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DISCUSSION PAPER SERIES

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ABSTRACT

Does Unemployment Worsen Babies' Health? A Tale of Siblings, Maternal Behaviour and Selection*

We study the effect of unemployment on birth outcomes by exploiting geographical variation in the unemployment rate across local areas in England, and comparing siblings born to the same mother via family fixed effects. Using rich individual data from hospital administrative records between 2003 and 2012, babies' health is found to be strongly pro-cyclical. A one-percentage point increase in the unemployment rate leads to an increase in low birth weight and preterm babies of respectively 1.3 and 1.4%, and a 0.1% decrease in foetal growth. We find heterogenous responses: unemployment has an effect on babies' health which varies from strongly adverse in the most deprived areas, to mildly favourable in the most prosperous areas. We provide evidence of three channels that can explain the overall negative effect of unemployment on new-born health: maternal stress; unhealthy behaviours - namely excessive alcohol consumption and smoking; and delays in the take-up of prenatal services. While the heterogenous effects of unemployment by area of deprivation seem to be explained by maternal behaviour. Most importantly, we also show for the first time that selection into fertility is the main driver for the previously observed, opposite counter-cyclical results, e.g., Dehejia and Lleras-Muney (2004). Our results are robust to internal migration, different geographical aggregation of the unemployment rate, the use of gender-specific unemployment rates, and potential endogeneity of the unemployment rate which we control for by using a shift-share instrumental variable approach.

JEL Classification: E24, I10, I12, J13

Keywords: unemployment rate, birth outcomes, birth weight, fertility, England

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* Our dataset comes from the Hospital Episode Statistics data (HES), and contain patient level information which is sensitive and confidential, and so cannot be posted on a data repository. We therefore request exemption from the Data Availability Policy, but can of course still provide a copy of the programs used to create the final results. Any author wishing to access this dataset for purposes of replication must apply via NHS Digital https://digital.nhs.uk. The authors have nothing to disclose.

1 Introduction

Early-life events are known to affect a large set of outcomes throughout the lifecycle (Almond and Currie, 2011a,b; Almond et al., 2018). Adverse prenatal conditions that result, for example, in low birth weight babies, have been found to lower adult height, IQ, earnings and educational attainment (e.g., Behrman and Rosenzweig, 2004; Black et al., 2007). A vast body of literature investigates the effects of economic recessions on child outcomes (Currie and Duque, 2016). Decrease in family resources, or employment, due to economic downturns suggests the vulnerability of young children, and particularly the ones of more disadvantaged families favouring the persistence of social and health inequalities (Currie et al., 2015b; Almond et al., 2018).

Nonetheless, there is still a lack of consensus concerning the relationship between economic downturns and babies' health. Dehejia and Lleras-Muney (2004) find that new-borns' health is counter-cyclical. Using administrative birth register data for the United States, they show that an increase in the unemployment rate leads to reductions in the incidence of low and very low birth weight, in congenital malformation, and in post-neonatal mortality. Other scholars have replicated this analysis, and more recently Aparicio Fenoll and González (2014) and van den Berg et al. (2016) have reached the same conclusions for respectively Spain and Sweden. In contrast, numerous studies have instead found new-born's health to be pro-cyclical (Lindo, 2011; Carlson, 2015; Olafsson, 2016; Alessie et al., 2018; Kaplan et al., 2017)[1], or insignificantly related (Salvanes, 2013).[2] In low-middle income countries, the consensus instead tends towards pro-cyclicality of babies' health (e.g., Bhalotra, 2010; Bozzoli and Quintana-Domeque, 2014). This leaves us an open debate on why such opposite findings exist, and whether they can or should be compared.

The aim of this paper is to reconcile these different results. We use a unique administrative dataset of 4.8 million births in England from 2003 until 2012, a period during which unemployment rates vary considerably.[3] By using individual data on mothers and their new-born children delivered in National Health Service (NHS) hospitals, we compare the health outcomes of babies born to the same mother to study how variations in local unemployment affect different birth outcomes (birthweight, low birthweight, very low birthweight, preterm and foetal growth). The focus on siblings, and the use of maternal fixed effects, allows us to control for selection into fertility with variations in labour market conditions.

We find that the health of English new-borns is negatively associated with an increase in the unemployment rate. Specifically, we find that overall a sibling born in a recession will on average be less healthy, *ceteris paribus*, on a range of metrics: a one-percentage point increase in the unemployment rate leads to an increase in low birth weight babies of 1.3%, an increase of preterm babies of 1.4%, and a 0.1% decrease in foetal growth.[4] We also find that these adverse effects of higher unemployment are *greater* the more deprived is the residential area of the mother, leading to a decrease in birthweight and foetal growth by respectively 0.15 and 0.14%. Conversely, for babies conceived in the richest areas, a one-percentage point increase in the unemployment rate is associated with an increase in birthweight and foetal growth by respectively 0.09% and 0.08%, but these estimates are only marginally significant at the 5 and 10% levels. We show that our results are robust to maternal migration, gender specific unemployment rate, a higher level of geographical aggregation of the unemployment rate, and potential endogeneity of the unemployment rate which we address by using a shift-share instrument or Bartik instrument.

We explain our findings by focusing on three mechanisms. Firstly, we explore how high unemployment, by creating financial distress, may affect the pregnancy outcome by increasing maternal stress. An increase of one-percentage point in the unemployment rate is found to increase the probability of stillbirth by 2.7%; and to increase the probability of having a female baby by 0.3%. This might indicate that more male births result in miscarriages given that males are more sensitive to foetal stress. Both results indicate selection in-utero (Hogue et al., 2013; Low, 2015).

A further mechanism that helps to explain not only the overall effect, but also the heterogenous effects of unemployment on health by degree of deprivation, is a differential change in unhealthy maternal behaviour. In line with previous research (Currie et al., 2015b), we find heterogenous responses. Mothers living in the poorest areas are more likely to be diagnosed for alcohol- (plus 2.6-3.6%) and smoking-related health problems (plus 8.2-9.3%), while mothers living in the richest areas undertake less of these risky behaviours as unemployment increases (9.6 and 2.5% reduction in respectively alcohol- and smoking-related diagnoses). These findings could explain our positive albeit only marginally significant associations of unemployment and babies' health.

Thirdly, we explore whether conceiving during an economic crisis results in a postponement of the first prenatal visit. We find that a one-percentage point increase in the unemployment rate leads to a postponement of the first visit by half a day. This finding suggests that a mother's opportunity cost of time to attend prenatal checks increases in a recession.

Finally, and arguably most importantly, we provide an empirical comparison of our findings with research that reaches opposing conclusions (e.g., Dehejia and Lleras-Muney, 2004). Given that sibling data is not always available, most studies have analysed the effect of unemployment on health by pooling all babies born in a period (all babies' sample), without controlling for maternal fixed effects. We show that when pooling all births, babies' health is counter-cyclical, supporting results by Dehejia and Lleras-Muney (2004) for the United States, and more recently by Aparicio Fenoll and González (2014) for Spain.

One hypothesis is that an all babies' sample where siblings cannot be identified does not allow for the control of time-invariant unobservable maternal characteristics leading therefore to different findings. We reject this hypothesis by showing that babies' health in the siblings sample is pro-cyclical both when using pooled OLS or maternal fixed effects. A more likely explanation is that babies who do not have siblings born in the ten year sample period, and who are thus excluded from the sibling analysis, are differently influenced by a change in unemployment. In fact, we find that the sub-sample of births whose mother delivers only once in the ten-year period (one-baby's sample) is driving the findings in the all babies' sample. We provide evidence that an increase in unemployment leads to different fertility depending on the socio-economic level of mother's area of residence. An increase in unemployment leads to a positive selection into the one-baby's sample where mothers who come from the least deprived areas, and are more likely to have healthier babies because they engage less in risky behaviours during pregnancy, conceive more. Viceversa, an increase in unemployment alters the make-up of the siblings' sample, increasing the proportion of mothers from deprived rather than affluent areas. Hence, selection into fertility seems to be the main reason for the counter- or pro-cyclical finding - different mothers decide to conceive at different points of the business cycle and behave differently once pregnant.

This paper contributes to the existing literature in several ways. Firstly, we contribute to the open discussion of the effects of unemployment on new-borns' health, by showing that differences in results seem to be due to selection into fertility and to the different behaviours that mothers from different social classes adopt during pregnancy. Secondly, English NHS administrative data for over 4.8 million births are used to study the effect of an economic downturn on health outcomes for new-born babies. Previous studies in the medical and social science literature have examined the relationship between economic conditions and birth outcomes in the UK by using small or selective samples or representative (but still small) surveys, such as the British Household Panel Survey or Millennium Cohort Study (e.g., Chevalier and O'Sullivan, 2007; Del Bono et al., 2012). By using data from 2003 to 2012, we benefit from the large variability in unemployment in the latest and worst recession since the 1930s. This differs from Dehejia and Lleras-Muney (2004), Salvanes (2013) or van den Berg et al. (2016), who use pre-Great Recession data, with only Olafsson (2016) using Great Recession data. Finally, we investigate the reasons for our findings, both the overall result and the heterogenous effects of unemployment on health by degree of deprivation in mother's area of residence. Always controlling for maternal time-invariant unobservable characteristics, we provide consistent empirical support for three mechanisms: maternal stress, maternal health behaviours, and prenatal care. Dehejia and Lleras-Muney (2004) and Aparicio Fenoll and González (2014) find that unemployment changes maternal behaviour, while Olafsson (2016) supports the stress in-utero channel.

This paper is structured in the following way. Section 2 presents the data and the empirical specification. Section 3 presents our main results, and analysis of heterogenous effects. Section 4 shows evidence on three potential mechanisms. In Section 6 we report our main robustness check analysis. Section 5 contrasts and explains the different findings in the siblings' sample and in the all babies' sample. Section 7 concludes and discusses future research.

2 Data and empirical strategy

2.1 Hospital Episode Statistics Data

This paper uses the administrative Hospital Episode Statistics (HES) data. HES data provide individual information concerning all inpatients and outpatients admitted to NHS hospitals from 1989-90. It includes also private patients treated in NHS hospitals. Each patient record contains detailed clinical information, patient characteristics, such as age, gender, residence, method of admission, and hospital of treatment. Since our focus is birth outcomes, we restrict our analysis to delivery admissions from 2003 until 2012. Each episode of delivery reports the following information: mother's age, mother's Lower Super Output Area (LSOA) of residence,[5] mother's ethnicity, length of gestation, gestation period in weeks at first antenatal assessment, result of the pregnancy (live birth or stillbirth), number and type of diagnoses, delivery information (method, type of doctors attending the delivery, etc.), date of admission to the hospital, date of discharge, as well as new-born's gender and birth weight. Moreover, the HES data contain information about the hospital where the delivery took place, the Primary Care Trust (PCT) of reference,[6]and the GP practice.

Our all babies' sample counts 4,778,918 singleton live births.[7] We exclude 22,777 stillbirths, which will be considered in our Mechanism section 4 to study selection in-utero. For our main analysis which compares siblings, we focus on mothers who had at least *two* live births in the period 2003-2012, leading to a sample of 2,384,935 births. About 77% of those are sibling pairs, 19% sibling triplets, and the rest mothers who delivered more than three offspring. Using the baby's birth weight and the length of gestation we create the following *outcome variables*: birth weight (BW) in grams, low birth weight babies (LBW - dummy variable equal to 1 if birth weight is below 2.5 kg), very low birth weight babies (VLBW - dummy variable equal to 1 if birth weight is below 1.5 kg), length of the pregnancy (in weeks), preterm (dummy variable equal to 1 if the length of pregnancy is below 37 weeks), and foetal growth in grams (ratio between BW and length of the pregnancy).

We also consider the following control variables: baby's gender, month of birth, and mother's ethnicity.[8]

To capture the supply of health care that might have changed in our period, we consider the following variables derived from NHS digital data and created at PCT level for each year: number of GPs per 1k population, the number of specialists per 1k population, the number of GP practices, and the number of hospitals. We also incorporate the number of midwives working in each hospital in each year.[9]

2.2 Unemployment rate

We use unemployment data from the UK Department of Work and Pensions (DWP). Persons above 17 years, who register at the local employment office, and are actively seeking work are called 'registered unemployed', and receive an unemployment benefit called Jobseeker's Allowance (JSA).[10] We use quarterly data on all claimants, from 2003 until 2012. We calculate a yearly average of claimants by year and Middle Layer Super Output Area (MSOA),[11] and divide this figure by the population at-risk aged 18-64 (working-age population) by year and MSOA, to obtain a measure of the proportion of people claiming unemployment-related benefit. We refer to this as the *unemployment rate* (UR). For our main analysis we consider unemployment as a whole, but as a robustness check we focus also on gender-specific unemployment, as it could affect babies' health in a different way (see for example van den Berg et al., 2016).

Figure 1 reports two maps representing the average unemployment rate pre-recession (2003-2007), and during and post-recession (2009-2012). As can be seen there is clear geographical variation in UR across local areas (MSOAs).

2.3 Descriptive statistics

The individual HES data are linked to the unemployment rate, and the health care services variables through the mother's area of residence[12] and the year at conception. The date of conception is estimated from the HES using the date of admission, the length of gestation and the week when the first antenatal health assessment is conducted.

Table 1 presents descriptive statistics of the main variables considered. In columns (1)-(4), the *all babies' sample* can be compared with the sample of mothers that are observed at least twice in our period or *siblings' sample* and the sample of mothers that delivered only once or *one-baby's sample*.

There are not major differences across the three samples. The proportion of LBW babies is 5-6%, the one of VLBW is about 0.7-0.8%, while there are about 6% preterm births. The unemployment rate is slightly higher in the siblings' sample (3.49) compared to the one for the one-baby's sample (3.46). As expected in the siblings' sample, families have bigger size with on average 1.38 children, compared to 0.93 in the one-baby's sample.[13] In the one-baby's sample we also observe a slightly higher prevalence of teen mothers (6.5%) and of old mothers over 35 (22%) compared to the same groups in the siblings' sample (respectively 5.4% and 16%). The opposite is true for the prevalence of 20-24 and 25-29 mothers.[14]

Our main empirical analysis focuses on the siblings' sample (Sections 2.4, 3, and 4), but the all babies' and one-baby's samples will be used to compare our findings with previous work in Section 5.

2.4 Empirical strategy

To study the effect of unemployment on babies' health, we compare outcomes of siblings born in different years, and adopt a mother fixed effect approach. This is possible as the hospital data allow us to identify mothers who gave birth more than once in the period observed. We can then control for unobserved time-invariant characteristics of the mother, which may be correlated with selectivity into fertility at times of high unemployment. We estimate the following equation using in turn (i) pooled OLS, and (ii) mother fixed effects (FE):

$$Y_{ijt} = \alpha_i + \beta UR_{jt} + \theta_t + \gamma_l(\delta_l \times t) + \epsilon_{ijt}, \quad (1)$$

where Y_{ijt} corresponds to a birth outcome (birth weight, LBW, VLBW, length of the pregnancy or preterm, or fetal growth) for infant of mother i living in MSOA j and conceived in year t. UR_{jt} corresponds to the unemployment rate for MSOA j and year t, and the coefficient of interest is β and indicates the effect of unemployment on the birth outcome. The mother fixed effect is captured by α_i and implicitly incorporates an MSOA fixed effect assuming that mothers do not migrate between MSOAs. Year fixed effects are captured by θ_t, while γ_l represents Local Authority (LA) (indexed with l), specific trends where δ_l corresponds to LA fixed effects and t to the time trend.[15] The year dummies control for any year specific factors that could affect both infant's health and the economy. The LA specific trends allow for omitted trends that vary by LA. The standard errors are clustered at the mother level.[16]

In our data we observe some mothers moving between MSOAs. In Section 6 as a robustness check, we rerun our analysis, using only mothers who have lived in the same MSOA during the period of analysis, automatically controlling for MSOA fixed effects and clustering the standard errors at MSOA level (as in van den Berg et al. (2016)). The results are similar to those from the main specification. Results are also robust if we aggregate unemployment at a higher geographical level (see Section 6).

In order to justify our claim that the results are causal, we assume that the residual does not contain MSOA-time varying shocks that might drive a correlation between infant's health and unemployment.[17] To relax this assumption and further control for the potential endogeneity of the unemployment rate, in Section 5 we adopt an instrumental variable approach (Bartik instrument) and find very similar results (see Section 6).

Equation (1) is estimated with and without extra individual controls (baby's gender, month of birth, and mother's ethnicity) and PCT- or hospital-level supply care controls.

3 Results

3.1 Main results

Table 2 reports the results obtained using the sample of siblings to estimate Equation (1) with pooled OLS without (Panel A), and then with (Panel B) the extra controls of month of birth, new-born's gender, mother's ethnicity, and health care supply characteristics. We then introduce a mother FE, without (Panel C) and with (Panel D) extra controls.

The evidence from the pooled OLS regressions (Panels A-B) suggests that an increase in the unemployment rate worsens new-borns' health: a one-percentage point increase in the unemployment rate leads to a decrease in birthweight and foetal growth by 0.7-0.9% depending on the specification. If unobserved mothers' characteristics are controlled for by adding mother fixed effects, the estimated coefficient of unemployment is slightly increased, and a broader range of health measures are statistically significant, as shown in Panels C and D. From Panel C, a one-percentage point increase in the unemployment rate leads to a decrease of 0.11% in both birthweight and foetal growth, together with an increase of 1% in LBW, and of 0.9% in preterm births. Also, the probability of VLBW increases by 1.7%, but only at a 10% level of significance. If extra controls are added (Panel D) the results reported in Panel C are slightly strengthened. The effect of unemployment on length of gestation is negative as expected in Panel D, but not statistically significant, while a one-percentage point increase in the unemployment rate leads to an increase of preterm babies by 1.4%.

The effects reported from the existing literature where mother fixed effects are included are mixed. Olafsson (2016) finds that having being exposed to the crisis in the first trimester leads to an *increase* in LBW by 1.9 percentage points in Iceland, van den Berg et al. (2016) show that an increase in the unemployment rate by one percentage points results in a *decrease* in LBW by 6-11% in Sweden, while Salvanes (2013) and Dehejia and Lleras-Muney (2004) find *no effect* for respectively Norway and California.

3.2 Heterogeneous effects by the Income Deprivation Domain

Given our finding that unemployment is bad for babies' health, we now consider whether there are heterogeneous effects. Disadvantaged women are more vulnerable to changes in economic conditions, they are more likely to work in low-quality jobs and they are more likely to encounter higher physical and mental health risks (Kim et al., 2008). They might also experience worse health compared to advantaged women (Currie et al., 2015b).

To account for heterogeneous effects of the unemployment, we group mothers by the level of poverty of their area of residence, as measured by the Economic Deprivation Index (EDI) built by the Ministry of Housing, Communities and Local Government. The EDI is a measure of deprivation produced by the UK government at LSOA level and is made up of two domains: Income and Employment. The two domains are given equal weighting within the overall EDI. The EDI is available for the years 1999-2009.[18]

For our analysis we consider only the Income Deprivation Domain (IDD), which represents the proportion of people aged under 60 in an area that are living in low income households claiming certain means-tested social security benefits. We construct an index of deprivation for every MSOA using the Income Deprivation Domain of the EDI data available at LSOA level. For our analysis we consider the 2002 IDD, one year before our period, so that it is not endogenously affected by changes to the unemployment rate during the years studied.[19]

We create a categorical variable that takes five values corresponding to the five quintiles of IDD, where the first quintile corresponds to the least deprived areas (IDD1), and the fifth quintile to the most deprived areas (IDD5).

Using the siblings' sample, in columns (2)-(3) of Table A.2 in the Appendix we report descriptive statistics of the main variables for the least and most deprived areas - IDD1 and IDD5. The health outcomes are worse for the poorest quintile of IDD, with higher proportions of LBW, VLBW and preterm babies. The unemployment rate is four times higher in the most deprived MSOAs. The least deprived areas have a higher percentage of White population, while in the poorest areas there are more Indians, Bangladeshi, Pakistani and Black mothers. The health care supply is slightly higher in the most deprived areas.

Table 3 shows the effect of unemployment on birth outcomes by deprivation. We estimate Equation (1) with a mother fixed effects specification, but we add dummies for the IDD quintiles and the interaction terms between unemployment rate and those dummies.[20] Every column corresponds to a different regression which includes the mother fixed effects, year fixed effects, LA-specific trends, infant's month of birth, new-born's gender, mother's ethnicity and the supply health care characteristics. Considering the four interaction terms UR $\times$ IDD2-UR $\times$ IDD5 in particular, we find that the more an area is deprived, the greater is the effect of unemployment in worsening a babies' health. Thus, babies born to mothers who live in the poorest areas (IDD5) are the ones whose health suffers most from an increase in unemployment: for these babies, a one-percentage point increase in the unemployment rate is associated with a decrease in birthweight, and foetal growth, by 0.15% and 0.14% respectively. Conversely, for babies conceived in the richest areas, a one-percentage point increase in the unemployment rate is associated with an increase in birthweight and foetal growth by respectively 0.09% and 0.08%, but this is only significant at the 5 and 10% levels, respectively.

4 Potential mechanisms

In this section we provide evidence on three potential channels through which unemployment affects infant's health: maternal stress, maternal health behaviour, and prenatal care.

4.1 Mechanism I: Stress in pregnancy

There is evidence showing a negative relationship between foetal exposure to maternal stress and low birth weight and premature births (Wadhwa et al., 1993; Persson and Rossin-Slater, 2018). Financial stress due to high unemployment may also affect the health of unborn children, and particularly the levels of miscarried, stillborn and aborted children. To test this channel, we attempt to discover if there is selection in-utero in two ways.[21] First, we study if the probability of having a stillborn depends on the unemployment rate. There is, in fact, evidence showing that stress in pregnancy increases the risk of stillbirth (Hogue et al., 2013). A stillbirth is registered by every hospital when a baby is born dead after 24 completed weeks of pregnancy, and our data allow us to observe mothers who delivered one or more babies in the period of observation, whether live or dead. Second, we study whether the probability of having a female baby is influenced by the unemployment rate. Male conceptions are more sensitive to foetal stress and are more likely to miscarry, so that a higher probability of having a female might indicate that male pregnancies are more likely to result in miscarriages (Low, 2015).

In Table 4 we define a new outcome variable, *Stillborn* equal to one if the baby is stillborn, and zero if he/she is a live birth. We estimate Equation (1) by regressing stillborn on unemployment in column (1), and on unemployment interacted with the 5 quintiles of IDD in column (2), and including always the full set of controls. A one-percentage point increase in the unemployment rate leads to a highly statistically significant increase of 2.7% in stillborn. Column (2) shows that there are no heterogeneous effects in the unemployment by IDD on stillborn. Also, Vlachadis and Kornarou (2013) and Michas et al. (2014) find an increase of 32% in stillbirth rate in Greece between 2008 and 2010. In columns (3) and (4), we regress a dummy variable equal to one if the baby is female and zero otherwise on the unemployment rate, and the unemployment rate interacted with the 5 quintiles of IDD, and including always the full set of controls. Column (3) shows that a one-percentage point increase in the unemployment rate leads to an increase of 0.14 percentage points, or 0.3%, in the probability of having a female baby. This is consistent with Olafsson (2016) who finds a 3.3 percentage points reduction in sex ratio at birth (less boys) among first-trimester children exposed to the 2008 financial crisis in Iceland. Column (4) shows that mothers who live in the richest areas tend to have more female babies (1.1% increase) than the mothers who live in the poorest areas (0.4% increase). This difference is statistically significant at the 5% level suggesting that there are no strong heterogeneous effects in the unemployment by IDD on infants' gender. Overall the results reported in Table 4 indicate a selection in-utero that may be linked to the financial stress experienced during the pregnancy.

4.2 Mechanism II: Maternal health behaviour

Other channels that can possibly explain the negative effect of unemployment on infant health include mothers' health behaviour during the pregnancy. Cigarette smoking and nutrition affect the intrauterine growth, while length of gestation is influenced by smoking and stress (Torche, 2011; Koppensteiner and Manacorda, 2016). Heavy prenatal alcohol exposure has been found to have negative effects on new-borns by crossing the placenta and passing to the foetal, and by decreasing the supply of oxygen and food (Jones and Smith, 1973; West et al., 1994; Goodlett and Horn, 2001).

The HES data do not contain information about the mother's behaviours, but they report diagnoses identified at the time of the delivery.[22] In particular, the first 20 diagnoses are reported using ICD-10 codes (up to three-digit code). In our sample of live births siblings, the most common first few diagnoses are related to the delivery,[23] while the remaining diagnoses refer to any other type of disease or health problem.

Following the medical literature (Currie et al., 2015a; Dietz et al., 2010; US Department of Health and Human Services and others, 2014), we identify diagnoses associated with alcohol consumption and smoking.[24] These diagnoses represent only very serious cases of either smoking or drinking addiction, or smoking or drinking related-diseases. However, given the lack of individual behavioural information, these data can still be quite informative. We create four new outcome variables, and Table A.1 in the Appendix reports the diagnoses selected to define each variable. The first one, *Alcohol*, is a dummy equal to one if the mother has at least one diagnosis related to alcohol use where the diagnosis correspond to Panel A, column (1) of Table A.1. The second, *Smoking*, is a dummy that takes value one if the mother has at least one diagnosis related to smoking where the diagnosis corresponds to Panel B, column (1) of Table A.1.[25] We then provide results for a less strong definition of alcohol and smoking, *Reduced alcohol* and *Reduced smoking*, using the diagnoses reported in column (2) of Panels A and B, respectively, of Table A.1.

Table 5 presents the results of eight regressions where the outcome is *Alcohol* in columns (1)-(2), *Reduced alcohol* in columns (5)-(6), *Smoking* in columns (3)-(4) and *Reduced smoking* in columns (7)-(8). In columns (1), (3), (5) and (7) we regress the outcomes on the unemployment rate, while in columns (2), (4), (6) and (8) we regress the outcomes on the unemployment rate and the unemployment rate interacted with the five quintiles of IDD, controlling for the full set of controls. Column (1) shows that a one-percentage point increase in the local unemployment rate is associated with an increase of 2.8% in alcohol-related diagnoses. Column (2) shows that there are heterogenous effects by area deprivation: the probability of being diagnosed for alcohol-related health problems is lower for the mothers who live in the least deprived areas (-9.6%), while the opposite is true for the ones living in the middle to most deprived areas (2.6-3.6%). Results reported in column (5) confirm the positive association between UR and alcohol-related illness, while results reported in column (6) show that this increase is mainly concentrated among the mothers who live in the most deprived areas, where a one-percentage point in the unemployment rate leads the poorest mother to be 17.4% more likely to be diagnosed for alcohol use (where p-value Test UR×IDD1-UR×IDD5=0.018). There are no other statistically significant heterogenous effects for *Alcohol*. Column (3) shows that a one-percentage point increase in the unemployment rate is associated with an increase of 5.8% in smoking-related diagnosis. Column (4) shows that this effect is driven by the poorest mothers (living in areas where IDD is 4 or 5) where a one-percentage point increase in the unemployment rate leads to an increase in *Smoking* by 8.2-9.3%, while a decrease by 2.5 for the richest mothers. As for the restricted definition of alcohol-related diagnosis, the evidence reported in columns (7) and (8) indicates that smoking increases if unemployment rate increases and this is largely due to a higher probability of smoking among the poorest mothers.

Overall, these results can explain our heterogenous findings (Table 3). Mothers who come from the most disadvantaged areas are more likely to engage in risky behaviour, such as smoking and drinking, following an economic downturn, leading to worsened new born health outcomes. There is some indication that the richest mothers smoke and drink less alcohol compared to the poorest ones (this effect disappears when we use a reduced set of diagnoses), and have healthier babies, even if the differences are only marginally statistically significant.

Similar findings are found by Currie et al. (2015b) who use American longitudinal data from the Fragile Families and Child Well-being Study to study the impact of the Great Recession on mothers' health. They adopt a mother fixed effects specification and find that unemployment increases smoking and drug use, and decreases self-reported health status particularly so for the disadvantaged mothers - African American, Hispanic, less educated or unmarried. Viceversa, White, married women and highly educated mothers report better mental health and physical health.[26]

4.3 Mechanism III: Prenatal care

We finally test whether unemployment has an effect on prenatal care, where prenatal care is measured as the gestation period in weeks at first antenatal assessment. In general, the earlier prenatal care is sought, the better it is for the mother and the baby (Kost et al., 1998).

Table 6 presents the results of two regressions where the outcome is *Week 1st visit* and corresponds to the week when the pregnant mother has her first antenatal appointment. In column (1) we regress *Week 1st visit* on the unemployment rate, while in column (2) we regress *Week 1st visit* on the unemployment rate and the unemployment rate interacted with the quintiles of IDD, including the full set of controls in both regressions. Column (1) shows that a one-percentage point increase in the unemployment rate leads to a postponement of the first visit by half a day, or an increase of 0.6% in the number of weeks before the first visit. Column (2) presents the results of regressions where prenatal care is regressed on the unemployment rate and the unemployment rate interacted with IDD. The results show that an increase of one-percentage point in the unemployment rate leads women from the least deprived areas to postpone their first visit by 2.2 days or 2.3%, while women from the most deprived areas delay by 0.9 days or 0.9%. Given that the NHS provides the recommended prenatal visits free-of-charge, the opportunity cost of time is a possible explanation for these results. In recessions, mothers might have less time to attend check-ups because the opportunity cost of time that might otherwise be used to find a job is higher, thus leading to a postponement of the first visit.[27] At the same time, mothers living in the poorest areas postpone prenatal care less than the ones living in the richest areas, suggesting that for them a substitution effect dominates the income effect. Nonetheless, this does not explain our heterogeneous findings, suggesting that the poorest mothers' health behaviour might be the main channel.

5 A tale of siblings, maternal behaviour and selection

We have found that unemployment in England has an effect on babies' health which varies from strongly adverse in the most deprived areas, to mildly favourable in the most prosperous areas. Our econometric strategy to control for maternal time-invariant characteristics and cyclical selectivity into childbirth is to compare siblings. As yet studies have not contrasted the findings from comparing the health of siblings born at different points of the cycle with those from studying the all babies' sample of which the siblings form a sub-sample. In this Section we perform some analysis of this nature by using different samples and methods to compare our results with previous work.

5.1 Replicating Dehejia and Lleras-Muney (2004)

We first study the influence of unemployment in the all babies' sample. We estimate the following equation:

$$Y_{ijt} = \alpha + \beta UR_{jt} + \theta_t + \xi_j + \gamma_l(\delta_l \times t) + \epsilon_{ijt}, \quad (2)$$

where the main differences with respect to Equation (1) are the lack of an individual fixed effect, the inclusion of MSOA fixed effects captured by ξ_j, and the standard errors clustered at MSOA level. Equation (2) is similar to that of Dehejia and Lleras-Muney

(2004) who consider United States birth register data and unemployment rate measured at the state-level.

In Table 7, Panels A and B, we show the results of this replication exercise where we estimate Equation (2). Every column reports only the estimated β coefficient which comes from different regressions that include the MSOA FE, year FE and LA-specific trends. In Panel B we also control for extra variables such as month of birth, new-born's gender, mother's ethnicity, and healthcare supply characteristics. Each column corresponds to a different birth outcome. The results reported in Panels A and B of Table 7 are similar and suggest that in the all babies' sample unemployment *improves* babies' health: an increase of one percentage point in the unemployment rate leads to an increase of 0.05% in birthweight, 0.04-0.06% in foetal growth, a 1.1-1.2% decrease in LBW, and a 1.1 decrease in VLBW but this is only statistically significant when the extra controls are included. These results therefore closely resemble those of previous studies of various developed countries; in particular, Dehejia and Lleras-Muney (2004), and Aparicio Fenoll and González (2014) who find a reduction in LBW by 0.24-0.5% for the United States and by 6% for Spain. These findings conflict with the findings obtained by comparing siblings, and confirm that if we had only performed a conventional analysis on the all babies' sample, we would have concluded that unemployment is beneficial for babies health.

The sharp contrast in the results obtained using English data for all babies compared to siblings raises the question of why unemployment is bad for health in the siblings' sample, but frequently good for health in studies of the all babies' sample? To address this, we first discuss the effects of unemployment on new-borns' health in the siblings' sample excluding the maternal fixed effects. The estimates are reported in Panels A and B of Table 2. The coefficients estimated with and without maternal fixed effects are broadly similar qualitatively, showing that the fixed effects contribute only a modest amount to closing the gap between the adverse influence of unemployment found in the siblings' sample, and the strongly positive influence of unemployment found in the all babies' sample.

An alternative explanation is that babies who do not have siblings born in the ten year sample period, and who are excluded from the sibling analysis, are differently influenced by a change in unemployment. To explore this, we estimate Equation (2) to study the effects of unemployment on babies' health for the sample of mothers who had only one baby in our period of observation (one baby's sample), and who were excluded from the sibling study. The results are reported in Table 7, Panels C and D, where we see that unemployment has a *positive effect* on the health of these babies, and with a substantially *larger elasticity* than the one found in the all babies' sample, as reported in Panels A and B of the same Table. For example, in the one baby's sample, we find that if the unemployment rate increases by one percentage point, VLBW decreases by 4.7%, as compared to 2.0% in the all births sample. In contrast, results based on the same analysis but focusing on the siblings' sample (without maternal fixed effects, Table 2- Panels A and B), show that unemployment adversely affects babies' health.

5.2 Fertility and unemployment

Why is the health of new-borns in the one baby's sample differently affected by the unemployment than the health of new-borns in the siblings' sample? To answer this question we explore fertility selection into the siblings' sample and into the one-baby's sample. Several studies show how a negative economic shock influences different women in their fertility decisions. In particular, Dehejia and Lleras-Muney (2004) find that White mothers giving birth during a recession are less educated, while the opposite is true for Black mothers who tend to be higher socioeconomic status. Aparicio Fenoll and González (2014) and Salvanes (2013) instead find that low-educated women are overrepresented when there is an economic downturn.[28]

As a measure of fertility, we consider the variable parity (the number of times that a woman has given birth) that is available for most of mothers. We first focus on the siblings' sample. In Table 8 we estimate Equation (1) where the outcome of interest is parity regressed on the local unemployment rate in column (1) and on unemployment rate interacted with the quintiles of IDD in column (2) always including all the controls. The results in Column (1) show that a one-percentage point increase in the unemployment rate leads to a decrease of 0.17% in parity. From column (2), we see that this decrease is accentuated for women who live in the richest areas (-0.73%), while it is less for women who live in the most deprived areas (-0.15%).

We have seen that babies born in poor areas tend to have worse health outcomes compared to the ones born in rich areas (see Table A.2 in the Appendix). As a consequence, the change in the births composition where more babies are born from deprived areas could explain our overall finding that unemployment reduces new-born health.

Recessions commonly lead to a postponement of childbearing, which is often later compensated during times of economic prosperity (Currie and Schwandt, 2014; Sobotka et al., 2011). Table 8 presents the results of two regressions of maternal age on unemployment rate, without (column (4)) and with (column (5)), the interaction with the IDD quintiles, always including all the controls. There are no statistically significant associations indicating that maternal age does not change with unemployment, so we do not find a postponement of fertility following economic downturn.

We now replicate Table 8 focusing on the effect of unemployment on parity in the one-baby's sample. Table 9 reports results based on Equation (2). We find that a one-percentage point increase in the unemployment rate leads to a decrease in parity by 0.94% (column (1)) compared with the 0.17 found in the siblings' sample. More interestingly, column (2) shows that when the unemployment rate goes up by one-percentage point, then parity *increases* by 2.16% among mothers who live in the richest areas, while it *decreases* by 1.52% among the mothers who come from the poorest areas. In contrast to Table 8, Table 9 shows a significant increase in maternal age by 0.15% (column (3)), where a one-percentage point increase in the unemployment rate leads mothers' age to go down by 0.55% when living in the least deprived MSOAs, and to go up by 0.18% when living in the most deprived MSOAs. These results, which sharply contrast the heterogenous effects reported in Table 8 (column (2)), indicate that economic downturns lead to a *positive selection into the one-baby's sample*, and a *negative selection into the siblings' sample*. The counter-cyclical results obtained in the all babies' sample are in fact dominated by the one-baby's sample. While our data do not allow to study the probability of becoming a mother, we can explore if unemployment affects the probability of having a first birth versus higher order births. This is particularly interesting in the one-baby's sample where the increase in parity for the richest mothers could be in partly due to an increase in first born. Nonetheless, further analysis (available upon request) shows no such heterogenous effects by birth order.

In summary, this Section shows that the influence of unemployment in the siblings' sample is not closely mirrored by the results found in the all babies' sample from which the siblings are drawn, a strategy often used if sibling information is unavailable. The difference between the counter- versus pro-cyclical findings seems to be mainly driven by selection into fertility. Recession may indeed encourage the selection into the siblings' sample of mothers less likely to come from a prosperous area, more likely to engage in risky behaviours, and as a consequence, be less likely to have healthy babies. Unfortunately, with the data available we are not able to investigate how these fertility decisions are made which deserve further research.

6 Robustness checks

6.1 Mothers who never moved

The results presented so far refer to mothers who have delivered more than one baby, but who may have moved to a different MSOA between pregnancies. In our sample, about 36% of mothers change MSOA of residence from one birth to the next. Given that moving is a choice, our estimates may be biased by endogeneity due to selective migration: namely that mothers with specific characteristics, correlated with health outcomes, choose to move.

In this Section we test if our results are robust when using the sub-sample of mothers who remain residents in the same MSOA. In Panel A of Table 10 we estimate Equation (1) including the full set of controls, and clustering the standard errors at MSOA level. A one-percentage point increase in the unemployment rate is associated with a 0.3%

decrease in birthweight and foetal growth, which are larger reductions compared to the analysis reported in Panel D of Table 2. While the effects of unemployment on LBW and preterm are higher than the ones reported in Panel D of Table 2, they are statistically significant only at the 10% level. Overall, our results are robust to the sub-sample of mothers who never moved even if larger in magnitude.[29]

6.2 Gender-specific unemployment

In this section we examine the implications of using gendered variations in the unemployment rate. Male unemployment is usually more sensitive to the business cycle because men tend to work more. However, participation of women in the English labour market is high and the majority of women have a job.[30] Aparicio Fenoll and González (2014) only consider male unemployment in the Spanish context, and van den Berg et al. (2016) show that in Sweden, the effect of unemployment on infants' health is mainly driven by male unemployment. In this section we test whether male or female unemployment affects new-born health in the same way, and compare the findings with those above which use the total unemployment rate.

Panels B1 and B2 of Table 10 report the estimates of Equation (1) using respectively male and female unemployment rate, and controlling as above for year fixed effects, LA-specific trends, and the extra controls. Panel B1 shows that a one percentage point increase in male unemployment leads to a decrease of 0.06% in birthweight, and in foetal growth, with an increase of 0.9% in LBW and 0.8% in preterm babies. These effects are slightly smaller in magnitude compared to the main ones reported in Panel D of Table 2. Panel B2 shows that a one percentage point increase in female unemployment leads to a decrease of 0.2% in birthweight, and foetal growth, an increase of 1.9% in LBW, and of 2.6% in preterm babies. The estimates using female unemployment are more than twice the estimates when overall unemployment rate is considered, suggesting that mothers unemployment has higher detrimental effects on babies' health.

6.3 Different geographical aggregation

Economic conditions might have different effects on health depending on their level of geographical aggregation (Lindo, 2015). In the main analysis we have used unemployment rates at the level of MSOA, but data at LA level can be considered. Estimates based on more disaggregated analysis (MSOAs) can be more precise and improve power because they consider variation in unemployment that is masked by more aggregated (LAs) measures. However, unemployment rates at LA include possible spillover effects across MSOAs within a LA. Panel C of Table 10 reports the analysis at LA level. The estimates at LA level are slightly larger than those at MSOA level, in line with spillover effects from neighbouring areas that are not taken into account in MSOA level analysis.

6.4 Endogeneity and measurement error of unemployment rate

The mother fixed effects control for any time-invariant maternal characteristics. However, unobservable time-varying characteristics could be correlated with both unemployment and the health of babies, leading to biased estimates. Moreover, the unemployment rate might be measured with error. To overcome these problems one solution is to find an instrumental variable that is associated with the local area unemployment rate but not with the birth outcomes. Building on the work of Bartik (1991) and Blanchard et al. (1992), we construct an instrumental variable called the Bartik IV.[31] We instrument for the MSOA-level unemployment rate using a predicted MSOA-level unemployment rate, which we create as the weighted average of the national-level unemployment rates across industries, where the weights are the MSOA-level fraction of the employed working-age population in each industry a few years before the start of our sample period, i.e. 2001.

More formally, our estimated industry-predicted unemployment rate is:

$$UR_{jt} = \delta(\Sigma_k w_{jk} URUK_{kt}) + \rho_t + \pi_l(\tau_l \times t) + \upsilon_{jt}, \tag{3}$$

where w_{jk} are the weights corresponding to the fraction of employed working-age individuals in each industry k and MSOA j in 2001; $URUK_{kt}$ is the national level unemployment rate in each industry, k, in each time period, t; ρ_t are year fixed effects, while π_l capture LA-specific trends. The error terms are υ_{jt}. The weights or shares of employed individuals in each industry in 2001 are extracted from the Census 2001, while the total unemployment rate at industry level comes from the Labour Force Survey from 2003 to 2012.[32] The weights are fixed at 2001 and they do not reflect sorting into industries over time.

For this robustness check, we use an instrumental variable fixed effects (IVFE) estimator, where the first stage is Equation (3), and the second stage is Equation (1). Second stage results are reported in Panel D of Table 10. The F test is always above 10 indicating that the instrument is relevant. The results are similar even if slightly larger in magnitude compared to the main ones reported in Panel D of Table 2.

7 Conclusions

In this paper we study the effects of unemployment on babies' health in England. We link English individual-level administrative hospital data on deliveries to unemployment rates measured at MSOA-level for the period 2003-2012. By exploiting geographical variation in the unemployment rate across local areas in England, and comparing siblings born to the same mother, we find the health of new-born babies to be strongly pro-cyclical, i.e. babies born during more prosperous times tend to have more favourable health statuses. Moreover, the results suggest that babies born to mothers who live in the poorest areas are the ones whose health is most affected by higher unemployment. We provide evidence of three channels that can explain the negative effect of unemployment on new-born health: maternal stress; unhealthy behaviours - namely excessive alcohol consumption and smoking; and delays in the take-up of prenatal services. While the heterogenous effects of unemployment by IDD seem to be explained by maternal behaviour.

Most importantly, we reconcile our results with previous counter-cyclical findings by showing that selection into fertility is the main driver. We use our data for England to discuss if studies that consider all babies' samples, the data basis for most studies (e.g., Dehejia and Lleras-Muney, 2004; Aparicio Fenoll and González, 2014), provide a good indication of the findings of a sibling study that use a sub-sample of the same data. We show that in the all babies' sample, necessarily estimated without mother fixed effects, the results suggest that unemployment *is good* for babies health, and thus not a valid approximation to a study of the sub-sample of siblings. The results found in the all babies' sample are driven by the sub-sample of mothers who deliver only one baby in the ten-year period. The two samples, one-baby and siblings, are in fact populated by different mothers who behave in different ways once pregnant, leading to the opposite counter- or pro-cyclical findings. Thus, studies unable to study this selectivity may fail to give reliable estimates of the effects of unemployment on child health.

Given that inequality begins before birth, and that large differences in health at birth have important consequences for later outcomes, such as education, earnings and disability (Currie, 2011), policy interventions or safety net programs that target the most vulnerable individuals, and especially so in recessions, should help reduce long-run adult inequality. In addition, future research should focus on studying fertility choices to better understand how people from different social classes make the decision to conceive during booms or busts.

Notes

[1]The medical literature is vast and usually reports new-born's health to be pro-cyclical. See for example Eiríksdóttir et al. (2013); Varea et al. (2016); Kana et al. (2017) that focus on the recent financial recession in Europe, and Ensor et al. (2010) for a review on the effect of the economic recession on infant mortality.

[2]Related research on the impact of different welfare programs, such as food or cash transfers, finds positive effects of such programs on birth outcomes (Almond et al., 2011; Hoynes et al., 2011; Currie and Rajani, 2015; Hoynes et al., 2015).

[3]The great recessions in the UK was characterised with a drop of 6 percentage points in GDP for six successive quarters from Q2 2008 until Q3 2009 (Coulter, 2016). According to OECD data, during the recession, there was also a fall in the UK labour participation rate from 76.8 per cent in 2008 to 76.3 per cent in 2010. In April 2008 the unemployed people were 1.61 million with a sharp climb after the second quarter of 2008 reaching 2.47 million by June 2009 (Pessoa and Van Reenen, 2014)

[4]Our results are consistent across a variety of health outcomes, and not only focus on birth weight which has been recently found to provide a limited picture of the prenatal environment (Conti et al., 2018).

[5]The LSOA is a small geographical area developed by the Office for National Statistics containing on average 1,500 individuals and 650 households. For more details see `http://www.neighbourhood.statistics.gov.uk`

[6]PCTs are large administrative bodies, responsible for commissioning primary, community and secondary health services from providers.

[7]This sample excludes twins and other multiple births. It also excludes both private and home births that are not accounted in the HES data, and represent about 5% of the total births. The percentage of women giving birth at home is very low in the UK, it was 2.6% in 2005 and 2.3% and in 2015 (including stillbirths) (ONS, 2016).

[8]Unfortunately, ethnicity is missing for many individuals, but we include in the ethnicity variable the category ethnicity unknown.

[9]We do not control for the quality of health services in this study. Even if in the years

post-recession the total healthcare expenditure slightly decreased (ONS 2012), different reforms were implemented to increase quality and efficiency in the NHS (e.g., Gaynor et al., 2016). See also http://webarchive.nationalarchives.gov.uk/20160107055132/ and http://www.ons.gov.uk/ons/dcp171766_361313.pdf

[10]The data can be downloaded from here: http://tabulation-tool.dwp.gov.uk/NESS/BEN/iben.htm

[11]There are about 7,200 MSOAs in England and Wales, with an average population of about 7,800 per MSOA. We focus on MSOA instead of LSOA because LSOA are small areas that can be considered more the neighbourhood than the labour market area.

[12]The mother's area of residence is at LSOA level. We link every LSOA with its respective MSOA or PCT to merge the HES with the other data.

[13]Parity is the order of the pregnancy including also stillbirths. For example, if a mother's first pregnancy results in a stillborn then parity is 1, and if the second is a live birth then parity is 2.

[14]This suggests that the 10 years' time window allows to observe only one birth for the old mothers (first observed at the beginning of the period) or the teen mothers (first observed at the end of the period).

[15]In England there are about 150 LAs that most of the times overlap with PCTs, and health care and social services are usually provided at the LA/PCT level.

[16]Results do not change if we cluster the standard errors at MSOA level. This analysis is available upon request.

[17]We estimate our regressions including birth order and results do not change (results available upon request).

[18]The index is constructed in a consistent manner over time and can be used to track the progress of deprived neighbourhoods. A different index is the Index of Deprivation (IMD) that includes also other domains such as education, skills and training deprivation, health deprivation and disability, crime, barriers to housing and services, and living environment deprivation. This index is only available for 2004, 2007 and 2010. The EDI was produced to overcome the difficulties in comparing the different IMDs as different methodologies

were used.

[19]We also consider the Income component of the 1999 EDI and use the 2001 population to create the index at MSOA level (population estimates at MSOA level do not exist pre-2001). Results do not change and are available upon request.

[20]The IDD quintiles are time-invariant and are identified because about 36% of the mothers have migrated from one MSOA to another. See Robustness analysis in Section 6.1.

[21]Unfortunately, there are no available or accessible data on miscarriages nor on abortions.

[22]There are no longitudinal UK surveys with information on pregnancies representative at the MSOA level that allow an alternative test of these behavioural mechanisms. The British Household Panel Survey (BHPS) and the Understanding Society surveys are longitudinal surveys representative of the UK and cover our period of observation, but the sample size for siblings is very small and not representative at the MSOA level.

[23]The most frequent first diagnosis is 'Perineal laceration during delivery' (26%, ICD-10 O70) followed by 'Single spontaneous delivery' (15%, ICD-10 O80), and 'Labour and delivery complicated by foetal stress' (14%, ICD-10 O68).

[24]For further details about these diagnosis see `https://nccd.cdc.gov/dph_ardi/info/icdcodes.aspx` and `https://www.aaaai.org`

[25]Our results for smoking are the same if we exclude diagnosis for cancer (ICT-10 codes starting with C) from the list. Results are available upon request.

[26]Unfortunately, the nutrition channel cannot be explored with the data at hand. However, a recent study by Griffith et al. (2013) find that because of the recession, British households cut real expenditure on food, substituting towards processed sweet and savoury food and away from fruit and vegetables. They also show that households with young children reduced real expenditure, calories and real expenditure per calorie more, on average, than other households. Jofre-Bonet et al. (2018) also finds that the Great Recession in England led to a decrease in fruit intake, an increase in BMI and in the likelihood of being obese.

[27]Similar results are obtained by Bhalotra (2010), while opposite findings are suggested by Ruhm (2000) and Dehejia and Lleras-Muney (2004).

[28]An other example is by Chevalier and Marie (2017) who show that following the economic uncertainty due to the collapse of the Berlin wall and the communist regime, women from low SES were more likely to conceive.

[29]Any potential form of endogeneity in the unemployment rate, including endogeneity due to selective migration, is taken into account in Section 6.4.

[30]In 2014, almost as many women with children (74.1%) participated in the labour force as women with no children (75%) (ONS, 2017).

[31]The Bartik instrument has also been recently used in the English context in a paper that investigates the effect of economic conditions on intimate partner violence (Anderberg et al., 2016).

[32]We use a classification of 15 industries.

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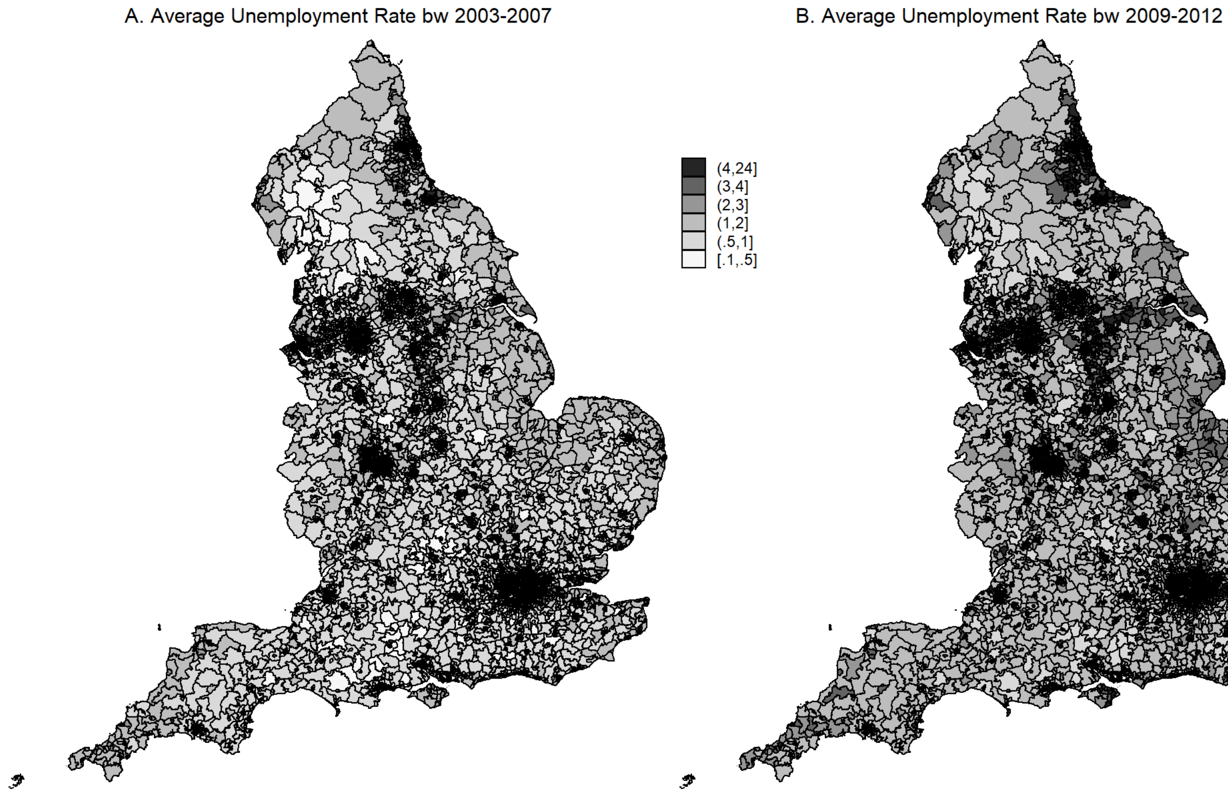
8 Figures and tables

Figure 1: Unemployment rate in England



Note: Map A. reports the averaged unemployment rate between 2003 and 2007 in each Middle Layer Super Output Area (MSOA), while map B. reports the averaged unemployment rate between 2009 and 2012 in each MSOA. The unemployment rate corresponds to the ratio of job seeker allowance claimants divided by the working age population at MSOA-year level.

Table 1: Descriptive statistics

Variables	All babies' sample Mean (1)	*SD (2)*	Siblings' sample Mean (3)	*SD (4)*	One-baby's sample Mean (5)	*SD (6)*
Outcomes						
Birth weight in grams	3365.069	*567.254*	3379.261	*565.178*	3350.955	*566.941*
Prop born below 2500 g	0.056	*0.23*	0.053	*0.224*	0.059	*0.235*
Prop born below 1500 g	0.008	*0.089*	0.007	*0.085*	0.008	*0.09*
Length of gestation	39.23	*2.102*	39.22	*2.075*	39.254	*2.115*
Prop born preterm	0.062	*0.241*	0.06	*0.237*	0.064	*0.244*
Foetal growth	85.57	*13.158*	85.96	*13.132*	85.16	*13.13*
Variable of interest						
Unemployment rate (UR)*	3.48	*2.384*	3.491	*2.39*	3.464	*2.374*
Controls from HES						
Prop female babies	0.491	*0.5*	0.491	*0.5*	0.491	*0.5*
Prop ethnicity White	0.728	*0.445*	0.745	*0.436*	0.708	*0.455*
Prop ethnicity Indian Bangladeshi and Pakistani	0.09	*0.286*	0.104	*0.305*	0.073	*0.261*
Prop ethnicity Black	0.051	*0.221*	0.055	*0.228*	0.047	*0.211*
Prop ethnicity Other	0.067	*0.25*	0.067	*0.25*	0.068	*0.251*
Prop ethnicity Unknown	0.064	*0.244*	0.029	*0.168*	0.104	*0.306*
External controls						
GPs per 1k pop at LA	0.97	*0.191*	0.965	*0.184*	0.977	*0.198*
Specialists per 1k pop at LA	0.81	*0.644*	0.812	*0.646*	0.809	*0.642*
N. GP practices at LA	0.168	*0.043*	0.168	*0.043*	0.168	*0.044*
N. of hospital at LA	0.008	*0.008*	0.008	*0.007*	0.009	*0.009*
N. midwives per hospital at LA	150.972	*69.154*	150.91	*69.349*	150.991	*68.908*
Mechanisms						
Prop stillbirth	0.005	*0.069*	0.006	*0.078*	0.003	*0.058*
Parity	1.176	*1.351*	1.381	*1.356*	0.932	*1.302*
Prop Parity 1	0.364	*0.481*	0.256	*0.436*	0.492	*0.5*
Prop Parity 2	0.341	*0.474*	0.392	*0.488*	0.282	*0.45*
Prop Parity 3	0.164	*0.371*	0.2	*0.4*	0.122	*0.328*
Prop Parity >=4	0.13	*0.336*	0.152	*0.359*	0.103	*0.304*
Prop mothers less than age 20	0.059	*0.236*	0.054	*0.226*	0.065	*0.246*
Prop mothers between age 20 and 24	0.194	*0.396*	0.208	*0.406*	0.177	*0.382*
Prop mothers between age 25 and 29	0.277	*0.447*	0.293	*0.455*	0.258	*0.438*
Prop mothers between age 30 and 35	0.281	*0.45*	0.282	*0.45*	0.281	*0.45*
Prop mothers greater than 35	0.189	*0.391*	0.163	*0.369*	0.219	*0.413*
Weeks 1st antenatal visit	14.063	*8.367*	13.872	*8.121*	14.284	*8.641*
Alcohol diagnosis†	0.005	*0.069*	0.005	*0.069*	0.005	*0.070*
Reduced alcohol diagnosis†	0.0004	*0.019*	0.0003	*0.0187*	0.0004	*0.021*
Smoking diagnosis†	0.107	*0.309*	0.108	*0.310*	0.111	*0.315*
Reduced smoking diagnosis†	0.001	*0.026*	0.001	*0.025*	0.001	*0.027*
Observations	4,778,918		2,384,935		2,377,723	

Note: * The unemployment rate corresponds to the the job-seekers allowance rate. All the statistics refer to the sample of live births, except the proportion of stillbirth. † Alcohol diagnosis (Reduced alcohol diagnosis) is a dummy equal to one if the mother has at least one diagnosis related to alcohol use where the diagnosis are listed in Panel A, column (1) (column (2)) of Table A.1. Smoking diagnosis (Reduced smoking diagnosis) is a dummy that takes value one if the mother has at least one diagnosis related to smoking where the diagnosis are listed in Panel B, column (1) (column (2)) of Table A.1.

Table 2: The effects of unemployment on different infants outcomes. Siblings' sample.

	(1) Birthweight	(2) LBW	(3) VLBW	(4) Length gestation	(5) Preterm	(6) Foetal growth
Panel A: Pooled OLS						
	OLS	OLS	OLS	OLS	OLS	OLS
UR	-3.06551***	0.00048	0.00020*	-0.00558	0.00098**	-0.06280***
	(0.77290)	(0.00030)	(0.00012)	(0.00375)	(0.00039)	(0.02072)
% change	-0.0908	0.892	2.579	-0.0143	1.469	-0.0729
Mean of Dep. Var.	3377	0.0537	0.00790	39.13	0.0666	86.18
Observations	2,384,935	2,384,935	2,384,935	2,010,159	2,010,159	2,006,421
Panel B: Pooled OLS, Extra controls						
	OLS	OLS	OLS	OLS	OLS	OLS
UR	-2.63992***	0.00042	0.00017	-0.00063	0.00054	-0.06060***
	(0.79131)	(0.00031)	(0.00012)	(0.00348)	(0.00038)	(0.02048)
% change	-0.0782	0.790	2.230	-0.00162	0.811	-0.0703
Mean of Dep. Var.	3378	0.0535	0.00783	39.14	0.0663	86.18
Observations	2,200,291	2,200,291	2,200,291	1,853,673	1,853,673	1,850,366
Panel C: Mother FE						
	FE	FE	FE	FE	FE	FE
UR	-3.79299***	0.00057***	0.00013*	0.00390**	0.00060***	-0.09900***
	(0.33459)	(0.00017)	(0.00007)	(0.00159)	(0.00020)	(0.00864)
% change	-0.112	1.064	1.661	0.00996	0.889	-0.115
Mean of Dep. Var.	3377	0.0537	0.00789	39.12	0.0676	86.10
Observations	2,375,935	2,375,935	2,375,935	1,824,578	1,824,578	1,818,540
Panel D: Mother FE, Extra controls						
	FE	FE	FE	FE	FE	FE
UR	-3.70376***	0.00070***	0.00013*	-0.00102	0.00094***	-0.08366***
	(0.35367)	(0.00018)	(0.00008)	(0.00166)	(0.00021)	(0.00906)
% change	-0.110	1.299	1.684	-0.00260	1.397	-0.0972
Mean of Dep. Var.	3377	0.0536	0.00785	39.13	0.0676	86.08
Observations	2,111,515	2,111,515	2,111,515	1,617,870	1,617,870	1,612,674

Note: Standard errors in parentheses. Every coefficient corresponds to a separate regression. Birthweight (in grams), length of pregnancy (in weeks), and foetal growth are continuous variables, while the other outcomes are dummy variables. In Panel A and B, every regression is estimated with pooled OLS and it includes year FE, MSOA FE and LA-specific trends. In Panel C and D, every regression is estimated with mother FE, and it includes year FE, and LA-specific trends. In Panel B and D, we control for the following extra variables: month of birth, new-born's gender, mother's ethnicity, number of GP practices in the PCT, number of hospitals in the PCT, number of GPs in the PCT, number of specialists doctors in the PCT and the number of midwives working in each hospital where the delivery occurred. The standard errors are clustered at the MSOA Level in Panel A and B, and at the mother level in Panel C and D.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3: Heterogeneous effects of unemployment on different infants' outcomes by Income Deprivation Domain (IDD). Siblings' sample.

	(1) Birthweight FE	(2) LBW FE	(3) VLBW FE	(4) Length gestation FE	(5) Preterm FE	(6) Foetal growth FE
UR	3.08411**	-0.00060	-0.00044	0.00421	-0.00025	0.06740*
	(1.48673)	(0.00079)	(0.00028)	(0.00629)	(0.00094)	(0.03590)
IDD2	-3.58285	0.00130	-0.00077	0.00686	0.00152	-0.16274
	(3.72683)	(0.00175)	(0.00069)	(0.01786)	(0.00228)	(0.10153)
IDD3	-0.26042	0.00124	-0.00124*	-0.02025	0.00309	0.02803
	(3.75465)	(0.00179)	(0.00072)	(0.01835)	(0.00231)	(0.10171)
IDD4	7.53653*	0.00148	-0.00133*	-0.01229	0.00101	0.22734**
	(4.01223)	(0.00196)	(0.00080)	(0.01966)	(0.00248)	(0.10893)
IDD5	18.35641***	0.00007	-0.00159*	-0.02521	-0.00254	0.43979***
	(4.06737)	(0.00203)	(0.00084)	(0.01972)	(0.00250)	(0.10845)
UR×IDD2	-3.21264*	0.00072	0.00028	-0.00785	0.00065	-0.06027
	(1.85094)	(0.00093)	(0.00034)	(0.00795)	(0.00111)	(0.04601)
UR×IDD3	-5.03219***	0.00093	0.00033	-0.00500	0.00088	-0.10757***
	(1.67894)	(0.00086)	(0.00032)	(0.00743)	(0.00104)	(0.04167)
UR×IDD4	-7.62871***	0.00091	0.00057*	-0.00700	0.00126	-0.17665***
	(1.59448)	(0.00083)	(0.00030)	(0.00697)	(0.00100)	(0.03943)
UR×IDD5	-8.23795***	0.00133*	0.00062**	-0.00332	0.00154	-0.18695***
	(1.50098)	(0.00080)	(0.00028)	(0.00642)	(0.00095)	(0.03650)
Observations	2,111,515	2,111,515	2,111,515	1,617,870	1,617,870	1,612,674
Mean of Dep. Var.	3377	0.0536	0.00785	39.13	0.0676	86.08
P-value Test UR×IDD1=UR×IDD2	0.0415	0.408	0.205	0.360	0.638	0.0908
P-value Test UR×IDD1=UR×IDD3	0.00703	0.332	0.170	0.477	0.551	0.0172
P-value Test UR×IDD1=UR×IDD4	0.000321	0.338	0.0687	0.378	0.419	0.000750
P-value Test UR×IDD1=UR×IDD5	0.000125	0.218	0.0524	0.548	0.336	0.000369
P-value Test UR×IDD2=UR×IDD3	0.224	0.770	0.866	0.673	0.789	0.217
P-value Test UR×IDD2=UR×IDD4	0.00177	0.790	0.269	0.893	0.449	0.00131
P-value Test UR×IDD2=UR×IDD5	0.000137	0.343	0.152	0.430	0.231	0.000155
P-value Test UR×IDD3=UR×IDD4	0.0226	0.962	0.303	0.714	0.565	0.0194
P-value Test UR×IDD3=UR×IDD5	0.00151	0.416	0.159	0.724	0.255	0.00220
P-value Test UR×IDD4=UR×IDD5	0.459	0.314	0.774	0.349	0.576	0.632

Note: Standard errors in parentheses. Every column corresponds to a separate regression. Five quintiles of IDD are built using only the Income Deprivation Domain of the 2002 EDI. IDD1 corresponds to the IDD first quintile, the richest, IDD5 corresponds to the IDD fifth quintile, the poorest. Every regression is estimated with mother FE and it includes year FE, LA-specific trends, and the extra controls that are month of birth, new-born's gender number of GP practices in the PCT, number of hospitals in the PCT, number of GPs in the PCT, number of specialists doctors in the PCT and the number of midwives working in each hospital where the delivery occurred. Standard errors are clustered at the mother level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Selection in-utero and heterogeneity by IDD quintiles. Siblings' sample.

	(1) Stillborn FE	(2) Stillborn FE	(3) Female baby FE	(4) Female baby FE
UR	0.00016***	0.00040*	0.00142***	0.00529***
	(0.00006)	(0.00021)	(0.00042)	(0.00193)
IDD2		0.00033		-0.00027
		(0.00055)		(0.00469)
IDD3		-0.00048		0.00115
		(0.00056)		(0.00465)
IDD4		-0.00035		0.00254
		(0.00067)		(0.00495)
IDD5		0.00017		-0.00249
		(0.00067)		(0.00497)
UR×IDD2		-0.00032		-0.00004
		(0.00027)		(0.00239)
UR×IDD3		-0.00005		-0.00355*
		(0.00025)		(0.00214)
UR×IDD4		-0.00013		-0.00331
		(0.00024)		(0.00204)
UR×IDD5		-0.00026		-0.00332*
		(0.00022)		(0.00194)
Observations	2,148,031	2,148,031	2,128,102	2,128,102
R-squared	0.43391	0.43391	0.45744	0.45744
Mean of Dep. Var.	0.00590	0.00590	0.490	0.490
% change	2.721		0.290	
P-value Test UR×IDD1-UR×IDD2		0.101		0.185
P-value Test UR×IDD1-UR×IDD3		0.297		0.0234
P-value Test UR×IDD1-UR×IDD4		0.213		0.0258
P-value Test UR×IDD1-UR×IDD5		0.115		0.0247
P-value Test UR×IDD2-UR×IDD3		0.231		0.0603
P-value Test UR×IDD2-UR×IDD4		0.405		0.0632
P-value Test UR×IDD2-UR×IDD5		0.767		0.0474
P-value Test UR×IDD3-UR×IDD4		0.663		0.865
P-value Test UR×IDD3-UR×IDD5		0.182		0.850
P-value Test UR×IDD4-UR×IDD5		0.402		0.996

Note: Standard errors in parentheses. Every column corresponds to a separate regression. Stillbirth is a dummy equal to 1 if the pregnancy resulted in a stillbirth, and 0 in a live birth. Female baby is a dummy equal to one if the infant is a female, and 0 if a male. In columns (1) and (2) we consider siblings who are either live or stillbirth, while in columns (3) and (4) we only consider live births. Every regression is estimated with mother FE and it includes year FE, LA-specific trends, and the extra controls that are month of birth, number of GP practices in the PCT, number of hospitals in the PCT, number of GPs in the PCT, number of specialists doctors in the PCT and the number of midwives working in each hospital where the delivery occurred. In columns (1) and (2) we also control for new-born's gender. Standard errors are clustered at the mother level.
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Maternal health behaviour and heterogeneity by IDD quintiles. Siblings' sample.

VARIABLES	(1) Alcohol diagnosis	(2) Alcohol diagnosis	(3) Smoking diagnosis	(4) Smoking diagnosis	(5) Reduced alcohol diagnosis	(6) Reduced alcohol diagnosis	(7) Reduced smoking diagnosis	(8) Reduced smoking diagnosis
UR	0.00013***	-0.00045***	0.00625***	-0.00265***	0.00005**	-0.00006	0.00005**	-0.00015**
	(0.00005)	(0.00015)	(0.00024)	(0.00095)	(0.00002)	(0.00004)	(0.00002)	(0.00008)
IDD2		-0.00017		-0.00166		-0.00001		-0.00002
		(0.00039)		(0.00219)		(0.00011)		(0.00018)
IDD3		-0.00096**		-0.00288		-0.00011		-0.00002
		(0.00041)		(0.00229)		(0.00012)		(0.00018)
IDD4		-0.00111**		-0.02841***		0.00006		0.00014
		(0.00049)		(0.00253)		(0.00015)		(0.00021)
IDD5		-0.00069		-0.03809***		-0.00020		-0.00002
		(0.00049)		(0.00261)		(0.00019)		(0.00022)
UR×IDD2		0.00026		0.00266**		0.00003		0.00014
		(0.00019)		(0.00115)		(0.00005)		(0.00009)
UR×IDD3		0.00062***		0.00541***		0.00006		0.00014*
		(0.00018)		(0.00107)		(0.00005)		(0.00008)
UR×IDD4		0.00062***		0.01264***		0.00005		0.00013
		(0.00017)		(0.00103)		(0.00005)		(0.00008)
UR×IDD5		0.00057***		0.01145***		0.00012***		0.00019**
		(0.00016)		(0.00096)		(0.00004)		(0.00008)
Observations	2,128,101	2,128,101	2,128,101	2,128,101	2,128,101	2,128,101	2,128,101	2,128,101
Mean of Dep. Var.	0.00467	0.00467	0.107	0.107	0.000344	0.000344	0.000567	0.000567
% change	2.812		5.836		14.29		8.225	
P-value Test UR×IDD1-UR×IDD2		0.022		0.006		0.223		0.055
P-value Test UR×IDD1-UR×IDD3		0.001		2.47e-05		0.125		0.056
P-value Test UR×IDD1-UR×IDD4		0.0004		0		0.159		0.066
P-value Test UR×IDD1-UR×IDD5		0.001		0		0.018		0.023
P-value Test UR×IDD2-UR×IDD3		0.045		0.004		0.630		0.988
P-value Test UR×IDD2-UR×IDD4		0.040		0		0.749		0.843
P-value Test UR×IDD2-UR×IDD5		0.047		0		0.079		0.427
P-value Test UR×IDD3-UR×IDD4		0.981		0		0.887		0.832
P-value Test UR×IDD3-UR×IDD5		0.719		0		0.133		0.295
P-value Test UR×IDD4-UR×IDD5		0.684		0.0377		0.106		0.183

Note: Standard errors in parentheses. Every column corresponds to a separate regression. Alcohol diagnosis (Reduced alcohol diagnosis) is a dummy equal to one if the mother has at least one diagnosis related to alcohol use where the diagnosis are listed in Panel A, column (1) (column (2)) of Table A.1. Smoking diagnosis (Reduced smoking diagnosis) is a dummy that takes value one if the mother has at least one diagnosis related to smoking where the diagnosis are listed in Panel B, column (1) (column (2)) of Table A.1. Every regression is estimated with mother FE and it includes year FE, LA-specific trends, and the extra controls that are month of birth, number of GP practices in the PCT, number of hospitals in the PCT, number of GPs in the PCT, number of specialists doctors in the PCT and the number of midwives working in each hospital where the delivery occurred. Standard errors are clustered at the mother level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: First antenatal visit and heterogeneity by IDD quintiles. Siblings' sample.

	(1) Week 1st visit FE	(2) Week 1st visit FE
UR	0.07963***	0.31467***
	(0.00736)	(0.03157)
IDD2		-0.40759***
		(0.07564)
IDD3		-0.55429***
		(0.07411)
IDD4		-0.37344***
		(0.08013)
IDD5		-0.60679***
		(0.08282)
UR×IDD2		0.09626**
		(0.03862)
UR×IDD3		-0.03225
		(0.03419)
UR×IDD4		-0.15503***
		(0.03331)
UR×IDD5		-0.18696***
		(0.03179)
Observations	1,712,456	1,712,456
Mean of Dep. Var.	13.54	13.54
% change	0.588	
P-value Test UR×IDD1-UR×IDD2		0.000762
P-value Test UR×IDD1-UR×IDD3		4.12e-08
P-value Test UR×IDD1-UR×IDD4		0
P-value Test UR×IDD1-UR×IDD5		0
P-value Test UR×IDD2-UR×IDD3		2.04e-05
P-value Test UR×IDD2-UR×IDD4		0
P-value Test UR×IDD2-UR×IDD5		0
P-value Test UR×IDD3-UR×IDD4		1.34e-08
P-value Test UR×IDD3-UR×IDD5		0
P-value Test UR×IDD4-UR×IDD5		0.0560

Note: Standard errors in parentheses. Every column corresponds to a separate regression. Week 1st visit corresponds to the pregnancy week when the mother had her first hospital visit. Every regression is estimated with mother FE and it includes year FE, LA-specific trends, and the extra controls that are month of birth, new-born's gender, number of GP practices in the PCT, number of hospitals in the PCT, number of GPs in the PCT, number of specialists doctors in the PCT and the number of midwives working in each hospital where the delivery occurred. Standard errors are clustered at the mother level.
*** p<0.01, ** p<0.05, * p<0.1

Table 7: The effects of unemployment on different infants outcomes in the all babies' and one-baby's samples.

	(1) Birthweight OLS	(2) LBW OLS	(3) VLBW OLS	(4) Length gestation OLS	(5) Preterm OLS	(6) Foetal growth OLS
Panel A: All babies' sample						
UR	1.61362***	-0.00062***	-0.00012	-0.00472	0.00002	0.05571***
	(0.52720)	(0.00021)	(0.00009)	(0.00465)	(0.00039)	(0.01761)
% change	0.0480	-1.084	-1.448	-0.0121	0.0251	0.0650
Mean of Dep. Var.	3364	0.0569	0.00859	39.15	0.0686	85.73
Observations	4,778,918	4,778,918	4,778,918	4,024,253	4,024,253	4,015,434
Panel B: All babies' sample, Extra controls						
UR	1.60387***	-0.00067***	-0.00017*	0.00159	-0.00042	0.03396**
	(0.54974)	(0.00022)	(0.00009)	(0.00312)	(0.00030)	(0.01532)
% change	0.0477	-1.176	-2.030	0.00405	-0.621	0.0396
Mean of Dep. Var.	3364	0.0567	0.00855	39.16	0.0682	85.72
Observations	4,466,443	4,466,443	4,466,443	3,757,446	3,757,446	3,749,161
Panel C: One-baby's sample						
UR	4.28047***	-0.00138***	-0.00034***	-0.00018	-0.00092*	0.10013***
	(0.73841)	(0.00031)	(0.00012)	(0.00579)	(0.00051)	(0.02350)
% change	0.128	-2.331	-3.806	-0.000456	-1.321	0.117
Mean of Dep. Var.	3352	0.0594	0.00897	39.18	0.0697	85.31
Observations	2,377,723	2,377,723	2,377,723	1,999,903	1,999,903	1,994,900
Panel D: One-baby's sample, Extra controls						
UR	4.25165***	-0.00150***	-0.00042***	0.00530	-0.00126***	0.08089***
	(0.78680)	(0.00033)	(0.00013)	(0.00403)	(0.00041)	(0.02164)
% change	0.127	-2.527	-4.695	0.0135	-1.827	0.0949
Mean of Dep. Var.	3353	0.0593	0.00894	39.19	0.0691	85.28
Observations	2,226,684	2,226,684	2,226,684	1,871,253	1,871,253	1,866,556

Note: Standard errors in parentheses. Every coefficient corresponds to a separate regression. Birthweight (in grams), length of pregnancy (in weeks), and foetal growth are continuous variables, while the other outcomes are dummy variables. Every regression is estimated with pooled OLS and includes year FE, MSOA FE and LA-specific trends. The extra controls in Panels B and D are month of birth, new-born's gender, mother's ethnicity, number of GP practices in the PCT, number of hospitals in the PCT, number of GPs in the PCT, number of specialists doctors in the PCT and the number of midwives working in each hospital where the delivery occurred. Standard errors are clustered at MSOA level.
*** p<0.01, ** p<0.05, * p<0.1

Table 8: Fertility and heterogeneity by IDD quintiles. Siblings' sample.

VARIABLES	(1) Parity FE	(2) Parity FE	(3) Mother's age FE	(4) Mother's age FE
UR	-0.00223***	-0.00973***	-0.00001	0.00085
	(0.00039)	(0.00142)	(0.00030)	(0.00122)
IDD2		0.00436		-0.00008
		(0.00358)		(0.00309)
IDD3		0.00101		0.00111
		(0.00374)		(0.00313)
IDD4		0.02164***		-0.00281
		(0.00404)		(0.00330)
IDD5		-0.00416		-0.00692**
		(0.00423)		(0.00337)
UR×IDD2		-0.00502***		-0.00189
		(0.00177)		(0.00152)
UR×IDD3		-0.00167		-0.00187
		(0.00164)		(0.00137)
UR×IDD4		-0.00262*		-0.00063
		(0.00154)		(0.00130)
UR×IDD5		0.00769***		-0.00025
		(0.00145)		(0.00123)
Observations	1,902,718	1,902,718	2,128,102	2,128,102
Mean of Dep. Var.	1.339	1.339	28.51	28.51
% change	-0.167		-2.25e-05	
P-value Test UR×IDD1-UR×IDD2		0.108		0.279
P-value Test UR×IDD1-UR×IDD3		0.00508		0.271
P-value Test UR×IDD1-UR×IDD4		0.0123		0.545
P-value Test UR×IDD1-UR×IDD5		6.12e-10		0.649
P-value Test UR×IDD2-UR×IDD3		0.0260		0.984
P-value Test UR×IDD2-UR×IDD4		0.0867		0.277
P-value Test UR×IDD2-UR×IDD5		0		0.132
P-value Test UR×IDD3-UR×IDD4		0.435		0.179
P-value Test UR×IDD3-UR×IDD5		0		0.0494
P-value Test UR×IDD4-UR×IDD5		0		0.576

Note: Standard errors in parentheses. Every column corresponds to a separate regression. Parity is the order of the pregnancy. Every regression is estimated with mother FE and it includes year FE, LA-specific trends, and the extra controls that are month of birth, new-born's gender, number of GP practices in the PCT, number of hospitals in the PCT, number of GPs in the PCT, number of specialists doctors in the PCT and the number of midwives working in each hospital where the delivery occurred. Standard errors are clustered at the mother level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 9: Fertility and heterogeneity by IDD quintiles. One-baby's sample.

VARIABLES	(1) Parity OLS	(2) Parity OLS	(3) Mother's age OLS	(4) Mother's age OLS
UR	-0.00927***	0.02126**	0.04404***	-0.16132***
	(0.00360)	(0.00981)	(0.00995)	(0.03991)
UR×IDD2		-0.00049		0.05488
		(0.01073)		(0.04086)
UR×IDD3		-0.00431		0.14010***
		(0.00938)		(0.03682)
UR×IDD4		-0.01662*		0.17455***
		(0.00934)		(0.03628)
UR×IDD5		-0.03623***		0.21335***
		(0.00939)		(0.03679)
Observations	1,690,757	1,690,757	2,209,487	2,209,487
R-squared	0.12131	0.12139	0.09757	0.09762
Mean of Dep. Var.	0.984	0.984	29.52	29.52
% change	-0.943		0.149	
P-value Test UR×IDD1-UR×IDD2		0.236		0.00359
P-value Test UR×IDD1-UR×IDD3		0.160		4.51e-05
P-value Test UR×IDD1-UR×IDD4		0.0400		7.20e-06
P-value Test UR×IDD1-UR×IDD5		0.00224		8.36e-07
P-value Test UR×IDD2-UR×IDD3		0.653		0.00423
P-value Test UR×IDD2-UR×IDD4		0.0553		2.43e-05
P-value Test UR×IDD2-UR×IDD5		2.76e-05		3.51e-08
P-value Test UR×IDD3-UR×IDD4		0.0391		0.0712
P-value Test UR×IDD3-UR×IDD5		3.08e-08		7.63e-05
P-value Test UR×IDD4-UR×IDD5		0.000122		0.00304

Note: Standard errors in parentheses. Every column corresponds to a separate regression. Every regression is estimated with pooled OLS and it includes MSOA FE, year FE, LA-specific trends, and the extra controls that are month of birth, new-born's gender, number of GP practices in the PCT, number of hospitals in the PCT, number of GPs in the PCT, number of specialists doctors in the PCT and the number of midwives working in each hospital where the delivery occurred. Standard errors are clustered at the MSOA level.
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Robustness checks on the effects of unemployment on different infants outcomes. Siblings' sample.

	(1) Birthweight	(2) LBW	(3) VLBW	(4) Length gestation	(5) Preterm	(6) Foetal growth
Panel A: Same MSOA, no movers						
	FE	FE	FE	FE	FE	FE
UR	-9.39509***	0.00101*	0.00026	0.01183**	0.00128*	-0.25027***
	(1.41136)	(0.00058)	(0.00023)	(0.00571)	(0.00070)	(0.03828)
% change	-0.278	1.938	3.342	0.0302	1.908	-0.290
Mean of Dep. Var.	3385	0.0522	0.00770	39.12	0.0669	86.28
Observations	1,350,508	1,350,508	1,350,508	1,039,956	1,039,956	1,036,552
Panel B1: Male UR						
	FE	FE	FE	FE	FE	FE
Male UR	-2.20277***	0.00047***	0.00009	-0.00070	0.00055***	-0.04920***
	(0.24307)	(0.00013)	(0.00005)	(0.00115)	(0.00014)	(0.00626)
% change	-0.0652	0.871	1.085	-0.00179	0.810	-0.0572
Mean of Dep. Var.	3377	0.0536	0.00785	39.13	0.0676	86.08
Observations	2,111,515	2,111,515	2,111,515	1,617,870	1,617,870	1,612,674
Panel B2: Female UR						
	FE	FE	FE	FE	FE	FE
Female UR	-6.71516***	0.00102***	0.00020*	-0.00077	0.00177***	-0.15016***
	(0.54400)	(0.00028)	(0.00012)	(0.00250)	(0.00032)	(0.01376)
% change	-0.199	1.912	2.498	-0.00198	2.614	-0.174
Mean of Dep. Var.	3377	0.0536	0.00785	39.13	0.0676	86.08
Observations	2,111,513	2,111,513	2,111,513	1,617,868	1,617,868	1,612,672
Panel C: UR at Local Authority level						
	FE	FE	FE	FE	FE	FE
UR LA	-9.07363***	0.00154***	0.00029	0.03787***	0.00230***	-0.29709***
	(1.18908)	(0.00059)	(0.00025)	(0.00548)	(0.00069)	(0.03035)
% change	-0.269	2.868	3.753	0.0968	3.398	-0.345
Mean of Dep. Var.	3377	0.0536	0.00786	39.13	0.0677	86.08
Observations	2,112,620	2,112,620	2,112,620	1,618,900	1,618,900	1,613,702
Panel D: Bartik IV						
	IVFE	IVFE	IVFE	IVFE	IVFE	IVFE
UR	-5.27041***	0.00067*	-0.00004	0.00029	0.00150***	-0.11431***
	(0.78927)	(0.00040)	(0.00017)	(0.00370)	(0.00046)	(0.02014)
% change	-0.156	1.246	-0.542	0.000749	2.222	-0.133
Mean of Dep. Var.	3377	0.0536	0.00785	39.13	0.0676	86.08
Observations	2,111,508	2,111,508	2,111,508	1,617,863	1,617,863	1,612,667
IV-Fstat	99756	99756	99756	81059	81059	80701

Note: Standard errors in parentheses. Every coefficient corresponds to a separate regression. Birthweight (in grams), length of pregnancy (in weeks), and foetal growth are continuous variables, while the other outcomes are dummy variable. Every regression is estimated with mother FE and it includes year FE, LA-specific trends, and the extra controls that are month of birth, new-born's gender, number of GP practices in the PCT, number of hospitals in the PCT, number of GPs in the PCT, number of specialists doctors in the PCT and the number of midwives working in each hospital where the delivery occurred. Panel D reports the second stage of a 2SLS estimation using the Bartik instrument. In Panel A standard errors are clustered at the MSOA level, while in Panel B1, B2, C and D at the mother level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix A Appendix

Table A.1: Alcohol and smoking related diagnosis.

Panel A: ALCOHOL DIAGNOSIS	(1) Alcohol diagnosis	(2) Reduced alcohol diagnosis
Alcohol dependance	F10	F10.1, F10.2, F10.9
Alcohol polyneuropathy	G62.1	G62.1
Degeneration of nervous system due to alcohol	G31.2	
Alcoholic myopathy	G72.1	
Alcohol cardiomyopathy	I42.6	I42.6
Alcoholic gastritis	K29.2	K29.2
Alcoholic liver disease	K70-K70.4, K70.9	K70.3
Fetal alcohol syndrome	Q86.0	
Foetus and new-born affected by maternal use of alcohol	O35.4, P04.3	
Alcohol-induced chronic pancreatitis	K86.0	
Acute pancreatitis	K85	
Chronic pancreatitis	K86.1	
Epilepsy	G40, G41	
Oesophageal varices	I85, I98.2	
Gastro-oesophageal hemorrhage	K22.6	
Liver cirrhosis, unspecified	K74.3-K74.6, K76.0, K76.9	
Portal hypertension	K76.6	
Alcohol poisoning	X45, Y15, T51.0, T51.1, T51.9	T51.0, T51.1
Suicide by and exposure to alcohol	X65	
Excessive blood level of alcohol	R78.0	
Alcohol-induced pseudo-Cushing's syndrome	E24.4	
Evidence of alcohol involvement determined by blood alcohol content	Y90	
Evidence of alcohol involvement determined by level of intoxication	Y91	
Alcohol use counseling and surveillance	Z71.4	

Panel B: SMOKING DIAGNOSIS	Smoking diagnosis	Reduced smoking diagnosis
Nicotine dependance	F17	
New-born affected by maternal use of tobacco	P04.2	
Toxic effect of tobacco and nicotine	T65.2	
Tobacco use counseling; not elsewhere classified	Z71.6	
Tobacco use not otherwise specified	Z72	
Contact with and exposure to environmental tobacco smoke	Z77.2	
Personal history of nicotine dependence	Z87.8	
Malignant neoplasms of lip, oral cavity and pharynx	C00-C14	C00-C14
Malignant neoplasms of oesophagus	C15	C15
Malignant neoplasms of stomach	C16	C16
Malignant neoplasms of pancreas	C25	C25
Malignant neoplasms of larynx	C32	C32
Malignant neoplasms of trachea, lung bronchus	C33-C34	C33-C34
Malignant neoplasms of cervix uteri	C53	
Malignant neoplasms of kidney and renal pelvis	C64-C65	C64-C65
Malignant neoplasms of urinary bladder	C67	C67
Malignant neoplasms of acute myeloid leukaemia	C92.0	C92.0
Coronary hear disease	I20-I25	I20-I25
Other heart disease	I00-I09, I26-I28, I29-I51	I51
Cerebrovascular disease	I60-I69	I60-I69
Atherosclerosis	I70	I70
Aortic aneurysm	I71	I71
Other arterial disease	I72-I78	
Chronic lower respiratory diseases	J40-J47	J40-J44
Influenza and pneumonia	J10-J11, J12-J18	J10-J11, J12-J18
Respiratory conditions due to smoke inhalation	J70.5	
Prenatal conditions related to smoking	K55.0, P00.0, P01.0, P01.1, P01.5, P02.0, P02.1, P02.7, P07.0-P07.3, P10.2, P22.0-22.9, P25.0-P28.1, P36.0-P36.9, P52.0-P52.3, P77	

Note: These diagnoses come mainly from the following sources: Currie et al. (2015a); Dietz et al. (2010); US Department of Health and Human Services and others (2014), and `https://nccd.cdc.gov/dph_ardi/info/icdcodes.aspx`.

Table A.2: Descriptive statistics by Income Deprivation Domain (IDD)

	Siblings' sample				One-baby's sample			
	IDD Q1		IDD Q5		IDD Q1		IDD Q5	
	Mean (1)	*SD (2)*	Mean (3)	*SD (4)*	Mean (5)	*SD (6)*	Mean (7)	*SD (8)*
Variables								
Outcomes								
Birth weight in grams	3465.578	*543.37*	3302.117	*574.402*	3415.727	*552.103*	3285.23	*578.426*
Prop born below 2500 g	0.038	*0.19*	0.067	*0.251*	0.046	*0.209*	0.072	*0.259*
Prop born below 1500 g	0.005	*0.074*	0.009	*0.096*	0.007	*0.081*	0.01	*0.101*
Lengh of gestation	39.337	*1.89*	39.083	*2.26*	39.34	*1.975*	39.117	*2.301*
Prop born preterm	0.049	*0.216*	0.07	*0.256*	0.056	*0.229*	0.074	*0.261*
Fetal growth	87.923	*12.569*	84.293	*13.479*	86.628	*12.695*	83.784	*13.565*
Variable of interest								
Job-seekers allowance rate	1.354	*1.045*	5.789	*2.349*	1.422	*1.124*	5.894	*2.42*
Controls from HES data								
Prop female babies	0.484	*0.5*	0.5	*0.5*	0.49	*0.5*	0.497	*0.5*
Prof ethnicity White	0.884	*0.32*	0.589	*0.492*	0.804	*0.397*	0.588	*0.492*
Prof ethnicity Indian/ Bangladeshi/Pakistani	0.031	*0.175*	0.184	*0.387*	0.028	*0.166*	0.13	*0.336*
Prof ethnicity Black	0.011	*0.103*	0.112	*0.315*	0.011	*0.106*	0.099	*0.298*
Prof ethnicity Other	0.037	*0.188*	0.096	*0.294*	0.043	*0.202*	0.096	*0.294*
Prof ethnicity Unknown	0.037	*0.189*	0.02	*0.139*	0.114	*0.317*	0.088	*0.283*
External controls								
GPs per 1k pop at LA	0.989	*0.183*	0.963	*0.187*	0.996	*0.198*	0.975	*0.199*
Specialists per 1k pop at LA	0.635	*0.383*	1.02	*0.806*	0.649	*0.4*	1.014	*0.811*
N. GP practices at LA	0.144	*0.032*	0.187	*0.043*	0.146	*0.034*	0.188	*0.044*
N. of hospital at LA	0.008	*0.008*	0.009	*0.004*	0.008	*0.01*	0.008	*0.005*
N. midwives per hospital at LA	141.861	*64.573*	166.814	*76.258*	144.953	*63.814*	166.341	*79.409*
Mechanisms								
Prop stillbirth	0.005	*0.069*	0.007	*0.084*	0.004	*0.061*	0.006	*0.076*
Parity	1.205	*1.137*	1.607	*1.522*	0.846	*1.183*	1.11	*1.466*
Perc Parity 1	0.26	*0.439*	0.229	*0.42*	0.492	*0.5*	0.458	*0.498*
Perc Parity 2	0.445	*0.497*	0.347	*0.476*	0.316	*0.465*	0.262	*0.44*
Perc Parity 3	0.194	*0.396*	0.215	*0.411*	0.117	*0.321*	0.135	*0.342*
Perc Parity >=4	0.101	*0.301*	0.209	*0.407*	0.076	*0.265*	0.145	*0.352*
Prop mothers less than age 20	0.024	*0.153*	0.075	*0.264*	0.036	*0.186*	0.091	*0.287*
Prop mothers between age 20 and 24	0.112	*0.315*	0.277	*0.448*	0.115	*0.319*	0.229	*0.42*
Prop mothers between age 25 and 29	0.241	*0.428*	0.319	*0.466*	0.221	*0.415*	0.277	*0.448*
Prop mothers between age 30 and 35	0.364	*0.481*	0.219	*0.413*	0.333	*0.471*	0.234	*0.423*
Prop mothers greater than 35	0.258	*0.438*	0.109	*0.312*	0.296	*0.457*	0.17	*0.375*
Weeks 1st antenatal visit	13.175	*7.807*	14.722	*8.553*	13.843	*8.632*	15.076	*9.034*
Alcohol diagnosis†	0.003	*0.059*	0.006	*0.076*	0.004	*0.064*	0.006	*0.075*
Reduced alcohol diagnosis†	0.0001	*0.012*	0.001	*0.024*	0.000	*0.016*	0.001	*0.025*
Smoking diagnosis†	0.073	*0.261*	0.132	*0.338*	0.086	*0.280*	0.130	*0.336*
Reduced smoking diagnosis†	0.0004	*0.02*	0.001	*0.027*	0.000	*0.022*	0.001	*0.030*
Observations	301,805		746,231		316,279		670,815	

Note: * The unemployment rate corresponds to the the job-seekers allowance rate. All the statistics refer to the sample of live births, except the proportion of stillbirth. † Alcohol diagnosis (Reduced alcohol diagnosis) is a dummy equal to one if the mother has at least one diagnosis related to alcohol use where the diagnosis are listed in Panel A, column (1) (column (2)) of Table A.1. Smoking diagnosis (Reduced smoking diagnosis) is a dummy that takes value one if the mother has at least one diagnosis related to smoking where the diagnosis are listed in Panel B, column (1) (column (2)) of Table A.1.