The birthweight effects of a universal child benefit in pregnancy: evidence from England and Wales

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Abstract

This paper takes advantage of a unique policy experiment in the UK to estimate the impact of paying a universal child benefit during pregnancy. Using administrative birth registrations microdata from England and Wales, I evaluate the impact of the Health in Pregnancy Grant, a universal and short-lived policy that paid the equivalent of child benefit in a lump sum to all pregnant mothers from 2009 to 2011. I exploit an arbitrary eligibility rule to implement a regression discontinuity design in the date of birth of the child. I find that the policy led to increases in mean birthweight at population level. Effects were largest for the smaller babies, youngest mothers and those living in deprived areas. My findings suggest that there are significant health returns to paying universal child benefits during pregnancy and that the potential for birthweight gains from windfall increases in income is larger than previously thought.

Keywords: birthweight, child health, cash transfer, child benefit
JEL classification: I14, I38

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[1]This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors. The author is grateful for helpful comments from Professor Daniel Sturm, Dr Kitty Stewart, Dr Berkay Özcan, Dr Giulia Giupponi, Dr Tania Burchardt and Dr Tammy Campbell. This work was produced using statistical data from the Office of National Statistics (ONS). 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.

1. Introduction

Child health is internationally recognised by parents, health systems and welfare states as a form of human capital, to which an increasing proportion of private and public expenditure is dedicated (Kelly et al., 2020; Burchardt and Obolenskaya, 2016; World Health Organization, 2019). Universal child benefits from birth have emerged as a popular mechanism for subsidising the costs of children and supporting child health and wellbeing (UNICEF, 2020). In expanding the budget constraint of the family and mothers in particular, child benefit income has been shown to benefit children's health by subsidising parental investment in children's nutrition and wellbeing, by reducing family stress, and even by reducing unhealthy behaviours such as poor diet, smoking and alcohol use (Benzeval, Bond, Campbell, Egan, Lorenc, Petticrew and Popham, 2014; Gregg, Waldfogel and Washbrook, 2006; Milligan and Stabile, 2011)

However, it is also clear that inequalities in child health start before birth: nutrition, stress and unhealthy behaviours during pregnancy are important determinants of health at birth and beyond (Averett and Wang, 2013; Cowan and Tefft, 2012). Despite this, few countries have extended universal child benefits to pregnancy, and even fewer have framed this as a child health issue. An exception to this was the UK Labour government who, in 2009, introduced a universal Health in Pregnancy Grant, a one-off lump-sum of the equivalent value of child benefit (190 GBP) in the third trimester of pregnancy. The UK government claimed that this would address "the serious problem of underweight babies in this country." Did it?

I answer the question of whether a universal child benefit during pregnancy improves birthweight by taking advantage of this unique policy experiment from the UK. Using administrative birth registrations data from England and Wales, I estimate the impact of a universal windfall cash transfer during pregnancy by conducting a regression discontinuity design in the date of birth of the child. By exploiting an exogenous and discontinuous increase in the probability of receiving the grant at a cut-off based on date of birth, I demonstrate that the grant led to a small but significant increase in mean birthweight. Effects were larger for smaller babies, younger mothers and those living in deprived areas.

My main contribution is to expand economic knowledge about the potential gains of bringing universal child benefits into the prenatal phase. There is some evidence that child benefits paid after birth can lead to improvements in children's physical health (Milligan and Stabile, 2011). However, very little is known about the impact of these kinds of child benefits on health outcomes at birth, and it is plausible that health effects will be stronger at birth than during childhood. The UK's Health in Pregnancy Grant represents a unique international policy experiment, bringing the rationale behind universal child benefit into the prenatal period with the aim of boosting health at birth. In finding that this policy led to significant increases in average birthweight, I demonstrate that there are significant health returns to universal child benefits during pregnancy.

My second contribution is to leverage this unique policy experiment to demonstrate that the potential for birthweight gains from windfall increases in income is larger than previously thought and, in doing so, to make a significant contribution to the literature on the causal effects of income and birthweight. My estimates of the impact of this unique case study of a 190 GBP increase in income are comparable with, and indeed in some cases larger than, other quasi-experimental estimates of the impact of a 1000 USD income increase in the US (Hoynes, Miller and Simon, 2015; Mocan, Raschke and Unel, 2015; Chung, Ha and Kim, 2016). In other words, paying a universal child benefit during pregnancy produces birthweight effects that are comparable with those of general windfall increases in income of approximately four times the financial value. This suggest that universal cash transfers during pregnancy may be a particularly efficient method of improving and narrowing inequalities in child health.

I use rich administrative birth registrations microdata to implement a regression discontinuity design in babies' date of birth to obtain causal estimates of the impact of the Health in Pregnancy Grant on birthweight in England and Wales. My identification strategy exploits the existence of an arbitrary eligibility rule for the Health in Pregnancy Grant, whereby mothers of all babies with an expected date of delivery on or after 6 April 2009 were eligible. This enables quasi-experimental conditions for the estimation of an intention to treat effect. I conduct separate regression discontinuity analyses for subgroups to test the causal mechanisms at play.

I find that the Health in Pregnancy Grant led to a small but significant increase in mean birthweight at population level. These gains were concentrated on the smallest babies, younger mothers (aged 25 and under), and those living in areas with high levels of deprivation. My results are robust to parametric and non-parametric approaches, different bandwidths and polynomial specifications. They are strongly supported by a series of sensitivity tests. In particular, I demonstrate that there was no robust treatment effect at placebo cut-offs on 6 April in non-treatment years of 2007, 2008, 2010 and 2011. This rules out the possibility that the beginning of the financial year at the treatment cut-off induced a mechanical increase in birthweight, and supports my finding that the increase in birthweight is causally linked to the introduction of the grant.

The structure of this paper is as follows. Section 2 outlines the context of the Health in Pregnancy Grant and reviews the literature on the impact of increases in income on birthweight. Section 3 describes the data and details my identification strategy and methodology. Section 4 presents the results of my analysis in three parts: the estimated treatment effect on birthweight; a heterogeneity analysis by birthweight, maternal age and English index of income deprivation; and a series of robustness checks to validate my identification strategy. Section 5 discusses these results and puts forward limitations of the analysis. Finally, Section 6 concludes and reflects on key policy implications.

2. Background

2.1. The Health in Pregnancy Grant, 2009-2011

In April 2009, the UK Labour government introduced the Health in Pregnancy Grant (HPG), a cash transfer of the equivalent of child benefit over the third trimester (190 GBP) as a lump sum to all pregnant women in the United Kingdom. Mothers of all babies due on or after 6 April 2009 were eligible for the HPG (Wright, 2009). In order to claim the grant, pregnant women were required to visit their midwife or doctor for an antenatal check-up from the twenty-fifth week of pregnancy, when they would be invited to fill a simple application form for the HPG. After sending the form to the UK tax authorities in a free-post envelope, recipients received a letter confirming their application. They could expect to receive their money within seven days, through a direct payment into their bank or building society account (Directgov, 2010). In a government podcast to promote the HPG, it was stated that women could spend the money "on whatever you like," including fresh fruit and vegetables, cots, nappies, buggies, and any other lump sums associated with having a healthy baby (Directgov, 2010). In being universal and unconditional, the HPG built on the principle of universal child benefit and gave mothers the choice of how to use the money to help with the costs of children.

The distinctiveness of the HPG, however, was that it extended the principles of child benefit and income support into pregnancy with the ambition of improving birth outcomes. The Labour government envisioned two main mechanisms through which the HPG would improve birthweight: improved nutrition and reduced stress. In a parliamentary standing committee session in 2008, a health minister argued that the HPG would "address the serious problem of underweight babies in this country" (Public Bills Committee, 2008, c.103).

However, the HPG was controversial from the start. Opposition MPs portrayed the grant as a "gimmick" (Public Bills Committee, 2008, c.460). Childbirth charities raised concerns that the grant was poorly designed for nutritional impact, as the third trimester was too late in pregnancy for significant nutritional gains (Public Bills Committee, 2008, c.86). Political opponents also claimed that the grant would be squandered on "booze, fags, bingo or plasma screen televisions" and that its universalism was an inefficient use of resources (Public Bills Committee, 2008, cc.90-91; 103).

The HPG fell under renewed scrutiny with the election of the Conservative-Liberal Democrat Coalition in 2010 and pressures for fiscal consolidation after the financial crisis. Less than two years after being rolled out across the UK, in January 2011 the Coalition government abolished the HPG.

2.2. Background literature and mechanisms

The hypothesis that the HPG improved birthweight is contested by the only existing study of the grant (Leyland et al., 2017) which concludes that the HPG had no impact on birthweight, prematurity or maternal health in Scotland. The study uses an interrupted time series analysis, which makes it difficult to control for contemporary events that coincided with the treatment period (April 2009 to April 2011). In particular, the paper does not account for the exogenous negative shock of the economic recession on birth outcomes during that period: the economic downturn from 2008-09 may have introduced downward bias in Leyland et al. (2017)'s estimation of a treatment effect. In contrast, an RD methodology – as implemented in this paper – enables quasi-experimental conditions to be created such that potential outcomes are 'as if' randomised at the treatment cut-off (6 April 2009), and such exogenous shocks are controlled for.

This paper focuses exclusively on birthweight, as it was the primary (and contested) policy objective of the HPG, and it is the main indicator of infant health available in the birth registrations data. While the relative importance of birthweight to health has declined in recent years (Maruyama and Heinesen, 2020; Goisis, Özcan and Myrskylä, 2017), and its impact on health dissipates with age (Maruyama and Heinesen, 2020), it is widely acknowledged to be one of the most important health outcomes in the life course and has a significant effect on child health. Low birthweight is associated with higher levels of infant mortality, particularly in the first 28 weeks after birth (Maruyama and Heinesen, 2020; Brooks-Gunn and Duncan, 1997). In adulthood, low birthweight can lead to "permanent health capital loss" (Maruyama and Heinesen, 2020, p.18) including shorter adult height, higher blood pressure, Type-2 diabetes and lower offspring birthweight (Silverwood et al., 2013; Victora et al., 2008). Low birthweight is also associated with lower educational attainment and achievement, grade repetition, and lower adult income (Maruyama and Heinesen, 2020; Currie and Rossin-Slater, 2015; Currie and Moretti, 2003; Behrman and Rosenzweig, 2004; Black, Devereux and Salvanes, 2007). Finally, it has been shown that low birthweight is intergenerationally transmitted (Currie and Moretti, 2003) and therefore plays a role in the "the reproduction of inequality over generations" (Strully, Rehkopf and Xuan, 2010, p.535).

There are three main mechanisms through which the Health in Pregnancy Grant may have impacted on birthweight: income, antenatal health engagement, and maternity leave. First, the HPG is likely to have boosted birthweight via the windfall increase in income of 190 GBP. There is robust and wide-ranging evidence that increases in income during pregnancy contribute towards improved health at birth (Cooper and Stewart, 2017; Chersich et al., 2016). This includes permanent, marginal and windfall (one-off) increases in income, such as the Earned Income Tax Credit (EITC) in the US (Cooper and Stewart, 2017; Hoynes, Miller and Simon, 2015; Almond, Hoynes and Schanzenbach, 2011; Strully, Rehkopf and Xuan, 2010). Among child health outcomes, birthweight and other neonatal outcomes appear the most sensitive to increases in income (Cooper and Stewart, 2017). Increases in income can be spent directly on goods and services that improve health and nutrition during pregnancy, such as fresh fruit and vegetables or wellbeing activities (Gregg, Waldfogel and Washbrook, 2006). There is anecdotal evidence from Mumsnet that some women spent the lump sum from the Health in Pregnancy Grant on 10-week courses of aquanatal swimming classes and antenatal yoga (Mumsnet, 2009). A recent randomised control trial found that the latter significantly reduces stress and depression (Newham, Wittkowski, Hurley, Aplin and Westwood, 2014). Increases in income can also improve birthweight by reducing stress and financial strain, even if they are not spent directly on health investments. Such reductions in stress can contribute towards reductions in unhealthy behaviours among pregnant women including maternal smoking, alcohol and drug consumption, which are known to affect birthweight (Averett and Wang, 2013; Cowan and Tefft, 2012; Gregg, Waldfogel and Washbrook, 2006; Strully, Rehkopf and Xuan, 2010).

Second, the HPG may have increased birthweight by incentivising earlier engagement with antenatal health services. One of the main reasons offered by the UK government for the grant being paid in the third trimester was so that it would be linked to seeking antenatal health advice at 25 weeks. Existing research on the HPG in Scotland suggests that it increased the odds of booking before 25 weeks by 10 percent (Leyland et al., 2017). If the HPG incentivised mothers to seek antenatal health advice at an earlier stage, it may have improved birthweight outcomes by increasing mothers' access to information about staying healthy during pregnancy, including on how to maintain a nutritious diet and on the risks of maternal smoking. A recent systematic review found that antenatal care is an important contributor to improved birth outcomes for disadvantaged and vulnerable mothers in high-income countries, though the evidence on specific interventions is less strong than the literature on income and birthweight (Hollowell, Oakley, Kurinczuk, Brocklehurst and Gray, 2011). Mothers who are teenage, non-White or living in deprived areas are among those who are more likely to engage with antenatal services at a later stage in the UK (Kapaya, Mercer, Boffey, Jones, Mitchell and Anumba, 2015). These groups may have been particularly incentivised by the HPG to attend antenatal check-ups at an earlier stage in their pregnancy.

Third, the HPG may have compensated women with poor access to paid maternity leave to take unpaid leave prior to the birth of their baby, thereby enabling them to get more rest during the period when babies gain weight very rapidly. Taking time off through unpaid maternity leave has been associated with increases in birthweight, but less so for poor, single and low-educated women due to the associated reduction in income (Rossin, 2011). The HPG may have enabled poorer women to circumvent such financial repercussions, to the benefit of their wellbeing and their babies' birthweights.

3. Data and methodology

3.1. Data and descriptives

I use birth registrations microdata for England and Wales from 2006 to 2012. As an administrative source of data, it benefits from a large sample size, with 1,402,979 observations (live births) between 6 April 2008 and 5 April 2010 (a two-year period around the treatment cut-off). Data were cleaned to drop duplicates, stillbirths (n=25,590) and implausible birthweight outliers (n=47,516).[2] The data enjoy a complete set of non-missing values for date of birth, birthweight and sex. Social class status is coded for a random sample of 10 percent of the population. The data include the postcode of mother's residence for all but 1,184 (0.08 percent) of the final sample. This was used to match in the English index of income deprivation at Lower Super Output Area (LSOA) level.[3]

Key covariates in the final sample are sex of the baby, maternal age, multiple birth status, English index of income deprivation score and combined occupational social class (for 10 percent of observations). Males tend to have slightly higher birthweights than females (Nascimento, Blanco Machin and Antonio Almeida Dos Santos, 2017). While the evidence is mixed, maternal age has been found to have a (non-linear) relationship with birthweight (Goisis, Schneider and Myrskylä, 2018). Younger mothers tend to have smaller babies on average and a higher incidence of low birthweight (Ghosh, Berild, Sterrantino, Toledano and Hansell, 2017). Multiple births are more prone to pre-term delivery and low birthweight (Blondel, Kogan, Alexander, Dattani, Kramer, Macfarlane and Wen, 2002), and are particularly relevant as the HPG was administered per pregnancy, not per birth (WiredGov, 2009). Consequently, mothers experiencing multiple births did not receive as much money per child, and a higher multiple birth rate is likely to attenuate the impact of the HPG. Birthweight is also patterned by social class: at the introduction of the HPG in 2009, babies born to parents from routine, manual

[2] After inspecting the birthweight distribution, outlier observations with a recorded birthweight less than 310 grams or larger than 5680 grams were dropped. This included missing values coded as '9999' or '9998'.

[3] Since the indices of multiple deprivation are not comparable across England and Wales (with the exception of a combined dataset for 2015-16), all analyses using deprivation data were for England only. Index of income deprivation scores from the nearest data point available to the introduction of the HPG, 2010, were matched into the births data at LSOA level for all observations with a postcode (99.92 percent of the sample).

and other occupations were 13 percent more likely to be low birthweight than babies born to parents in professional, managerial, intermediate or small employer occupations (Stewart and Reader, 2021). The index of income deprivation – which measures the proportion of the population in a given area who experience deprivation relating to low income, whether due to unemployment or low pay (Smith, Noble, Noble, Wright, McLennan and Plunkett, 2015) – is also associated with low birthweight (Dibben, Sigala and Macfarlane, 2006). These covariates are not used as controls, since the RD design renders that redundant and potential outcomes are quasi-randomised at the discontinuity (Lee and Lemieux, 2010). However, in Section 4.3 I conduct a covariate balance test as a robustness check.

Table 1 shows summary statistics for the final sample of births from 6 April 2008 to 5 April 2010. Treatment and control groups were balanced in terms of sex composition, teenage and advanced maternal age pregnancies, multiple births and lone parents, and broadly balanced for maternal age (with the mean for the treatment group being 0.04 years higher than the control group). However, mean birthweight in the treatment group was 5 grams larger than in the control group, while low birthweight was 0.2 percentage points lower and extremely low birthweight was 0.1 percentage points lower.

3.2. Identification strategy

I estimate the causal impact of the Health in Pregnancy Grant by exploiting variation in babies' dates of birth to conduct a regression discontinuity design. The policy design of the HPG renders it a promising candidate for a regression discontinuity, since it was simultaneously universal and administered on an arbitrary basis by expected date of delivery (EDD): all mothers of babies with an EDD on or after 6 April 2009 were eligible for the grant.[4] Since EDDs, proxied by date of birth, are difficult if not impossible to control, the HPG is a particularly compelling case for an RD design. This is because the running variable is plausibly random.

As claiming the HPG was dependent on attending an antenatal check-up from 25 weeks, it exhibited imperfect compliance. Although no official take-

[4] While arbitrary cut-offs were used both in the introduction and the abolition of the HPG (Howard, 2011, January 10), I focus exclusively on the introduction cut-off. The abolition of the HPG in January 2011 coincided with the restriction of the Sure Start Maternity Grant to the first child, which would make it more difficult to isolate the effect of the HPG's abolition.

	Main sample		Treatment group		Control group	
Variable	Mean	SD	Mean	SD	Mean	SD
Birthweight (grams)	3,336.46	598.52	3,339.14	597.53	3,333.75	599.51
Prop. low birthweight	0.071	0.257	0.070	0.255	0.072	0.258
Prop. extremely low birthweight	0.011	0.106	0.011	0.105	0.012	0.107
Maternal age (years)	29.45	6.07	29.43	6.08	29.47	6.05
Prop. teenage pregnancy	0.062	0.241	0.060	0.238	0.063	0.244
Prop. advanced maternal age	0.200	0.400	0.200	0.400	0.200	0.400
Prop. multiple births	0.031	0.173	0.031	0.174	0.031	0.172
Prop. male	0.512	0.500	0.512	0.500	0.512	0.500
Prop. lone parents	0.158	0.365	0.158	0.365	0.157	0.364
Index of income deprivation score	0.175	0.122	0.175	0.122	0.175	0.123
N	1,402,979		697,595		705,384	

Table 1: Summary statistics for the final sample, 2008-2010.
Notes: A birth is defined as low birthweight if its recorded birthweight is less than 2500g, and extremely low birthweight if less than 1500g. Teenage pregnancies have a recorded maternal age of 19 or under; advanced maternal age are 35 and over. Multiple births are those where more than one child is born as a result of a pregnancy (e.g., twins, triplets). Lone parents are coded if the mother's marital status indicates they are outside a marriage or civil partnership and they either register the birth as a sole registration or as a joint registration where the parents live at separate addresses. English index of income deprivation score is based on the area of mother's residence.

up data was record, estimates from tax authority expenditure data suggest high take-up rates of 92.9 percent.[5] This high but imperfect compliance introduces ‘fuzziness’ in the discontinuity, as the probability of being treated increases sharply from 6 April 2009 but falls short of reaching unity. Somewhat unusually, in my setup the fuzziness of the RD is compounded by another source. Since the birth registrations data do not include a due date variable, I use date of birth as a proxy for due date. This increases the fuzziness of the RD and, given the large sample size, can be expected to increase random measurement error. Some mothers whose babies were born before 6 April will have received the HPG as their due dates were on or after the cut-off, while others whose babies were born after 6 April will not have received it as their due dates were before the cut-off. This measurement error or ‘noise’ is likely to reduce statistical significance and lead to underestimation of the real treatment effect.

Since official take-up rates were not measured, it is not possible to estimate a treatment on the treated (TOT) or local average treatment effect (LATE) through a fuzzy RD approach. Instead, I estimate an intention to treat (ITT) effect utilising the traditional sharp RD approach. As such, I adopt a reduced form approach by looking at the relationship between an outcome (birthweight) and an instrumental variable (week of birth) which serves as a proxy for an explanatory variable (receipt of the HPG). I use week of birth, centered around the cut-off, instead of date of birth in order to maximize sample size.[6]

My basic empirical specification is the following:

$$Y_i = \beta_0 + \beta_1 HPG_i + \beta_2 WOB_i + \epsilon_i \tag{1}$$

[5]For the 2009–2010 financial year, reported expenditure on HPG grants – excluding administrative costs – totalled 137.8 million GBP in the UK as a whole (Her Majesty’s Revenues and Customs, 2010, p. 55). Given that the grant was a flat-rate lump sum of 190 GBP for all women, it can therefore be estimated that approximately 725,260 women received the grant in 2009-2010. This implies an extremely high take-up rate of the 781,000 maternities that were recorded in the UK in 2009 (Office of National Statistics, 2021; National Records of Scotland, 2021; Northern Ireland Statistics and Research Agency, 2020), of approximately 92.9 percent.

[6]In the parametric models, in line with the empirical specification shown the running variable (date/week of birth) is included in the regression as a control, so using date of birth leads to a smaller number of observations from which to draw (see Table 13, supplementary material).

, where Y_i is the outcome of choice (birthweight in grams, or a dummy variable equal to one if a baby weighs less than 2500g and zero otherwise), β_1 is the ITT effect of the HPG, HPG_i is a dummy variable equal to one if an individual was eligible and zero otherwise, WOB_i is week of birth, and i indexes individual births.

In estimating the regression discontinuity, I use both parametric and non-parametric approaches. In parametric models, I select the optimal bandwidth by minimising the Akaike Information Criterion (AIC) and Bayesian Information Criterion (BIC). In non-parametric models, I select my bandwidth by a cross-validation procedure that minimises the mean squared error (MSE) of the estimator (Cattaneo, Idrobo and Titiunik, 2019; Imbens and Lemieux, 2008; Lee and Lemieux, 2010). In all cases, I use standard errors that are clustered by week of birth. This helps to avoid the specification error caused by a discrete running variable within the non-parametric approach (Lee and Card, 2008).

OLS inference methods are invalid in an RD context because they do not account for the possibility of misspecification error in the local polynomial fit (Cattaneo, Idrobo and Titiunik, 2019). I therefore use robust bias-corrected confidence intervals. This produces smaller confidence intervals than conventional inference (based on parametric least-squares assumptions) or standard bias correction, and is the only RD inference procedure that replicates experimental estimates (Hyytinen, Meriläinen, Saarimaa, Toivanen and Tukiainen, 2018). By contrast, RD estimates using conventional confidence intervals or standard bias correction can lead to overestimation of effects (Hyytinen et al., 2018). However, since many RD papers use these approaches, I also report results using these alternative confidence intervals in Table A2 in the supplementary material, and they do indeed suggest even higher and statistically significant effects than in my preferred models.

4. Results

This section outlines the main results of my analysis. First, I estimate an RD treatment effect for the HPG using both parametric and non-parametric approaches. Second, I investigate whether the gains of the grant were larger for certain groups than others, namely by birthweight, maternal age and English index of income deprivation. Third, I conduct three robustness checks to validate the use of the RD methodology: placebo cut-off dates, a covariate balance test, and a McCrary manipulation test.

4.1. Main findings

I find a small positive effect on birthweight that is robust and statistically significant across almost all models, across parametric and non-parametric approaches, different bandwidths and polynomial specifications. Results from preferred regression models, selected based on goodness of fit, are shown in Table 2, with full results listed in Table 1 and Table 2 in the supplementary material.

Figure 1 illustrates a compelling discontinuous increase in birthweight based on my preferred specification, which indicates a statistically significant effect size of 11g (1.8 percent of a standard deviation).

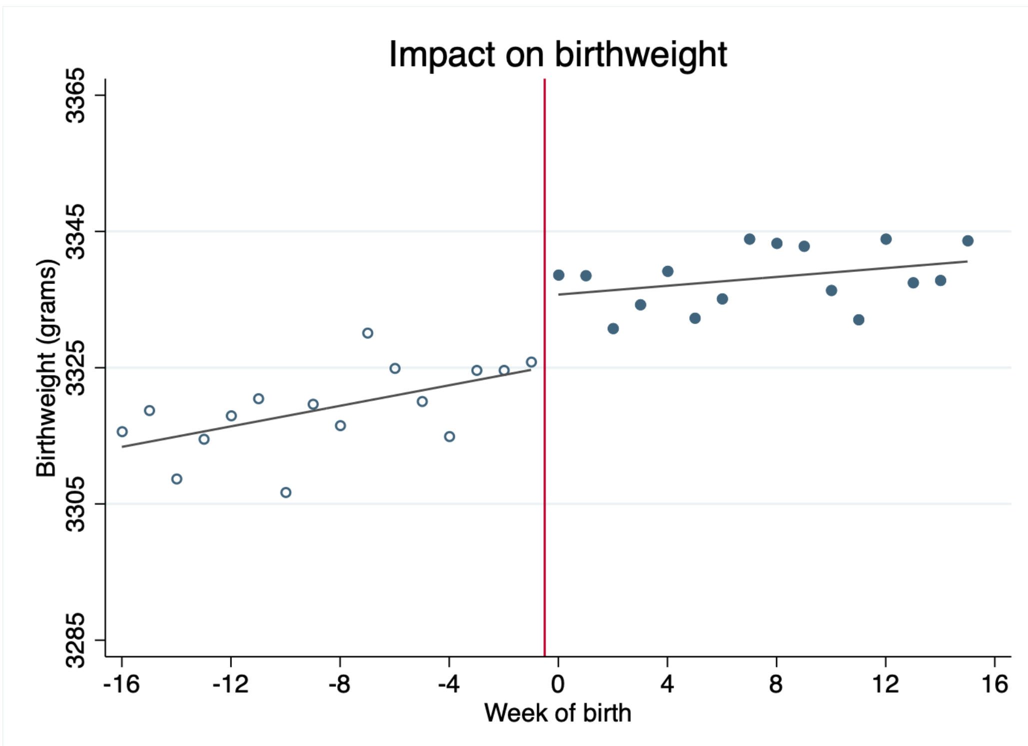

Figure 1: Impact of the Health in Pregnancy Grant on birthweight.
Notes: 1. Scatterplot shows collapsed mean birthweight data by week of birth with a fitted linear spline around the cut-off. 2. A bandwidth of 16 weeks (preferred parametric bandwidth) is used for graphical purposes.

At population level, these effect sizes were not of a sufficient magnitude to significantly reduce the probability of being low birthweight (less than 2500g).

	(1)	(2)	(3)	(4)	Control mean
	10.72***	10.39***	10.17***	10.38***	3323.67
	(3.291)	(2.778)	(2.593)	(2.664)	
Observations	67,553	425,620	425,620	425,620	
MSE-optimal h	X				
Robust bias-corrected	X				
Linear	X	X			
Linear interaction		X			
Quadratic			X		

Table 2: Regression discontinuity estimates for the intention-to-treat effect of the Health in Pregnancy Grant on birthweight.
Notes: Outcome is birthweight in grams. Standard errors, clustered by week of birth, in parentheses. Bandwidth refers to the size of the region either side of the cut-off (e.g., for a bandwidth of 16 weeks, the total region covered by the non-parametric analysis is 32 weeks). Model 1 is a non-parametric RD specific using the MSE-optimal bandwidth for each variable, with robust bias-corrected standard errors. Models 2-4 are parametric specifications all for a 16-week bandwidth around the cut-off: (2) is a linear polynomial; (3) is a linear interaction (i.e., spline); (4) is a quadratic polynomial. Preferred model is (1), the non-parametric model. Full results in Table 1 and Table 2 in the supplementary material. * p<0.1. ** p<0.05. *** p<0.01.

While all parametric and non-parametric models have negative coefficients for low birthweight and Figure 2 indicates a modest negative discontinuity, these results fail to reach significance for the most part. It is worth noting, however, that significant effects are reached when conventional confidence intervals are utilised in the non-parametric approach, an approach that many RD papers continue to use despite evidence that they can lead to overestimation of treatment effects (Hyytinen et al., 2018). Low birthweight effect sizes are consistent with expectations from back-of-the-envelope calculations from the main birthweight results: 2 percent of low birthweight babies are within 16.3 grams (the largest effect size identified) of the low birthweight cut-off (2500g), as corroborated by the low birthweight models. For certain subgroups with higher incidence of low birthweight (such as younger mothers), however, these models do suggest a significant reduction in the probability of being low birthweight, as shown in the next section.

4.2. Heterogeneous effects

Having established that the HPG led to a positive increase in birthweight on average across the population as a whole, I investigate whether the gains

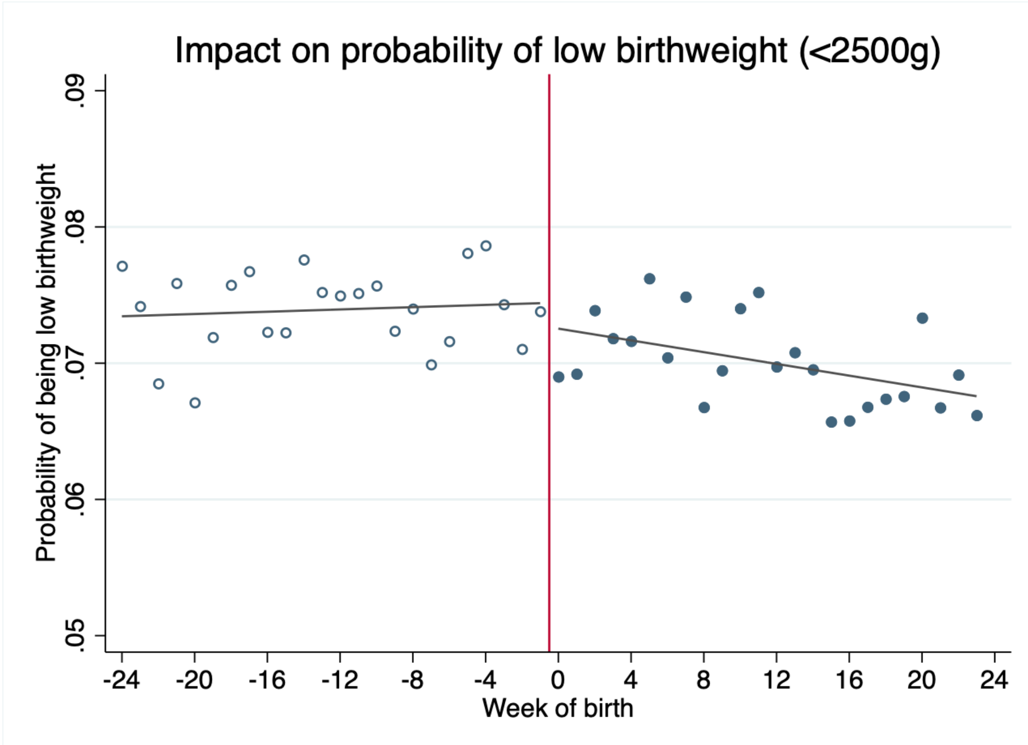

Figure 2: Impact of the Health in Pregnancy Grant on low birthweight status (2500g). *Notes:* 1. Scatterplot shows collapsed low birthweight data by week of birth with a fitted linear spline around the cut-off. 2. A bandwidth of 24 weeks (preferred parametric bandwidth) is used for graphical purposes.

of the grant were distributed differentially across groups.

Birthweight treatment effects are concentrated at the bottom of the birthweight distribution. When conducting separate RD analyses by birthweight decile, it is only the bottom decile of the birthweight distribution for whom there is a significant positive treatment effect, with this group seeing a statistically significant 16g (3.2 percent of a standard deviation) increase in birthweight at the cut-off. This bottom decile has a birthweight of 2279 grams on average, well below the low birthweight threshold of 2500 grams. This indicates that the gains of the grant were concentrated among the smallest babies, particularly those at risk of low birthweight.

There is consistent evidence from my preferred and non-parametric models that the grant had a larger impact on younger mothers aged 25 and

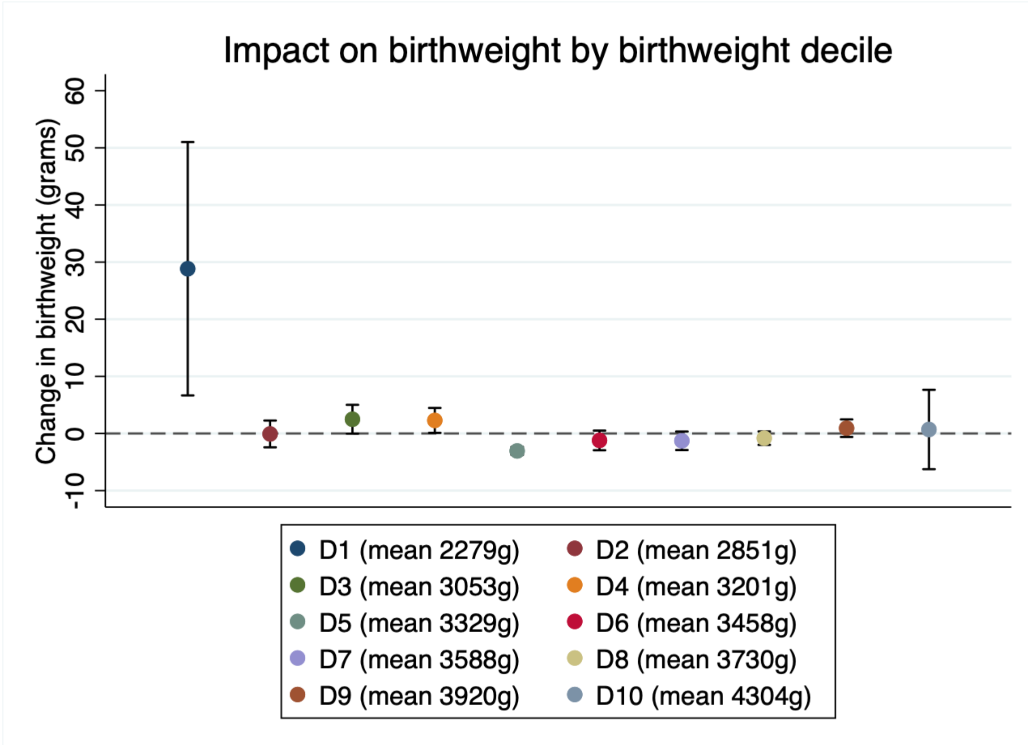

Figure 3: Heterogeneous treatment effects of the HPG across the birthweight distribution. *Notes:* 1. Each point estimate represents estimates from a separate RD regression for each birthweight quartile, using an 8-week bandwidth with robust bias-corrected standard errors (clustered by week of birth). 2. Birthweight deciles and quartiles are calculated among singleton births only (i.e., excluding multiple births). 3. Bars show 95 percent confidence intervals. 4. Full results are shown in Table 3 in the supplementary material.

under (see Table 5, supplementary material).[7] This is noteworthy given that younger mothers are associated with significantly lower birthweights on average (see Figure 4, supplementary material), and a relatively high incidence of low birthweight (see Figure 5, supplementary material). As Figure 4 shows, the relationship between effect size in my preferred model and maternal age is U-shaped, with mothers in the bottom quartile of the age distribution be-

[7]Parametric models are less supportive of heterogeneous effects across birthweight, maternal age and income deprivation. The parametric models should be treated with caution here, however, since the parametric assumption of normally distributed data is less likely to hold for these non-random subgroups of the population.

ing the only group with statistically significant effect sizes. Mothers in the bottom age quartile (25 years and under) experience a 29g increase in birthweight (5 percent of a standard deviation), a point estimate that is much higher than that for the overall population.

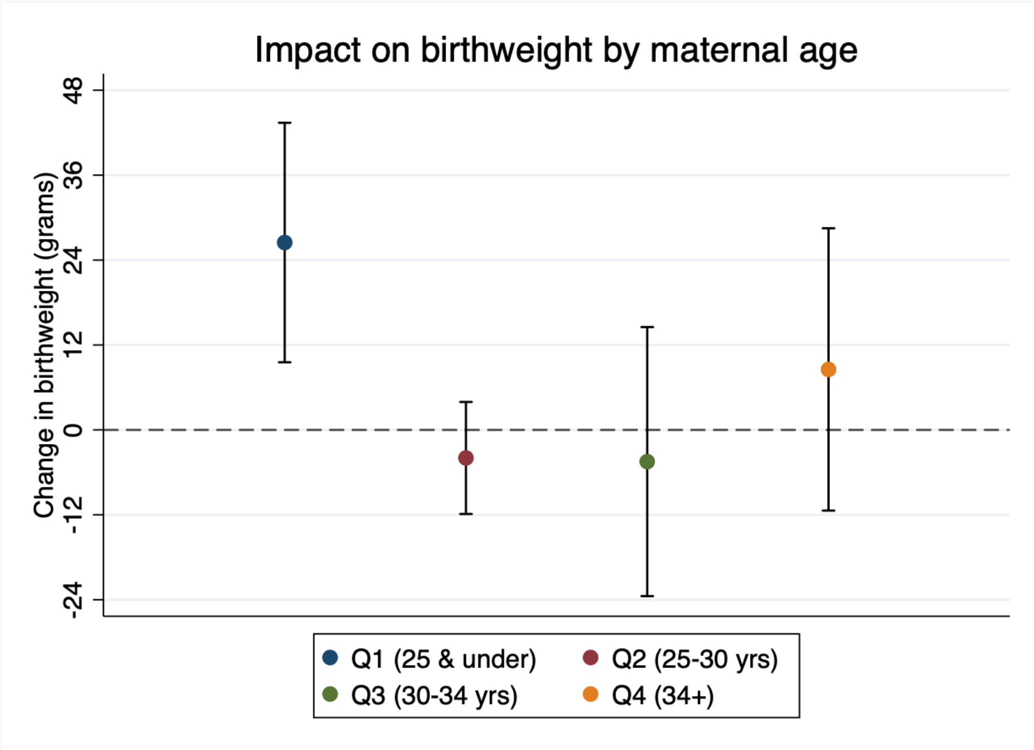

Figure 4: Heterogeneous treatment effects of the HPG by maternal age.
Notes: 1. Each point estimate represents estimates from a separate RD regression for each maternal age quartile, using an 8-week bandwidth with robust bias-corrected standard errors (clustered by week of birth). 2. Bars show 95 percent confidence intervals. 3. Full results by maternal age quartile and decile are shown in Table 5 and Table 6 in the supplementary material.

Furthermore, while the effect size for the overall population was not sufficient to reduce the probability of being low birthweight (less than 2500g), for younger mothers (aged 25 and under) the magnitude of the effect was sufficient to significantly reduce the probability of low birthweight by 0.9 percentage points, or 12 percent relative to this group's mean low birthweight rate of 7.4 percent.

Finally, matched data for births in England on the index of income deprivation indicate that the impact of the HPG was larger for mothers who live in more deprived areas.[8] Coefficients only reach statistical significance for the most deprived quartile, for whom there is a positive treatment effect of 20 grams in my preferred model (3.4 percent of a standard deviation). Differences in effect size by deprivation quartile are more muted than for maternal age, however, likely due to the ecological fallacy and the limitations of the index as a predictor of individual-level socio-economic status or income (see Table 7 and Figure 1, supplementary material).

4.3. Robustness checks

I conduct three robustness checks to test the validity of my identification strategy. First, I conduct placebo RD tests for the treatment date (6 April) in four different years, both before and after the HPG was implemented. Given the tendency of birthweight to fluctuate throughout the year by season and its natural incline in April, it is important to test whether the observed increase in birthweight at the treatment cut-off is due to (discontinuous) seasonal variation or a financial year effect. As Figure 5 demonstrates graphically, there is no robust evidence for a positive statistically significant jump in birthweight at any of these placebo cut-offs.

Second, I conduct covariate balance tests to check that the treatment effect is not attributable to a discontinuity in one of the predetermined covariates of birthweight. Multiple births and males are balanced around the cut-off across all models, and any effects for maternal age and the English index of income deprivation are highly sensitive to parametric or non-parametric specification, bandwidth and polynomial.

Third, I conduct a McCrary density test to rule out the possibility of pregnant mothers manipulating dates of birth in order to receive the grant. The identifying assumption of RD designs is that individuals lack precise control over the running variable in question. Institutional background knowledge is essential to validate this assumption (Eggers, Freier, Grembi and Nannicini, 2018). In the case of the HPG, the main source of manipulation of the running variable would be the postponement of conception in order to

[8]Indices of income deprivation are not comparable for England and Wales, so analysis by deprivation was conducted for births in England only. National Statistics Socio-Economic Classification (NS-SEC) occupation is recorded in the birth registrations data for 10 percent of observations only, so sample sizes were too small for meaningful inference.

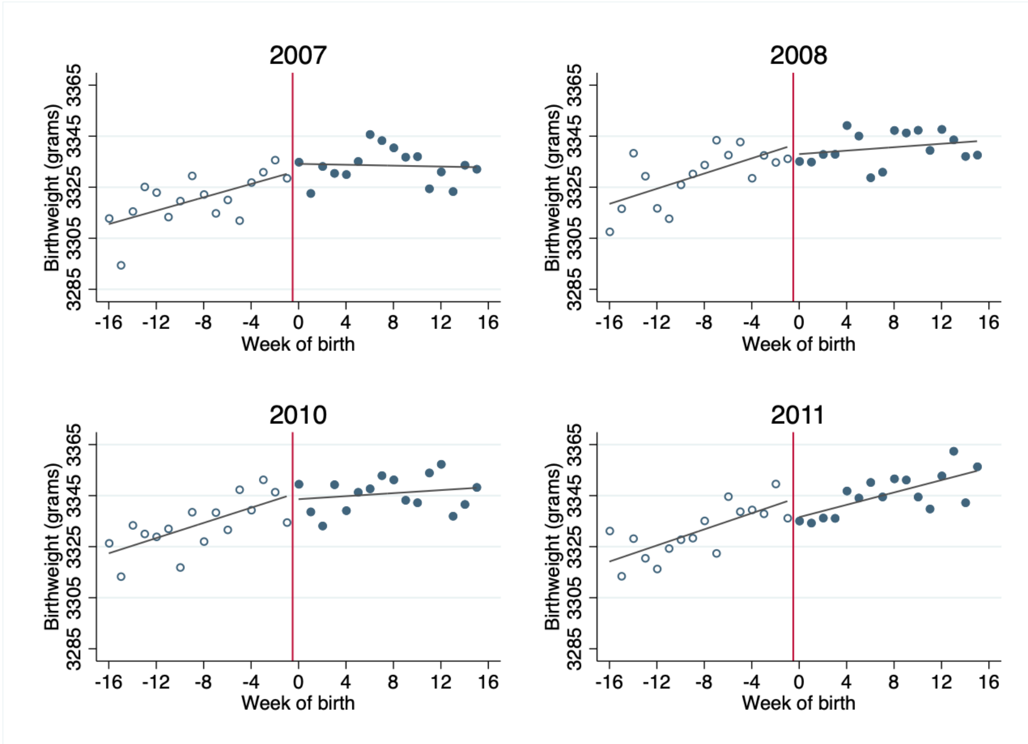

Figure 5: Placebo cut-off tests
Notes: 1. Scatterplot shows collapsed mean birthweight data by week of birth. 2. A bandwidth of 16 weeks (preferred parametric bandwidth in the main RD analysis) is used for graphical purposes and for consistent comparison with the treatment graph in Figure 1.

qualify for the grant. The first general discussion of eligibility requirements that mentions the approximate cut-off of April 2009, according to newspaper reports, appears to have been September 2007 (BBC News, 2007). However, no specific cut-off date was given, and it was not stated what point of gestation would have to been reached by April 2009 to be eligible. The possibility that prospective mothers had precise control over eligibility and the running variable is, therefore, remote. Additionally, a formal McCrary test fails to reject the null hypothesis of no discontinuity in the density at the cut-off: there is no significant evidence of manipulation or bunching at the cut-off.

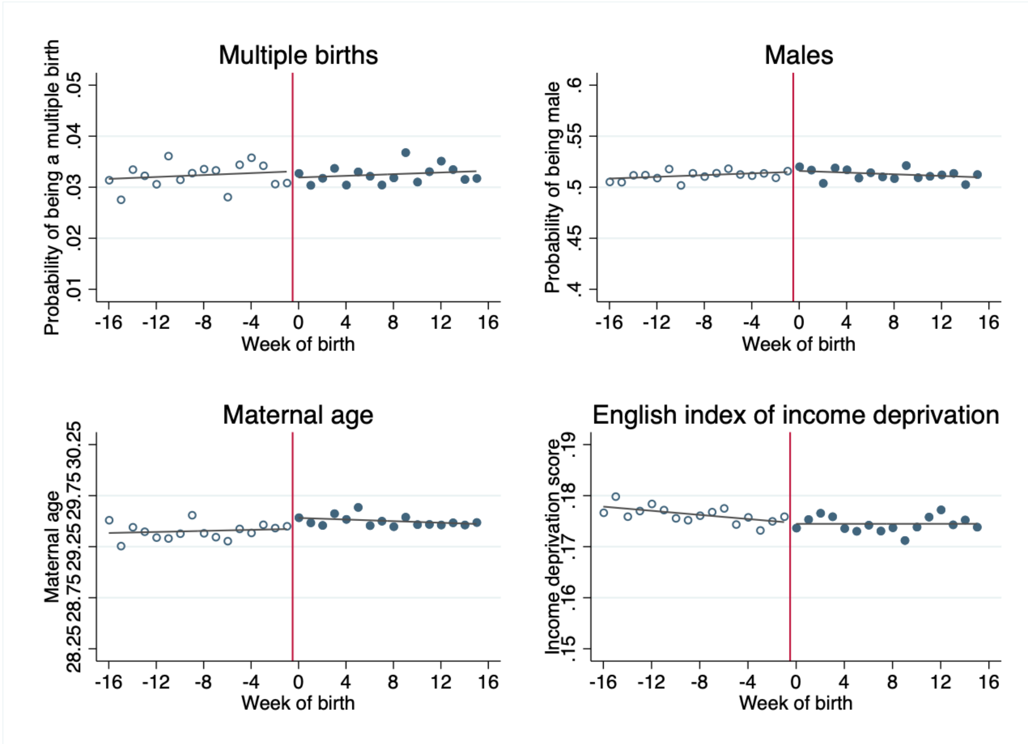

Figure 6: Covariate balance tests
Notes: 1. Multiple births are those where more than one child is born as a result of a pregnancy (e.g., twins, triplets). 2. English index of income deprivation data is matched to the data using mother's postcode of residence. 3. Scatterplot shows collapsed data for each variable by week of birth. 4. A bandwidth of 16 weeks (preferred parametric bandwidth in the main RD analysis) is used for graphical purposes and for consistent comparison with the treatment graph in Figure 1.

5. Discussion

My research suggests that paying a universal child benefit during pregnancy in England and Wales led to a small but significant increase in birthweight on average at population level. A significant positive effect on birthweight is shown to be robust across parametric and non-parametric approaches, different orders of the polynomial and different bandwidths. This contrasts sharply with regression discontinuity results for placebo cut-offs and baseline covariates – which demonstrate a lack of magnitude in the estimated effect, a lack of statistical significance and a lack of robustness. Both institutional context and a formal McCrary test support that manipulation of the running variable was unlikely.

A small positive increase in birthweight for the general population in the region of 11g (1.8 percent of a standard deviation) from a 190 GBP cash transfer is small but impressive relative to other studies' estimates of the impact of income on birthweight. Quasi-experimental evidence from the US on the impact of a 1000 USD (2000 prices) increase in EITC income suggested an increase in mean birthweight of 10g for a high-impact sample of single low-educated mothers (Hoynes, Miller and Simon, 2015). How did the HPG manage to achieve such comparatively large effect sizes despite its lower financial value?

There are three possible explanations. First, the HPG was tied to seeking antenatal health advice at 25 weeks of pregnancy, unlike the EITC and other windfall cash interventions. It is not possible to determine from birth registrations data whether the HPG was associated with improved engagement with antenatal care, but it is possible given findings to this effect in Scotland (Leyland et al., 2017). Further research by the author with Hospital Episode Statistics will investigate this within an English context. Second, unlike the other studied windfall income increases, the HPG was clearly labelled as a policy that was intended for mothers to use to improve health during pregnancy. Research has suggested that child benefit and child tax credit increase parental investment on healthy fruit and vegetables, books and toys in part due to the labelling of these benefits as designated for children; the same labelling effect may apply here with the HPG and healthy behaviours during pregnancy (Kaushal, Gao and Waldfogel, 2007). Third, unlike the EITC in the US, the HPG was universal and therefore less likely to attract stigma. This may have improved take-up rates among disadvantaged and younger mothers.

My heterogeneity findings are consistent with diminishing marginal returns to birthweight and income, as babies in the bottom decile of the birthweight distribution benefitted the most from the grant. This group, who are at risk of low birthweight, saw a larger 16g (3.2 percent of a standard deviation) boost to birthweights on average. In fact, these are the only group within the birthweight distribution who saw a significant positive impact. This is consistent with other studies which have found that birthweight effects due to increases in income are strongest at the bottom of the birthweight distribution and very weak at the top (Hoynes, Miller and Simon, 2015; Almond, Hoynes and Schanzenbach, 2011). This suggests that though the grant was universal and unconditional, its effects were targeted on those babies most in need of increases in birthweight. For the bottom birthweight decile, an effect size of 16g represents a sizeable proportion of their total weight and a significant contribution towards improved health and weight at birth.

The grant also had a larger effect for babies with young mothers (25 and under), of almost three times the magnitude of the overall birthweight treatment effect according to my preferred specification. This could be driven by a number of mechanisms. Since there is no official data on take-up, it is not possible to rule out the possibility that younger women were more likely to claim the grant and therefore to benefit from it. However, this is unlikely, as the grant required one-off attendance at a GP or midwife check-up and engagement with health services tends to be higher with older women (Department for Children, Schools and Families, 2009). Instead, there are three main candidate mechanisms for why the grant had a larger impact on younger women: income, antenatal health engagement, and maternity leave.

First, younger mothers under 25 are likely to have lower incomes than their older mothers, and consequently that the flat-rate HPG is likely to represent a larger proportion of their budget constraint. This may have led to greater reductions in stress and/or improvements in nutrition.

Second, younger and poorer mothers tend to have later engagement with antenatal health services, and the linking of the HPG to an antenatal check-up at 25 weeks may have improved this. A recent government publication found that 20.4 percent of women aged under 25 attended their antenatal appointment after 13 weeks of gestation, compared to 16.1 percent of women aged 25 or over (Public Health England, 2019). The linking of the HPG to seeking antenatal health advice at 25 weeks may have increased young mothers' access to information about staying healthy during pregnancy, thereby having a larger impact on birth outcomes than for other age groups. There is also some evidence that younger mothers can be more sensitive to 'learning effects' associated with cash transfers (Sosa-Rubí, Walker, Serván and Bautista-Arredondo, 2011). Younger mothers may therefore have been more responsive to the health advice and information surrounding the introduction of the HPG.

Third, younger women may have been more likely to view HPG as a form of wage replacement in place of paid maternity leave that compensates them for taking unpaid leave prior to the birth of their baby. This mechanism may be particularly salient for younger women because they are simultaneously more likely to be in manual or retail occupations that involve strenuous physical activity in the latter stages of pregnancy, and less likely to have comprehensive paid maternity packages (Equality and Human Rights Commission, 2016). Research by the Equality and Human Rights Commission found that younger women (aged 25 and under) were significantly more likely to experience pregnancy and maternity discrimination, with 1 in 10 young mothers reported that they left their employer due to health and safety concerns not being resolved and 15 percent discouraged from attending antenatal appointments (Equality and Human Rights Commission, 2016). Taking time off through unpaid maternity leave has been associated with increases in birthweight, but less so for poor, single and low-educated women due to the associated reduction in income (Rossin, 2011). The HPG may have enabled poorer and younger women to circumvent such financial repercussions, to the benefit of their wellbeing and birth outcomes.

6. Conclusions

Universal child benefits are a popular policy tool for tackling child poverty and improving child health. The benefits of birthweight to child health, adult health and socio-economic outcomes are clear. However, little to no research has considered what birthweight gains are to be made from paying universal child benefits during pregnancy. This paper fills that gap by being the first to consider the impact of a universal child benefit during pregnancy on birthweight.

I do so by exploiting a unique and short-lived policy experiment in the UK from 2009, when all pregnant mothers were eligible for a Health in Pregnancy Grant – a cash payment of the equivalent of child benefit in a lump sum during the third trimester of pregnancy. Using birth registrations data including date of birth, birthweight, maternal age, multiple birth status and postcode of mother's residence, I take advantage of an arbitrary eligibility rule for the Health in Pregnancy Grant based on expected due dates. I implement a regression discontinuity design in babies' dates of birth that enables me to overcome selection bias and to infer causal estimates of the impact of the policy.

I find that paying a universal child benefit during pregnancy led to small but significant increases in mean birthweight at population level. Effects were larger for the smallest babies at risk of low birthweight. Younger mothers (aged 25 and under) and mothers living in more deprived areas appear to have benefitted particularly from the policy. Though this gives some indication of the mechanisms through which effects were achieved, it is not possible with the data available to determine whether these effects were due to the direct impact of the income itself, behavioural effects on antenatal health engagement, or maternity leave timing.

This paper demonstrates that there are significant infant health gains to be made from paying universal child benefits during pregnancy. It also demonstrates that universal programmes can have impacts that disproportionately benefit disadvantaged groups. The gains of the HPG were specific to smaller babies, and the gains were larger for younger mothers in particular. Younger mothers may be a difficult group to reach, as they have lower access to maternity and health services, information and support networks (Department for Children, Schools and Families, 2009). The fact that the HPG delivered specifically for young mothers therefore suggests that the universalism of the grant may have been beneficial in offering non-stigmatised, accessible financial support for these women. Since the abolition of the HPG, the only remaining pregnancy-specific support in the UK – the Sure Start Maternity Grant and Healthy Start food vouchers – is means-tested, requiring a greater level of administrative input from the recipient. It is beyond the scope of this paper to compare the impact of the HPG with these means-tested programmes, but it is possible that the universalism of the HPG may have been an important factor for reaching younger mothers, thereby explaining the scale of its impact for this group.

My results also suggest that cash transfers do not have to be conditional in order to elicit positive behaviour. If the HPG was spent on plasma televisions, alcohol and cigarettes, as its critics had warned it would be, the treatment effect would be either zero or negative. The presence of a statistically significant increase in birthweight that was concentrated on smaller babies suggests, instead, that the HPG was spent on healthy nutrition, in ways that reduced antenatal stress or unhealthy behaviours, or some combination thereof. This supports the legitimacy of other labelled, universal and unconditional transfers to mothers that are founded on the principle of maternal choice, such as child benefit.

Many developed countries pay child benefits from birth as part of a wider 'cradle to grave' package of financial support that supports health across the life course. Fewer have experimented with starting financial support in utero. This paper demonstrates that there may be untapped gains to be made from doing so.

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