy increased fertility among the treated group, so we interpret this as consistent with a null hypothesis of no effect.

5.2 Differential changes in the probability of having a child, by occupation and family size

Next, we estimate the triple differences using a fuzzier but more exogenous measure of the likelihood of being treated by the two-child limit: low-income occupation. Figure 3 and Table 4 show the main results from this approach. The change in the probability of having a third or subsequent child conditional on being in a low-income occupation is not dissimilar from that shown in the previous section. But unlike in the previous analysis, other groups do not share this reduction. Consequently, the overall triple differences estimate is negative and suggests a reduction in the probability of having a child of approximately 0.24 percentage points. This suggests that our previous analysis, based on benefits receipt, may suffer from some bias due to compositional changes in who reports receiving benefits over time. The APS suggests that 1,445,596 women are in the treatment group (as defined here: women in families with low-income occupations with 2+ children) each year, on average. The changes in probability in Table 4 therefore imply a reduction in the number of births of approximately 12,000 a year, 3500 a year of which can be attributed to the two-child limit. This equates to less than 1 percent of total annual births in England and Wales. The estimated effect is not statistically significant.

5.3 Robustness checks

Our results above, while suggestive of something of a decline in fertility, do not reject the null hypothesis of no fertility effects of the two-child limit. Given the marginality of our results, we stress test them with three key robustness checks, two of which are reported here and one of which is detailed in Appendix A.

5.3.1 Regression discontinuity estimates

To test whether our findings are sensitive to our choice of identification strategy, we implement a regression discontinuity (RD) design in the date of birth of the child. The benefit of this approach is that enables us to leverage the exogeneity of the date of birth criterion for the two-child limit, and to focus on those born local to the cut-off who are arguably 'as if' randomised around it (Lee & Lemieux 2010). The downside is that any RD estimation of fertility effects are by definition a local estimate, and do not account for the possibility of gradual effects. Since we manually combine data from the administrative births and APS data and therefore have to abide by statistical disclosure control in outputting the birth totals by time period, the most granular level at which we can implement the RD without disclosure is week of birth (centred at the cut-off of 6 April 2017).

Table 2 in Online Appendix B details RD results for a variety of bandwidths, estimated exclusively for those in our main treatment group – women with 2 or more children in a low-income occupation. Across all bandwidths, the RD estimates suggest a robust but very small negative discontinuity at the cut-off. Our preferred estimate, based on graphical inspection of the data, is for a 24-week bandwidth and suggests a discontinuous reduction in the probability of having a third or subsequent child of 0.856 percentage points. Figure 4 illustrates the RD graphically by charting the probability of having a child within a small 24-week window either side of the cut-off. A noticeable but small (and insignificant) discontinuity is visible. For context, Figure 6 in Appendix B graphs the discontinuity for all groups within the triple differences: as in the triple differences, there is an apparent decline for the treatment group but no decline for the other groups. Overall, these RD estimates support those of our triple differences: there appears to be something of a decline in fertility among women affected by the policy, but the magnitude is very small.

5.3.2 Anticipation or lagged effects

While there do not appear to be large fertility effects at the introduction of the two-child limit, we consider the possibility that there may have been anticipation effects after the announcement of the policy in 2015, or lagged effects after the policy's introduction.

The policy was announced in the July 2015 budget for implementation in 2017. Some media coverage, particularly in the broadsheet press, stated clearly that the policy would be introduced in 2017 and that 'families who have a third child after April 2017 could be caught' (Grice 2015, Slack 2015). However, other media sources made no such clarification, meaning that some families may not have known about the birth cut-off (Whitehouse 2015). On the (albeit unlikely) assumption that parents had perfect information about the policy announcement, this would give families almost two years to respond. Parents who were considering having a third child might decide to do so quickly in advance of the birth cut-off, so as not to ensure they receive benefits for that child. Alternatively, parents may interpret the policy as a wider signifier of a lack of support for larger families and decide not to have a third child. The direction of these anticipation effects is therefore not clear in theory.

It is also possible that fertility effects may have been lagged: parents may only have become aware of the policy once it was rolled out and once greater media attention was drawn to the policy (Marsh 2017).

To test for anticipation or lagged treatment effects, we examine our multiple period estimates (see our discussion of parallel trends in Section 4.2) to check whether there are notable trends before or after the policy's formal introduction. As is clear from Figure 2 and Figure 3 in Online Appendix B, there is no evidence of anticipation effects between the announcement and implementation of the policy (i.e., between the dashed and solid vertical lines). Neither is there evidence for a lagged fertility response thus far.

6 Discussion

The overall conclusion of our analysis that the introduction of the two-child limit had a measurable, but relatively small impact on the number of births to women in households who were affected by the limit. Our estimates take into account pre-existing trends in the data, including the reduction in overall fertility during the period.

Our preferred estimate – the triple differences analysis by occupation in Section 5.2 – suggests a reduction in the probability of having a child of 0.24 percentage points (3.2 percent in relative terms). We can compute an implied elasticity of fertility with respect to income by comparing this to the level of benefit cap (the level at which total benefits are capped in the UK, which is 20,000 GBP a year for most of the country) – a useful albeit rough estimate of income levels among families likely to be affected. This implies that the loss of income (compared to the previous system) for parents choosing to have a third child is approximately 14 percent.[12] The implied elasticity is therefore 0.22. By contrast, the elasticity implied by Brewer, Ratcliffe & Smith (2012) – which found an increase in fertility among those affected of more than 15 percent in response to an income increase of about 10 percent – is closer to 1.5. Other papers – notably Milligan (2005) whose triple difference identification strategy is closest to our own – find even larger elasticities, with considerably larger fertility responses in response to much smaller changes in family income.

A review of 22 studies found a wide range of results (Stone 2020). However, almost all studies

[12]In reality, average household income among those affected by the two-child limit is likely to be higher than this. Our estimation of an elasticity here is therefore an upper bound.

found significant and positive results, with an increase in the present value of child benefits of 10 percent of a household's annual income producing between 0.5 and 4.1 percent higher birth rates, with Brewer, Ratcliffe & Smith (2012) towards the lower end of the range. Extrapolating Stone (2020)'s methodology, the impact of the two-child limit could easily be in excess of 100 percent of annual household incomes over the life of the child (although it could be less if parental income increases enough to reduce benefit entitlement). Our results look very much at the low end of the range found by the literature.

On the face of it, this is surprising. As discussed above, the most closely related research suggests that increases in child-related benefits led to more-or-less immediate, significant, and quite large, increases in fertility among affected groups, even though (in the case of the changes examined by Brewer, Ratcliffe & Smith (2012)) the increases were quite widely spread and some of the impacts were potentially ambiguous. Our results, by contrast, imply that large cuts, with (in theory) large and unambiguous impacts on the financial incentives to have children for the affected group, had at most small and gradual impacts. What explains this asymmetry?

One possible explanation may be that Brewer, Ratcliffe & Smith (2012), in common with the vast majority of the studies reviewed by Stone (2020), are estimating the impacts of more generous welfare policies. By contrast, however, Sandner & Wiynck (2022) look at a German welfare policy change which, as in the UK, resulted in substantial cuts to household income for families with children. They found a fertility reduction of 6.8 percent for a large (18 percent) reduction in household income, implying an elasticity of 0.38, very similar to our results. They conclude that welfare recipients' fertility reacts less strongly to financial incentives than the fertility of overall populations.

Establishing the causal mechanisms at work here is beyond the scope of this paper. However, it is worth considering possible candidates. To the extent that those impacted did not know about the policy change, the impact of financial incentives would be attenuated. Recent qualitative research found that there is a lack of awareness of the policy; approximately half of the participants affected by the two-child limit did not know about the policy when they conceived their third or subsequent child (Patrick & Andersen 2022). The nature of the qualitative sample – which was restricted to larger families affected either by the two-child limit or the benefit cap – means that there is likely to have been some selection bias (since women who had been deterred from having another child by the policy will have been excluded), but nevertheless these results do suggest that imperfect information may have contributed towards the limited fertility response. This is consistent with broader research suggesting that levels of information among welfare recipients about the structure of the welfare system are relatively low, and that this may act to moderate the impact of financial incentives (Card 2020).

A lack of awareness of the policy prior to conception directly reduces the likelihood of a significant fertility response. In the absence of information, families may not find out about the policy until they notify the tax-benefit authorities of the new child, and their change of circumstances, thus removing the possibility of a fertility response. In some cases, families may find out about the policy during their pregnancy if they have contact with advisers or civil servants, as documented in the emergent qualitative evidence base (Patrick & Andersen 2022).

Where a lack of information means that families only find out about the policy during a pregnancy, abortion is the only available fertility response. There are likely to be low numbers of people who become aware of the policy during their pregnancy and are willing to have an abortion directly due to its existence. A survey of women who had abortions during the pandemic suggested that 57 percent who were likely to be affected by the two-child limit said it was a relevant factor in their decision (British Pregnancy Advisory Service 2020). However, this was a small survey with a highly selected sample and as such we cannot draw substantive causal conclusions from it. Abortion microdata is very difficult to access in the UK and published data do not include a breakdown by both the number of previous live births and by socio-economic status. Nevertheless, we examined published data on abortions by the number of previous live births in England and Wales (Department of Health and Social Care 2021). There is no evidence in the data of a substantial abortion response among those with two or more children; existing trends (towards more abortions, and a shift in the age distribution of abortions towards older women) do not appear to have shifted substantially at the time of the policy change, as shown in Figure 7 in Appendix B.

However, it is worth noting that awareness of the policy may grow over time, especially as the number of affected families rises. This may increase the scale of the fertility response in the long run.

Other possible explanations are more speculative. It may be that, in contrast to Laroque & Salanié (2014)'s findings in France, in the UK women considering having a third or subsequent child are *less* responsive to economic incentives in fertility decisions. Religious and cultural factors may be relevant here: the two-child limit disproportionately affects orthodox Jewish and Muslim families (Work and Pensions Select Committee 2019). Several of the affected families in a qualitative study of the policy had religious beliefs which meant that they reported the two-child limit as having no impact on their conception decisions; adherence to their religious faith to them was more important than the financial incentives at play (Patrick & Andersen 2022). Cohen, Dehejia & Romanov (2013) found that the ultra-Orthodox Jewish population in Israel were less responsive in fertility behaviours to changes in financial incentives – namely a benefit cut affecting larger families.

Finally, it is of course well established in low-income countries with high fertility and high infant mortality that increased prosperity reduces fertility, and that decreased prosperity can increase it (Kleven & Landais 2017). The broad explanation here is that increased prosperity reduces infant mortality, so women need fewer children to be assured that one or more will reach adulthood; it also increases women's choice and agency, so that they can choose to have fewer children. The infant mortality effect is not normally relevant in developed economies. However, it is at least possible that even in the UK reducing access to economic resources has negative impacts on choice and agency, resulting in reduced access to contraception, worse mental health, and less interaction with health services; all of these could potentially increase fertility and thereby attenuate the response to financial incentives (Cesur, Gunes, Tekin & Ulker 2021, Kearney & Levine 2009).

7 Conclusion

Over the last thirty years, welfare states in the developed world have become concerned with the potential for moral hazard within the context of welfare and fertility: the assumption that expansions in benefits lead to expansions in fertility, and vice versa. In the 1990s, this motivated several US states to introduce ‘family caps’ under Clinton’s programme of welfare reform; today it can be seen in the handful of countries that cap means-tested child benefits at the third or fourth child (Patrick 2022). However, until 2017 no country has attempted to cap child benefits at the second child (Patrick 2022). Identifying the causal impact of the UK’s two-child limit on fertility therefore offers a unique opportunity to identify the causal impact of capping child benefits by family size. It also offers opportunities for credible causal identification through a triple differences design.

In this paper we show that capping child benefits at the second child leads to much smaller fertility effects than one might expect. This is surprising given the literature to date, which through the examination of benefits expansions has suggested relatively large elasticities between benefits and fertility (Brewer, Ratcliffe & Smith 2012, Laroque & Salanié 2014). We are not able to identify the reasons behind this asymmetry, but we speculate that imperfect information about the policy, the relative ‘stickiness’ of attitudes towards abortion (and indeed fertility preferences more generally), and the negative effect of benefit cuts on choice and agency, may have been important.

What are the policy implications of this? If capping child benefits does not have large impacts on fertility, then the implication of our findings is that the savings from the policy result almost exclusively from lower payments to poorer families, with only a marginal additional impact from reduced fertility. In having greater household needs and lower work intensity on average, larger families already faced a disproportionate risk of poverty, and child poverty in the UK among this group has increased sharply since 2013-14 (Bradshaw 2020, Stewart, Reeves & Patrick 2021). Since the two-child limit does not appear to have changed fertility behaviour or the number of births in larger families, it appears inevitable that it will increase child poverty further among larger families. Estimates from Child Poverty Action Group suggest that the policy is pushing 50,000 children a year into poverty, a stark statistic relative to our estimate of 5,400 fewer births (Child Poverty Action Group 2022). This has significant implications for inequalities in children's outcomes and development (Cooper & Stewart 2017).

Finally, in implying an asymmetry between the effects of benefits expansions and benefits reductions, our results have wider policy implications in showing that it cannot simply be assumed that doing the opposite of a policy will lead to equivalent results in the opposite direction. The UK government understood and framed the two-child limit as a policy that would have the opposite effects of previous benefits expansions: it would reduce fertility, possibly by a substantial amount (Treasury & DWP 2015). Yet our results suggest a relatively small impact on fertility. This underscores the need for robust causal evidence specific to the policy in question during the policymaking process.

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Tables

Table 1: Summary statistics for administrative births microdata in England and Wales, 2015-2019

	N	Mean	SD
Multiple birth	3,355,172	0.030	0.171
Stillbirth	3,355,172	0.004	0.064
Previous live births	3,343,253	0.967	1.158
Maternal age (years)	3,355,055	30.562	5.646
Paternal age (years)	3,175,878	33.420	6.714
Income deprivation score	3,355,172	14.947	9.981
Local incidence of the two-child limit (percent)	3,320,020	1.240	0.583
Low-income occupation	327,256	0.490	0.500
High-income occupation	327,256	0.510	0.500
Single parent	3,355,172	0.157	0.364

Note: The table displays the total number of observations, mean and standard deviation of each variable in the administrative births microdata. Multiple birth is a dummy variable equal to one if more than one child is born as a result of a pregnancy (e.g., twins, triplets), and zero otherwise. Income deprivation score represents the percentage of the local population who receive means-tested benefits. Local incidence of the two-child limit is the percentage of the population in the child's local authority who are affected by the two-child limit. NS-SEC occupation is coded for a random 10 percent sample of the data, hence the smaller number of observations. Our measure of occupation selects the highest-ranked occupation of the mother and father. Low-income occupation is a dummy variable equal to one if the highest ranking occupation of the mother or father is NS-SEC 3-7 and zero if it is NS-SEC 1 or 2. High-income occupation is a dummy variable equal to one if the highest ranking occupation of the mother or father is NS-SEC 1 or 2 and zero if it is NS-SEC 3-7. Single parent is a dummy variable equal to one either if the parent registers the birth on their own or reports that the biological parents live at different addresses.

Table 2: Annual population estimates from Annual Population Survey sample, women aged 16-45 in England and Wales, 2015-2019

Total population of interest (women aged 16-45)	10,334,903
No benefits, 0-1 child	6,783,295
No benefits, 2+ children	2,305,344
Benefits, 0-1 child	875,564
Benefits, 2+ children	993,833
High-income occupation, 0-1 child	4,154,722
High-income occupation, 2+ children	1,522,303
Low-income occupation, 0-1 child	3,212,283
Low-income occupation, 2+ children	1,445,596

Note: The table presents weighted averages of the number of women aged 16-45 in England and Wales in 2015, 2016, 2017, 2018 and 2019. Unweighted N=50,735 a year on average. Person-household weightings are used to correct for non-response. Our measure of occupation selects the highest-ranked occupation of the mother and father. A woman is coded with a low-income occupation if the highest ranking occupation of the family is NS-SEC 3-7. A woman is coded with a high-income occupation if the highest ranking occupation of the family is NS-SEC 1 or 2. A woman is coded as receiving benefits if she reports receiving one or more of Universal Credit, tax credits, housing benefit, Jobseeker's Allowance (JSA), or income support. The number of children refers to dependent children under 16 in the family.

Table 3: Triple difference estimates by benefits receipt, family size and quarter of birth

	2+ children		0-1 children	
Quarter	Benefits	No benefits	Benefits	No benefits
2015q1	0.0602	0.0447	0.0708	0.0603
	(0.0076)	(0.0048)	(0.0065)	(0.0025)
2015q2	0.0627	0.0476	0.0745	0.0622
	(0.0079)	(0.0051)	(0.0067)	(0.0026)
2015q3	0.0661	0.0508	0.0776	0.0646
	(0.0081)	(0.0053)	(0.0069)	(0.0027)
2015q4	0.0627	0.0474	0.0731	0.0644
	(0.0078)	(0.0050)	(0.0066)	(0.0026)
2016q1	0.0588	0.0435	0.0771	0.0625
	(0.0077)	(0.0047)	(0.0075)	(0.0026)
2016q2	0.0587	0.0435	0.0796	0.0648
	(0.0077)	(0.0047)	(0.0077)	(0.0027)
2016q3	0.0597	0.0442	0.0806	0.0674
	(0.0077)	(0.0047)	(0.0078)	(0.0027)
2016q4	0.0579	0.0415	0.0758	0.0632
	(0.0076)	(0.0046)	(0.0074)	(0.0026)
2017q1	0.0584	0.0401	0.0757	0.0615
	(0.0079)	(0.0044)	(0.0078)	(0.0026)
2017q2	0.0574	0.0400	0.0793	0.0640
	(0.0078)	(0.0044)	(0.0081)	(0.0026)
2017q3	0.0606	0.0419	0.0810	0.0667
	(0.0081)	(0.0046)	(0.0083)	(0.0027)
2017q4	0.0566	0.0392	0.0767	0.0649
	(0.0077)	(0.0043)	(0.0079)	(0.0027)
2018q1	0.0511	0.0373	0.0725	0.0614
	(0.0073)	(0.0042)	(0.0079)	(0.0026)
2018q2	0.0531	0.0391	0.0730	0.0637
	(0.0075)	(0.0043)	(0.0080)	(0.0026)
2018q3	0.0545	0.0400	0.0745	0.0661
	(0.0077)	(0.0044)	(0.0081)	(0.0027)
2018q4	0.0518	0.0381	0.0729	0.0643
	(0.0074)	(0.0042)	(0.0080)	(0.0026)
2019q1	0.0511	0.0359	0.0686	0.0606
	(0.0076)	(0.0040)	(0.0079)	(0.0026)
2019q2	0.0522	0.0365	0.0708	0.0629
	(0.0077)	(0.0041)	(0.0081)	(0.0026)
2019q3	0.0529	0.0381	0.0737	0.0660
	(0.0078)	(0.0041)	(0.0084)	(0.0027)
2019q4	0.0509	0.0355	0.0684	0.0631
	(0.0076)	(0.0040)	(0.0080)	(0.0026)
Pre	0.0606	0.0448	0.0761	0.0634
	(0.0009)	(0.0007)	(0.0009)	(0.0004)
Post	0.0538	0.0383	0.0738	0.0640
	(0.0009)	(0.0006)	(0.0010)	(0.0004)
First difference	-0.0067	-0.0065	-0.0023	0.0005
	(0.0017)	(0.0014)	(0.0019)	(0.0008)
Second difference	-0.0003		-0.0029	
	(0.0031)		(0.0027)	
Third difference	0.0026			
	(0.0058)			

Note: The table shows the annualised probability of having a child by family size and benefits receipt, by quarter. The two-child limit was introduced in April 2017 (i.e., 2017 Q2). Data on the number of lower- and higher-order births by NS-SEC category and year are from birth records; data on benefits probabilities and the number of women aged 16-45 by NS-SEC category and family size are from the Annual Population Survey. A woman is coded as receiving benefits if she reports receiving one or more of Universal Credit, tax credits, housing benefit, Jobseeker's Allowance (JSA), or income support. The number of children refers to dependent children under 16 in the family. The first difference subtracts the post from the pre averages. The second difference subtracts the first differences of those on benefits from those not on benefits. The third difference subtracts the second differences of higher-order births to those with 2+ children from lower-order births to those with 0-1 child. Standard errors are shown in brackets and are calculated manually from the births and APS data, as detailed in footnote 11.

Table 4: Triple difference estimates by family occupation, family size and quarter of birth

	2+ children		0-1 children	
Quarter	Low-income	High-income	Low-income	High-income
2015q1	0.0748	0.0363	0.0741	0.0631
	(0.0076)	(0.0027)	(0.0044)	(0.0020)
2015q2	0.0769	0.0400	0.0792	0.0637
	(0.0077)	(0.0029)	(0.0046)	(0.0020)
2015q3	0.0821	0.0420	0.0823	0.0662
	(0.0080)	(0.0030)	(0.0047)	(0.0021)
2015q4	0.0767	0.0398	0.0779	0.0676
	(0.0077)	(0.0029)	(0.0045)	(0.0021)
2016q1	0.0723	0.0353	0.0830	0.0601
	(0.0075)	(0.0026)	(0.0050)	(0.0019)
2016q2	0.0734	0.0343	0.0845	0.0633
	(0.0076)	(0.0026)	(0.0050)	(0.0020)
2016q3	0.0748	0.0347	0.0873	0.0657
	(0.0076)	(0.0026)	(0.0051)	(0.0020)
2016q4	0.0722	0.0323	0.0820	0.0615
	(0.0075)	(0.0025)	(0.0049)	(0.0019)
2017q1	0.0734	0.0304	0.0808	0.0590
	(0.0077)	(0.0023)	(0.0049)	(0.0019)
2017q2	0.0724	0.0304	0.0828	0.0623
	(0.0076)	(0.0023)	(0.0050)	(0.0019)
2017q3	0.0763	0.0319	0.0869	0.0642
	(0.0079)	(0.0024)	(0.0051)	(0.0020)
2017q4	0.0717	0.0295	0.0815	0.0638
	(0.0076)	(0.0023)	(0.0049)	(0.0020)
2018q1	0.0634	0.0286	0.0805	0.0615
	(0.0069)	(0.0023)	(0.0050)	(0.0020)
2018q2	0.0659	0.0300	0.0831	0.0636
	(0.0070)	(0.0023)	(0.0051)	(0.0020)
2018q3	0.0684	0.0301	0.0848	0.0666
	(0.0072)	(0.0024)	(0.0052)	(0.0021)
2018q4	0.0643	0.0293	0.0809	0.0658
	(0.0070)	(0.0023)	(0.0050)	(0.0021)
2019q1	0.0624	0.0275	0.0789	0.0602
	(0.0069)	(0.0022)	(0.0050)	(0.0020)
2019q2	0.0634	0.0282	0.0817	0.0624
	(0.0070)	(0.0022)	(0.0051)	(0.0020)
2019q3	0.0655	0.0290	0.0854	0.0656
	(0.0071)	(0.0022)	(0.0053)	(0.0021)
2019q4	0.0624	0.0269	0.0809	0.0629
	(0.0069)	(0.0022)	(0.0051)	(0.0020)
Pre	0.0752	0.0361	0.0812	0.0633
	(0.0008)	(0.0008)	(0.0006)	(0.0005)
Post	0.0669	0.0292	0.0825	0.0635
	(0.0008)	(0.0007)	(0.0006)	(0.0005)
First difference	-0.0082	-0.0069	0.0013	0.0002
	(0.0016)	(0.0015)	(0.0012)	(0.0010)
Second difference	-0.0013		0.0011	
	(0.0031)		(0.0022)	
Third difference	-0.0024			
	(0.0053)			

Note: The table shows the annualised probability of having a child by family size and occupation status, by quarter. The two-child limit was introduced in April 2017 (i.e., 2017 Q2). Data on the number of lower- and higher-order births by NS-SEC category and year are from birth records; data on the number of women aged 16-45 by NS-SEC category and family size are from the Annual Population Survey. A woman is coded with a low-income occupation if the highest ranking occupation of the family is NS-SEC 3-7. A woman is coded with a high-income occupation if the highest ranking occupation of the family is NS-SEC 1 or 2. The number of children refers to dependent children under 16 in the family. The first difference subtracts the post from the pre averages. The second difference subtracts the first differences of those on in low-income occupations from those in high-income occupations. The third difference subtracts the second differences of higher-order births to those with 2+ children from lower-order births to those with 0-1 child. Standard errors are shown in brackets and are calculated manually from the births and APS data, as detailed in footnote 11.

Figures

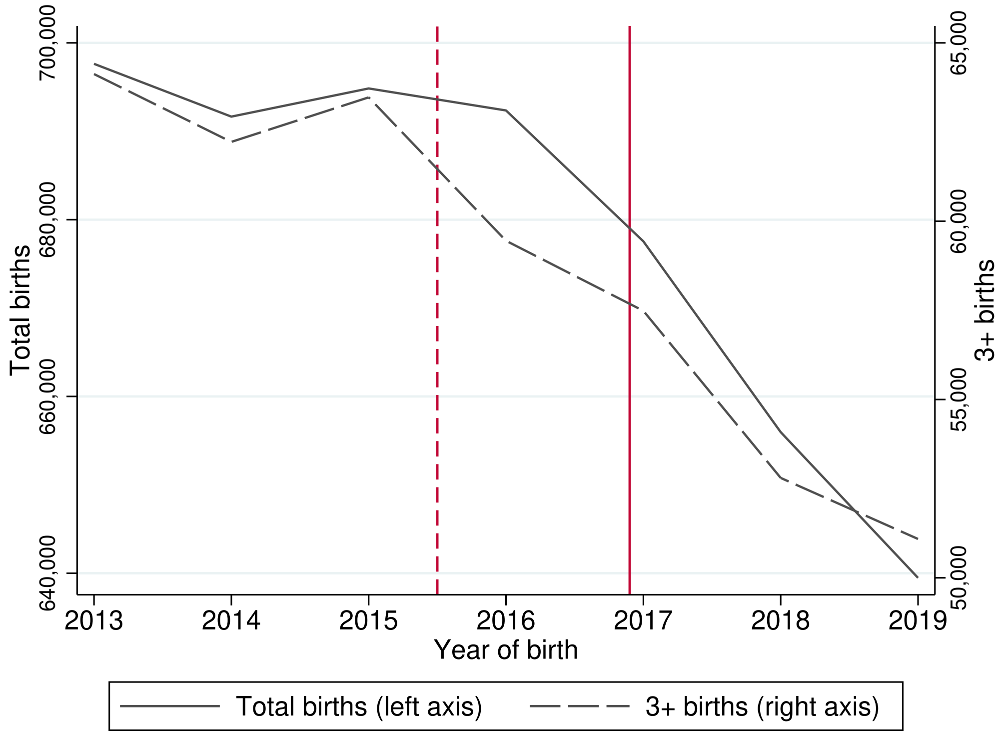

Figure 1: Number of total and 3+ births in England and Wales by year, 2013-2019

Note: The figure plots the total number of births in England and Wales on the primary axis (solid line) and the total number of third or subsequent births (dashed line) on the secondary axis by year. Data are presented annually, since the number of births is volatile by month and quarter of birth. The red solid line indicates the introduction of the two-child limit in April 2017 (here it is displayed just before 2017, as the data is annual and the policy affected the majority of 2017). The red dashed line indicates the announcement of the two-child limit in July 2015 (for the same reasons, here it is displayed just before 2016). Data from administrative births microdata for England and Wales.

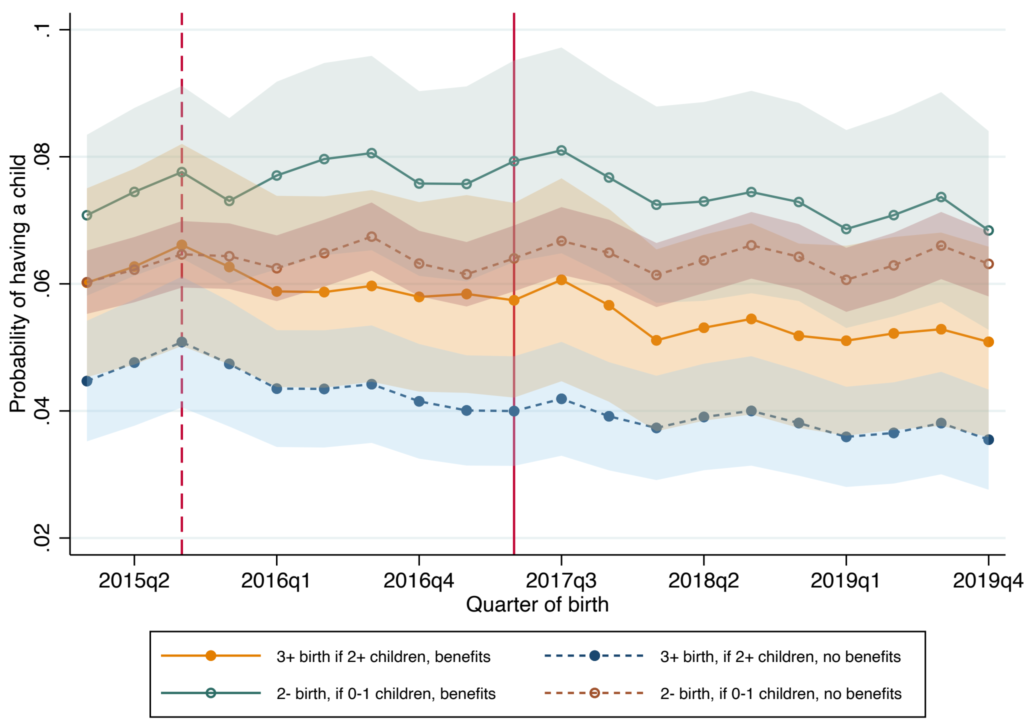

Figure 2: Probability of having a child by benefits receipt, family size and quarter of birth, 2015-2019

Note: The figure charts the annualised probability of a woman aged 16-45 having a child by quarter of birth, by family size and benefits receipt. A woman is coded as receiving benefits if she reports receiving one or more of Universal Credit, tax credits, housing benefit, Jobseeker's Allowance (JSA), or income support. The number of children refers to dependent children under 16 in the family. The sample is adult female respondents aged 16-45 (weighted N=10.3 million a year on average; unweighted N=50,735 a year on average). The shaded areas represent 95 percent confidence intervals. Data from Annual Population Survey and administrative births microdata.

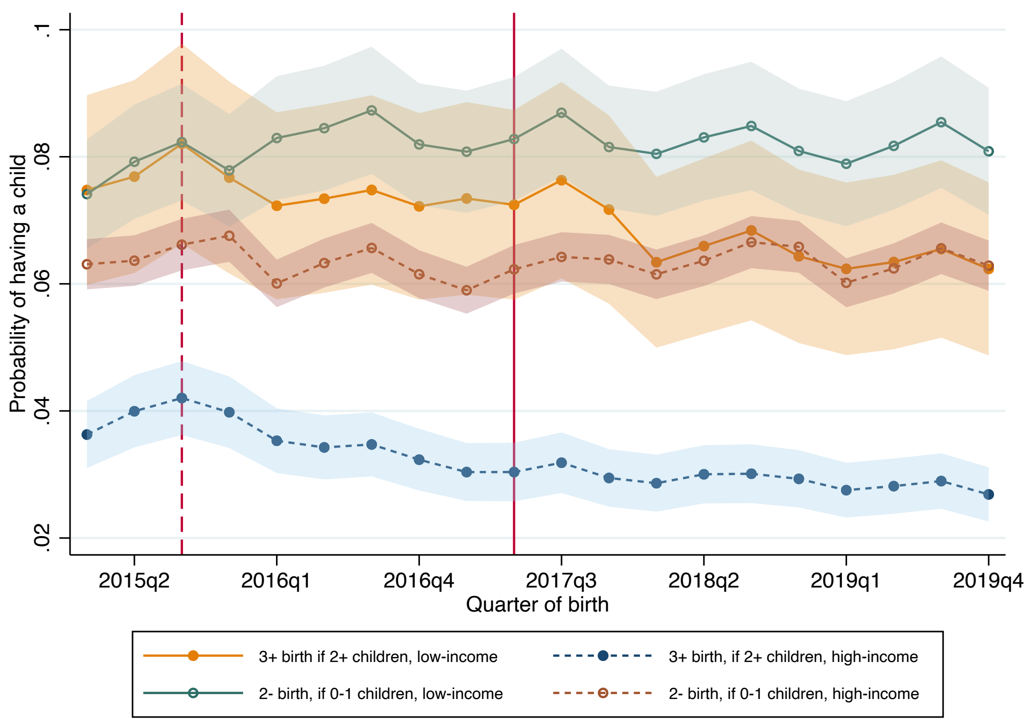

Figure 3: Probability of having a child by NS-SEC occupation, family size and quarter of birth, 2015-2019

Note: The figure charts the annualised probability of a woman aged 16-45 having a child by quarter of birth, by family size and family occupation status. A woman is coded with a low-income occupation if the highest ranking occupation of the family is NS-SEC 3-7. A woman is coded with a high-income occupation if the highest ranking occupation of the family is NS-SEC 1 or 2. The sample is adult female respondents aged 16-45 (weighted N=10.3 million a year on average; unweighted N=50,735 a year on average). The shaded areas represent 95 percent confidence intervals. Data from Annual Population Survey and administrative births microdata.

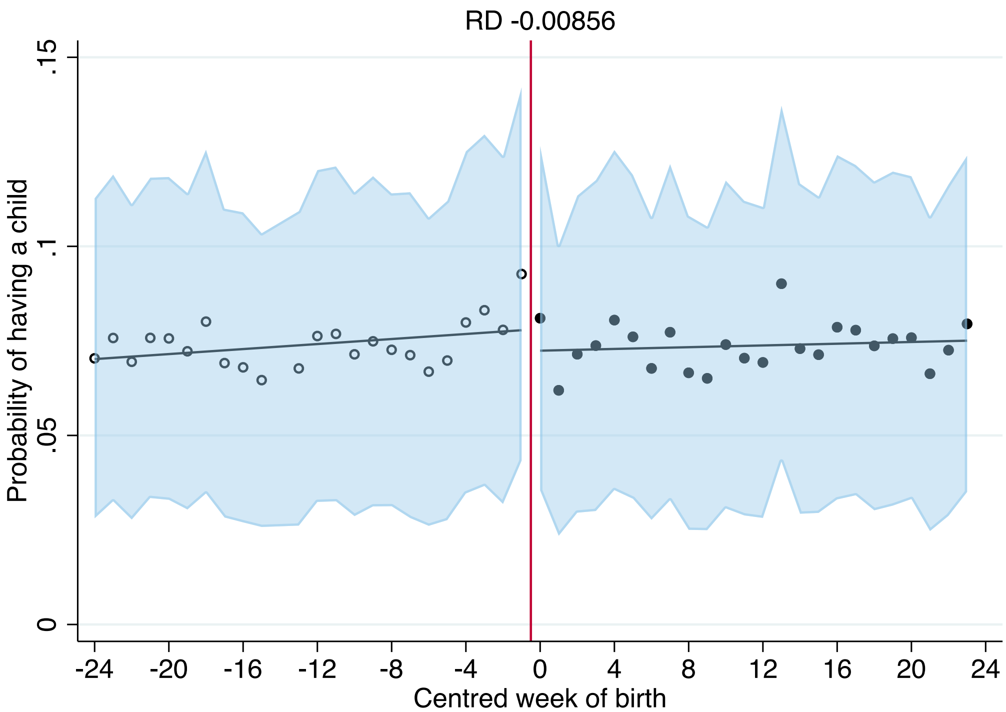

Figure 4: Regression discontinuity for women with 2+ children in low-income occupations

Note: The figure charts the annualised probability of having a child by week of birth (relative to the 6 April 2017 cut-off) among our preferred definition of the treatment group (women with 2+ children in low-income occupations) for a small 24-week bandwidth around the cut-off. A local linear polynomial is fitted to the data either side of the cut-off. A woman is coded with a low-income occupation if the highest ranking occupation of the family is NS-SEC 3-7. The shaded areas represent 95 percent confidence intervals. The regression discontinuity estimate from a non-parametric local linear regression for a 24-week bandwidth is shown. Data from Annual Population Survey and administrative births microdata.