The infant health effects of starting universal child benefits in pregnancy: evidence from England and Wales*

Mary Reader†

Last updated: September 12, 2022

Abstract

I take advantage of a unique policy experiment in the UK to identify the infant health effects of starting universal child benefits in pregnancy. Leveraging administrative birth registrations and hospital microdata from England, I study the effects of the Health in Pregnancy Grant, a universal cash transfer of 190 GBP to all pregnant mothers who visited their doctor or midwife from 2009 to 2011. I exploit an arbitrary eligibility rule to implement a regression discontinuity design in the date of birth of the baby. I find that the policy led to significant increases in birthweight and reductions in prematurity. These effects do not appear to be explained by earlier antenatal care, nutrition or smoking. Instead, my results are consistent with reductions in prenatal stress among those most at risk of it: low-income, younger and older mothers.

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

*I acknowledge funding from a STICERD research grant and a Titmuss Meinhardt research grant from the LSE. I am grateful to Daniel Sturm and Kitty Stewart for their advice and guidance. I thank Berkay Özcan, Giulia Giupponi, Elisabetta De Cao, Ania Zylbersztejn, Tania Burchardt, Tammy Campbell, and attendees of a CASE Researchers' Workshop for helpful discussions and comments on earlier drafts.

†Corresponding author. Email: m.reader@lse.ac.uk. STICERD, London School of Economics and Political Science, Houghton Street, London, WC2A 2AE, United Kingdom

Contents

List of Tables

List of Figures

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, Lee, Sibieta & Waters 2020, Burchardt & Obolenskaya 2016, World Health Organization 2019). Universal child benefits from birth are a popular and effective policy mechanism for subsidising the costs of children and supporting child health (UNICEF 2020, East 2020). In expanding the budget constraint of the family and mothers in particular, child benefits from birth can benefit children's health by subsidising parental investment in children's nutrition and wellbeing, reducing family stress, and discouraging unhealthy behaviours in the family such as poor diet, smoking and alcohol use (Benzeval, Bond, Campbell, Egan, Lorenc, Petticrew & Popham 2014, Gregg, Waldfogel & Washbrook 2006, Milligan & Stabile 2011).

It is also clear that inequalities in child health start *before* birth: nutrition, stress and unhealthy behaviours during pregnancy are important determinants of health in infancy and beyond (Averett & Wang 2013, Cowan & Tefft 2012). Despite this, few countries provide universal cash transfers during pregnancy, and those that do are not targeted at infant health.[1] An exception was the UK Labour government which, in 2009, introduced a universal Health in Pregnancy Grant (HPG), a one-off lump sum of the equivalent value of child benefit (190 GBP) in the third trimester of pregnancy. The cash transfer was conditional on attending an antenatal appointment with a doctor or midwife. The UK government claimed that the policy would address "the serious problem of underweight babies in this country." Did it?

In answering this question, I provide unique evidence on the infant health benefits of starting universal child benefits in pregnancy. I use two rich and exhaustive administrative datasets from birth registrations and hospital records to obtain causal estimates of the impact of the Health in

[1] France and Belgium both have universal one-off 'birth payments', but their role is to provide income support around the birth of a child rather than to improve infant health: they are either paid at birth (France) or at 28-32 weeks of pregnancy at the earliest (Belgium). The country that has come closest to the HPG is Finland, offers a universal maternity package with a choice between a 'baby box' of clothes and toys and a one-off 170 EUR cash transfer from the twenty-second week of pregnancy. However, since the box is worth more than the transfer, most women choose the baby box over the cash, including 95 percent of first-time mothers (Koivu et al. 2020).

Pregnancy Grant on infant health in England and Wales. My identification strategy exploits an arbitrary eligibility rule for the policy, whereby mothers of all babies with an expected date of birth on or after 6 April 2009 were eligible for the cash transfer. This enables me to conduct a regression discontinuity design in the date of birth of the baby and to estimate an intention to treat effect.

I find that the Health in Pregnancy Grant led to a small but significant increase in mean birthweight at population level. This appears to be driven by improvements in infant health at the bottom of the birthweight distribution. Effects on the probability of being classified as low birthweight (less than 2500g) and extremely low birthweight (less than 1500g) are consistently negative in sign (though not always statistically significant). Additionally, my results suggest that the policy reduced the probability of babies being born prematurely (before 37 weeks of gestation). Babies born to mothers in deprived areas, younger and older mothers benefited the most from these infant health effects. For younger mothers (aged 24 or under), the policy significantly reduced the probability of extremely low birthweight (less than 1500g) by 26 percent.

My results are robust to two separate administrative datasets, parametric and non-parametric approaches, different bandwidths and polynomial specifications. They are strongly supported by a series of sensitivity tests. In particular, I show that there are no placebo effects on 6 April in years when the HPG was not introduced. This rules out the possibility that the beginning of the financial year induced a mechanical increase in birthweight. There is also no evidence of women manipulating their expected or actual dates of birth in order to receive the grant. In sum, I find strong causal evidence that the policy boosted infant health.

There are two possible explanations for this: that the conditionality of the grant incentivised earlier engagement with antenatal healthcare, and/or that the cash itself had an impact. To understand these mechanisms, I use administrative hospital data on the date of the first antenatal health appointment to identify whether the grant incentivised earlier engagement. Effects on antenatal engagement are mixed and lack robustness. At best, the policy appears to have incentivised women to attend an appointment one day earlier than they would have otherwise. Back-of-the-envelope calculations suggest that the magnitude of this effect is extremely unlikely to explain the size of the birthweight effect (Cygan-Rehm & Karbownik 2022). This indicates that the mechanism for the infant health effects was not the conditionality of the policy but the cash itself.

How did the cash make a difference? The grant seems to have driven longer gestational length rather than faster intrauterine growth, which makes improved nutrition an unlikely explanation for the effect (Amarante, Manacorda, Miguel & Vigorito 2016). I also find no evidence that the grant made women less likely to smoke during pregnancy. The main remaining hypothesis is that the grant reduced stress among pregnant women, thereby reducing the risk of prematurity and boosting birthweight. My results are consistent with the stress hypothesis in two ways. First, stress predominantly affects birthweight via gestational length rather than intrauterine growth, and I find birthweight effects to be driven by the former rather than the latter (Torche 2011, Kramer, Séguin, Lydon & Goulet 2000). Stress during the third trimester has a particular effect on prematurity, and my findings suggest that the HPG reduced prematurity by 8 percent. Second, low-income, younger and older mothers are particularly at risk of prenatal stress, and these groups were the biggest winners from the grant (García-Blanco et al. 2017, Persson & Rossin-Slater 2018).

My main contribution is to expand economic knowledge about the gains of starting universal child benefits in the prenatal phase. There is evidence that child benefits paid *after* birth can improve children's physical health and even the infant health of subsequent children born into the family (Milligan & Stabile 2011, González & Trommlerová 2022). However, no existing research has investigated the infant health effects of starting the payment of child benefits *during* pregnancy. First-time pregnant mothers are not eligible for child benefits, but they are eligible if child benefits start in pregnancy. Notably, I show that the HPG reduced the probability of extremely low birthweight by 26 percent among younger mothers (aged 24 and under), many of whom will not be receiving child benefits. An important implication of this is that child benefits from birth exclude a crucial demographic who have the potential to benefit significantly from cash transfers during pregnancy. Moreover, in finding that this small cash grant led to significant increases in birthweight, I demonstrate that that the potential for infant health gains from windfall increases in income is larger than previously thought. 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 & Simon 2015, Mocan, Raschke & Unel 2015, Chung, Ha & Kim 2016). In other words, the birthweight effects of starting universal child benefits in pregnancy are comparable with windfalls of approximately four times the financial value in PPP terms (OECD 2021).[2] This suggests that starting child benefits in pregnancy may be a particularly efficient method of narrowing inequalities in infant and child health.

Second, I contribute to the literature on conditional cash transfers by demonstrating that the main causal mechanism for the infant health effects is unlikely to be the conditionality of attending an antenatal appointment. This suggests that, particularly in contexts with universal state-funded healthcare and high antenatal engagement, the benefits of conditionality may be limited. Instead, my results are consistent with the cash leading to reductions in stress during pregnancy. Policies to reduce prenatal stress – including financial support during pregnancy – may therefore be a more effective investment.

The structure of this paper is as follows. Section 2 describes the Health in Pregnancy Grant in detail and reviews the literature on the impact of increases in income on infant health. Section 3 describes the data and sets out my identification strategy. Section 4 presents regression discontinuity estimates of the infant health and antenatal effects of the HPG, documents treatment heterogeneity, and implements a series of robustness checks. Section 5 discusses potential interpretations of these results and how they relate to existing research. Finally, Section 6 concludes and reflects on key policy implications.

[2] 190 GBP in 2009 is equivalent to 267.60 USD in terms of purchasing power parity (OECD 2021).

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 with an expected date of birth 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 25th 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 (but not limited to) fresh fruit and vegetables, cots, nappies, pushchairs, and any other costs associated with having a healthy baby (Directgov 2010). Figure S1 is a screenshot of the UK government's website for the HPG. The government sent a clear message about the sorts of items that could be bought with the grant: the website includes illustrations of a birthing ball, a pushchair, a baby carrier, a cot, fruit and vegetables. In being a universal cash transfer, 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 having a baby.

The HPG was unique in extending the principles of child benefit into pregnancy with the ambition of improving infant health. The Labour government envisioned two mechanisms through which the HPG would improve birthweight and reduce prematurity: 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

Birthweight is widely acknowledged to be one of the most important health outcomes in the life course, with significant effects on subsequent child and adult health (Brooks-Gunn & Duncan 1997). Babies who are born with a low birthweight (less than 2500g) are more at risk of infant mortality, particularly in the first 28 weeks after birth (Maruyama & Heinesen 2020, Brooks-Gunn & Duncan 1997). In adulthood, low birthweight can lead to “permanent health capital loss” (Maruyama & 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 & Heinesen 2020, Currie & Rossin-Slater 2015, Currie & Moretti 2003, Behrman & Rosenzweig 2004, Black, Devereux & Salvanes 2007). Finally, it has been shown that low birthweight is intergenerationally transmitted (Currie & Moretti 2003) and plays a role in the “the reproduction of inequality over generations” (Strully, Rehkopf & Xuan 2010, p.535). This remains the case despite the decline in the relative importance of infant health in recent years (Maruyama & Heinesen 2020, Goisis, Özcan & Myrskylä 2017) and its dissipating importance over the life course (Maruyama & Heinesen 2020).

The two main biomedical determinants of birthweight are gestational length and the speed of intrauterine growth (González & Trommlerová 2022). Over and above its implications for birthweight, gestational length is an important infant health outcome in its own right because it leads to systematic disadvantages for pre-term babies as they progress into childhood and beyond. A growing literature on relative age effects has documented that children are, on average, at an advantage if they are older within their cohort (Campbell 2014, Crawford, Dearden & Greaves 2016). Premature babies (those born before 37 weeks) start their pre-school and school career weeks or even months earlier than their full-term peers. This has potentially long-lasting implications for academic attainment, designation with special education needs, teacher perceptions and even labour market prospects (Campbell 2014).

Some research suggests that cash transfers are more effective at improving intrauterine growth than gestational length. González & Trommlerová (2022) find that the payment of universal child benefits in the years *prior* to pregnancy in Spain does not have any significant impact on gestational length, and attribute the birthweight gains to intrauterine growth. Similarly Amarante et al. (2016) identify declines in low birthweight in response to means-tested cash transfers to low-income women in Uruguay, with the effect being driven by intrauterine growth rather than gestational age. Nevertheless, some studies do document effects on gestation. Hoynes et al. (2015)'s study of the Earned Income Tax Credit (EITC) finds that increases in birthweight were achieved through both a reduction in prematurity and faster intrauterine growth, with candidate drivers being reductions in maternal smoking and increases in antenatal care.

The relative magnitude of gestational length and intrauterine growth effects can provide an indication of the causal mechanisms behind birthweight effects. Amarante et al. (2016)'s reading of the medical literature is that smoking and genetic history play a role in both intrauterine growth and prematurity; nutrition on the other hand affects intrauterine growth but does not affect gestational length.[3] In other words, if infant health effects are driven exclusively by gestational length, nutrition is an unlikely mechanism. Maternal stress, on the other hand, seems to play a role pre-

[3] González & Trommlerová (2022)'s simultaneous identification of faster intrauterine growth and improvements in nutrition is consistent with this.

dominantly in gestational length and prematurity, rather than intrauterine growth (Torche 2011, Kramer, Séguin, Lydon & Goulet 2000).

Cash transfers have the potential to influence these biomedical mechanisms, and thereby infant health, in two respects. First, increases in income can improve infant health (González & Trommlerová 2022, Hoynes, Miller & Simon 2015, Almond, Hoynes & Schanzenbach 2011, Strully, Rehkopf & Xuan 2010). Cash transfers can be spent on goods and services that improve health and nutrition during pregnancy, such as fresh fruit and vegetables or wellbeing activities (Gregg, Waldfogel & Washbrook 2006). Even if the cash is not spent directly on health investment, it can improve infant health by reducing financial strain and stress. Evidence from domestic violence, family ruptures and earthquakes indicate that exposure to maternal stress has significant effects on infant health (Currie, Mueller-Smith & Rossin-Slater 2022, Persson & Rossin-Slater 2018, Torche 2011). Stress can also affect birth outcomes indirectly via maternal smoking, alcohol and drug consumption (De Cao, McCormick & Nicodemo 2022, Averett & Wang 2013, Cowan & Tefft 2012, Gregg, Waldfogel & Washbrook 2006, Strully, Rehkopf & Xuan 2010). Windfall cash transfers (such as the EITC) have been found to reduce maternal stress and depression, with consequent implications for infant health (Boyd-Swan, Herbst, Ifcher & Zarghamee 2016).

Second, cash transfers can boost infant health if they are conditional on other behaviours (such as antenatal attendance) which improve health during pregnancy. Attending antenatal appointments at an early stage during pregnancy enables medical professionals to detect abnormalities and risk factors (including conditions like pre-eclampsia). It can also facilitate access to information on how to stay healthy during pregnancy (including the importance of folic acid supplements, and the risks of maternal smoking). Cygan-Rehm & Karbownik (2022) exploit a Polish reform which made receipt of an existing 'baby bonus' (a universal one-off cash transfer after childbirth) conditional on attending antenatal care before 10 weeks. Since the cash transfer was already in place, the authors are able to isolate the causal effect of earlier timing of antenatal care. They find that earlier antenatal care has a modest but statistically significant effect on neonatal health, including birthweight. Exploiting an exogenous change in prenatal care usage due to a bus strike, Evans & Lien (2005) also show that prenatal care reduces the probability of smoking during pregnancy.

The HPG involved both cash and conditionality. Infant health effects could have proceeded from either, both or neither of these. As detailed in Section 2.1, the government pitched the 190 GBP grant as a lump sum that could be spent on healthy eating, wellbeing activities, or helping out with large costs in preparation for the arrival of a baby. The government also made the HPG conditional on attending antenatal healthcare: one of the main justifications for the grant being paid in the third trimester was that it would encourage women to seek antenatal health advice before 25 weeks. In fact, the only existing study of the HPG, in Scotland, found that the policy increased the odds of booking before 25 weeks by 10 percent (Leyland, Ouédraogo, Nam, Bond, Briggs, Gray, Wood & Dundas 2017).

While Leyland et al. (2017) find significant effects of the HPG on antenatal engagement, however, they find no effects on birthweight, prematurity or maternal health in Scotland (Leyland, Ouédraogo, Nam, Bond, Briggs, Gray, Wood & Dundas 2017). 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). Given evidence that recessions impact on infant health (De Cao, McCormick & Nicodemo 2022), it is possible that the global financial crisis and subsequent recession 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 local to the treatment cut-off (6 April 2009), and such exogenous shocks are controlled for.

3 Data and empirical strategy

3.1 Data

I use two separate comprehensive administrative datasets: birth registrations microdata for England and Wales,[4] and birth episodes from Hospital Episode Statistics (HES) for England.[5] Both are high-quality administrative datasets at the individual (birth) level.[6] While the hospital data offer a rich set of variables on births, a recent linkage study found that birth registrations offer superior data quality and completeness (Ghosh, Ashworth, Hansell, Garwood, Elliott & Toledano 2016). Birth registration is a statutory responsibility, whereas hospitals are not legally required to report certain details about the baby (including birthweight and gestational age) in hospital records.

I lead with results from the birth registrations microdata. I use the hospital data to obtain data on gestational age and antenatal health, which are necessary for two purposes: to investigate effects on prematurity and antenatal engagement as outcomes; and to validate the use of actual date of birth as the running variable by comparing results with actual and expected date of birth. In both datasets, I drop duplicate records and stillbirths, and create a main window dataset which covers a symmetric two-year period around the introduction of the HPG on 6 April 2009. Table 1 and 2 presents summary statistics for the birth registrations and hospital data respectively from April 2008 to April 2010.

3.1.1 Birth registrations microdata, 2006-2014

Birth registrations data provide the gold standard administrative data on births in England in terms of completeness and quality (Ghosh, Ashworth, Hansell, Garwood, Elliott & Toledano 2016).

[4]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 ONS research datasets which may not exactly reproduce National Statistics aggregates.

[5]Copyright © (2022), NHS Digital. Re-used with the permission of NHS Digital. All rights reserved.

[6]While there is an ongoing research project attempting to link these datasets (Coathup, Macfarlane & Quigley 2020), unfortunately the linked data is not available for other researchers at present.

Parents have a statutory responsibility to register a child being born within 42 days of the birth. The data are thereby exhaustive, covering all births that take place in England and Wales. Registration is completed by the mother and/or father at the local registry office. These details are digitised and processed by the Office for National Statistics (ONS). The ONS links the birth registration with a birth notification, which they receive from the doctor or midwife who was present at the baby's birth. The birth notification includes the birthweight of the baby, as measured in grams by the doctor or midwife at the time of birth.

The final birth registrations microdata are pseudonymised at the individual level for researchers to use. The dataset includes variables on the actual date of birth of the baby, birthweight (grams), stillbirth status, multiple birth status, sex of the baby, maternal and paternal age, and the postcode of the mother's residence. These variables are close to 100 percent complete. My main outcome variable – birthweight – is a discrete variable measured immediately after birth in grams, and is normally distributed (see Figure S2).[7] I construct two variables to capture the medical definitions of low birthweight (babies born weighing less than 2500g) and extremely low birthweight (babies born weighing less than 1500g).

The data do not include any data on income or earnings. While the ONS does code a random 10 percent sample of the data with the National Statistics Socio-economic Classification (NS-SEC) occupation category of the mother and the father, the sample size is too small for RD estimation with small bandwidths. Instead, I use the postcode of the mother's address to match in the index of income deprivation at Lower Super Output Area (LSOA) level (a small neighbourhood of approximately 1500 people). The index of income deprivation is a score capturing the proportion of the population in the area who are in receipt of means-tested benefits and likely on a low-income.[8]

Unfortunately, the birth registrations microdata do not contain gestational age. As a result, it is not possible to observe expected date of birth, upon which eligibility for the Health in Pregnancy

[7] After inspecting the birthweight distribution, I code outlier observations with a recorded birthweight less than 265 grams (n=3,127) or larger than 5650 grams (n=615) as missing. This includes missing values coded as '9999' or '9998'.

[8] I match index of income deprivation scores for England and Wales from the nearest available data point to the introduction of the HPG (2015) to the births data at LSOA level by the mother's postcode of residence.

Grant was based. I therefore use actual date of birth as a proxy for expected date of birth. I discuss the implications of this in Section 3.2 and Section 4.4.1.

3.1.2 Hospital Episode Statistics microdata, 2006-2014

My second source of data is administrative hospital records (Hospital Episode Statistics) from the National Health Service (NHS) in England. The NHS is the main provider of healthcare in the UK and offers universal healthcare which is free at the point of use, funded by general taxation. English NHS hospitals collect these data so that hospital activity can be remimbursed by the government through payment-by-results.[9] The vast majority (97 percent) of registered births in England take place in NHS hospitals (see Table 1). The data exclude the remaining 3 percent of births that take place in private hospitals, at home or elsewhere.

The hospital microdata include a bespoke variable for the actual week of birth relative to the 6 April 2009 cut-off,[10] gestational age at birth (weeks), gestational age at the first antenatal appointment (weeks), the number of babies in the pregnancy, maternal age at birth (years) and ethnicity.

For most births, gestational age at birth (in weeks) is derived from an ultrasound dating scan conducted during an antenatal appointment at a specialist maternity department in hospital at approximately 10-14 weeks of gestation (Redshaw & Heikkila 2010). A specialist sonographer carries out an ultrasound scan to confirm gestational age and the expected date of birth, as well as to identify viability and multiple pregnancies. Mothers will usually arrive at this antenatal appointment with an existing estimate of their expected date of birth from their doctor or midwife,

[9] While the NHS is UK-wide, healthcare is devolved to the four nations of the UK (England, Wales, Scotland and Northern Ireland), so Hospital Episode Statistics only cover England.

[10] The secure data application process for Hospital Episode Statistics is lengthy and approval from the Confidentiality Advisory Group is required for access to daily date of birth. Since expected date of birth is not directly recorded in birth episodes, the only way of calculating the expected due date of the baby is to combine data on the gestational at birth (in weeks) and the actual week of birth of the baby. It is therefore not necessary to know the daily date of birth for these purposes. Given this, I requested a bespoke actual week of birth variable relative to the treatment cut-off of 6 April 2009 to be created by NHS Digital. Actual dates of birth from 31 March-5 April are coded with an actual week of birth of -1; 6-13 April 2009 are coded as 0; 14-21 April 2009 are coded as 1; and so on. Expected week of birth is then calculated in line with Equation 1.

based on their self-reported first day of their last menstrual period (LMP).[11]

Expected date of birth is not recorded directly in the hospital data. Instead I calculate the expected week of birth (e_{wob}) using gestational age at birth in weeks and the actual week of birth relative to the cut-off of 6 April 2009 (wob):

$$e_{wob} = wob + (40 - gestage) \tag{1}$$

, where wob is the actual week of birth relative to 6 April 2009 and $gestage$ is gestational age at birth in weeks.[12]

Finally, the hospital data include variables on the gestational age of the baby at the first antenatal appointment and the specific date of that appointment. This enables me to investigate whether the conditionality of the HPG incentivised earlier engagement with antenatal healthcare.

3.2 Empirical strategy

I identify the infant health effects of the Health in Pregnancy Grant by exploiting variation in the date of birth of the baby with a regression discontinuity design. The policy design of the HPG makes it an ideal candidate for a regression discontinuity, since it was simultaneously universal and administered on an arbitrary basis by expected date of birth: all mothers of babies with an expected date of birth on or after 6 April 2009 were eligible for the grant.[13] To the best of my knowledge, no other relevant policy affecting pregnant women was introduced discontinuously on

[11] If the mother participates in a dating ultrasound scan and the ultrasound estimate differs by more than 7 days from the LMP estimate, then the gestational age is amended to the ultrasound estimate and recorded in the hospital database. I drop implausible gestational ages based on the birthweight data in line with the method described in Herbert, Wijlaars, Zylbersztejn, Cromwell & Hardelid (2017).

[12] Since missingness of gestational age is not random, I replace expected week of birth with the actual week of birth if the former is missing to avoid sample selection. Results are virtually unchanged when doing this.

[13] 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 and the abolition of the baby tax credit, which may have confounding effects on birthweight and even fertility, thereby making it difficult to isolate the effect of the HPG's abolition.

6 April 2009.

Since claiming the HPG was dependent on attending an antenatal check-up, it exhibited imperfect compliance. Estimates from tax authority expenditure data suggest high take-up rates of 92.9 percent.[14] 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.

Since receipt of the HPG is not measured in the data, 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 with a traditional sharp RD. I estimate the reduced form relationship between an outcome (e.g., birthweight) and an instrumental variable (date of birth) which serves as a proxy for an explanatory variable (receipt of the HPG).

A further source of 'fuzziness' in my empirical strategy is that while eligibility for the HPG was based on expected date of birth (e_{dob}), the birth registrations data only include actual date of birth (dob). In my main results, I use actual date of birth as a proxy for expected date of birth. My basic empirical specification is the following:

$$Y_i = \beta_0 + \beta_1 hpg_i + \beta_2 dob_i + X_i + \theta_d + \epsilon_i \tag{2}$$

, where Y_i is the outcome of choice (e.g., birthweight in grams), hpg_i is a dummy variable equal to one if the baby's actual date of birth was on or after 6 April 2009, dob_i is actual date of birth relative to 6 April 2009, ϵ_i is the error term, and i indexes individual births. β_1 is the coefficient of interest and represents the ITT effect of the HPG.

Using actual date of birth (dob) as a proxy for expected date of birth (e_{dob}) can be expected to introduce some measurement error in the running variable. One concern is that this measurement

[14] 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, I estimate 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.

error could be non-random. Some late babies ($dob > e_{dob}$) will be categorised as treated when they were not, while some premature babies ($e_{dob} > dob$) may be categorised as untreated when they were. Both of these effects would introduce upward bias in my estimated treatment effect, since late babies are heavier and premature babies lighter on average. In Section 4.4.1, I test for sensitivity to this in two ways. First, I leverage the hospital data to validate the use of actual date of birth by comparing effects with actual and expected week of birth as the running variable.[15] Second, I use a 'donut' RD to exclude small bands of observations around the cut-off, as recommended by Barreca, Guldi, Lindo & Waddell (2011) in the case of measurement error in the running variable. My results are statistically indistinguishable when these tests are applied. This suggests that any systematic measurement error in the running variable is minor, and does not present a threat to identification.

In estimating the regression discontinuity, I use both parametric and non-parametric approaches. In parametric models, I include linear or quadratic trends in the running variable (dob_i). As a robustness check I include a vector of controls (X_i)[16] and day-of-the-week fixed effects (θ_d), to account for seasonality. In non-parametric models, I fit a local polynomial either side of the cut-off of degree 1 (linear) or degree 2 (quadratic). My preferred specification is a non-parametric local linear regression: this allows the slope of the data to change either side of the cut-off, thereby providing a better approximation of the fit either side of the cut-off (Hahn, Todd & Van der Klaauw 2001, Cattaneo, Idrobo & Titiunik 2019).[17] I cluster standard errors by the running variable.[18]

[15] As explained in footnote 10, the hospital data does not include daily date of birth or expected date of birth. Instead, I use data on the actual week of birth and calculate the expected week of birth using gestational age, in line with Equation 1. This enables me to compare results with actual and expected week of birth.

[16] In the birth registrations data, I control for maternal age, multiple birth status, sex of the baby, and income deprivation score. In the hospital data, I control for maternal age, multiple birth status, and whether the baby's recorded ethnicity is White British.

[17] As shown in Section 3.3, the non-parametric models are also completely clear in a covariate balance test, whereas maternal age exhibits a significant effect in a couple of parametric models (these effects appear to reflect seasonal variation, as they disappear when controls and day-of-the-week fixed effects are added). This is an additional reason why I prefer a non-parametric approach: it enables me to abstract from global trends and isolate discontinuous variation local to the cut-off.

[18] This is recommended for non-parametric RD analyses with a discrete running variable due to the possibility of specification error (Lee & Card 2008).

3.3 Identification checks

The identifying assumption of RD designs is that individuals lack precise control over the running variable in question. This could be violated in two ways within this context. First, women could postpone conception to push their expected or actual date of birth until after the 6 April 2009 cut-off, thereby making them eligible for the HPG. Second, women could mislead healthcare workers about the date of their last menstrual period in order to manipulate their expected date of birth. Alternatively, they could persuade healthcare workers to record an inaccurate expected date of birth at their ultrasound dating scan.

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 could have precisely timed their conception in order to get the grant is, therefore, remote. In addition, the hypothesis that women could persuade healthcare workers to misrepresent their expected date of birth seems implausible. Survey evidence from 2009 indicates that at the time of the policy 90 percent of pregnant women attended an ultrasound dating scan, which is the main way of assessing gestational age and expected date of birth (Redshaw & Heikkila 2010). Gestational age is objectively estimated from the crown-rump length (CRL) in the ultrasound scan by a specialist sonographer, who is subject to regular retrospective quality assurance (Royal College of Radiologists 2014, Lodge et al. 2014, Salomon et al. 2013). Additionally, misrepresenting expected dates of birth would carry clinical risks. The accurate recording of gestational age is an important piece of clinical information for most obstetric and neonatal decisions and procedures, both before and after birth. Medical professionals need to know an accurate gestational age in order to assess small-for-gestational-age (SGA) status, intrauterine growth retardation (IUGR), and the optimal timing of induction (National Childcare Trust 2020, Salomon et al. 2013).

I test for manipulation around the cut-off empirically with a McCrary test, which examines whether there is discontinuous bunching in the density of births around the cut-off. If women had manipulated their dates of birth, one would expect a significant positive McCrary estimate and graphical evidence of bunching to the right of the cut-off. Figure S3 and Figure S4 show McCrary manipulation plots for the running variable in the birth registrations data (actual date of birth) and the hospital data (actual week of birth and expected week of birth). McCrary RD estimates and p-values are included on each graph. They are all negative and statistically insignificant: the null hypothesis of no bunching cannot be rejected. This indicates that there was no manipulation of expected or actual dates of birth by women or healthcare workers.

I test the identifying assumption that births below the cut-off are a valid counterfactual for those above the cut-off further by implementing covariate balance tests. Table S1 and Table S2 display summary statistics separately for the treatment and control group (as defined by being above or below the 6 April 2009 cut-off) with the birth registrations and hospital data respectively. Treatment and control groups appear balanced by key covariates. Table S3 presents a full set of formal covariate balance results. Multiple births, female babies, local deprivation rates, teenage mothers, births in NHS hospitals, and parents in low socio-economic status (SES) occupations are all clearly balanced around the cut-off. Two parametric models indicate significant results for maternal age, but these disappear when a non-parametric model is used and when controls are added. Figure S5 illustrates this graphically for key covariates: there are no visible discontinuities around 6 April 2009. This lends support to the assumption that births are 'as if' randomised either side of the cut-off, thereby supporting the validity of a regression discontinuity approach.

4 Results

I present my findings in four parts. First, I estimate the intention to treat effect of the HPG on infant health, and investigate the extent to which this is driven by intrauterine growth or longer gestational length. Second, I leverage data on antenatal health booking to test whether the infant health effects are driven by the cash or the conditionality of the grant. Third, I document heterogeneity in the effects of the grant. Finally, I present two major robustness checks to check for non-random measurement error in the running variable and for effects at placebo cut-offs.

4.1 Effects on infant health

4.1.1 Birthweight

Table 3 uses the birth registrations data to present RD estimates of the birthweight effects of the Health in Pregnancy Grant for a 16-week bandwidth either side of the cut-off. I find a small increase in mean birthweight in the region of 9-12 grams which is robust to parametric and non-parametric specification, different orders of the polynomial, and the addition of day-of-the-week fixed effects and controls. The coefficients for low birthweight and extremely low birthweight are always negative in sign, but significance is variable. As shown in Table S4, Figure S6 and Figure S7, these results are stable across different bandwidths.

Figure 1 illustrates mean birthweight effects graphically. There is a clear discontinuous increase in the birthweight data at the introduction of the HPG. Since the birthweight data for a 16-week bandwidth is very linear, I select a non-parametric local regression with a linear polynomial (column 1 of Table 3) as my preferred specification. RD estimates and standard errors from this model are labelled on the graph. This specification indicates an increase in mean birthweight of 10.5 grams (0.32 percent relative to the control mean) which is statistically significant at the 5 percent level.

As shown in Figure S8 and Figure S9, there are modest discontinuities in low and extremely low birthweight, but these are not statistically significant for the most part. My preferred specification (a non-parametric local linear regression) indicates a non-significant reduction in the probability of being low birthweight of 0.3 percentage points (4 percent relative to the control mean). Meanwhile there is a non-significant decline in the probability of extremely low birthweight of 0.1 percentage points (8 percent).

In order to investigate the relative birthweight gains across the birthweight distribution, I follow the approach of González & Trommlerová (2022) and Hoynes, Miller & Simon (2015) in constructing a series of dummy variables which are equal to one if the baby weighs less than a certain threshold from 3500g to 1500g, in increments of 100g. I conduct separate RD regressions with each dummy variable as the outcome, using my preferred specification. Since the proportion of babies born below the threshold decreases with the threshold, in Figure 3 I present these effects relative to the mean value for each dummy variable. The size of the relative reduction in the proportion of babies weighing less than the threshold increases as the threshold becomes smaller. The probability of weighing less than 1500g reduced by approximately 10 percent, while the probability of weighing less than 2750g fell by approximately 2.5 percent. This indicates that while there were no significant effects on low or extremely low birthweight, the relative gains of the grant were greatest for the smallest babies.

4.1.2 Gestational length and prematurity

Next, I use the hospital data on gestational age to investigate effects on gestational length and prematurity. Table 4 demonstrates that positive effects on gestational age at birth are small, ranging from 0.06-0.1 weeks (less than a day). The HPG does, however, appear to have made a sizeable difference to babies at risk of being born prematurely. As illustrated in Figure 2 and Table 4, the HPG seems to have led to a significant decline in the proportion of babies who are born prematurely (before 37 weeks) of 0.5-0.8 percentage points (7-11 percent relative to previous levels).

To investigate whether this explains the birthweight effects documented above, in Table S5 I use the hospital data to add gestation fixed effects to my birthweight RD model (González & Trommlerová 2022). This tests whether the increases in birthweight are attributable to longer gestational length or to faster intrauterine growth. The birthweight effects disappear when gestation fixed effects are added: for a given gestation, babies are no heavier due to the grant. This indicates that the birthweight gains are explained by longer gestation and the reduction of prematurity, rather than faster intrauterine growth.

4.2 Effects on antenatal health engagement

The Health in Pregnancy Grant was available to pregnant women from the 25th week of pregnancy, and was conditional on having attended an antenatal appointment. Since changes in antenatal usage could contribute to infant health effects, I leverage the hospital data to test whether the grant incentivised women to attend their first antenatal assessment earlier.

The RD estimates from Table 4 suggest a small reduction in mean gestational age at the first antenatal assessment in the region of 0.09-0.3 weeks (1-2 percent relative to the control mean). However, placebo cut-off tests for this variable in Table S6 indicate that the treatment effects are very similar to placebo effects in other years. This suggests that the significant effects in Table 4 for this variable are spurious and are unlikely to reflect the HPG itself. In Table S7 I investigate whether there are valid antenatal health effects concentrated at specific stages of pregnancy by listing results for four dummy variables which capture attendance during the first, second, or third trimester, and before 25 weeks. The point estimates are significant and positive for the first trimester, and tend to be null or negative for the second and third trimester, which seems to suggest that any incentivisation occurred in the first trimester. Similarly, however, these dummy variables do not perform well in placebo cut-off tests.[19] Taken together, these results indicate that the null hypothesis of no treatment effect of the HPG on antenatal care cannot be decisively rejected.

4.3 Heterogeneous treatment effects

While the HPG was universal, effects are unlikely to have been uniform across the population: diminishing returns imply that 190 GBP make a greater difference to low-income and disadvantaged women. I test for such treatment heterogeneity by running separate RD analyses by income deprivation and by maternal age quartile.

Interestingly, Figure 4 shows that birthweight effects are u-shaped by maternal age, with younger

[19] Placebo cut-off tests for these variables are available on request.

and older mothers benefiting the most in both absolute and relative terms.[20] Effects on mean birthweight are not statistically significant when splitting the sample by maternal age quartile, due to the smaller sample size. However, the point estimate for younger (24 and under) and older (34 and over) mothers is 14 grams (0.44 and 0.43 percent relative to each group's control mean) – roughly a 50 percent increase on the average treatment effect across the population. Effects on low and extremely low birthweight are even more u-shaped across the maternal age distribution: the point estimates for the middle two quartiles are approximately zero, while they are negative for younger and older mothers. Notably, younger mothers (aged 24 and under) experience a significant reduction in the probability of being extremely low birthweight of 0.3 percentage points (26 percent in relative terms). They also benefit from a non-significant reduction in low birthweight of 0.5 percentage points (7 percent). Meanwhile, older mothers (aged 34 and over) see a non-significant reduction in extremely low birthweight of 0.4 percentage points (15 percent in relative terms), and a non-significant reduction in low birthweight of 0.6 percentage points (8 percent).[21]

Additionally, Figure S11 shows that birthweight effects are driven predominantly by babies born in areas with above-median levels of deprivation. Babies born in deprived areas experience a significant 13 gram increase in birthweight on average; estimates are positive (8 grams) but not statistically significant for less deprived areas. There are no notable differences in the low and extremely low birthweight effects by deprivation.[22]

[20] For a finer categorisation of maternal age, Figure S10 plots RD estimates by maternal age decile. Effects are more erratic and do not tend to reach statistical significance when deciles are utilised, hence my use of quartiles in the main results.

[21] I do not report heterogeneity results from the hospital data for a couple of reasons. First, the hospital data only include postcode of residence for a handful of observations, so the index of income deprivation cannot be used. Second, just 67 percent of babies' records have data on maternal age. When combined with the completeness of the birthweight, gestational age, and antenatal appointment data, this means that approximately half of the sample cannot be used in maternal age heterogeneity analyses. Since the remaining sample are likely to be systematically different from the missing observations, such results would be biased.

[22] While the index of income deprivation is a useful predictor of low-income in the absence of individual-level income data, it is relatively blunt measure which involves measurement error: some high-income women live in poor areas, and vice versa. This may explain why the heterogeneity results by deprivation are more muted than by maternal age.

4.4 Robustness checks

In Table S4 and Figure S6 I demonstrate that my RD estimates are robust to different choices of bandwidth and to the use of robust bias-corrected standard errors (Cattaneo, Idrobo & Titiunik 2019, Hyytinen, Meriläinen, Saarimaa, Toivanen & Tukiainen 2018). This is also the case when a uniform kernel is used in the non-parametric approach (see Figure S7). In this section I present results from two more major robustness checks. First, I test whether my results are explained by systematic measurement error in the running variable. Second, I conduct four placebo cut-off tests at 6 April in 2007, 2008, 2010 and 2012.

4.4.1 Measurement error in the running variable

As mentioned in Section 3.2, one potential problem with my empirical strategy would be if using actual date of birth as a proxy for expected date of birth led to systematic measurement error in the running variable, causing upward bias in my estimates. I probe this possibility in two ways.

First, I exploit the hospital microdata to validate the use of actual date of birth by comparing results with actual and expected week of birth as the running variable. As shown in Table S8, results are consistently positive and statistically significant when using expected week of birth. Figure 5 compares RD graphs and estimates with actual week of birth as the running variable against those with expected week of birth. The magnitude of the RD estimate is attenuated by approximately 10.7 percent when using expected week of birth instead of actual week of birth, but effects remain statistically indistinguishable. A clear discontinuity persists in the data when using expected week of birth in Figure 5.[23] Applying this attenuation rate to my main estimates from Table 3 implies birthweight increases in the region of 7.7-10.3 grams.

[23] To verify that it does not matter whether daily actual date of birth or actual week of birth is used, in Table S9 I show results from the birth registrations data with actual week of birth as the running variable. Results are almost identical to those with actual date of birth in Table 3. When comparing results with actual week of birth across the two datasets, RD estimates are larger and more variable in the hospital data (12-36 grams) than the births data (9-12 grams) when using actual week of birth as the running variable. This is likely to be due to the superior data quality of the birth registrations data: the births data contain complete birthweight data on all births that took place in England and Wales, whereas the hospital data only record birthweight for 75 percent of births.

My second test for measurement error in the running variable is a 'donut RD' to check whether my results are sensitive to the exclusion of a small number of observations around the cut-off. If systematic measurement error in the running variable is a problem, then it should be more of an issue for observations that are close to the cut-off: these are the observations which will be 'miscategorised' in the control or treatment group and thereby create upward bias in the treatment effect (Barreca, Guldi, Lindo & Waddell 2011). Results for a series of donut sizes are listed in Table S10 and Table S11. The magnitude of the RD estimate decreases incrementally as larger donuts are applied, until it starts to increase again after 2 weeks. The smallest point estimate from the donut test is 8.5 grams. These results indicate that the observed birthweight effects are robust to adjusting for measurement error in the running variable, and that this bias turns out to be minor: at least an increase of 8-10 grams withstands the donut test. This aligns almost perfectly with the result of the above expected date of birth test and provides reassurance that the observed treatment effects are not an artefact of measurement error.

4.4.2 Placebo cut-off tests

Next, I conduct placebo RD tests for the treatment date (6 April) in four different years, both before and after the introduction of the policy.[24] Birthweights can vary by season, and there is a natural incline in April. Additionally, 6 April represents the start of the financial year in the UK. It is important to check whether the observed infant health effects are simply the reflection of discontinuous variation around this date. Full placebo cut-off results for birthweight are given in Table 5. There are no significant birthweight effects, in either direction, at any of the placebo cut-offs. Figure 6 illustrates this by plotting mean birthweight around each placebo cut-off: no discontinuities are visible. Similarly, Figure S12 verifies there are no significant placebo effects when expected date of birth is used as the running variable in the hospital data.

There are, additionally, no negative placebo effects for low or extremely low birthweight, as il-

[24] I do not use 2011 as a placebo cut-off because the Health in Pregnancy Grant was abolished on 16 April 2011 (alongside other changes to financial support affecting families with children and pregnant women), so 2011 is not plausibly a 'placebo'.

lustrated by Figures S13 and S14 respectively.[25] Placebo cut-off results for prematurity with the hospital data are listed in Table S12. These are less clean than the birthweight placebos, but they are mostly null or positive, and where they are negative they are very sensitive to specification. This suggests that the robust observed negative treatment effect on prematurity is likely to be attributable to the policy.

5 Discussion

This paper exploits a unique policy experiment from England and Wales to show that paying universal child benefits from pregnancy can lead to small but significant improvements in infant health. The Health in Pregnancy Grant led to significant increases in birthweight and reductions in prematurity. These effects are robust across parametric and non-parametric approches, different orders of the polynomial and different bandwidths. This contrasts sharply with regression discontinuity results for placebo cut-offs and baseline covariates, which are almost exclusively null. Both institutional context and a formal McCrary test suggest that manipulation of the running variable was unlikely.

After adjusting for minor measurement error in the running variable, my findings from Section 4.4.1 indicate that I can be confident of a 8-10 gram intention to treat effect from the 190 GBP cash transfer. This represents 1-2 percent of a standard deviation: 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 10 grams for a high-impact sample of single low-educated mothers (Hoynes, Miller & Simon 2015). The highest-impact sample in my study – younger mothers aged 24 or under – benefited from a 14 gram increase in mean birthweight, by comparison. Younger mothers also saw a remarkable and significant 26 percent reduction in extremely low birthweight. The scale of the effects for this group may go some way to explaining the impressive effects of the HPG relative

[25] Low birthweight displays a significant positive effect in 2010, but if anything this lends further support to the assumption that 6 April does not led to spurious negative effects on low birthweight.

to other policies: while most cash transfers target women with existing children (through child benefits or tax credits), the HPG was available to all women, including first-time mothers.

The birthweight effects of the policy appear to be driven by the bottom of the birthweight distribution, which is consistent with diminishing marginal returns to birthweight and income. This corroborates 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 (González & Trommlerová 2022, Hoynes, Miller & Simon 2015, Almond, Hoynes & Schanzenbach 2011). In other words, although the grant was universal, its effects were targeted on those babies most in need of increases in birthweight. The HPG also appears to have increased gestational length, concentrated on babies at risk of prematurity.

A further contribution of this paper is to offer evidence on possible causal mechanisms, notably the conditionality and the cash itself. The policy implications of these two mechanisms are very different. At one extreme, if the infant health effects were wholly explained by an increase in antenatal attendance due to the conditionality of the grant, then it could be cheaper and more efficient to mandate antenatal attendance or encourage it through other means. At the other extreme, if there were no changes in antenatal attendance then it would suggest that only the cash matters. Unconditional cash transfers could then be just as effective, and administratively simpler to implement.

While I find some minor significant effects on earlier antenatal health engagement, these are very similar to placebo effects in other years. Even taking the highest estimates at face value, a back-of-the-envelope calculation indicates that they are too small to explain the magnitude of the birthweight effect. In a similar policy setup involving a cash incentive for antenatal care, Cygan-Rehm & Karbownik (2022) find that a 10 percentage point increase in the antenatal care during the first trimester increases birthweight by 0.14 percent. At best, my results indicate a 3.3 percentage-point increase in antenatal care usage during the first trimester (see Table S8). Applying Cygan-Rehm & Karbownik (2022)'s effect sizes, my observed increase in antenatal usage would imply an increase in birthweight of 0.046 percent. Instead, I observe an increase in birthweight of 0.32 percent. It seems reasonable to conclude that the primary mechanism for the infant health effects was the cash itself, rather than the conditionality.

How could the cash itself have had such an effect? There are three main possibilities: subsidising better nutrition; reducing the likelihood of smoking; and reducing prenatal stress. As explained in Section 2.2, the literature generally suggests that improved nutrition is instrumental to the speed of intrauterine growth but not to the length of gestation. In Section 4.1.2, I find that birthweight effects are driven by longer gestation, rather than faster intrauterine growth. It is therefore unlikely that the effects are explained by improved nutrition during pregnancy. In Figure S15, I examine published government data on the proportion of women who report to be smoking at the time of delivery by quarter of birth. If the HPG had reduced smoking, one would expect to see a sharper decline in population-level smoking rates among pregnant women during the policy's implementation (between 2009 Q2 and 2011 Q1 inclusive) because the policy was universal. Figure S15 shows no evidence of such a decline. This makes it extremely unlikely that the infant health effects are due to reductions in smoking.

By a process of elimination, the final mechanism is stress. My analysis of this as a mechanism remains speculative, since there is no direct data to verify stress levels. However, the stress hypothesis is consistent with my results in two respects.

First, research suggests that stress predominantly affects birthweight via gestational length and prematurity, rather than intrauterine growth (Torche 2011). In Section 4.1.2, I show that the birthweight effects of the HPG are explained by gestational length and not by intrauterine growth. I also document a reduction in prematurity of 0.6 percentage points (8 percent in relative terms). Maternal stress during the third trimester has a particular effect on prematurity because it leads to the release of maternal glucocorticoids, which stimulate excess production of the corticotrophin releasing hormone (CRH) and in turn cause early maturation of fetal organs and early delivery (Foureaux Koppensteiner & Manacorda 2016, Torche 2011). Since the HPG was paid during the third trimester, it is possible that it mitigated against stress-induced prematurity of this kind.

Second, if stress is the main mechanism then one would expect the effects of the grant to be larger for groups at greater risk of prenatal stress. This is precisely what I observe: babies born to mothers in deprived areas, younger (24 and under) and older (34 and over) mothers were the biggest winners from the HPG. This is unlikely to be due to differential take-up rates of the grant: take-up overall was extremely high, at 93 percent, and existing evidence suggests that there were no significant differences in antenatal incentivisation (Leyland et al. 2017).[26] It is possible that the heterogeneity reflects the elevated risk of lower birthweight among these groups, and the fact that the grant benefited smaller babies more. But it is also plausible that the heterogeneity reflects differences in prenatal stress. Poverty and financial instability are associated with "an accumulation of multiple chronic stressors" and higher cortisol levels, which are likely to affect women living in deprived areas and younger women (Kramer, Séguin, Lydon & Goulet 2000, Cohen, Doyle & Baum 2006). The arrival of a baby typically involves large fixed costs during the third trimester – buying a pushchair, a cot or other baby equipment – which may not be met easily by such groups. Additionally, evidence shows that older mothers face a higher risk of prenatal stress, anxiety and depression (García-Blanco, Monferrer, Grimaldos, Hervás, Balanzá-Martínez, Diago, Vento & Cháfer-Pericás 2017). Advanced maternal age involves higher risks of adverse health outcomes during conception, pregnancy and birth, including reduced fertility, miscarriage, stillbirth and chromosomal abnormalities (García-Blanco et al. 2017). Older women are also more likely to have additional caring responsibilities and greater professional commitments, which increase stress during pregnancy (García-Blanco et al. 2017).

Unlike some other windfall income increases, the HPG was clearly labelled as a lump sum for mothers to use to improve health and wellbeing 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 (Kaushal, Gao & Waldfogel 2007). The same labelling effect may have applied with the HPG and stress reduction during pregnancy. Some users on the public online forum *Mumsnet* reported using the HPG to reduce stress, help with financial difficulties or to promote wellbeing. One woman

[26]Leyland et al. (2017) find that deprived mothers attended antenatal care 0.5 weeks earlier and non-deprived mothers attended 0.2 weeks earlier, these differences are not statistically significant. They also find no differences by single parent status.

reported using the 190 GBP to buy a pushchair; another said it would reduce her overdraft; another reported investing in a 10-week course of aqua-natal swimming and yoga classes (Mumsnet 2009). These examples are, of course, anecdotal and subject to selection bias. Nonetheless, they give a contemporary insight into the spending decisions of self-identified recipients of the HPG. In the absence of direct data which quantifies stress levels, this indicates that it is plausible that the HPG was spent on activities which have been shown to reduce stress. Antenatal yoga, for instance, has been found to reduce stress and depression significantly (Newham, Wittkowski, Hurley, Aplin & Westwood 2014). It is also plausible that receiving a lump sum of 190 GBP could reduce stress by having a positive financial effect on low-income women anxious about meeting the costs of having a baby. A final possibility is that the HPG could have reduced stress by allowing low-income women to take earlier unpaid maternity leave prior to the birth of the baby, thereby enabling them to get more rest during the period when babies gain weight very rapidly.[27] Without direct data on stress, these explanations remain speculative. However, by eliminating the two other possible mechanisms (nutrition and smoking), examining the consistency of my results with stress, and using contemporary, anecdotal evidence to show that it is possible that some recipients spent the HPG on stress-reducing activities, it is reasonable to conclude that stress reduction is the most likely causal mechanism.

6 Conclusion

Universal child benefits are a popular policy tool for tackling child poverty and improving child health. The importance of infant health to child health, adult health and socio-economic outcomes is clear. Yet no research has considered what the potential infant health gains are from bringing universal child benefits into pregnancy. This paper fills that gap by being the first to consider the infant health effects of starting universal child benefits in pregnancy.

[27] 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, thereby reducing stress. Unfortunately, there is no available data on maternity leave to test this hypothesis, so it remains speculative.

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 administrative birth registrations and hospital data, I take advantage of an arbitrary eligibility rule for the Health in Pregnancy Grant based on expected date of birth. I implement a regression discontinuity design in the date of birth of the baby, which enables me to overcome selection bias and to identify the causal effects of the policy.

I find that starting universal child benefits in pregnancy leads to small but statistically significant increases in mean birthweight at population level. Smaller babies, mothers living in deprived areas, younger mothers (aged 24 and under) and older mothers (aged 34 and over) benefit the most. These effects do not appear to be explained by increases in antenatal care. Instead the cash itself seems to have been important. Suggestive evidence indicates that nutrition and smoking are unlikely channels: stress reduction, mediated through longer gestation, is a more plausible candidate.

These results have striking policy implications. First, they demonstrate that there are significant infant health gains to be made from starting universal child benefits in pregnancy, including for younger mothers who are less likely to benefit from other child-related benefits. This is particularly relevant given recent calls to extend the US Child Tax Credit to pregnant mothers (Romney Campaign 2022). Second, since the conditionality appears to have played at best a minor role in the effects of the grant, it seems reasonable to conclude that infant health effects could be found for unconditional cash transfers. Third, my finding that stress is a plausible causal mechanism underscores recent research on the effects of maternal stress on infant health (Currie, Mueller-Smith & Rossin-Slater 2022, Persson & Rossin-Slater 2018), and suggests that wider policies to reduce maternal stress could have significant benefits to infant health.

Many developed countries pay universal 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 this financial support in utero. This paper demonstrates that doing so may provide hitherto unrealised infant health benefits.

References

Almond, D., Hoynes, H. W. & Schanzenbach, D. W. (2011), 'Inside the war on poverty: The impact of food stamps on birth outcomes', *The Review of Economics and Statistics* **93**(2), 387–403.

Amarante, V., Manacorda, M., Miguel, E. & Vigorito, A. (2016), 'Do Cash Transfers Improve Birth Outcomes? Evidence from Matched Vital Statistics, and Program and Social Security Data', *American Economic Journal. Economic Policy* **8**(2), 1–43.

Averett, S. & Wang, Y. (2013), 'The effects of earned income tax credit payment expansion on maternal smoking', *Health Economics* **22**(11), 1344–1359.

Barreca, A. I., Guldi, M., Lindo, J. M. & Waddell, G. R. (2011), 'Saving babies? Revisiting the effect of very low birth weight classification', *The Quarterly Journal of Economics* **126**(4), 2117–2123.

BBC News (2007), 'Eat well cash for mothers-to-be', http://news.bbc.co.uk/1/hi/health/6984122.stm. Last accessed: 05/05/21.

Behrman, J. R. & Rosenzweig, M. R. (2004), 'Returns to birthweight', *The Review of Economics and Statistics* **86**(2), 586–601.

Benzeval, M., Bond, L., Campbell, M., Egan, M., Lorenc, T., Petticrew, M. & Popham, F. (2014), 'How does money influence health? Technical Report', https://www.jrf.org.uk/report/how-does-money-influence-health. Last accessed: 05/05/21.

Black, S. E., Devereux, P. J. & Salvanes, K. G. (2007), 'From the Cradle to the Labor Market? The Effect of Birth Weight on Adult Outcomes', *The Quarterly Journal of Economics* **122**(1), 409–439.

Boyd-Swan, C., Herbst, C. M., Ifcher, J. & Zarghamee, H. (2016), 'The earned income tax credit, mental health, and happiness', *Journal of Economic Behavior and Organization* **126**, 18–38.

Brooks-Gunn, J. & Duncan, G. J. (1997), 'The effects of poverty on children', *The Future of Children* **7**(2), 55–71.

Burchardt, T. & Obolenskaya, P. (2016), *Public and private welfare*, Social policy in a cold climate: Policies and their consequences since the crisis, 1st edn, Policy Press, New York.

Campbell, T. (2014), 'Stratified at seven: in-class ability grouping and the relative age effect', *British Educational Research Journal* **40**(5), 749–771.

Cattaneo, M., Idrobo, N. & Titiunik, R. (2019), *A practical introduction to regression discontinuity designs: Foundations*, Cambridge University Press, Cambridge.

Chung, W., Ha, H. & Kim, B. (2016), 'Money transfer and birth weight: Evidence from the Alaska permanent fund dividend', *Economic Inquiry* **54**(1), 576–590.

Coathup, V., Macfarlane, A. & Quigley, M. (2020), 'Linkage of maternity hospital episode statistics birth records to birth registration and notification records for births in England 2005–2006: Quality assurance of linkage', *BMJ Open* **10**(10).

Cohen, S., Doyle, W. & Baum, A. (2006), 'Socioeconomic status is associated with stress hormones.', *Psychosomatic medicine* **68**(3), 414.

Cowan, B. & Tefft, N. (2012), 'Education, Maternal Smoking, and the Earned Income Tax Credit', *The B.E. Journal of Economic Analysis and Policy* **12**(1).

Crawford, C., Dearden, L. & Greaves, E. (2016), 'When you are born matters: Evidence for England', *Institute for Fiscal Studies* . Last accessed: 08/07/21.

Currie, J. & Moretti, E. (2003), 'Mother's education and the intergenerational transmission of human capital: Evidence from college openings', *The Quarterly Journal of Economics* **118**(4), 1495–1532.

Currie, J., Mueller-Smith, M. & Rossin-Slater, M. (2022), 'Violence While in Utero: The Impact of Assaults during Pregnancy on Birth Outcomes', *The Review of Economics and Statistics* **104**(3), 525–540.

Currie, J. & Rossin-Slater, M. (2015), 'Early-life origins of life-cycle well-being: Research and policy implications: Early-life origins of life-cycle well-being', *Journal of Policy Analysis and Management* **34**(1), 208–242.

Cygan-Rehm, K. & Karbownik, K. (2022), 'The effects of incentivizing early prenatal care on infant health', *Journal of Health Economics* **83**, 102612–102612.

De Cao, E., McCormick, B. & Nicodemo, C. (2022), 'Does unemployment worsen babies' health? A tale of siblings, maternal behaviour, and selection', *Journal of Health Economics* **83**, 102601–102601.

Directgov (2010), 'Information for mums-to-be: Health in Pregnancy Grant', https://bit.ly/3bk8duq. Last accessed: 05/05/21.

East, C. N. (2020), 'The effect of food stamps on children's health: Evidence from immigrants' changing eligibility', *Journal of Human Resources* **55**(2), 387–427.

Evans, W. N. & Lien, D. S. (2005), 'The benefits of prenatal care: Evidence from the PAT bus strike', *Journal of Econometrics* **125**(1), 207–239.

Foureaux Koppensteiner, M. & Manacorda, M. (2016), 'Violence and birth outcomes: Evidence from homicides in Brazil', *Journal of Development Economics* **119**, 16–33.

García-Blanco, A., Monferrer, A., Grimaldos, J., Hervás, D., Balanzá-Martínez, V., Diago, V., Vento, M. & Cháfer-Pericás, C. (2017), 'A preliminary study to assess the impact of maternal age on stress-related variables in healthy nulliparous women', *Psychoneuroendocrinology* **78**, 97–104.

Ghosh, R. E., Ashworth, D. C., Hansell, A. L., Garwood, K., Elliott, P. & Toledano, M. B. (2016), 'Routinely collected English birth data sets: Comparisons and recommendations for reproductive epidemiology', *Archives of Disease in Childhood - Fetal and Neonatal Edition* **101**(5), F451–F457.

Goisis, A., Özcan, B. & Myrskylä, M. (2017), 'Decline in the negative association between low birth weight and cognitive ability', *Proceedings of the National Academy of Sciences - PNAS* **114**(1), 84–88.

González, L. & Trommlerová, S. (2022), 'Cash transfers before pregnancy and infant health', *Journal of Health Economics* **83**, 102622–102622.

Gregg, P., Waldfogel, J. & Washbrook, E. (2006), 'Family expenditures post-welfare reform in the uk: Are low-income families starting to catch up?', *Labour Economics* **13**(6), 721–746.

Hahn, J., Todd, P. & Van der Klaauw, W. (2001), 'Identification and estimation of treatment effects with a regression-discontinuity design', *Econometrica* **69**(1), 201–209.

Her Majesty's Revenues and Customs (2010), 'HMRC Departmental Accounts 2009-10', https://bit.ly/3u0Y5NB. Last accessed: 05/05/21.

Herbert, A., Wijlaars, L., Zylbersztejn, A., Cromwell, D. & Hardelid, P. (2017), 'Data Resource Profile: Hospital Episode Statistics Admitted Patient Care (HES APC)', *International Journal of Epidemiology* **46**(4), 1093–1093i.

Hoynes, H., Miller, D. & Simon, D. (2015), 'Income, the earned income tax credit, and infant health', *American Economic Journal. Economic Policy* **7**(1), 172–211.

Hyytinen, A., Meriläinen, J., Saarimaa, T., Toivanen, O. & Tukiainen, J. (2018), 'When does regression discontinuity design work? Evidence from random election outcomes', *Quantitative Economics* **9**(2), 1019–1051.

Kaushal, N., Gao, Q. & Waldfogel, J. (2007), 'Welfare reform and family expenditures: How are single mothers adapting to the new welfare and work regime?', *The Social Service Review* **81**(3), 369–396.

Kelly, E., Lee, T., Sibieta, L. & Waters, T. (2020), 'Public Spending on Children in England: 2000 to 2020', https://bit.ly/33AGYYs. Last accessed: 05/05/21.

Koivu, A., Phan, Y., Abuhamed, J., Perry, B., Atkins, S., Perkiö, M. & Koivusalo, M. (2020), 'The baby box: Enhancing the wellbeing of babies and mothers around the world', https://bit.ly/3Pj45Mr. Last accessed: 04/08/22.

Kramer, M., Séguin, L., Lydon, J. & Goulet, L. (2000), 'Socio-economic disparities in pregnancy outcome: Why do the poor fare so poorly?', *Paediatric and Perinatal Epidemiology* **14**(3), 194–210.

Lee, D. S. & Card, D. (2008), 'Regression discontinuity inference with specification error', *Journal of Econometrics* **142**(2), 655–674.

Leyland, A. H., Ouédraogo, S., Nam, J., Bond, L., Briggs, A. H., Gray, R., Wood, R. & Dundas, R. (2017), 'Evaluation of Health in Pregnancy Grants in Scotland: A natural experiment using routine data', *Public Health Research* **5**(6), 1–278.

Lodge, V., Lomas, K., Jaworskyj, S. & Thomson, H. (2014), 'Obstetric audit: the Bradford way', *Ultrasound* **22**(3), 158–167.

Maruyama, S. & Heinesen, E. (2020), 'Another look at returns to birthweight', *Journal of Health Economics* **70**, 102269–102269.

Milligan, K. & Stabile, M. (2011), 'Do child tax benefits affect the well-being of children? Evidence from Canadian child benefit expansions', *American Economic Journal. Economic Policy* **3**(3), 175–205.

Mocan, N., Raschke, C. & Unel, B. (2015), 'The impact of mothers' earnings on health inputs and infant health', *Economics and Human Biology* **19**, 204–223.

Mumsnet (2009), '£190 grant for mums to be (news to me anyway)', https://www.mumsnet.com/Talk/childbirth/740256-pound-190-grant-for-mums-to-be-news-to-me. Last accessed: 05/05/21.

National Childcare Trust (2020), 'What is the 12-week scan for?', https://www.nct.org.uk/pregnancy/tests-scans-and-antenatal-checks/what-12-week-scan-for. Last accessed: 11/08/22.

National Records of Scotland (2021), ‘Births: Archive: Historical data back to 2001.’, https://www.nrscotland.gov.uk/statistics-and-data/statistics/statistics-by-theme/vital-events/general-publications/vital-events-reference-tables/archive. Last accessed: 05/05/21.

Newham, J. J., Wittkowski, A., Hurley, J., Aplin, J. D. & Westwood, M. (2014), ‘Effects of antenatal yoga on maternal anxiety and depression: A randomised controlled trial’, *Depression and Anxiety* **31**(8), 631–640.

Northern Ireland Statistics and Research Agency (2020), ‘Birth statistics’, https://www.nisra.gov.uk/publications/birth-statistics. Last accessed: 05/05/21.

OECD (2021), ‘Purchasing power parities’, https://bit.ly/3JbkH5Z. Last accessed: 03/02/22.

Office of National Statistics (2021), ‘Births in England and Wales: Summary Tables’, https://bit.ly/3paDmXT. Last accessed: 05/05/21.

Persson, P. & Rossin-Slater, M. (2018), ‘Family Ruptures, Stress, and the Mental Health of the Next Generation’, *The American Economic Review* **108**(4-5), 1214–1252.

Public Bills Committee (2008), ‘Savings Accounts and Health in Pregnancy Grant Bill, 3rd sitting, cc 85–480’, https://bit.ly/3eE6fY0. Last accessed: 05/05/21.

Redshaw, M. & Heikkila, K. (2010), ‘Delivered with care: A national survey of women’s experience of maternity care 2010’, https://www.npeu.ox.ac.uk/assets/downloads/reports/Maternity-Survey-Report-2010.pdf. Last accessed: 14/07/22.

Romney Campaign (2022), ‘Romney, colleagues fight for pregnant moms to receive tax relief’, https://www.romney.senate.gov/romney-colleagues-fight-for-pregnant-moms-to-receive-tax-relief/. Last accessed: 04/08/22.

Rossin, M. (2011), ‘The effects of maternity leave on children’s birth and infant health outcomes in the United States’, *Journal of Health Economics* **30**(2), 221–239.

Royal College of Radiologists (2014), ‘Standards for the provision of an ultrasound service’, https://bit.ly/3SJtRfM. Last accessed: 12/08/22.

Salomon, L., Alfirevic, Z., Bilardo, C., Chalouhi, G., Ghi, T., Kagan, K., Lau, T., Papageorghiou, A., Raine-Fenning, N., Stirnemann, J., Suresh, S., Tabor, A., Timor-Tritsch, I., Toi, A. & Yeo, G. (2013), ‘ISUOG Practice Guidelines: performance of first-trimester fetal ultrasound scan’, *Ultrasound in Obstetrics and Gynecology* **41**(1), 102–113.

Silverwood, R. J., Pierce, M., Hardy, R., Sattar, N., Whincup, P., Ferro, C., Savage, C., Kuh, D. & Nitsch, D. (2013), ‘Low birth weight, later renal function, and the roles of adulthood blood pressure, diabetes, and obesity in a British birth cohort’, *Kidney International* **84**(6), 1262–1270.

Strully, K. W., Rehkopf, D. H. & Xuan, Z. (2010), ‘Effects of prenatal poverty on infant health: State earned income tax credits and birth weight’, *American Sociological Review* **75**(4), 534–562.

Torche, F. (2011), ‘The effect of maternal stress on birth outcomes: Exploiting a natural experiment’, *Demography* **48**(4), 1473–1491.

UNICEF (2020), ‘Universal child benefits: Policy issues and options’, https://www.unicef.org/media/72916/file/UCB-ODI-UNICEF-Report-2020.pdf. Last accessed: 05/05/21.

Victora, C. G., Adair, L., Fall, C., Hallal, P. C., Martorell, R., Richter, L. & Singh Sachdev, H. (2008), ‘Maternal and child undernutrition: Consequences for adult health and human capital’, *The Lancet* **371**(9609), 340–357.

World Health Organization (2019), ‘Global spending on health: A world in transition’, https://bit.ly/2RaI8qQ. Last accessed: 05/05/21.

Wright, M. (2009), ‘New grant aimed at improving health of expectant mums’, https://www.theguardian.com/money/2009/feb/08/pregnancy-grant. Last accessed: 05/05/21.

Tables

Table 1: Summary statistics from birth registrations, 2008-2010

	N	Mean	SD	Prop. complete
Birthweight (grams)	1,403,178	3335.964	599.626	0.99
Low birthweight	1,403,178	0.071	0.257	0.99
Extremely low birthweight	1,403,178	0.012	0.107	0.99
Multiple birth	1,415,022	0.031	0.174	1.00
Female baby	1,415,022	0.488	0.500	1.00
Maternal age (years)	1,413,446	29.513	6.074	1.00
Teenage mother	1,413,446	0.061	0.240	1.00
Index of income deprivation	1,414,862	0.151	0.101	1.00
Born in a NHS hospital	1,415,022	0.967	0.180	1.00
Lower socio-economic status (SES)	130,018	0.284	0.451	0.09

Note: Data from administrative birth registrations for England and Wales. The sample is registered live births in England and Wales born between 6 April 2008 and 5 April 2010. Each column reports the number of observations, mean, standard deviation and the proportion of observations with a complete (non-missing) value respectively for each variable. Low and extremely low birthweight are dummy variables equal to one if the baby's recorded birthweight is less than 2500g and 1500g respectively, and zero if it is higher. Multiple birth is a dummy variable equal to one if than one child is born as a result of a pregnancy (e.g., twins, triplets), and zero otherwise. Maternal age is a continuous variable measured in years. Teenage pregnancies have a recorded maternal age of 19 or under. English index of income deprivation score is based on the area of mother's residence and captures the proportion of the population in the area who are in receipt of means-tested state benefits. Socio-economic status (SES) as defined by occupation is only coded for a random 10 percent sample of the data. Lower socio-economic status is a dummy variable equal to one if the highest status occupation of the mother and father is a routine, manual or other occupation, and zero if is a higher status occupation.

Table 2: Summary statistics from Hospital Episode Statistics, 2008-2010

	N	Mean	SD	Prop. complete
Birthweight (grams)	966,566	3330.964	607.100	0.75
Low birthweight	966,566	0.071	0.258	0.75
Extremely low birthweight	966,566	0.013	0.112	0.75
Gestational age at birth (weeks)	927,806	39.210	2.081	0.72
Premature birth	927,806	0.070	0.256	0.72
Postnatal stay (days)	1,283,722	2.224	6.940	1.00
Multiple birth	1,058,630	0.029	0.168	0.82
Gestational age at first antenatal assessment	820,529	13.974	8.274	0.64
First antenatal assessment during first trimester	820,529	0.700	0.458	0.64
First antenatal assessment during second trimester	820,529	0.199	0.399	0.64
First antenatal assessment during third trimester	820,529	0.090	0.287	0.64
First antenatal assessment before 25 weeks	820,529	0.889	0.314	0.64
Maternal age (years)	870,120	28.948	6.083	0.67
White British	1,106,130	0.678	0.467	0.86
Indian, Pakistani or Bangladeshi	1,106,130	0.089	0.284	0.86
Black, Asian or Minority Ethnic (BAME)	1,106,130	0.244	0.430	0.86

Note: Data from administrative Hospital Episode Statistics for England. The sample is live births in English NHS hospitals born within 52 weeks of 6 April 2009. Each column reports the number of observations, mean, standard deviation and the proportion of observations with a complete (non-missing) value respectively for each variable. Low and extremely low birthweight are dummy variables equal to one if the baby's recorded birthweight is less than 2500g and 1500g respectively, and zero if it is higher. Multiple birth is a dummy variable equal to one if than one child is born as a result of a pregnancy (e.g., twins, triplets), and zero if it is a singleton birth. Gestational age at birth is measured in weeks and is estimated from either an ultrasound dating scan or the date of the mother's last menstrual period. Premature birth is a dummy variable equal to one if the baby was born before 37 weeks of gestation and zero if they were born afterwards. Postnatal stay is the number of days between the birth of the baby and the end of the hospital episode. Gestational age at first antenatal assessment is measured in weeks. First antenatal assessment during the first/second/third trimester are a dummy variables equal to one if the first appointment took place from 0-13/14-26/27-40 weeks of gestation respectively, and zero if the appointment took place at another point during pregnancy. First antenatal assessment before 25 weeks is a dummy variable equal to one if the first appointment took place prior to the 25th week of pregnancy, and zero if it took place afterwards. Maternal age is a discrete variable measured in years. White British is a dummy variable equal to one if the baby is recorded with a 'British (White)' ethnicity, and zero if another ethnicity is recorded. Indian, Pakistani or Bangladeshi is a dummy variable equal to one if the baby is recorded with either 'Indian (Asian or Asian British)', 'Pakistani (Asian or Asian British)', or 'Bangladeshi (Asian or Asian British)' ethnicity, and zero if another ethnicity is recorded. Black, Asian or Minority Ethnic (BAME) is a dummy variable equal to one if the baby is recorded with either one of the following ethnicities, and zero if another ethnicity is recorded: 'White and Black Caribbean (Mixed)', 'White and Black African (Mixed)', 'White and Asian (Mixed)', 'Any other mixed background', 'Indian (Asian or Asian British)', 'Pakistani (Asian or Asian British)', 'Bangladeshi (Asian or Asian British)', 'Any other black background', 'Caribbean (Black or Black British)', 'African (Black or Black British)', 'Any other black background', 'Chinese (other ethnic group)', or 'Any other ethnic group'.

Table 3: Effect of the Health in Pregnancy Grant on birthweight

	Regression discontinuity					Control mean
	(1)	(2)	(3)	(4)	(5)	(6)
Birthweight (grams)	10.465**	11.528*	10.255**	8.699**	10.166**	3318.719
	(4.967)	(6.634)	(4.702)	(3.590)	(4.699)	
Low birthweight	-0.003	-0.005*	-0.002	-0.001	-0.002	0.074
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	
Extremely low birthweight	-0.001	-0.002*	-0.001	-0.000	-0.001	0.012
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
N	421,992	421,992	421,992	421,992	421,992	208,098
Linear trend			X	X		
Quadratic trend					X	
Local linear polynomial	X					
Local quadratic polynomial		X				
Day of the week FE				X		
Controls				X		

Note: The table reports the coefficient β_1 from Equation 2 for different outcome variables. Column 1 is a non-parametric RD with a local linear polynomial fitted either side of the cut-off, with a triangular kernel and conventional standard errors. Column 2 is a non-parametric RD with a local quadratic polynomial fitted either side of the cut-off, with a triangular kernel and conventional standard errors. Column 3 is a parametric RD with a linear trend. Column 4 is a parametric RD with a linear trend, day-of-the-week fixed effects and a vector of controls (maternal age (years), multiple birth status, income deprivation score, and the sex of the baby). Column 5 is a parametric RD with a quadratic trend. All estimates are based on a 16-week bandwidth. Standard errors are clustered at the running variable (actual date of birth) and are listed in parentheses. ***p<0.01, ** p<0.05, * p<0.1. Data from administrative birth registrations for England and Wales. The sample is registered live births in England and Wales born within 16 weeks of 6 April 2009.

Table 4: Effect of the Health in Pregnancy Grant on gestational length, prematurity and antenatal engagement

	Regression discontinuity					Control mean	Observations
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gestational age at birth (weeks)	0.072***	0.097***	0.065***	0.064***	0.069***	39.166	270,966
	(0.018)	(0.018)	(0.015)	(0.017)	(0.018)		
Premature birth	-0.006***	-0.008***	-0.005**	-0.005**	-0.006***	0.075	270,966
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)		
Gestational age at first antenatal assessment	-0.091	0.122**	-0.298**	-0.223**	-0.228**	13.944	237,761
	(0.057)	(0.052)	(0.115)	(0.093)	(0.096)		
Linear trend			X		X		
Quadratic trend				X			
Local linear polynomial	X						
Local quadratic polynomial		X					
Controls					X		

Note: The table reports the coefficient β_1 from Equation 2 for different outcome variables, with actual week of birth as the running variable. Dependent variables are defined as in Table 2. Column 1 is a non-parametric RD with a local linear polynomial fitted either side of the cut-off, with a triangular kernel and conventional standard errors. Column 2 is a non-parametric RD with a local quadratic polynomial fitted either side of the cut-off, with a triangular kernel and conventional standard errors. Column 3 is a parametric RD with a linear trend. Column 4 is a parametric RD with a quadratic trend. Column 5 is a parametric RD with a linear trend and a vector of controls (maternal age (years), multiple birth status, and whether the baby's recorded ethnicity is White British). All estimates are based on a 16-week bandwidth. Standard errors are clustered at the running variable (actual week of birth) and are listed in parentheses. ***p<0.01, ** p<0.05, * p<0.1. Data from administrative Hospital Episode Statistics for England. The sample is live births in English NHS hospitals born within 16 weeks of 6 April 2009.

Table 5: Placebo cut-off tests

	Regression discontinuity					Control mean
	(1)	(2)	(3)	(4)	(5)	(6)
April 2007	0.978	-7.309	3.588	0.007	0.007	3320.543
	(4.877)	(6.760)	(4.589)	(0.000)	(0.000)	
N	406,218	406,218	406,218	406,218	406,218	201,201
April 2008	-1.849	2.393	-2.479	0.007	0.007	3329.303
	(4.476)	(6.095)	(4.362)	(0.000)	(0.000)	
N	427,667	427,667	427,667	427,667	427,667	211,832
April 2010	-3.892	-3.920	-1.938	0.007	0.007	3333.373
	(5.577)	(8.379)	(4.793)	(0.000)	(0.000)	
N	429,320	429,320	429,320	429,320	429,320	214,596
April 2012	4.575	5.778	2.914	0.007	0.007	3336.081
	(3.995)	(5.297)	(4.079)	(0.000)	(0.000)	
N	437,756	437,756	437,756	437,756	437,756	218,799
Linear trend			X	X		
Quadratic trend					X	
Local linear polynomial	X					
Local quadratic polynomial		X				
Day of the week FE				X		
Controls				X		

Note: The table reports the coefficient β_1 from Equation 2 for a series of placebo cut-offs when the HPG was not introduced: 6 April 2007, 6 April 2008, 6 April 2010 and 6 April 2012. Birthweight (grams) is the dependent variable. Column 1 is a non-parametric RD with a local linear polynomial fitted either side of the cut-off, with a triangular kernel and conventional standard errors. Column 2 is a non-parametric RD with a local quadratic polynomial fitted either side of the cut-off, with a triangular kernel and conventional standard errors. Column 3 is a parametric RD with a linear trend. Column 4 is a parametric RD with a linear trend, day-of-the-week fixed effects and a vector of controls (maternal age (years), multiple birth status, income deprivation score, and the sex of the baby). Column 5 is a parametric RD with a quadratic trend. All estimates are based on a 16-week bandwidth. Standard errors are clustered at the running variable (actual date of birth) and are listed in parentheses. ***$p<0.01$, ** $p<0.05$, * $p<0.1$. Data from administrative birth registrations for England and Wales. The sample is registered live births in England and Wales born within 16 weeks of 6 April in each year.

Figures

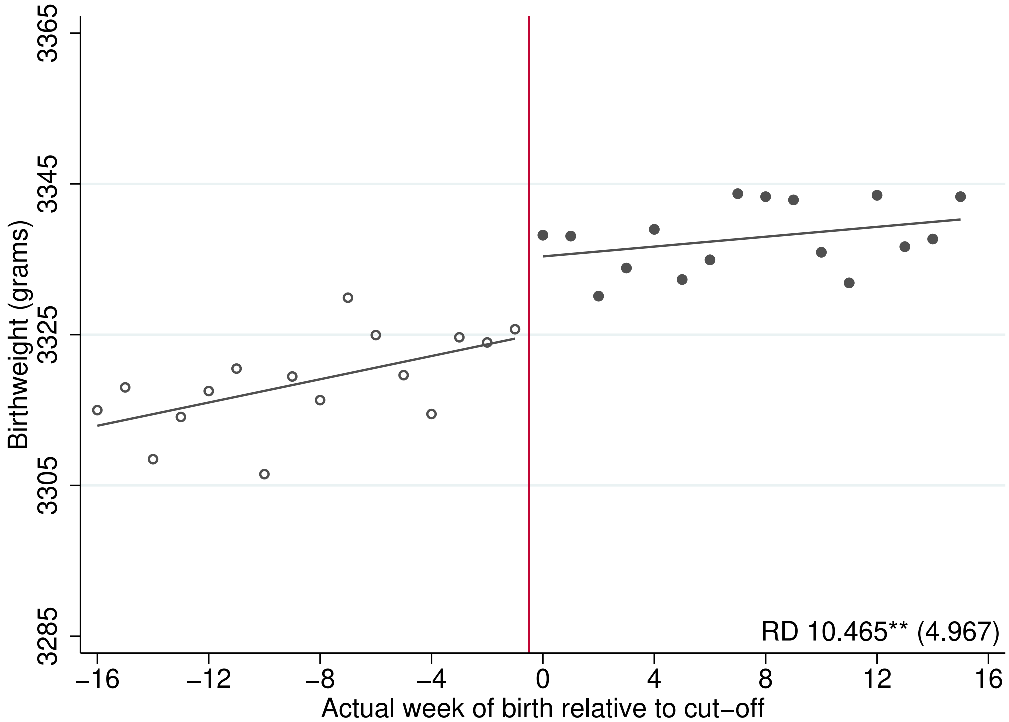

Figure 1: Effect of the Health in Pregnancy Grant on mean birthweight

Note: The figure plots mean birthweight by actual week of birth for a 16-week bandwidth, with a local linear polynomial fitted either side of the cut-off. The red vertical line indicates the introduction of the Health in Pregnancy Grant on 6 April 2009. The figure also reports the coefficient β_1 from Equation 2 using a non-parametric local linear regression. Standard errors are clustered at the running variable (actual date of birth) and are listed in parentheses. ***p<0.01, ** p<0.05, * p<0.1. Data from administrative birth registrations for England and Wales. The sample is registered live births in England and Wales born within 16 weeks of 6 April 2009.

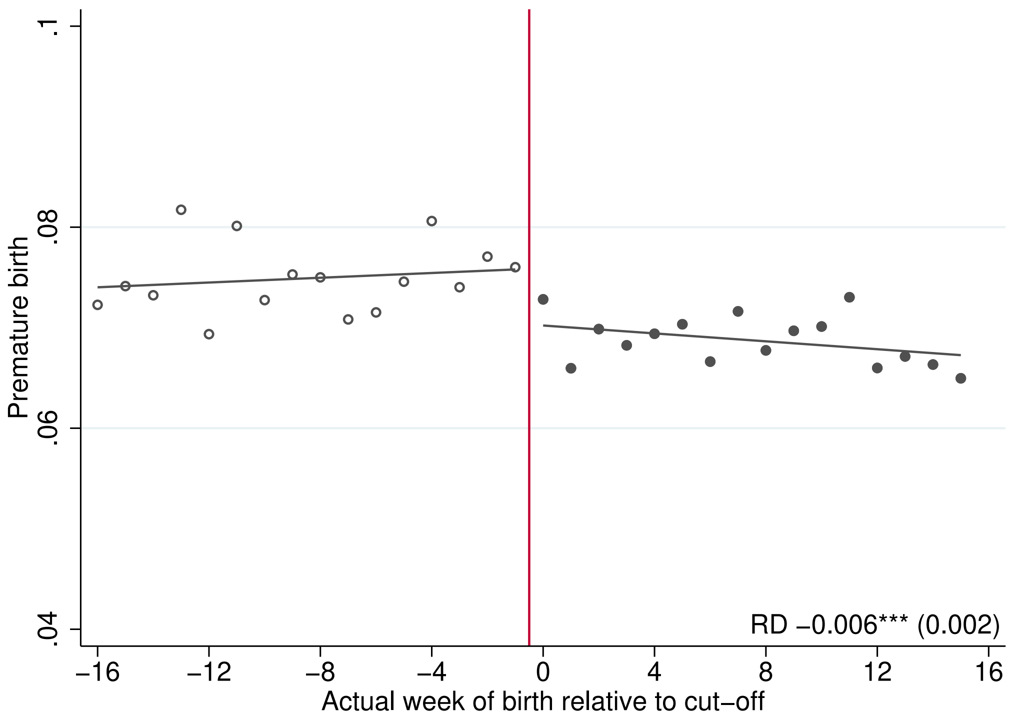

Figure 2: Effect of the Health in Pregnancy Grant on the probability of being born premature (<37 weeks)

Note: The figure plots the proportion of babies who are born before 37 weeks of gestation by actual week of birth for a 16-week bandwidth, with a local linear polynomial fitted either side of the cut-off. The red vertical line indicates the introduction of the Health in Pregnancy Grant on 6 April 2009. The figure also reports the coefficient β_1 from Equation 2 using a non-parametric local linear regression. Standard errors are clustered at the running variable (actual week of birth) and are listed in parentheses. ***p<0.01, ** p<0.05, * p<0.1. Data from administrative Hospital Episode Statistics for England. The sample is live births in English NHS hospitals born within 16 weeks of 6 April 2009.

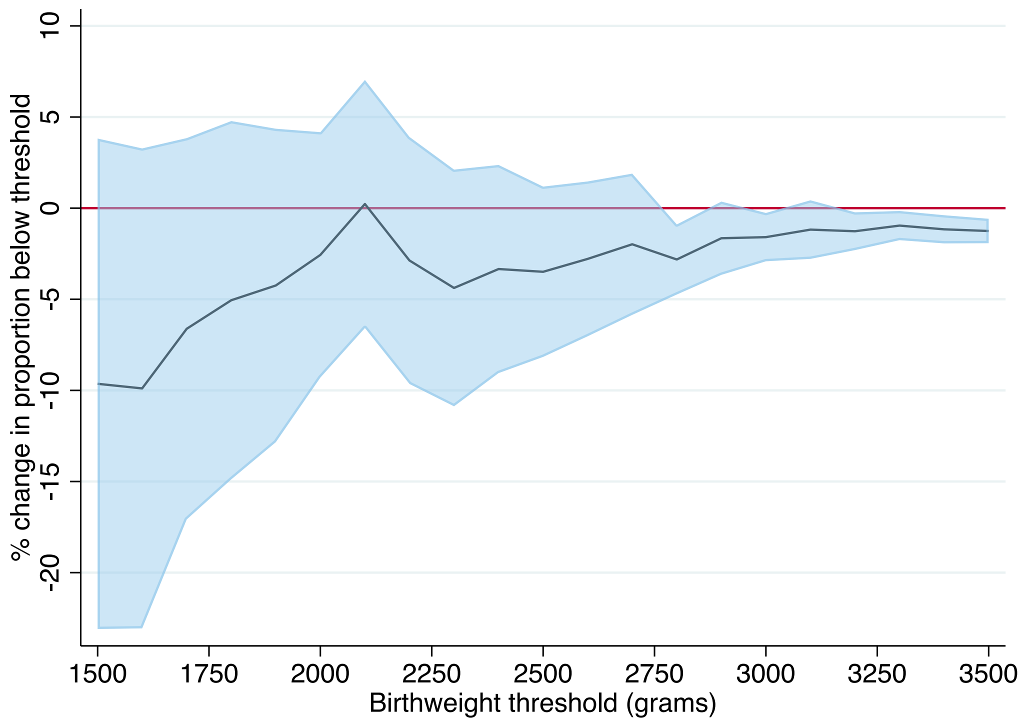

Figure 3: Relative birthweight effects across the birthweight distribution

Note: Following González & Trommlerová (2022), the figure plots the coefficient β_1 from Equation 2 with 21 separate RD regressions as a percentage of the control mean, where the dependent variable is a dummy variable equal to one if the baby weighs less than a certain threshold from 3500g to 1500g, in increments of 100g. A non-parametric local linear regression for a 16-week bandwidth is used. Blue shaded areas represent 95 percent confidence intervals. Standard errors are clustered at the running variable (actual date of birth). Data from administrative birth registrations for England and Wales. The sample is registered live births in England and Wales born within 16 weeks of 6 April 2009.

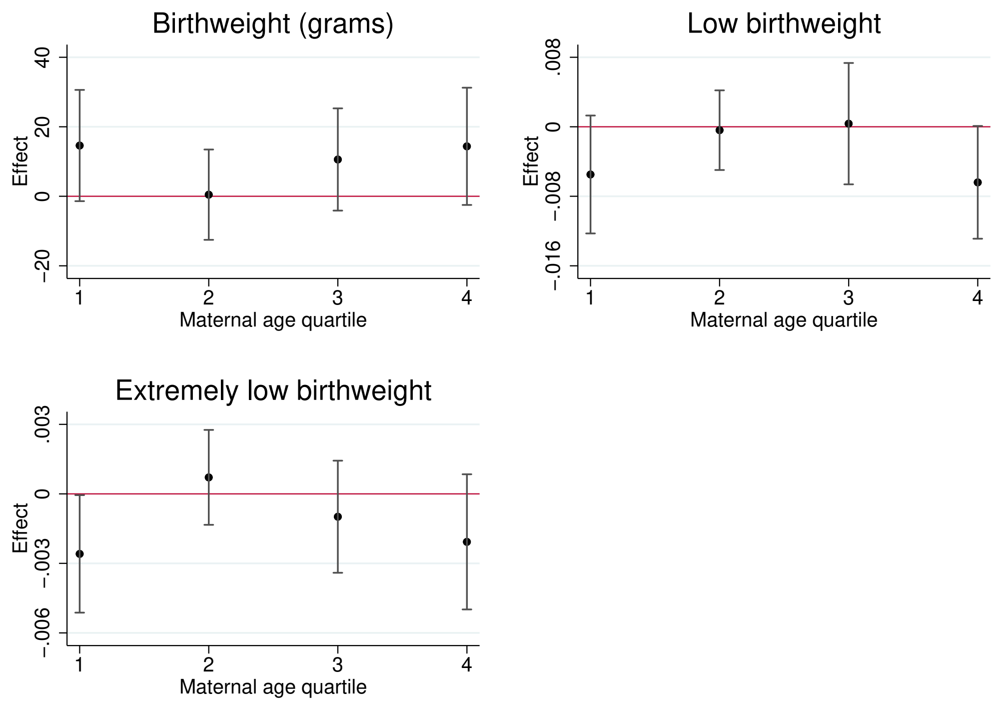

Figure 4: Birthweight effects by maternal age quartile

Note: Each panel plots the coefficient β_1 from Equation 2, estimated separately for each maternal age quartile. The dependent variables are birthweight (grams), low birthweight and extremely low birthweight respectively. A non-parametric local linear regression for a 16-week bandwidth is used. The ranges of the maternal age quartiles are as follows: 24 and under (1st quartile); 24-29 years; 29-34 years; and 34 and over (4th quartile). Error bars represent 90 percent confidence intervals. Standard errors are clustered at the running variable (actual date of birth). Data from administrative birth registrations for England and Wales. The sample is registered live births in England and Wales born within 16 weeks of 6 April 2009.

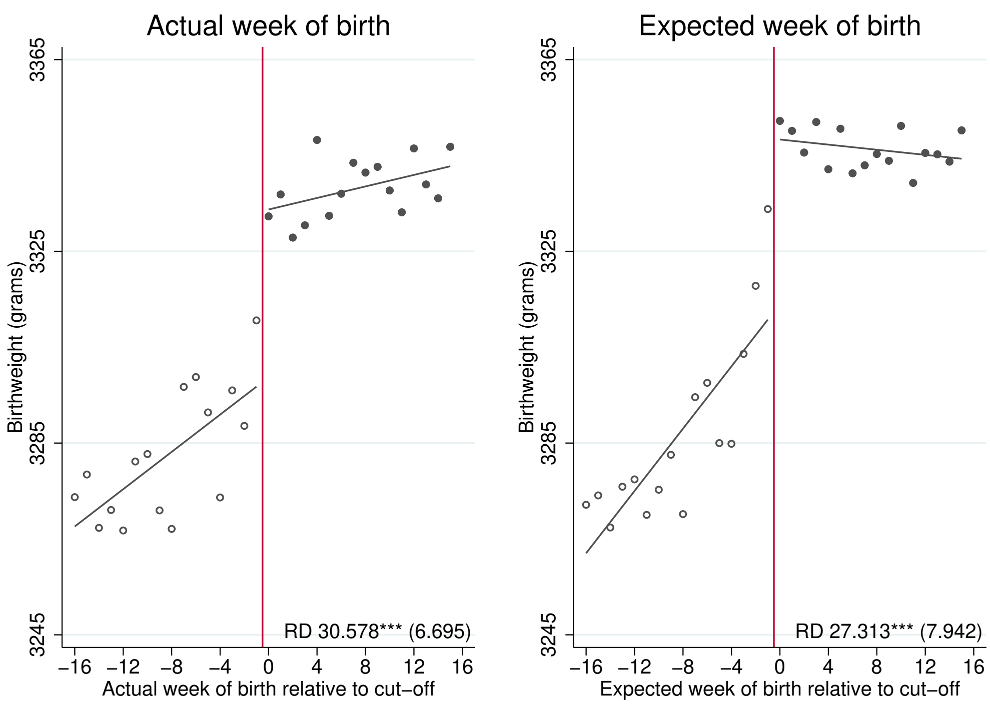

Figure 5: Results with actual vs. expected week of birth as the running variable

Note: The figure compares results with actual and expected week of birth as the running variable. In the left panel, the running variable is actual week of birth; in the right panel, the running variable is expected week of birth. Each panel plots mean birthweight by the relevant running variable for a 16-week bandwidth, with a local linear polynomial fitted either side of the cut-off. The red vertical line indicates the introduction of the Health in Pregnancy Grant on 6 April 2009. Each panel also reports the coefficient β_1 from Equation 2 using a non-parametric local linear regression. Standard errors are clustered at the running variable and are listed in parentheses. ***p<0.01, ** p<0.05, * p<0.1. Data from administrative Hospital Episode Statistics for England. The sample is live births in English NHS hospitals with an actual/expected week of birth within 16 weeks of 6 April 2009 respectively.

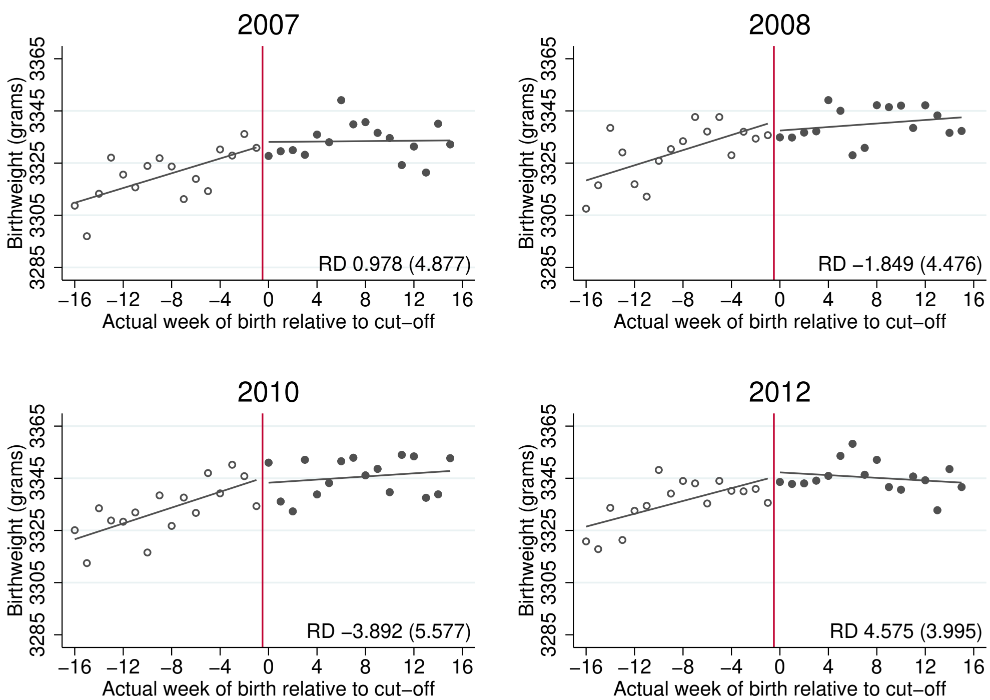

Figure 6: Placebo cut-off tests

Note: The figure shows results from placebo cut-off tests on 6 April in four years when the policy was not introduced: 2007, 2008, 2010 and 2012. Each panel plots mean birthweight by actual week of birth for a 16-week bandwidth, with a local linear polynomial fitted either side of the placebo cut-off. The red vertical line indicates the placebo cut-off on 6 April for each year. The figure also reports the coefficient β_1 from Equation 2 using a non-parametric local linear regression. Standard errors are clustered at the running variable (actual date of birth) and are listed in parentheses. ***p<0.01, ** p<0.05, * p<0.1. Data from administrative birth registrations for England and Wales. The sample is registered live births in England and Wales born within 16 weeks of 6 April for each year.