Economic conditions and the health of newborns: Evidence from comprehensive register data *

Gerard J. van den Berg † Alexander Paul ‡ Steffen Reinhold §

October 2019

Abstract

We examine whether economic downturns are beneficial to health outcomes of newborn infants in developed countries. For this we use merged population-wide registers on health and economic and demographic variables, including the national medical birth register and intergenerational link registers from Sweden covering 1992–2004. We take a rigorous econometric approach that exploits regional variation in unemployment and compares babies born to the same parents so as to deal with possible selective fertility based on labour market conditions. We find that downturns are beneficial; an increase in the unemployment rate during pregnancy reduces the probability of having a birth weight less than 1,500 grams or of dying within 28 days of birth. Effects are larger in low socio-economic status households. Health improvements cannot be attributed to the parents' own employment status. Instead, the results suggest pathways more general than individual employment.

*We thank the Editor Michele Pellizzari, two anonymous Referees, Anna Sjögren and participants at a seminar at the University of Aarhus and at EEA/ESEM and EALE conferences for useful comments and suggestions. Also, we thank Statistics Sweden and Socialstyrelsen for the use of the micro data registers and Hans-Martin von Gaudecker and Mårten Palme for their efforts in making these data accessible. We thank IFAU Uppsala, in particular Linus Liljeberg and Olof Åslund, for the data on unemployment rates. Financial support from the German Science Foundation DFG Grant RE 3086/1-1 and the European Research Council through Starting Grant No. 313719 is acknowledged. This paper was previously titled: "Economic Conditions, Parental Employment and Health of Newborns".

†Department of Economics, University of Bristol, United Kingdom and IFAU Uppsala, IZA, ZEW and CEPR.

‡Department of Economics and Business Economics, Aarhus University, and TrygFonden's Centre for Child Research, Denmark. Email: alexander.paul@econ.au.dk (corresponding author)

§E.CA Economics, Germany and Max Planck Institute for Social Law and Social Policy (MEA)

1 Introduction

A currently expanding literature examines how up- and downturns of the economy affect the health of newborn children. This research is motivated by evidence that health at birth impacts both health and labour market outcomes later in life (Currie 2009) and that relatively mild shocks in utero can have substantive adverse effects on the individual in the long run (the so-called fetal origins hypothesis, for reviews see Almond and Currie 2011; Almond et al. 2018). Understanding the impact of economic conditions on newborn health as well as the underlying mechanisms is therefore important in order to realize efficiency gains through reallocation of resources to early-life periods. It also contributes to accurately estimating the cost of the business cycle and determining the need for stabilization policies (see Barlevy, 2004, for a review). In addition, to gauge the effectiveness of health-improving interventions after the first year of life, it is useful to improve our understanding of the determinants of early-life health.

Previous work for developing countries has found strong evidence that recessions tend to increase infant mortality, while booms tend to lower it.[1] In contrast with this evidence, it has been suggested that the effect of the cycle differs in developed countries, with newborn health improving in recessions. The pivotal study by Dehejia and Lleras-Muney (2004), using U.S. state-level data, estimates that an increase in the unemployment rate by one percentage point lowers both the infant mortality rate and the incidence of very low birth weight (below 1,500 grams) by 0.5 percent.

There are several reasons for why babies' health suffers less from recessions in developed countries than in developing countries (see also the discussion in Ferreira and Schady 2009). First, while spending on public health care has been shown to decline during downturns in developing countries (Cutler et al. 2002; Paxson and Schady 2005), fiscal policy generally tends to be countercyclical rather than procyclical in developed countries (Lane 2003). Second, recessions are often shorter in developed countries, and given the higher level of health spending, marginal reductions are less severe. Third, credit markets are more widespread, allowing

1. See Cutler et al. (2002) for Mexico, Paxson and Schady (2005) for Peru, Lin (2006) for Taiwan and Bhalotra (2010) for India. Baird et al. (2011) using a dataset from 59 developing countries in Africa, Latin America and Asia, find that a 5 percent reduction in GDP per capita increases the number of infant deaths by 1 to 2 per 1,000 children born. A notable exception is Miller and Urdinola (2010), who document that higher world coffee prices raise infant mortality in Colombia in coffee-growing regions. Higher prices lead to higher income but also to lower time-intensive investments in child health, due to increased labour supply.

mothers to smooth income and thus spending on health care and nutrition.

Studies on effects of economic fluctuations in developed countries on health of the *adult* population confirm that contemporaneous health improves in recessions. Pioneering work by Ruhm (2000) and many subsequent studies provide strong evidence for the procyclicality of the total mortality rate.[2] Several of the channels linking the business cycle to adult health also apply to babies, both in utero and shortly after birth.[3] This includes channels that are related to parental job loss, since downturns give rise to displacements and lower chances of re-employment. Job loss reduces the available income that can be spent on tobacco and alcohol. Smoking and drinking during pregnancy are highly detrimental to newborns' health. It has been shown that these behaviors are less prevalent in downturns (Ruhm 2000; Ruhm and Black 2002). Further, as a result of job loss, the mother's opportunity cost of time decreases, so she may become more engaged in time-intensive activities that benefit babies' health, such as prenatal care, physical exercise or breast-feeding (Miller and Urdinola 2010), and her exposure to hazardous working conditions decreases. One may also consider channels that are not propelled through actual job loss. First, recessions are associated with less traffic and lower air pollution levels Chay and Greenstone (2003). Air pollution, in turn, has been shown to be an important determinant of newborn health (see Currie et al., 2014, for a review of the literature). Second, while job loss has been associated with stress, lower workloads in recessions might reduce job-related stress for those who do not lose their job, and positive spillovers among parents are likely. There is plenty of evidence that stress affects birth outcomes, in particular during the first trimester of pregnancy (Camacho 2008; Torche 2011; Mansour and Rees 2012; Bozzoli and Quintana-Domeque 2014; Foureaux Koppensteiner and Manacorda 2016; Lee and Orsini 2018). Third, economic upturns might be characterized by a shortage of medical staff, resulting in lower availability and quality of prenatal and neonatal care (Stevens et al. 2015). The latter mechanism, however, could also work in the opposite direction if the enlarged tax revenues brought about by upturns are used to increase the quantity and quality of

2. Gerdtham and Ruhm (2006) show that this relationship also holds in a panel of 23 OECD countries. See van den Berg, Gerdtham, et al. (2017) for recent evidence on procyclicality of mortality in the current labour force in Sweden, exploiting regional variation in unemployment rates over time and relating them to outcomes at the individual level. Ruhm (2015) suggests that effect sizes have tapered down over time. In our setting, the observation window is rather small so that we do not expect major changes along those lines.

3. Recently, Page et al. (2019) provided evidence on the cyclical effects on the health of children (aged 0-17 years). They found health to be positively associated with the labour market conditions of men, but negatively with those of women. This supports the view that the business cycle affects health not only in adulthood, but throughout the whole life cycle.

medical staff (Konetzka et al. 2018).

In the light of the importance of the issues at hand, it is perhaps surprising that there is only little evidence for developed countries. Margerison-Zilko (2010) does an extensive literature search and finds about 15 studies, almost all of which concern aggregate data. One major complication in estimating the effect of the cycle is that women who give birth in a recession may systematically differ from those who give birth in a boom. Dehejia and Lleras-Muney (2004) argue that low-educated women – who do not suffer from skill depreciation – prefer to give birth in recessions when the wage they would receive is low. The authors provide evidence that the fraction of low-educated mothers indeed rises in times of high unemployment, at least for white mothers. The effect is reversed for black mothers, a finding that Dehejia and Lleras-Muney (2004) attribute to credit constraints. In this line of reasoning, low-educated black mothers would also prefer to give birth in recessions, but cannot afford to do so since credit constraints prevent them from smoothing income over time. Salvanes (2013) and Aparicio and González (2014) find that low-educated mothers are overrepresented in recessions.

The composition of newborns has also been studied in the literature on long-run health effects of conditions at birth. After all, late-life health problems among cohorts exposed to adverse early-life conditions may be affected by selective fertility in the corresponding birth years. Most of these studies do focus on what are now developed countries; however, the birth cohorts are from years in which governmental social safety nets were largely absent. Some studies examine how exposure relates to birth rates or to the composition of newborns in terms of observed characteristics of the families into which they are born, following the line of reasoning that if such a relation exists then it is also more likely that there are systematic differences between exposed and non-exposed in terms of unobserved characteristics of the families. Van den Berg and Modin (2013) provide an overview of those studies (see e.g. Kåreholt 2001; van den Berg et al. 2009; van den Berg et al. 2011). They all conclude that the composition of newborns does not vary systematically over the business cycle.

One approach to deal with compositional changes over the business cycle is to compare babies born to the same mother at different stages of the cycle. Econometrically, this may be achieved by including mother fixed effects in the model equations, which requires individual-level data. Interestingly, when Dehejia and Lleras-Muney (2004), who study the U.S., control for mother fixed effects using a subsample from California, the positive effects on the health of newborns become all insignificant, suggesting that selection had been the main driver. Other studies of developed countries employing fixed-effects identification strategies also fail to establish a significant relationship with the cycle (Salvanes, 2013, for Norway and Aparicio and González, 2014, for Spain).[4] Most studies of developing countries find their results unaltered when accounting for selection bias (Paxson and Schady 2005; Bhalotra 2010; Baird et al. 2011).

Van den Berg and Modin (2013) consider individual records from Swedish birth cohorts 1915–1929, where birth weight was recorded at birth by health care workers. Note that at that time, Sweden was not yet a developed economy according to today's standards. They find no relationship between the national business cycle and birth weight, both in basic analyses and in fixed-effects analyses with mother-specific fixed effects.Van den Berg, Lindeboom, Popławska, et al. (2017) use family-specific fixed-effects in the analysis of long-run effects of conditions at birth among Dutch birth cohorts around 1850 on individual longevity, and they subsequently examine the distribution of the estimated unobserved family-specific fixed effects over the various birth years. They find no evidence of an association between conditions around birth on the one hand, and the unobserved family-specific "frailty" determinant of longevity on the other hand.

This paper contributes to existing literature in several important ways. First, we shed new light on the relationship between economic conditions and newborn health in developed countries using data from Sweden. The data come from population-wide registers spanning the years 1992 to 2004; they include comprehensive information of infant health and conditions around birth, from the neonatal and patient registers. All information is recorded at the individual level and in real time by professional health care workers. We match these data with monthly local-labour-market unemployment rates which provide indicators of the business cycle. Exploiting geographical variation in unemployment within Sweden, we control for variables that may confound a relation between economic conditions and newborns' health.

Second, we improve upon previous strategies of controlling for selective fertility. The data provide identifiers of the mother and father, which enable us to compare health outcomes of babies born to the same parents. In this way, we control for the possibility that parents select into pregnancy depending on the state of the business cycle. Most existing studies on developed countries could at most use mother fixed effects to address selective fertility. An exception is Aparicio and González

4. In robust specifications with parental and time fixed effects, Aparicio and González (2014) find a negative effect of unemployment only on late fetal death. It is significant at the 10% significance level; however, it vanishes when additionally accounting for province time trends.

(2014), who also use parents fixed effects but have less detailed unemployment information.[5] Unlike all these studies, this paper is the first that controls for selective fertility and finds robust evidence for countercyclical effects on newborn health: Higher unemployment significantly reduces the incidence of neonatal mortality and very low birth weight. Our point estimates suggest that a one percentage point increase in the unemployment rate is associated with a ca. 10% decrease in these health outcomes, but standard errors are quite large. The effect is entirely driven by the unemployment rate of men. We also find evidence for selective fertility over the cycle, which underlines the importance of controlling for parental fixed effects.[6]

Third, we exploit the rich socio-economic and demographic information about the parents to explore some of the underlying mechanisms. First of all, we consider the role of actual parental unemployment, which is more prevalent in times of recessions. This sheds some light on whether health-enhancing activities – due to lower opportunity cost of time – or reduced smoking and drinking – due to lower available income – drive the estimated effects. We find that parental job loss does not play an important role in mediating the beneficial effects of recessions. As a next step, we investigate whether the effects vary by socio-economic status (SES) of the parents. Low-SES parents tend to be hit harder by recessions. We also find that recessions decrease the occurrence of premature birth. Somewhat speculatively, these findings are in line with a pathway through air pollution which is plausibly reduced in recessions and has been shown to disproportionately affect low-SES families and to provoke premature birth in prior work. Our paper also contributes to the literature on birth weight determinants. In particular, the effect sizes on the incidence of low birth weight may be compared to those due to other interventions (Kramer 1987; Currie and Cole 1993; Kaestner and Lee 2005).

This paper is structured as follows: Sections 2 and 3 explain the data and econometric method, respectively. Section 4 presents the results, starting with an analysis of selective fertility. We then report baseline effects on newborn health, followed by

5. Aparicio and González (2014) construct yearly unemployment rates at the province level based on the Labor Force Survey for the second quarter of each year. In contrast, we use monthly unemployment rates at the arguably more meaningful local-labour-market level based on registry data from the public employment office. We can also compute gender- and age-specific rates of unemployment, which we show to have differential effects on newborn health.

6. Tapia Granados and Ionides (2008, 2011) and Svensson and Krüger (2012) consider time series on mortality and economic conditions at the national level for Sweden. As a by-product of their analyses, they find some evidence for a positive association between infant mortality and national-level indicators of the business cycle. Catalano et al. (1999), studying Sweden and using Norway to control for independent variation in newborn health, find a positive relationship between male unemployment and very low birth weight. Yet none of these papers controls for selection into childbirth.

a sensitivity analysis, and an investigation into heterogeneity and potential mechanisms. Section 5 concludes.

2 Data

2.1 Unemployment data from the HÄNDEL register

We start this section with a brief outline of some notable features of Swedish society. Sweden has a large welfare state acting as a social safety net. Every citizen has access to the tax-funded public health care sector. Private health insurance and patient cost-sharing only play a tiny role.[7] Income inequality is among the lowest in the world and consumer credit is widely available. Female labour force participation is relatively high.[8] Sweden has traditionally had a high level of prenatal and neonatal care, as reflected in one of the smallest infant mortality rates worldwide (World Bank 2019b). We therefore suspect that fluctuations in the quality and availability of medical care over the cycle are rather limited. At the same time, there are reasons to suspect that boom years are not particularly detrimental to health either. Specifically, since overtime work is regulated through collective bargaining agreements, the stress caused by overtime hours in booms is limited.

We should point out that our observation window does not include the 2008 recession and its aftermath. However, Sweden experienced a severe downturn in the early 1990s, with GDP per capita shrinking in three consecutive years between 1991 and 1993 (World Bank 2019a).

For the purposes of our study, we construct a dataset from two sources: monthly unemployment data at the municipality level and, secondly, population-wide administrative data on newborn infants and parental characteristics at the individual level. The former are discussed in the current subsection and the latter in the next subsection.

The unemployment data come from the so-called HÄNDEL register created by Swedish public employment offices. HÄNDEL captures all persons in Sweden who register as "openly" unemployed with the employment office. Persons who classify themselves as unemployed in surveys because they are temporarily unemployed (e.g. due to a job change) or expect to be unemployed soon (e.g. due to a short-term

7. See e.g. Tertilt and van den Berg (2015), for a description of the Swedish health care system.

8. According to OECD data, labour force participation among women aged 15–64 years amounted to ca. 78% in Sweden in the period 1990-2004, compared with an OECD average of ca. 58%.

contract or the notification of lay-off), but do not register with the employment office, are not included in HÄNDEL. However, Carling et al. (2001) report that more than 90% of the individuals who are ILO-unemployed according to labour force surveys are also registered as unemployed.

From the HÄNDEL registers starting in January 1992, the number of unemployed individuals by month and municipality can be deduced, stratified by gender, age group (18–24, 18–30, 18–40 and 18–64 years) as well as the interaction of gender and age group. These numbers can then be divided by the corresponding numbers of individuals in the population, to obtain the unemployment-to-population ratio. We simply refer to these as "the" unemployment rates. Unfortunately, the registers at our disposal do not allow for observation of the size of the labour force.

If the labour market that is relevant from the individual's perspective extends to or even centers in a municipality other than the municipality of residence, then the unemployment rate in the municipality of residence is only an incomplete indicator of economic conditions. In fact, an individual can (and might find it optimal for job search to) register with an employment office in a different municipality. To capture spillovers from surrounding areas, we aggregate municipality-level unemployment rates to the local labour market level. This approach also alleviates concerns about measurement error in municipality-level unemployment.[9] We use the definition of local labour markets provided by *Tillväxtanalys* (formerly Nutek), the Swedish Agency for Growth Policy Analysis (Tillväxtanalys 2005). Mainly based on commuting patterns in 2003, this definition divides Sweden into 72 non-overlapping so-called functional analysis regions (FA-regions).[10] The basic idea is to construct regions that include both the place of residence and the place of work for the majority of people. Previous papers using FA-regions are, for example, Eliasson et al. (2012) and Moretti and Thulin (2013). Clearly, the benefits of aggregation to local labour markets must be weighed against the reduced power due to ignoring idiosyncratic variations of economic conditions within smaller regional units. We therefore explore the sensitivity of our results to various degrees of aggregation (Lindo 2015).

The upper panel of Figure 1 illustrates the evolution of the unemployment rate for six randomly selected local labour markets between 1992 and 2004. Reflecting

9. For example, measurement error could arise because an individual moves to another municipality without registering with the new employment office.

10. There are two steps in the formation of FA-regions: First, a municipality is defined as independent if the share of commuters to any other municipality does not exceed 20 percent in the working population and the share of commuters to any single municipality does not exceed 7.5 percent. Second, municipalities that are found not to be independent are merged with connected independent ones to form a FA-region. For more details, see ITPS (2008, pp. 195–196).

the deep recession that occurred in Sweden in the early 1990s, unemployment is relatively high at the beginning of the time period with values of more than 20 percent. Unemployment then sinks to a low around 2001/02 and subsequently rises again. To capture business cycles, we use a detrended version of the unemployment rate stripped of permanent differences across local labour markets and month-specific national shocks as well as seasonal variations. The detrended time series is illustrated in the lower panel of Figure 1. Note that the residual variation in unemployment after detrending is fairly large. For some local labour markets there appear to be secular trends in unemployment towards the end of the time period. It is unclear whether these trends are driven by third factors that might also affect newborn health outcomes or whether they constitute independent variation in unemployment. We check the sensitivity of our results to controlling for local-labour-market-specific time trends in the results section.

Since we are interested in how economic conditions during pregnancy shape birth outcomes, our main measure of unemployment will be the average unemployment rate in the nine-month period following conception, where the measurement of conception is explained in the next subsection. In additional analyses, we estimate effects by trimester and study the impact of periods before and after pregnancy.

2.2 Individual register data

We merge the unemployment data with an individual-level administrative dataset that integrates a number of different registers. The linkage of registers is possible thanks to a unique personal identifier that each individual gets assigned at birth. Because we are interested in the effect of labour market conditions during pregnancy, we use the Vital Statistics register and the Medical Birth register to identify all infants whose month of conception was after January 1992, the earliest month for which we have unemployment data.[11]

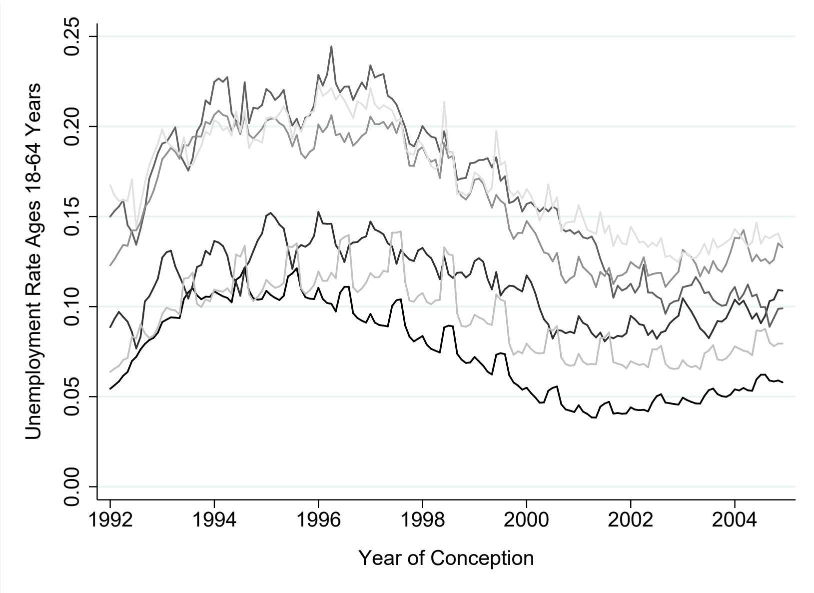
The Medical Birth register also contains data on birth weight, Apgar scores[12] and

11. We define the month of conception to be the month of the first day of the last menstrual cycle. Since this variable is sometimes missing or inaccurate, we also construct the month of conception using the more accurate variables birth month and gestation length. If the month of conception as given in the data differs from the constructed month by more than 1 month or is entirely missing, we replace it with the constructed month. If gestation length is missing we only retain the month of conception if its implied gestation length – given birth month – ranges between 5 and 11 and set it to missing otherwise. We ignore birth records for which both month of conception and gestation length are missing. Following this procedure, the share of birth records that cannot be assigned a month of conception is equal to 2.5%.

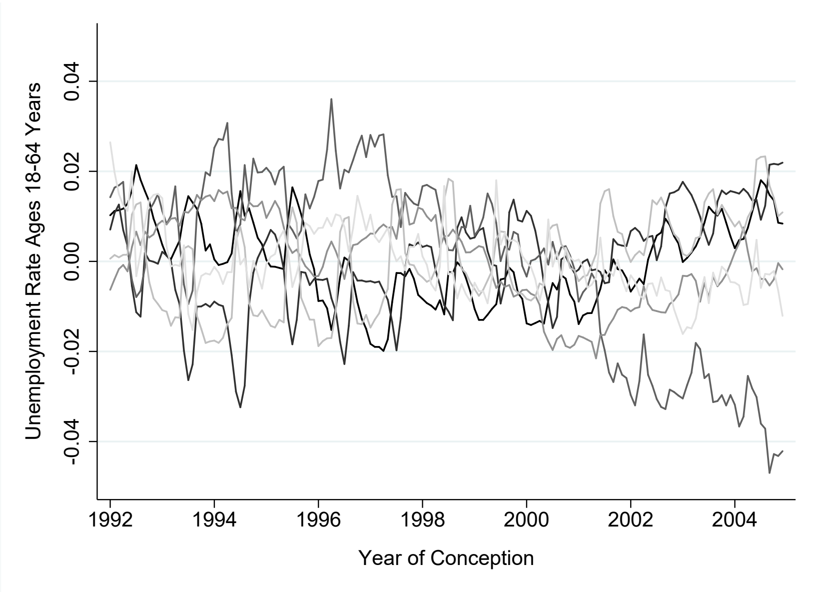
12. The Apgar score is a summary measure for the health of newborn infants. It ranges between 0

Figure 1: Unemployment rate (18–64 years) for a few local labour markets

Raw unemployment



Deviations in unemployment



Notes: Monthly unemployment rates (18–64 years) for six randomly selected local labour markets. Deviations in unemployment are after detrending the unemployment rate by taking out permanent differences across local labour markets as well as month-specific national shocks, which account for countrywide fluctuations in unemployment such as seasonal variation.

neonatal mortality, i.e. whether a newborn infant died within 28 days after birth.

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and 10, with higher values indicating better health. It is taken 1, 5 and 10 minutes after birth.

For infant mortality, i.e. deaths within a year of birth, we add information from the Cause of Death register, which includes deaths up until 2005, so that infant mortality is observable up until 2004. Finally, the Medical Birth register also indicates the mother's municipality of residence, which – together with the month of conception – allows us to determine local labour market conditions around the time of birth.

Where municipality of residence is not available in the Medical Birth register, we take it from the mother's socio-economic and demographic data records – the so-called LISA register. This register also provides maternal income, earnings, unemployment benefits, marital status and education. The same variables are available for the father too. However, since the Medical Birth register only indicates the mother but not the father, for fathers we have to rely on the Intergenerational Link register, which does not provide father links for children born in 2005 and later. This restriction implies that the inclusion of parents fixed effects in the empirical analysis limits the sample to the time period 1992 to early 2004.[13] To determine the birth order of a newborn infant, we count the number of children that the mother has given birth to in the past. Finally, we match records from the National Inpatient register to obtain information about hospitalizations of both the mother during pregnancy and the child after birth.

2.3 Sample

The starting point for our sample is the universe of newborn infants that were conceived in 1992 or later and born in Sweden in 2004 or earlier, as dictated by the availability of unemployment data and paternal information (see the previous subsection). We apply a number of restrictions to obtain the final sample: First, we disregard all parents from those municipalities that did not remain the same over the time period we study. More specifically, there were four municipalities that were each split into two.[14] Besides measurement error in unemployment rates due to employment offices not following the splits carefully, there might be idiosyncratic shocks to affected municipalities. Therefore, for each split, we ignore both the municipality that retained the original name and the one that was newly created. Second, we focus on singleton births. Multiples such as twins and triplets have typically quite low birth weight, which adds noise to the analysis. Moreover, since labour market conditions during pregnancy are identical for multiples, within-

13. Babies conceived later in 2004 are born in 2005, so that we do not have father information.

14. The splits were as follows: Bollebygd broken out of Boras (1995), Nykvarn broken out of Södertälje (1999), Knivsta broken out of Uppsala and Lekeberg broken out of Örebro (both 2003).

multiples comparisons are not informative for the relationship between economic conditions and newborn health outcomes. Finally, we limit attention to mothers who were aged above 18 for comparability with Dehejia and Lleras-Muney (2004), who imposed the same restriction.

After excluding infants whose father is still unknown (to us), which applies to about 6% of births, we are left with 874,584 babies conceived between 1992 and early 2004. They are born to 590,543 distinct pairs of parents. A woman might be part of several parent pairs if she has children with different partners. Of women who have at least two children in the time period we study, 14.9% have them with two or more different partners. The corresponding number for men is a little smaller (12.3%), but recall that we exclude babies for whom the father is unknown.

In an econometric model with parents fixed effects, identification rests on parent pairs with at least two births. There are 245,036 parent pairs in the sample that fulfill this criterion (529,078 births). Table 1 provides descriptive statistics for both the whole sample and the regression sample that we ultimately use in the analysis. The two samples differ slightly in terms of observable characteristics. In the regression sample, first-born children – some of whom do not (yet) have younger siblings – and third-order or higher children – some of whom have older siblings too old to be included – are underrepresented relative to second-born children. Moreover, since we focus on couples having multiple children, the parents in the regression sample are more often married and higher educated on average.

The main health outcomes of interest in this paper are neonatal mortality (death within 28 days of birth) and very low birth weight (birth weight less than 1,500 grams, VLBW). The incidence of these variables is relatively low. In the regression sample, only about 0.22% (2.2 out of 1,000 infants) suffer from neonatal mortality and about 0.43% (4.3 out of 1,000 infants) have a birth weight less than 1,500 grams. The slightly higher rate of neonatal mortality in the regression sample than in the whole sample (0.17%) cannot be explained by the aforementioned differences in observable characteristics. It is plausible that parents experiencing the death of a newborn child are more likely to have another child and to have it sooner, thus entering the regression sample at a higher rate. To ease interpretation of the estimated effects, we scale up neonatal mortality and very low birth weight to express them as per 1,000 infants in the regressions below.

Table 1: Summary statistics by sample

	Whole sample			Regression sample		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Neonatal mortality	0.0017	0.0409	874,584	0.0022	0.0466	529,078
Infant mortality	0.0027	0.0519	874,584	0.0035	0.0591	529,078
Weight (in grams)	3,583.7353	564.2316	871,541	3,605.6433	552.5971	527,178
Weight $<$ 1,500 grams (VLBW)	0.0052	0.0717	871,541	0.0043	0.0657	527,178
Gestational age (in days)	278.9414	12.6664	874,462	279.0808	12.2125	529,010
Gestational age $<$ 37 weeks	0.0494	0.2168	874,462	0.0453	0.2079	529,010
Gestational age $<$ 32 weeks	0.0064	0.0799	874,462	0.0055	0.0737	529,010
Small for gestational age	0.0211	0.1438	870,369	0.0180	0.1330	526,467
Apgar score (5 min) $<$ 5	0.0057	0.0750	867,894	0.0054	0.0734	524,959
Unemployment 18-40 years - month	0.1378	0.0534	874,584	0.1386	0.0531	529,078
Unemployment 18-40 years - pregnancy	0.1376	0.0529	874,584	0.1370	0.0523	529,078
Unemployment 18-64 years - pregnancy	0.1086	0.0347	874,584	0.1092	0.0346	529,078
Unemployment men 18-40 years - pregnancy	0.1443	0.0587	874,584	0.1423	0.0575	529,078
Unemployment women 18-40 years - pregnancy	0.1305	0.0486	874,584	0.1313	0.0487	529,078
Birth Order						
1	0.4195	0.4935	874,584	0.3806	0.4855	529,078
2	0.3738	0.4838	874,584	0.4336	0.4956	529,078
3	0.1467	0.3538	874,584	0.1310	0.3374	529,078
4	0.0412	0.1987	874,584	0.0360	0.1864	529,078
Mother's age						
Below 25 years	0.1981	0.3985	874,584	0.2019	0.4014	529,078
25-35 years	0.7120	0.4529	874,584	0.7307	0.4436	529,078
Above 35 years	0.0900	0.2862	874,584	0.0675	0.2508	529,078
Mother's marital status						
Single	0.5763	0.4942	873,842	0.5659	0.4956	528,878
Married	0.3875	0.4872	873,842	0.4122	0.4922	528,878
Divorced	0.0362	0.1869	873,842	0.0219	0.1463	528,878
Mother's education						
Primary and lower secondary	0.0684	0.2524	850,651	0.0559	0.2298	522,248
Secondary education and vocational	0.5673	0.4955	850,651	0.5630	0.4960	522,248
Graduate and postgraduate	0.3643	0.4812	850,651	0.3811	0.4856	522,248
Mother's country of birth						

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Sweden	0.9599	0.1962	874,577	0.9664	0.1801	529,076
Developed countries	0.0211	0.1436	874,577	0.0174	0.1308	529,076
Developing countries	0.0190	0.1366	874,577	0.0162	0.1261	529,076
Father's age						
Below 25 years	0.1017	0.3022	874,584	0.0984	0.2979	529,078
25-35 years	0.6952	0.4603	874,584	0.7273	0.4453	529,078
Above 35 years	0.2032	0.4023	874,584	0.1743	0.3793	529,078
Father's marital status						
Single	0.5721	0.4948	872,711	0.5585	0.4966	528,208
Married	0.3893	0.4876	872,711	0.4134	0.4924	528,208
Divorced	0.0385	0.1925	872,711	0.0281	0.1652	528,208
Father's education						
Primary and lower secondary	0.1165	0.3209	860,821	0.1015	0.3020	525,351
Secondary education and vocational	0.5698	0.4951	860,821	0.5653	0.4957	525,351
Graduate and postgraduate	0.3136	0.4640	860,821	0.3332	0.4713	525,351
Father's country of birth						
Sweden	0.9572	0.2025	874,523	0.9625	0.1899	529,060
Developed countries	0.0241	0.1535	874,523	0.0215	0.1450	529,060
Developing countries	0.0187	0.1355	874,523	0.0160	0.1254	529,060
Father's unemployment: No wage	0.0817	0.2740	873,178	0.0719	0.2584	528,390
Mother's unemployment: No wage	0.1041	0.3054	874,566	0.1030	0.3040	529,075
Father's unemployment: No reimbursements	0.0540	0.2259	802,043	0.0445	0.2063	495,874
Mother's unemployment: No reimbursements	0.0953	0.2937	803,340	0.0930	0.2904	496,523

Notes: Summary statistics for selected variables by sample. The regression sample focuses on parents with at least two births, see Section 2.3. Month unemployment is the unemployment rate in the month of conception. Pregnancy unemployment is the average unemployment rate in the nine months following conception. Developed countries include EU-15 (excl. Sweden), Norway, North America and Oceania. Developing countries include the rest of Europe, Africa, South America, Asia and Soviet Union. "No wage" takes on the value 1 if a gross wage of zero is reported in the statement of income submitted to the tax agency. "No reimbursements" takes on the value 1 if no work-related reimbursements are received.

3 Econometric specification

Before studying how economic conditions impact newborn health outcomes, we first investigate how the composition of parents of newborns changes over the business cycle due to selective fertility. If parental characteristics are associated with newborn health, then compositional changes induced by the cycle confound the relationship between economic conditions and newborn health. Analyzing selective fertility yields insights into the sources of this potential bias. It also sheds light on the determinants of fertility decisions, which are of independent interest.[15]

We study selective fertility over the business cycle using the following equation, which in the next step we adapt to estimate health effects. Specifically, in line with the literature,

$$Y_{lt} = \alpha + \beta \text{Unemployment rate}_{lt} + \delta_t + \lambda_l + \theta_l t + \varepsilon_{lt}, \quad (1)$$

where Y_{lt} is an outcome relating to all births conceived in month t by parents living in local labour market l. Specifically, Y_{lt} is the birth rate – the number of births per 1,000 women aged 18–49 years – or the share of parents belonging to some demographic subgroup, such as low-educated individuals. The parameter β captures the effect of economic downturns, as measured by the unemployment rate, on the outcome. δ_t are month-fixed effects that capture nationwide fluctuations in unemployment in the month of conception. These are included to control for third factors that affect economic conditions (such as labour market policies or long-run increases in educational attainment) and also correlate with newborns' health outcomes. As a result, the identifying variation in unemployment stems from regional variation in transitory economic conditions. The λ_l are local-labour-market fixed effects that account for persistent differences in unemployment across local labour markets, as illustrated in Figure 1. In some specifications, we also allow for local-labour-market-specific linear time trends ($\theta_l t$). These may help reduce omitted variable bias further but come at the cost of increasing estimation uncertainty.

Given that local labour markets vary considerably in population size and a few small regions do not encounter a single birth in some months, we use the number of

15. Rather than arising from deliberate fertility decisions, differential fertility by demographic group might also arise due to a differential propensity for fetal loss (Bhalotra 2010) or differential mobility to low-unemployment regions (Lindo 2015). The former may be more prevalent in developing countries. We return to this issue below.

births as weights in the regression. This also makes our results more comparable with the individual-level analysis later on. To account for serial correlation in the error term, we cluster standard errors at the level of the local labour market.

To estimate health effects, we adopt the following equation which is similar in spirit to equation 1, except that it is specified at the individual (newborn) level. Accordingly, we include parental fixed effects. This results in the key model equation of the paper,

$$Y_{it} = \alpha + \beta \text{Unemployment rate}_{l(i)t} + \delta_t + \rho_i + \theta_{l(i)} t + X_i' \gamma + \varepsilon_{it}. \tag{2}$$

Here, i refers to a pair of parents consisting of mother and father. Y_{it} is a health outcome such as whether the infant has a very low birth weight ($<$ 1,500 grams) or suffers from neonatal mortality (death within 28 days of birth).[16] By including parental fixed effects ρ_i, we essentially identify β by comparing babies born to the same parents but at different stages of the business cycle. This accounts for selective fertility over the cycle. In sensitivity analyses, we include parental characteristics that may vary across siblings, such as marital status and birth order (X_i). We once again cluster standard errors at the level of the local labour market.

Finally, we observe that a small fraction of parents move to a different local labor market between births. If the decision to move is driven by factors that depend on the economic conditions in a local labour market and also affect newborn health, then our estimate of β might be biased. We address this endogeneity problem by assigning the municipality of the mother's first observed birth during the sample period to all later-born children. This intent-to-treat approach implies that ρ_i absorbs local-labour-market fixed effects since all births belonging to the same parents are attributed to the same local labour market. This approach also enables us to cluster standard errors at the level of the local labour market in equation 2. For the sake of consistency, we use the same reassignment of municipalities before aggregating micro data to the local-labour-market level to estimate equation 1.

16. In specifying linear probability models rather than binary choice models such as logit or probit we follow the literature, but we also test a logit specification as part of the robustness checks.

4 Results

4.1 Setting the stage: preparatory analyses on the relevance of selection

We start with estimating the effect of downturns on the birth rate – defined as the number of births per 1,000 women aged 18–49 years in the overall population. Here we use the overall unemployment rate among individuals aged between 18 and 64 years in the month of conception. Recall from subsection 2.3 that the sample in the health regressions below only includes babies conceived in 1992 or later and born in Sweden in 2004 or earlier. Consistent with this restriction, we thus focus in this exercise on months of conception between January 1992 and March 2004.

Table C.1 in the appendix shows that recessions have no significant effect on the overall birth rate. When we stratify the analysis by parental characteristics, we find a negative impact on the rate of parents that are young, low-educated and Swedish. Moreover, there is a positive effect on the birth rate among high-educated and married parents and parents from developing countries. Specifically, a 1-percentage-point increase in the unemployment rate implies a rise in the birth rate among mothers from developing countries of about 7-8%. Table C.2 shows that results are similar when using the average unemployment during pregnancy, rather than unemployment in the month of conception.

We investigate the effect of the cycle on the composition of births more directly by regressing shares of demographic groups on unemployment (see Table 2). By comparing Tables C.1 and 2 we see that changes in birth rates do not always result in notable changes in the composition. There is a significantly negative effect on the share of low-educated mothers as well as married and high-educated fathers, and a significantly positive effect on the share of mothers that are divorced and parents that come from developing countries. When using the average unemployment rate during pregnancy (see Table C.3 in the appendix), we additionally find that the share of single parents significantly increases while the negative effect on high-educated fathers disappears.[17]

17. Our findings are consistent with some findings in existing studies on the compositional impact of the cycle in recent years (see the references in Section 1). In particular, similar to the Norwegian study by Salvanes (2013), we observe that the share of married mothers tends to decrease with higher unemployment. In agreement with Salvanes (2013) and Aparicio and González (2014), we fail to detect a clear pattern in parental age. However, Dehejia and Lleras-Muney (2004) find fewer young- and more medium-aged mothers in recessions in the United States, and Lindo (2015), also using U.S. data, reports an increase in teen births in recessions. Finally, we find low-educated mothers to be

In each of the Tables C.1 and 2, we conduct significance tests for a large number of regression estimates. This involves the risk that some of our significant findings are actually false positives. Note, however, that 25% and 14% of all estimates are significant at the 5% level in Tables C.1 and 2, respectively, many more than would be expected by chance. This suggests that the business cycle does cause systematic changes in the composition of birth cohorts.

Table 2: Effect of unemployment in month of conception on composition of birth cohorts nine months later

	Mother		Father	
	Baseline	With trends	Baseline	With trends
Birth Order 1	-0.0224	-0.0450		
	(0.0749)	(0.1004)		
% change	-0.06%	-0.12%		
Birth Order 2	-0.0211	0.0852		
	(0.0837)	(0.0892)		
% change	-0.06%	0.23%		
Birth Order 3	0.0266	-0.0465		
	(0.0680)	(0.0725)		
% change	0.16%	-0.28%		
Birth Order 4	-0.0112	-0.0243		
	(0.0370)	(0.0328)		
% change	-0.21%	-0.45%		
Age - Below 25 years	-0.0113	0.0132	0.0010	0.0025
	(0.0886)	(0.1011)	(0.0655)	(0.0627)
% change	-0.05%	0.05%	0.01%	0.02%
Age - 25-35 years	0.1309	-0.0118	0.0519	0.0353
	(0.1360)	(0.1090)	(0.0813)	(0.0786)
% change	0.19%	-0.02%	0.08%	0.05%
Age - Above 35 years	-0.1196	-0.0014	-0.0529	-0.0378
	(0.0858)	(0.0486)	(0.0700)	(0.0704)
% change	-1.41%	-0.02%	-0.26%	-0.19%
Marital status - Single	0.3293	0.0704	0.3351	0.0935
	(0.2440)	(0.1212)	(0.2195)	(0.1186)
% change	0.52%	0.11%	0.53%	0.15%
Marital status - Married	-0.4328	-0.1496	-0.3771	-0.1094
	(0.2633)	(0.1313)	(0.2274)	(0.1150)
% change	-1.32%	-0.46%	-1.15%	-0.33%
Marital status - Divorced	0.1035**	0.0792*	0.0420	0.0159
	(0.0329)	(0.0373)	(0.0367)	(0.0374)
% change	2.67%	2.04%	1.14%	0.43%
Education - Primary and lower secondary	-0.0202	-0.1612**	0.1478	-0.0197
	(0.0701)	(0.0606)	(0.0935)	(0.0767)
% change	-0.29%	-2.30%	1.22%	-0.16%
Education - Secondary education and vocational	0.1695	0.0919	0.1931	0.0651
	(0.1577)	(0.0828)	(0.1749)	(0.0913)
% change	0.27%	0.14%	0.29%	0.10%
Education - Graduate and postgraduate	-0.1492	0.0694	-0.3409**	-0.0454
	(0.1765)	(0.0871)	(0.1315)	(0.1145)
% change	-0.50%	0.23%	-1.64%	-0.22%

Continued on next page

underrepresented in recessions, which is in line with the work by Bhalotra (2010) for India, but in contrast to several studies of developed countries Dehejia and Lleras-Muney (2004), Salvanes (2013), and Aparicio and González (2014).

Country of birth - Sweden	-0.1321	-0.0295	-0.2099*	0.0045
	(0.1038)	(0.0527)	(0.1003)	(0.0459)
% change	-0.14%	-0.03%	-0.22%	0.00%
Country of birth - Developing countries	0.1143**	0.0529*	0.1385**	0.0094
	(0.0423)	(0.0233)	(0.0535)	(0.0193)
% change	11.88%	5.49%	18.00%	1.22%
Country of birth - Developed countries	0.0178	-0.0234	0.0715	-0.0139
	(0.0685)	(0.0441)	(0.0557)	(0.0346)
% change	0.80%	-1.06%	3.14%	-0.61%

Notes: Each coefficient comes from a separate OLS regression of the share of infants with the same month of conception in a given subgroup on the unemployment rate in the age group 18-64 years in the month of conception. Percentage changes divide the unemployment effect by the mean level of the outcome in the observations used in the regression. Both coefficients and percentage changes are for a 1-percentage-point increase in the unemployment rate, but coefficients are scaled up by 100 to express them in percentage points. Sample includes months January 1992 to March 2004. Controls are month fixed effects, local-labour-market fixed effects and local-labour-market-specific linear time trends where indicated. Regressions are weighted by the number of births. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

In Section A in the online appendix, we provide evidence that parental characteristics are indeed correlated with newborns' health. Although the relation between economic conditions and newborn health might be biased as a result, we also show that the direction of the bias is unclear and that selective fertility based on *observable* characteristics plays only a negligible role in generating the relation. However, there may be unobserved variables that govern fertility over the cycle, potentially leading to fluctuations in aggregate newborns' health. This is of course why, in the key analyses in the subsequent subsections, we include parental fixed effects. In doing so, we control for time-invariant parental characteristics, both observed and unobserved.[18]

Section A also zooms in on the subset of parents that have at least one child with VLBW or neonatal mortality and shows that there is enough demographic variation within this group to allow for heterogeneity analysis, which we will turn to when discussing mechanisms (see subsection 4.5). Finally, Section A discusses the role of the selectivity of abortions over the cycle using a dataset from one Swedish region (Scania (in Swedish: Skåne); see Tertilt and van den Berg 2015; Nilsson and Paul 2018).

18. Selection into pregnancy might also occur independently of the cycle. If a disproportionate number of women from a certain demographic group become pregnant and give up their jobs in response to pregnancy, then this generates a mechanical shift in the unemployment rate that will be correlated with the level of newborn health specific to this group. Note that also this type of selective fertility will be captured by parental fixed effects. Moreover, note that maternal leave laws exist in Sweden, meaning that women on maternal leave are not counted as unemployed and even encouraged to work during most of the pregnancy so as to maximize the replacement rate while on leave.

4.2 Baseline effects on newborns' health

We next turn to the micro-level analysis of how unemployment affects newborn health. We estimate versions of equation 2, which controls for parents fixed effects to address selective fertility. The baseline results are presented in column 1 of Table 3.

We present estimates for our preferred indicator of economic conditions, which is the unemployment rate at the local labour market level among men aged 18-64 years, averaged across the nine months following conception. Alternative indicators will be discussed in subsection 4.3.

There is a negative and significant effect of recessions on both very low birth weight and neonatal mortality.[19] The size of the effect is quite large. A one-percentage point (= 0.01) increase in the unemployment rate is associated with an 11 percent decrease in both very low birth weight and neonatal mortality. This is an order of magnitude larger than the health effects implied by compositional changes with respect to some observable variables such as marital status computed in subsection 4.1. Hence, our results cannot be driven by fluctuations in these variables.

The estimates are also an order of magnitude larger than comparable estimates from earlier literature, which ranged between 0.5 and 0.7% for very low birth weight and between 0.2 and 0.6% for neonatal mortality (Dehejia and Lleras-Muney 2004; Lindo 2013; Aparicio and González 2014). However, this comparison of point estimates is misleading for two reasons: First, the standard errors of our estimates are quite large, and 95% confidence intervals suggest that the effects could in fact be as small 1-2%, much closer to earlier findings. Second, our estimates, unlike the others, are based on regressions that controls for parents fixed effects, which may give rise to stronger effect sizes. Table C.6 in the appendix illustrates the importance of controlling for parents or mother fixed effects. It shows that omitting them and instead only including local-labour-market fixed effects renders the estimates smaller and insignificant (columns 2 and 7).[20]. This suggests that those parents and mothers who select into pregnancy when unemployment is high tend to have sicker children, thus counteracting the positive impact on health.[21]

19. Coefficients are for a one-percentage point (= 0.01) increase in the unemployment rate and scaled up by 1,000 to improve readability.

20. Our results differ from those of Dehejia and Lleras-Muney (2004), who do not find significant effects once they control for mother fixed effects. However, they can only conduct this exercise for a Californian subsample, which yields effect sizes different from the national sample even when not controlling for mother fixed effects.

21. Furthermore, the effect on neonatal mortality becomes insignificant when only controlling for mother rather than parents fixed effects (column 3 of Table C.6). This is primarily driven by including stillbirths and adding mothers whose child's father is unknown. Stillbirths are known to be caused

Table 3: Baseline effect of unemployment on health

		LLM-specific time trends		
	Baseline	Linear trends	Quadratic trends	Maternal controls
Weight < 1,500 grams (VLBW)	(1)	(2)	(3)	(4)
Unemployment	-0.577**	-0.652**	-0.555	-0.570**
	(0.209)	(0.244)	(0.322)	(0.205)
% change	-13.39%	-15.12%	-12.88%	-13.23%
- Upper bound	-3.90%	-4.02%	1.79%	-3.92%
- Lower bound	-22.88%	-26.22%	-27.55%	-22.55%
Mean × 1,000	4.308	4.308	4.308	4.308
N	525,728	525,728	525,728	525,528
Neonatal mortality	(1)	(2)	(3)	(4)
Unemployment	-0.246*	-0.259	-0.157	-0.233*
	(0.107)	(0.139)	(0.211)	(0.110)
% change	-11.31%	-11.90%	-7.22%	-10.71%
- Upper bound	-1.64%	0.57%	11.74%	-0.81%
- Lower bound	-20.98%	-24.36%	-26.17%	-20.60%
Mean × 1,000	2.177	2.177	2.177	2.176
N	529,078	529,078	529,078	528,878

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the average unemployment rate among 18-64-year-old men in the nine months following conception. Percentage changes divide the unemployment effect by the mean level of the outcome in the observations used in the regression. Both coefficients and percentage changes are for a 1-percentage-point increase in the unemployment rate. Coefficients and means are scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Column 4 additionally controls for birth order, a third-order polynomial in mother's age, and mother's marital status. Upper/lower bound refer to the upper and lower bound of the 95% confidence interval. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

We test the robustness of this estimate by allowing for labour-market-specific time trends in columns 2 and 3. As it turns out, adding time trends affects the estimate only slightly. However, the residual variation in unemployment shrinks considerably, as reflected in enlarged standard errors, especially with quadratic trends. For this reason and because regional time trends are more likely to emerge for a longer time span – ours being relatively short compared with e.g. Dehejia and Lleras-Muney (2004) – our preferred specification will not include time trends in the following.

In column 4, we additionally control for birth order, a third-order polynomial

by idiosyncratic factors, so including them attenuates the effect of economic conditions. Mothers whose child's father is unknown appear to have sicker children in recessions, thereby diminishing the positive effect of recessions on newborn health.

in mother's age, and mother's marital status. These variables might help reduce bias in the estimation, but note that inference is hampered by the identification challenge posed by the simultaneous inclusion of mother's age, parental fixed effects and calendar time. With this in mind, coefficients actually hardly change with the inclusion of these variables. We therefore do not include them in our preferred specification.

Finally, Table C.7 in the appendix explores the sensitivity of our results to a logit specification. For both the model with and without fixed effects, we obtain estimates very similar to the ones from our baseline linear probability specification.

4.3 Sensitivity analysis

4.3.1 Timing of effects

For our estimates to capture the health effect of the business cycle, we expect to find significant correlations with unemployment during, but not so much before or after pregnancy. To examine this, Table 4 also presents regressions for the 9-months periods before and after pregnancy. It is reassuring to see that the estimates become generally insignificant and smaller in absolute size as we move away from pregnancy. An exception is the significant coefficient on neonatal mortality in the 9 months before pregnancy. We investigate this using Table C.8 in the appendix, which splits the analysis further by trimester of pregnancy. The significant effect is driven by the 7-9 months before birth, a finding that we presume is random. The general observation that the estimates are also negative before and after pregnancy is not surprising in light of the strong correlation in the unemployment rate over time. Overall, Table 4 justifies our choice of using the average unemployment rate during pregnancy as our baseline indicator of the cycle.

Table C.8 also reveals that effects are not substantially different across the three trimesters. Therefore, we cannot conclude that the cyclical sensitivity of newborn health varies within pregnancy.[22]

22. This is also confirmed by Table C.9 in the appendix, where we simultaneously include all three trimesters in the same regression. Given that unemployment exhibits high serial correlation, it does not come as a surprise that none of the individual coefficients is any longer significant.

Table 4: Effects by 9-months-period before, during or after pregnancy

VLBW	*Before* pregnancy: 10-18 months (1)	*Before* pregnancy: 9 months (2)	*During* pregnancy (3)	*After* pregnancy: 9 months (4)	*After* pregnancy: 10-18 months (5)
Unemployment	-0.082	-0.338	-0.577**	-0.353	-0.246
	(0.238)	(0.226)	(0.209)	(0.200)	(0.200)
Neonatal mortality	(1)	(2)	(3)	(4)	(5)
Unemployment	-0.051	-0.318**	-0.246*	-0.182	-0.251
	(0.156)	(0.123)	(0.107)	(0.127)	(0.145)

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among 18-64-year-old men. Coefficients are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

4.3.2 Male and female unemployment

Table 5 investigates whether male and female unemployment affect health outcomes differently. It shows that the effect of downturns is entirely driven by male unemployment, for which coefficients are larger and more precisely estimated. Male unemployment is typically a better proxy for the business cycle than female unemployment. One reason is that men are over-represented in the private sector, where employment is sensitive to the cycle, rather than the public sector, where employment is more stable. Using annual county-level GDP data for the period 2000–2011, we also find that in Sweden male unemployment is more strongly related to GDP than female unemployment. Moreover, note from Table 1 that the standard deviation of female unemployment is lower than that of male unemployment. Because male unemployment appears to be a better indicator of the business cycle, we will focus on it in the following. We return to this point in subsection 4.6.1, when discussing mechanisms.[23]

23. Some studies emphasize the differential effect of male and female employment on fertility and child outcomes (Schaller 2016; Page et al. 2019). Closest to our paper is Page et al. (2019), who use gender-specific shift-share employment indices to examine the effect of labor demand shocks on child (rather than newborn) health. They find that female employment rates affects child health negatively, while the male employment rates have beneficial effects. The authors explain these findings by providing evidence that higher female employment leads mothers to reduce their time spent with the child, while male employment increases health care utilisation and insurance coverage. Especially the latter effect is unlikely to be present in a country like Sweden that offers cheap health care to everyone independent of employment (see subsection 2.1). More generally, the distinction between male and female employment appears to be important when the effect of economic downturns operates via parental job loss, which we argue in subsection 4.6.1 is not the case in our setting. As general effects of the business cycle that are not synonymous to job loss appear to be better captured by male unemployment, we focus on it in the rest of the paper.

Table 5: Effect by gender

	Unemployment		
	Male	Female	Overall
Weight < 1,500 grams (VLBW)	(1)	(2)	(3)
Unemployment	-0.577**	-0.297	-0.543*
	(0.209)	(0.176)	(0.219)
% change	-13.39%	-6.90%	-12.61%
Neonatal mortality	(1)	(2)	(3)
Unemployment	-0.246*	-0.095	-0.214*
	(0.107)	(0.095)	(0.096)
% change	-11.31%	-4.38%	-9.82%

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among 18-64-year-old individuals of the indicated gender in the nine months following conception. Percentage changes divide the unemployment effect by the mean level of the outcome in the observations used in the regression. Both coefficients and percentage changes are for a 1-percentage-point increase in the unemployment rate, but coefficients are scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

4.3.3 Age groups

In Table C.10 in the appendix, we explore how the effect varies depending on the age group used to compute the unemployment rate.[24] If the business cycle primarily affects newborn health via parental unemployment, focusing on unemployment among younger individuals – including most parents – might yield more precise estimates. However, including older individuals will increase the number of observations. This reduces measurement error in the unemployment rate, especially if newborn health is not so much influenced by parental unemployment, but rather general economic conditions.

In Table C.10, note that the size of the estimate rises as we include older men. This partly reflects a mechanical inflation of coefficients as a result of adding individuals for whom unemployment varies less with the cycle, so that changes in health are attributed to smaller fluctuations in the unemployment rate. However, larger – and more often significant – coefficients are also an indicator of reduced measurement error. The unemployment rate among men aged 18–64 years produces the largest and most significant effects, so we will choose this as our baseline.

24. See Table C.11 for corresponding regressions for female unemployment.

4.3.4 Regions and level of aggregation

As discussed earlier, we have chosen to compute unemployment rates at the level of the local labour market, but alternative regional units are conceivable. In particular, we consider the levels of counties and municipalities. There are 72 local labour markets compared with 283 municipalities and 21 counties, so these two levels are finer and coarser, respectively. Figure B.1 illustrates the three levels of aggregation with a map of Sweden.[25]

In Table C.12, we report results for the unemployment rate aggregated to the municipality and county level. See Table C.15 for corresponding regressions for female unemployment. For neonatal mortality, Table C.12 shows that the relationship between economic conditions and health strengthens with higher levels of aggregation. This finding confirms previous results for infant mortality by Lindo (2015). It can be explained by spillover effects from surrounding areas within the same local labour market or county, respectively, which are ignored when focusing solely on municipality-level unemployment. For very low birth weight, the pattern is slightly different in that the strongest relation is found with local-labour-market economic conditions, while the coefficient on county economic conditions is small and insignificant.

We explore these patterns further in Table C.13. Following Lindo (2015), we simultaneously include economic conditions at the municipality level and economic conditions aggregated over all *other* municipalities in the same local labour market (column 1) or in the same county (column 2). Note that because economic conditions are highly correlated within regions, there might be a lack of power to disentangle these differential effects.[26] Starting with neonatal mortality, we find that it is negatively related to the unemployment rate in both the municipality and the rest of the local labour market, although none of the coefficients is individually significant (column 1). This suggests spillover effects from surrounding municipalities within the same local labour market. Such spillovers appear even more pronounced when instead including county-level economic conditions, whose coefficient is significant at the 10 percent level (column 2). Further disentangling county-level effects, col-

25. Each of the 283 municipalities belongs to only one local labour market. In contrast, one local labour market might extend to several counties, although in total the number of local labour markets (72) is larger than the number of counties (21). More precisely, 9 local labour market extend to 2 counties and one local labour market to 3 counties.

26. This exercise is not possible for municipalities that are identical to their local labor market or county. We confirm in Table C.14 that the resultant reductions in sample size do not substantively affect our baseline results.

umn 3 shows that estimates are similar no matter whether the other municipalities within the same county are inside and outside of the municipality's local labour market. Turning to very low birth weight, the results look different: Municipality-level economic conditions are strongly and significantly related to health. The effect of local-labour-market economic conditions is of the same sign, but weaker. In contrast, the relation with county-level unemployment, in particular outside of the local labor market, goes into the opposite direction (recession worsening newborn health), but the standard errors are quite large.

In sum, we find evidence that larger effects at higher levels of aggregation are driven by substantial spillover effects from surrounding areas in line with previous work. An exception is very low birth weight, for which county-level economic conditions, especially outside the local labor market, have imprecisely estimated effects of the opposite sign. This masks the strong relation with municipality-level unemployment and yields a small and insignificant effect coefficient on aggregate county-level unemployment. In contrast, total local-labour-market economic conditions are always more strongly related to newborn health than municipality economic conditions alone, in line with the notion that local labour markets capture the relevant economic activity from an individual perspective. This is why local-labour-market economic conditions are our preferred level of aggregation. Another advantage of using local labour markets rather than municipalities is that movers are more likely to stay within local labour markets than within municipalities. This limits the measurement error introduced by assigning the municipality of the mother's first observed birth to all later-born children.

4.4 Effects on other health outcomes

Above we found that recessions change the incidence of neonatal mortality, i.e. deaths within 28 days of birth, by -0.246 (using the effect of male unemployment in Table 5). In Table 6, we report estimates of the effect on infant mortality – deaths within 1 year of birth – and postneonatal mortality – deaths after 28 days and within 1 year of birth. Note that the coefficient of infant mortality (-0.133) is about the same size as or smaller than the coefficient on neonatal mortality, and less significant. This has two implications: First, since the effect is not significantly larger, recessions have no effect on deaths later than 28 days after birth, also shown by the insignificant estimate for postneonatal mortality. Second, since the effect is somewhat smaller, some of the deaths not happening within 28 days of birth might just be deferred to a

Table 6: Effect of male unemployment on other health outcomes

	Infant mortality	Postneonatal mortality	Weight (in grams)	Apgar score (5 min) < 5	Small for gestational age
Unemployment	-0.133	0.113	-1.157	0.150	-0.274
	(0.150)	(0.100)	(1.376)	(0.245)	(0.496)
% change	-3.80%	8.50%	-0.03%	2.77%	-1.52%
Mean × 1,000	3.508	1.331	3,605.7	5.413	17.998
N	529,078	529,078	525,728	521,866	524,459
	Gestational age < 32 Weeks	**Gestational age < 37 Weeks**	**Hospitalizations pregnancy**	**Hospitalizations 1 year**	**Hospitalizations 3 years**
Unemployment	-0.605**	0.131	0.001	0.000	0.004
	(0.202)	(0.568)	(0.002)	(0.003)	(0.005)
% change	-11.07%	0.29%	0.63%	0.25%	1.11%
Mean × 1,000	5.464	45.266	0.135	0.168	0.370
N	528,961	528,961	529,078	526,336	474,331

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among 18-64-year-old men in the nine months following conception. Percentage changes divide the unemployment effect by the mean level of the outcome in the observations used in the regression. Both coefficients and percentage changes are for a 1-percentage-point increase in the unemployment rate. Except for weight, coefficients and means are scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

later point in time within the first year. However, the estimated effect is still negative and not significantly different from that on neonatal mortality. This indicates that some lives are actually saved in the long run.

Table 6 also explores the effects of recessions on absolute birth weight, the 5-minute Apgar score and small for gestational age (SGA). For any given gestational age, the SGA definition gives upper bounds of birth weight below which an infant is deemed "light" or "small" for gestational age. We also look at indicators for being born before 32 completed weeks of gestation ("very preterm" according to the WHO classification) and before 37 completed weeks of gestation ("preterm"). Finally, we investigate the effect on the number of hospitalizations, both during pregnancy and within 1 and 3 years of birth. There are no significant effects on these outcomes, except for the likelihood of being born with less than completed 32 weeks of gestation. We return to this finding below when discussing mechanisms.

4.5 Heterogeneity

4.5.1 Economic crisis in early 1990s

Sweden encountered a severe economic crisis at the beginning of the 1990s with GDP per capita shrinking in every year from 1991 to 1993. As a consequence, the unemployment rate escalated to 30% and more. A marginal increase in the unemployment rate from 29 to 30% in times of crisis might have different effects on newborn health than a marginal increase from 5 to 6% in normal times. The positive effects of downturns are mitigated if the income shocks associated with crises become so large that they cannot be buffered anymore, even in a developed country with social welfare and functioning capital markets. However, Ruhm (2016) using U.S. data finds that national-level crises tend to amplify the positive effects of downturns.[27]

In Table C.16 in the appendix, we present results from regressions in which we interact the unemployment rate with an indicator for the early-1990s crisis, using alternative year ranges to define the crisis. When we define the crisis to include the recession years 1992/1993, there is no indication that unemployment would have a differential effect on health in these years. However, the picture changes when adding the year 1994, when unemployment was still high even though the economy already started to grow again. Consistent with Ruhm (2016), we find that unemployment is associated with even larger reductions in VLBW in times of crisis (significant at the 10% level). This also holds true if we extend the year range further to 1996, until which high levels of unemployment prevailed. As a whole, the estimates suggest that downturns are beneficial to newborn health both in times of crisis and non-crisis, with effects appearing to be even larger on very low birth weight in times of crisis.

4.5.2 Socioeconomic Status

Detailed micro data allows us to study whether effects sizes vary by socio-economic status of the parents, by marital status or by the gender of the child. In the general population, Haaland and Telle (2015) find no evidence that the effect of the cycle would depend on socio-economic status.

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27. Bremberg (2003), who also studies the Swedish economic crisis in the early 1990s, finds no health effects on children of the crisis *per se*. This is slightly different from the question asked here, but nevertheless surprising in light of our finding that recessions are beneficial. However, his approach does not properly control for trends in health over time.

The first column of Table 7 allows for differential effects of recessions for fathers with different levels of educational attainment.[28] The coefficient in the first row gives the effect on mothers who only have primary or secondary education, which is the reference category in this regression. The estimate of -0.852 is much larger than our baseline estimate of -0.577 from Table 3. The other coefficient in the same column (just below) refers to the interaction of graduate and postgraduate education with unemployment. It is significantly positive and so large that it cancels out the effect on low-educated fathers. The results are similar for father's education. In sum, the negative effect of unemployment on very low birth weight seems entirely driven by low-educated parents, while effects are absent for high-educated parents.

In column 3, we also study effects by family income, which is another indicator for socio-economic status. This indicator ranks given parents in the distribution of family income of all parents with a baby conceived in the same year.[29] Our reference group are the parents in the bottom quarter of the income distribution and we contrast them with those in the top quarter. With very low birth weight as a health outcome, there are no differential effects of unemployment between top- and bottom-income parents. If we compare single with married mothers and boys with girls, the effects of unemployment do not differ either. Overall, these results provide suggestive evidence that the positive effects of recessions on very low birth weight are stronger for low-SES parents.

In the bottom part of Table 7, we repeat the above analysis for neonatal mortality. Here, recessions do not become less beneficial with increasing parental education. However, there is a positive, albeit insignificant, interaction effect for top-income-quarter parents.

4.6 Mechanisms

Our analysis so far has established a positive relationship between economic downturns and newborns' health. In analyzing potential mechanisms, we distinguish two

28. We define educational attainment as the education level obtained in 2006, the last year in which we observe this variable. In order to ensure that education is completed in this year, we restrict attention to individuals who are at least 26 years old at the end of 2006. This restriction excludes only about 1% of all observations. For individuals who have no education level information in 2006, we instead use the highest value ever obtained, regardless of age.

29. Ideally, we would like to base this indicator on the income distribution of potential rather than actual parents to prevent bias due to selective fertility. However, we observe family income only for couples who are married or already have common children. We would therefore ignore many potential first-time parents. To reduce bias, we also experiment with ranking today's parents according to today's income distribution of the previous year's parents. The results are very similar.

Table 7: Heterogeneity of unemployment effect by subgroup

	Education				
	Mother	Father	Family income	Marital status	Gender
Weight < 1,500 grams (VLBW)	(1)	(2)	(3)	(4)	(5)
Unemployment	-0.852**	-0.720**	-0.814	-0.590**	-0.588**
	(0.253)	(0.242)	(0.444)	(0.212)	(0.199)
Graduate and postgraduate	0.834*				
	(0.378)				
Graduate and postgraduate		0.587			
		(0.390)			
Top 25%			-0.116		
			(0.318)		
Married				-0.078	
				(0.124)	
Girl					0.021
					(0.070)
Mean - Reference	4.604	4.463	4.581	4.273	4.332
Mean - Interaction	3.809	3.953	4.017	4.160	4.283
% - Reference	-18.51%	-16.14%	-17.77%	-13.8%	-13.56%
% - Interaction	-0.48%	-3.36%	-23.16%	-16.04%	-13.23%
N	518,941	522,040	229,246	514,039	525,728
Neonatal mortality	(1)	(2)	(3)	(4)	(5)
Unemployment	-0.210	-0.229	-0.371	-0.195	-0.287**
	(0.146)	(0.130)	(0.279)	(0.123)	(0.105)
Graduate and postgraduate	-0.111				
	(0.214)				
Graduate and postgraduate		-0.094			
		(0.311)			
Top 25%			0.276		
			(0.191)		
Married				-0.145	
				(0.101)	
Girl					0.086
					(0.054)
Mean - Reference	2.261	2.249	2.247	1.844	2.458
Mean - Interaction	2.025	2.023	2.178	2.514	1.879
% - Reference	-9.27%	-10.17%	-16.5%	-10.57%	-11.69%
% - Interaction	-15.84%	-15.96%	-4.36%	-13.53%	-10.72%
N	522,248	525,351	230,846	517,297	529,078

Continued on next page

Notes: This table explores heterogeneous unemployment effects for different subgroups. The first line in each panel reports the unemployment effect in the respective reference subgroup. Reference subgroups are: (1) Primary and secondary, (2) Primary and secondary, (3) Bottom 25%, (4) Single, (5) Boy. Unemployment refers to the unemployment rate among 18-64-year-old men in the nine months following conception. Percentage changes divide the unemployment effect by the mean level of the outcome in the observations used in the regression. Both coefficients and percentage changes are for a 1-percentage-point increase in the unemployment rate. Coefficients and means are scaled up to express them as per 1,000 infants. Controls are parents fixed effects as well as subgroup-specific month fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

main categories. The first category refers to channels that are related to parental job loss and includes more time for health-enhancing activities and lower consumption of tobacco and alcohol. The second category includes all channels that are not synonymous to parental job loss, including reductions in air pollution and stress as well as higher availability of prenatal and neonatal care. For each of the two categories, we now evaluate whether it can rationalize the above findings.

4.6.1 Parental job loss

Recall from subsection 4.3.2 that the effect of the cycle on newborn health was entirely driven by the male unemployment rate, with the female unemployment rate being virtually uncorrelated with newborn health. At the same time, while uncorrelated with newborn health, female unemployment is a strong indicator of the mother's employment status. Table C.17 in the appendix presents regressions of two binary unemployment indicators on male and female unemployment separately.[30]

Irrespective of the indicator used, female unemployment is a much better predictor of mother's unemployment than male unemployment (columns 2 and 6 versus columns 1 and 5). The last two columns of Table C.17 also show that female unemployment decreases log family earnings more than male unemployment. These

30. The first indicator ("no wage") takes on the value one if a gross wage of zero is reported in the annual statement of income submitted to the tax agency in the year of conception. The second indicator ("no reimbursements") is defined analogously, except for being more comprehensive in the sense that – in addition to gross wage – it also accounts for work-related reimbursements such as sickness or pregnancy benefits and income from self-employment. However, it is not available to us in the year 2003. Both indicators have the limitation that they designate those individuals as unemployed who voluntarily receive zero work-related income, thus introducing measurement error. In our context, this particularly affects students. But note that for some students the continuation of education might only be an involuntary response to bad labour market conditions. Moreover, studying and being unemployed are not too different in terms of available income though not time.

Table 8: Effect of parental unemployment ("no wage")

	Baseline	Mother		Father		Both parents	
Weight < 1,500 grams (VLBW)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unemployment	-0.577**	-0.576**	-0.569**	-0.570**	-0.566**	-0.571**	-0.571**
	(0.209)	(0.209)	(0.203)	(0.207)	(0.208)	(0.207)	(0.208)
No wage		-0.004	0.001	-0.018*	-0.012	-0.026*	-0.029
		(0.005)	(0.015)	(0.007)	(0.017)	(0.011)	(0.039)
No wage × Unemployment			-0.005		-0.004		0.002
			(0.012)		(0.012)		(0.029)
N	525,728	525,725	525,725	525,045	525,045	525,045	525,045
	Baseline	Mother		Father		Both parents	
Neonatal mortality	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unemployment	-0.246*	-0.244*	-0.256*	-0.243*	-0.250*	-0.244*	-0.242*
	(0.107)	(0.107)	(0.107)	(0.107)	(0.108)	(0.107)	(0.106)
No wage		0.003	-0.008	-0.004	-0.013	0.002	0.010
		(0.004)	(0.010)	(0.005)	(0.017)	(0.012)	(0.045)
No wage × Unemployment			0.009		0.007		-0.006
			(0.008)		(0.013)		(0.032)
N	529,078	529,075	529,075	528,390	528,390	528,390	528,390

Notes: In each column, all coefficients come from the same regression. Unemployment refers to the unemployment rate among 18-64-year-old men in the nine months following conception. "No wage" takes on the value 1 if a gross wage of zero is reported in the statement of income submitted to the tax agency. Coefficients involving the unemployment rate are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

observations stand in stark contrast with our finding that the effects on newborn health are entirely driven by male rather than female unemployment. With female unemployment strongly affecting mother's unemployment but not newborn health, we conclude that mother's unemployment and, consequently, more time available for prenatal care is only a negligible channel in linking downturns to improved newborn health. It also follows that income reductions – and associated decreases in the consumption of detrimental goods – do not qualify as a likely channel either. These findings demonstrate that parental unemployment plays no major role in explaining the positive health effects of recessions.

In Table 8, we present a more direct test of the role of parental unemployment. Column 1 reproduces our baseline regression with the unemployment rate during pregnancy as the only regressor apart from controls. In column 2, we add an indicator ("no wage") for mother's unemployment as an additional covariate. Note that mother's unemployment, even in the case of holding the mother fixed, might be endogenous to third factors also affecting newborn health, such as age. Its coefficient must therefore be treated with caution. However, including this variable controls for the indirect effect of the unemployment rate that operates via mother's unemployment and isolates the direct effect.

For both very low birth weight and neonatal mortality, the coefficient of the unemployment rate does not change at all, confirming that the effect on newborn health does not operate through mother's unemployment. In column 3, we add an interaction term of mother's unemployment with the unemployment rate. The coefficient of the interaction is insignificant, suggesting that the unemployment rate affects employed and unemployed mothers in a similar way. Columns 4 and 5 repeat the analysis for father's and parents' joint unemployment, respectively, and yield comparable results. Table C.18 in the appendix reports the same set of regressions for the "No Reimbursements" indicator of parental unemployment, with results being essentially unaltered.

Finally, in Table C.19 in the appendix, we regress newborn health on first differences – rather than absolute levels – in the unemployment rate. First differences capture changes in the unemployment rate, such as a large-scale job loss due to layoffs. They exhibit no variation when unemployment remains constant at a high or low level. If a job loss has strong immediate effects that fade out over time, then first differences should give different results than levels of unemployment. Table C.19 shows the corresponding estimates for first differences in overall, male and female unemployment. There is no robust evidence that job loss captured by first differences in unemployment affects newborn health.

Overall, we find that parental employment status cannot account for the beneficial health effects of recessions. This is in line with prior work showing that job loss due to displacement actually affects individual health negatively rather than positively (e.g. Sullivan and von Wachter 2009; Eliason and Storrie 2009). Lindo (2011) and Carlson (2015) both provide evidence that negative effects of job loss carry over to newborn children in the form of reduced birth weight. Recall from Section 4.3.2 that male unemployment affects newborn health much more than female unemployment and also correlates more strongly with the business cycle. Taken together, these findings suggest that the cycle operates through channels more general than individual unemployment.

4.6.2 Channels other than job loss: air pollution, stress and availability of medical care

Among the alternative mechanisms linking downturns to newborn health are reduced stress, less traffic and air pollution as well as higher availability of prenatal and neonatal care. Availability of care might decrease under favorable economic conditions if it becomes difficult to recruit medical staff in a tighter labour market. Stevens et al. (2015) suggest that this mechanism explains cyclically sensitive mortality among individuals in elderly care in the U.S. However, cyclical variation in staffing plays hardly any role in Sweden due to its public health care system (see also subsection 2.1). Moreover, while neonatal care may affect the likelihood that a newborn infant dies, it is hardly relevant for weight at birth, for which we find positive effects just like we do for infant survival. Finally, financial barriers to prenatal care are virtually absent, so that there is no reason to expect differential effects by socioeconomics status of the parents as seen in Table 7.

Air pollution has been shown to rise in recessions to due less traffic (Chay and Greenstone 2003). We verify this relationship for Sweden by establishing a generally negative, and sometimes significant, correlation between the level of various pollutants and the municipality-level unemployment rate of men aged 18-64, which is also the measure of economic conditions for which we found the strongest health effects. The relationship is weaker for the unemployment rate of younger men and women (Table C.20 in the appendix).[31]. An enormous literature has demonstrated the impact of pollution on newborn health (Chay and Greenstone (2003), Knittel et al. (2016), see Currie et al. (2014) for a review), particularly in the first and third semester (Currie et al. 2009; Lavaine and Neidell 2017). Moreover, children of low-SES parents tend to suffer disproportionately from pollution. Reasons include sorting of low-SES families into neighborhoods with more pollution and elevated vulnerability due to lower baseline health. In the Swedish context, Jans et al. (2018) show that pollution induced by episodes of inversion causes considerably more harm

31. We obtained pollution data from the IVL Swedish Environmental Research Institute. Unfortunately, the data are only available at the seasonal level (summer/winter) and missing for many municipalities and years, especially summer seasons. Data for nitrogen dioxide (NO_2), for instance, is only available for 108 out of 283 municipalities and only for 1051 out of 11,037 season-municipality observations between 1992 and 2011. Data availability is even worse for other pollutants. To avoid losing additional precision, we abstain from aggregating to local labour markets. We regress pollution on the unemployment rate using a version of equation (1) without trends, replacing local-labour-market with municipality fixed effects and month fixed effects with season-year fixed effects. We also experimented with merging the pollution data with our micro-level dataset, but unfortunately the sample size decreases so dramatically that we can no longer precisely estimate our baseline effect on newborn health.

to low-SES children. In light of this evidence, our finding that economic conditions affect especially low-SES babies is not at odds with the hypothesis that air pollution is a mediating factor. We view it as a topic for further research to explore this more directly by using data with pollution levels at the appropriate level of geographical aggregation.

Some additional support for the role of air pollution comes from exploiting knowledge about the production function of birth weight. Following Kramer (1987), birth weight is determined by length of gestation and by intrauterine growth. While intrauterine growth is affected by cigarette smoking and nutrition, the length of gestation is – among others – sensitive to air pollution (Currie and Walker 2011). Recall from subsection 4.4 that recessions decrease the incidence of being born with less than 32 completed weeks of gestation ("very preterm"). This decrease has about the same size as the baseline decrease in very low birth weight from column 1 of Table 3, suggesting that a short gestation accounts for almost all of the reductions in very low birth weight. In contrast, the effect on the incidence of small for gestational age (SGA), which is an indicator of intrauterine growth (Kramer 1987), is not significantly different from zero.

Finally, recessions potentially reduce stress. Mothers, even if not experiencing job loss, enjoy fewer hours of work, which leaves more time for health-improving activities during pregnancy. In addition, for both the mother and father, lower workloads decrease job-related stress and positive spillovers among parents are likely. A large body of evidence documents the impact of stress on birth outcomes, in particular during the first trimester of pregnancy (Camacho 2008; Torche 2011; Mansour and Rees 2012; Bozzoli and Quintana-Domeque 2014; Foureaux Koppensteiner and Manacorda 2016). Moreover, gestation length responds to stress (Torche 2011; Foureaux Koppensteiner and Manacorda 2016; Persson and Rossin-Slater 2018) and stress reductions might particularly affect low-educated individuals who are disproportionately employed in sectors sensitive to the business cycle, such as manufacturing or simple services.

However, some skepticism about stress is warranted: First, there is extensive evidence that unemployment and underemployment are associated with *more* rather than *less* stress (e.g. Zivin et al. 2011; Rosenthal et al. 2012). Catalano and Serxner (1992) even demonstrate a link between threats to employment and low birth weight. Second, stress reduction due to fewer working hours would be difficult to reconcile with our finding that the effects on unemployed mothers appear to be similarly large. Lastly, when splitting the analysis by trimester, estimates are too imprecise to identify a specific phase of pregnancy that might drive the results (Table C.8). This exercise favors neither stress nor, for that matter, pollution as a potential channel. Overall, however, our results are most consistent with air pollution playing a major role in linking recessions with improvements in newborns' health.

5 Conclusion

Downturns improve newborns' health outcomes. A one-percentage-point increase in the unemployment rate is associated with an approximately 10% reduction in the incidence of having a birth weight below 1,500 grams and of dying within 28 days after birth. The reduction in the infant mortality rate is at least in part persistent and not offset by delayed death later in the first year of life.

Using detailed micro-level information about the parents, we shed some light on what are the underlying mechanisms. Parental job loss does not act as a mediating factor. We find that downturns disproportionately affect low-SES parents.

It seems that the reduction in mortality can be accounted for by a reduction of premature births, i.e. less than 32 weeks of gestation. Prior work has documented that low-SES children are more vulnerable to air pollution. Moreover, premature birth has been attributed to air pollution in earlier literature. Given that air pollution decreases during downturns, it seems that air pollution is a possible pathway for the effects we find. We view it as a topic for further research to extend our analysis with local pollution data.

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Appendix

A Additional analyses of selective fertility

A.1 Correlation of parental characteristics with newborn health

If parental characteristics are correlated with newborns' health, then compositional changes in birth cohorts caused by the business cycle entail changes in average health outcomes among newborns. We explore the implications of such selective fertility for average health outcome levels using Table C.3. Recalling from Table 1 that the average number of VLBW infants is 0.0052 and that of infants dying with 28 days within birth (neonatal mortality) is 0.0017, Table C.4 provides summary statistics of VLBW and neonatal mortality for demographic subgroups of the population. As for mother's education, more highly educated mothers are less likely to have VLBW children. No such clear-cut pattern is visible for neonatal mortality, but on average a smaller fraction of low-educated mothers in recessions – ceteris paribus – tends to improve health among newborn infants. Similarly, babies born to married mothers suffer from neonatal mortality significantly more often. However, the pattern is opposite for VLBW, so that the effect on average health remains unclear. Regarding country of origin, mothers from developing countries have a higher propensity to give birth to babies that suffer from VLBW or neonatal mortality. An increase in the proportion of these mothers in recessions would imply reductions in average newborn health. Overall, while the evidence from Table C.4 clearly demonstrates that newborn health varies by demographic group, it remains inconclusive about the direction of the effect that compositional changes induced by recessions have on average health outcome levels.

A.2 Health effects generated by shifts in observable characteristics

The selection on observables means that the correlation between economic conditions and newborns' health provides a biased estimate of a causal effect. However, note that compositional changes related to the characteristics included in Table C.4 generate only negligible health effects. As an example, consider the shift from low-educated mothers to medium-educated and high-educated mothers by about 0.0032 for a 1 percentage point increase in the unemployment rate.[32] From Table C.4, the average incidence of VLBW among high- and medium-educated mothers is about 0.0021 lower relative to low-educated mothers. Given an average VLBW of 0.0052, this implies that a change in the unemployment rate of 1 percentage point will decrease VLBW by only about 0.1%.

A.3 Comparison of observable characteristics of parents with and without at least one child with VLBW or neonatal mortality

When using parental fixed effects, identification comes from parent pairs with at least two births. Moreover, at least two births of a parent pair have to differ in, first, the economic conditions under which they were conceived and, second, the newborns' health outcome of interest. As our indicator of economic conditions is a continuous variable, the first condition is mechanically fulfilled. The second condition is fulfilled if parents experience a specific health outcome such as VLBW in some but not all of their children. As shown above, the prevalence of VLBW and neonatal mortality, while being low overall, varies by demographic group. As a consequence, among the parents who contribute to identifying the effect of interest, the fraction of those belonging to a demographic group in which a certain health outcome (such as VLBW) is relatively frequent should be disproportionately high. This is confirmed by Table C.5, in which we compare the characteristics of those parents in the regression sample that never had a child with VLBW or neonatal mortality ("no child") and those parents that experienced VLBW or neonatal mortality in at least one but not all of their children ("at least one"). Consistent with the findings from above, it can be seen that mothers and fathers who exhibit variation in either health outcome are significantly more likely to be old, non-single, low-educated and non-Swedish and have babies with higher birth orders. At the same time, there remains sufficient demographic variation to explore heterogeneity in effects.

A.4 Selectivity of abortions over the cycle

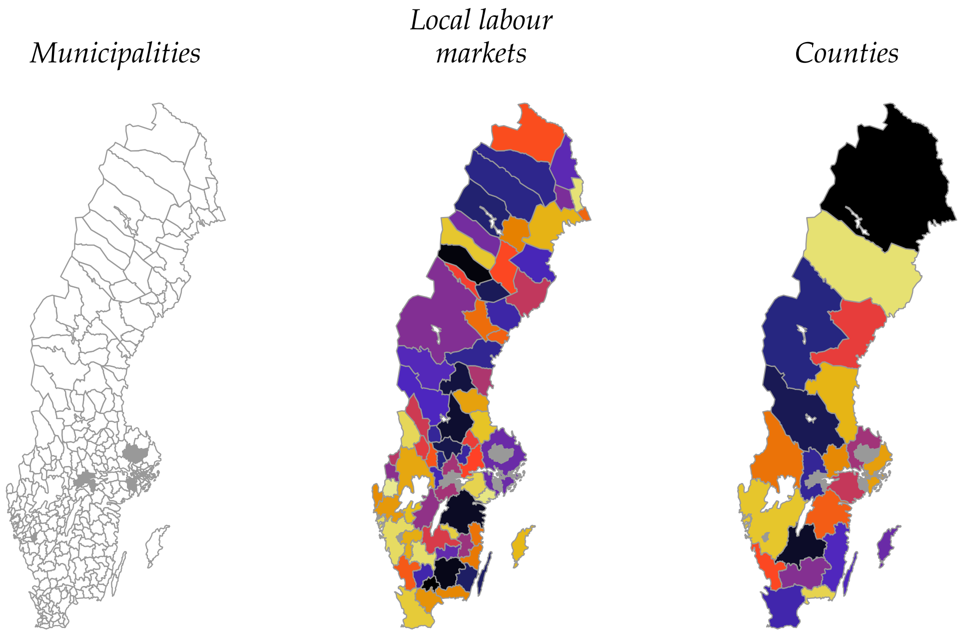
In this context it is interesting to examine medical abortions as a means to control fertility. If the result of an abortion is that the family ends up with exactly one newborn child in our observation window then the abortion effectively causes the

32. In the specification with trends, add up the reductions in the shares of low-educated mothers, -0.16, and the combined increase in the share of medium-/high-educated mothers, 0.16. Divide the result by 100, since reported coefficients are scaled up by this factor.

family to be omitted from the sample used in the fixed effects analyses. By analogy to the paragraph above, this should not affect the results if the model specification is correct. However, if, for example, effects of recessions are heterogeneous across families, and if this is not taken into account, then selectivity of abortions across the cycle may affect the results. Dehejia and Lleras-Muney (2004) discuss earlier studies and conclude that the evidence for an association between economic conditions and the abortion rate is inconclusive. Medical abortions are ambulatory and thus not observed in the inpatient registers. Hence they are not included in our data. Instead, they are recorded in a different register called the Outpatient Register. For a small number of years we have access to the latter for one region in Sweden (Scania (in Swedish: Skåne); see Tertilt and van den Berg 2015; Nilsson and Paul 2018). In this region we observe a positive association between recessions and the medical abortion rate. However, given that the association is not large, and given that abortions constitute only a small fraction of the birth rate, and given that the potential health outcomes in the absence of an abortion should not be dramatically worse than those among actual newborns, we are confident that abortions do not affect the estimation results below. It is also useful to point out that the results below when stratified by parental characteristics appear to be similar for different subgroups, so that effect heterogeneity does not seem to be a key issue.

B Figures

Figure B.1: Levels of regional aggregation in Sweden



Notes: The maps show regional aggregation in Sweden by municipalities, local labour markets and counties. In our sample, there are 283 muncipalities, 72 local labour markets and 21 counties. Grey-shaded areas indicate municipalities that have been excluded from the sample due to changes over time – see Section 2.3.

C Tables

Table C.1: Effect of unemployment in month of conception on birth rate nine months later

	Mother		Father	
	Baseline	With trends	Baseline	With trends
Overall	-0.2267	0.1818		
	(0.1427)	(0.2599)		
% change	-0.51%	0.41%		
Birth Order 1	0.0193	0.2328		
	(0.0904)	(0.1594)		
% change	0.11%	1.28%		
Birth Order 2	-0.0716	0.1175		
	(0.0709)	(0.1047)		

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% change	-0.41%	0.67%		
Birth Order 3	-0.0308 (0.0583)	-0.0668 (0.0450)		
% change	-0.35%	-0.77%		
Birth Order 4	-0.0495 (0.0349)	-0.0849* (0.0388)		
% change	-1.36%	-2.34%		
Age - Below 25 years	-0.1608 (0.0989)	-0.1398 (0.0784)	-0.1292* (0.0504)	-0.1141* (0.0490)
% change	-1.34%	-1.17%	-1.95%	-1.72%
Age - 25-35 years	0.0023 (0.1401)	0.2855 (0.2422)	-0.1297 (0.1132)	0.1369 (0.1725)
% change	0.01%	0.93%	-0.42%	0.44%
Age - Above 35 years	-0.0214 (0.0481)	0.1007 (0.0803)	0.0398 (0.0607)	0.1559 (0.1269)
% change	-0.43%	2.02%	0.40%	1.56%
Marital status - Single	-0.1684 (0.1313)	-0.0776 (0.1686)	-0.1655 (0.1221)	-0.0746 (0.1603)
% change	-0.59%	-0.27%	-0.58%	-0.26%
Marital status - Married	-0.1067 (0.1415)	0.1884* (0.0872)	-0.0726 (0.1272)	0.2164* (0.0993)
% change	-0.67%	1.18%	-0.45%	1.35%
Marital status - Divorced	0.0179 (0.0339)	0.0080 (0.0412)	0.0098 (0.0347)	0.0048 (0.0377)
% change	0.64%	0.29%	0.36%	0.18%
Education - Primary and lower secondary	-0.0194 (0.0324)	-0.0839* (0.0399)	0.0448 (0.0760)	-0.0389 (0.0594)
% change	-0.45%	-1.96%	0.66%	-0.57%
Education - Secondary education and vocational	-0.2675* (0.1083)	-0.0840 (0.1186)	-0.3467** (0.1208)	-0.2149* (0.0932)
% change	-0.96%	-0.30%	-1.17%	-0.72%
Education - Graduate and postgraduate	0.0229 (0.0961)	0.5045** (0.1859)	0.0729 (0.1122)	0.5537* (0.2368)
% change	0.16%	3.59%	0.68%	5.13%
Country of birth - Sweden	-0.3329* (0.1561)	0.0913 (0.2162)	-0.3701* (0.1614)	0.1049 (0.2167)
% change	-0.77%	0.21%	-0.85%	0.24%
Country of birth - Developing countries	0.1073** (0.0359)	0.0903** (0.0297)	0.1465** (0.0469)	0.0606 (0.0368)
% change	8.45%	7.11%	12.68%	5.24%
Country of birth - Developed countries	-0.0387 (0.1027)	-0.0850 (0.0757)	-0.0621 (0.0916)	-0.0836 (0.0709)
% change	-1.70%	-3.74%	-2.79%	-3.75%

Notes: Each coefficient comes from a separate OLS regression of the birth rate on the unemployment rate in the age group 18-64 years in the month of conception. Birth rates are defined as the number of births with the same month of conception in the given subgroup per 1,000 women aged 18–49 years in the overall population. Percentage changes divide the unemployment effect by the mean level of the outcome in the observations used in the regression. Both coefficients and percentage changes are for a 1-percentage-point increase in the unemployment rate, but coefficients are scaled up by 12 to obtain annualized figures. Sample includes months January 1992 to March 2004. Controls are month fixed effects, local-labour-market fixed effects and local-labour-market-specific linear time trends where indicated. Regressions are weighted by the number of births. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.2: Effect of unemployment during pregnancy on birth rate

	Mother		Father	
	Baseline	With trends	Baseline	With trends
Overall	-0.0201	0.4886		
	(0.2151)	(0.3659)		
% change	-0.05%	1.09%		
Birth Order 1	0.0880	0.3497		
	(0.1365)	(0.2154)		
% change	0.48%	1.92%		
Birth Order 2	0.0090	0.2607		
	(0.1041)	(0.1368)		
% change	0.05%	1.50%		
Birth Order 3	-0.0191	-0.0917		
	(0.0580)	(0.0666)		
% change	-0.22%	-1.05%		
Birth Order 4	-0.0220	-0.0584		
	(0.0410)	(0.0357)		
% change	-0.61%	-1.61%		
Age - Below 25 years	-0.1187	-0.0919	-0.1143*	-0.0751
	(0.1025)	(0.0913)	(0.0582)	(0.0613)
% change	-0.99%	-0.77%	-1.72%	-1.13%
Age - 25-35 years	0.1375	0.5183	-0.0136	0.2999
	(0.2001)	(0.3215)	(0.1629)	(0.2625)
% change	0.45%	1.70%	-0.04%	0.97%
Age - Above 35 years	0.0291	0.1640	0.1018	0.2450
	(0.0667)	(0.0866)	(0.0814)	(0.1421)
% change	0.58%	3.28%	1.02%	2.45%
Marital status - Single	-0.0036	0.2134	0.0040	0.2125
	(0.1637)	(0.2128)	(0.1532)	(0.2028)
% change	-0.01%	0.74%	0.01%	0.74%
Marital status - Married	-0.0617	0.2063	-0.0330	0.2360
	(0.1321)	(0.1466)	(0.1248)	(0.1646)
% change	-0.39%	1.29%	-0.21%	1.47%
Marital status - Divorced	0.0173	0.0101	0.0094	0.0081
	(0.0474)	(0.0499)	(0.0391)	(0.0428)
% change	0.62%	0.36%	0.35%	0.30%
Education - Primary and lower secondary	0.0004	-0.0792	0.1186	0.0476
	(0.0422)	(0.0512)	(0.0893)	(0.0774)
% change	0.01%	-1.84%	1.74%	0.70%
Education - Secondary education and vocational	-0.0916	0.1857	-0.2018	0.0150
	(0.1325)	(0.1713)	(0.1573)	(0.1387)
% change	-0.33%	0.67%	-0.68%	0.05%
Education - Graduate and postgraduate	0.1110	0.6285*	0.1449	0.6495*
	(0.1679)	(0.2517)	(0.1876)	(0.2948)
% change	0.79%	4.47%	1.34%	6.02%
Country of birth - Sweden	-0.1236	0.3776	-0.1901	0.3629
	(0.2059)	(0.3087)	(0.2169)	(0.3165)
% change	-0.29%	0.87%	-0.44%	0.84%
Country of birth - Developing countries	0.1104**	0.1145**	0.1558**	0.0822*
	(0.0391)	(0.0344)	(0.0484)	(0.0401)
% change	8.69%	9.01%	13.49%	7.12%
Country of birth - Developed countries	0.0041	-0.0077	-0.0975	-0.1163
	(0.1108)	(0.0743)	(0.1232)	(0.1087)
% change	0.18%	-0.34%	-4.38%	-5.22%

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Notes: Each coefficient comes from a separate OLS regression of the birth rate on the average unemployment rate in the age group 18-64 years in the nine months during pregnancy. Birth rates are defined as the number of births with the same month of conception in the given subgroup per 1,000 women aged 18–49 years in the overall population. Percentage changes divide the unemployment effect by the mean level of the outcome in the observations used in the regression. Both coefficients and percentage changes are for a 1-percentage-point increase in the unemployment rate, but coefficients are scaled up by 12 to obtain annualized figures. Sample includes months January 1992 to March 2004. Controls are month fixed effects, local-labour-market fixed effects and local-labour-market-specific linear time trends where indicated. Regressions are weighted by the number of births. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.3: Effect of unemployment during pregnancy on composition of birth cohorts

	Mother		Father	
	Baseline	With trends	Baseline	With trends
Birth Order 1	-0.0375	-0.0859		
	(0.0970)	(0.1347)		
% change	-0.10%	-0.22%		
Birth Order 2	-0.0126	0.1637		
	(0.1001)	(0.1028)		
% change	-0.03%	0.45%		
Birth Order 3	0.0552	-0.0587		
	(0.0837)	(0.0792)		
% change	0.33%	-0.35%		
Birth Order 4	-0.0242	-0.0417		
	(0.0495)	(0.0433)		
% change	-0.45%	-0.77%		
Age - Below 25 years	-0.0375	-0.0131	-0.0253	0.0010
	(0.1013)	(0.1072)	(0.0826)	(0.0885)
% change	-0.16%	-0.05%	-0.21%	0.01%
Age - 25-35 years	0.1851	0.0873	0.1016	0.1080
	(0.1296)	(0.1160)	(0.1124)	(0.1145)
% change	0.27%	0.13%	0.15%	0.16%
Age - Above 35 years	-0.1476	-0.0742	-0.0763	-0.1089
	(0.0966)	(0.0710)	(0.0879)	(0.0921)
% change	-1.74%	-0.87%	-0.38%	-0.54%
Marital status - Single	0.3227	0.1660	0.3427	0.1944
	(0.2154)	(0.1269)	(0.1958)	(0.1323)
% change	0.51%	0.26%	0.54%	0.31%
Marital status - Married	-0.4102	-0.2234	-0.3657	-0.1818
	(0.2324)	(0.1378)	(0.1972)	(0.1239)
% change	-1.25%	-0.68%	-1.11%	-0.55%
Marital status - Divorced	0.0875*	0.0574	0.0230	-0.0126
	(0.0405)	(0.0538)	(0.0413)	(0.0432)
% change	2.25%	1.48%	0.62%	-0.34%
Education - Primary and lower secondary	-0.0264	-0.1886**	0.1477	-0.0539
	(0.0841)	(0.0723)	(0.1230)	(0.0958)
% change	-0.38%	-2.69%	1.22%	-0.45%
Education - Secondary education and vocational	0.0816	0.0679	0.0727	-0.0015
	(0.1800)	(0.1110)	(0.2130)	(0.1046)
% change	0.13%	0.11%	0.11%	-0.00%
Education - Graduate and postgraduate	-0.0552	0.1207	-0.2204	0.0554
	(0.1853)	(0.1075)	(0.1564)	(0.1246)
% change	-0.19%	0.41%	-1.06%	0.27%
Country of birth - Sweden	-0.1391	-0.0776	-0.2634**	-0.0794
	(0.1071)	(0.0737)	(0.0985)	(0.0531)
% change	-0.14%	-0.08%	-0.27%	-0.08%
Country of birth - Developing countries	0.1281**	0.0857**	0.1599**	0.0509*
	(0.0457)	(0.0279)	(0.0549)	(0.0236)
% change	13.31%	8.90%	20.78%	6.62%
Country of birth - Developed countries	0.0110	-0.0080	0.1035	0.0285
	(0.0717)	(0.0587)	(0.0576)	(0.0395)
% change	0.50%	-0.36%	4.55%	1.25%

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Notes: Each coefficient comes from a separate OLS regression of the share of infants with the same month of conception in a given subgroup on the average unemployment rate in the age group 18-64 years in the nine months during pregnancy. Percentage changes divide the unemployment effect by the mean level of the outcome in the observations used in the regression. Both coefficients and percentage changes are for a 1-percentage-point increase in the unemployment rate, but coefficients are scaled up by 100 to express them in percentage points. Sample includes months January 1992 to March 2004. Controls are month fixed effects, local-labour-market fixed effects and local-labour-market-specific linear time trends where indicated. Regressions are weighted by the number of births. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.4: Average health by subgroup

	Weight < 1,500 grams (VLBW)				Neonatal mortality			
	Mean	SD	N	t-value	Mean	SD	N	t-value
Birth Order								
1	0.0069	0.0826	365,360	-	0.0018	0.0430	366,857	-
2	0.0036	0.0600	325,955	-18.86	0.0014	0.0369	326,955	-5.08
3	0.0039	0.0623	127,930	-13.39	0.0017	0.0413	128,309	-1.04
4	0.0052	0.0720	35,919	-4.11	0.0021	0.0453	36,020	0.83
Mother's education								
Primary and lower secondary	0.0070	0.0831	57,938	-	0.0017	0.0412	58,167	-
Secondary education and vocational	0.0054	0.0730	480,948	-4.43	0.0017	0.0413	482,555	0.05
Graduate and postgraduate	0.0044	0.0665	308,805	-6.88	0.0016	0.0403	309,929	-0.39
Mother's marital status								
Single	0.0052	0.0721	501,789	-	0.0014	0.0377	503,559	-
Married	0.0049	0.0696	337,456	-2.24	0.0020	0.0450	338,621	6.45
Divorced	0.0074	0.0858	31,555	4.45	0.0019	0.0435	31,662	1.88
Mother's age								
Below 25 years	0.0049	0.0700	172,649	-	0.0017	0.0408	173,229	-
25-35 years	0.0048	0.0691	620,483	-0.69	0.0016	0.0398	622,661	-0.71
Above 35 years	0.0086	0.0924	78,409	9.95	0.0024	0.0487	78,694	3.55
Mother's country of birth								
Sweden	0.0051	0.0714	836,613	-	0.0017	0.0407	839,500	-
Developing countries	0.0067	0.0816	16,569	2.46	0.0021	0.0458	16,644	1.24
Developed countries	0.0052	0.0718	18,352	0.08	0.0021	0.0460	18,433	1.34
Father's education								
Primary and lower secondary	0.0063	0.0791	99,932	-	0.0020	0.0446	100,311	-
Secondary education and vocational	0.0052	0.0717	488,951	-4.19	0.0016	0.0400	490,519	-2.56
Graduate and postgraduate	0.0046	0.0679	268,955	-5.88	0.0017	0.0412	269,991	-1.79
Father's marital status								
Single	0.0052	0.0721	497,558	-	0.0015	0.0383	499,301	-
Married	0.0049	0.0698	338,621	-2.10	0.0020	0.0448	339,785	5.77

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Divorced	0.0071	0.0840	33,497	4.01	0.0014	0.0374	33,625	-0.33
Father's age								
Below 25 years	0.0053	0.0728	88,602	-	0.0015	0.0392	88,911	-
25-35 years	0.0047	0.0687	605,873	-2.23	0.0016	0.0398	608,001	0.32
Above 35 years	0.0065	0.0803	177,066	3.76	0.0021	0.0453	177,672	3.02
Father's country of birth								
Sweden	0.0051	0.0711	834,158	-	0.0017	0.0408	837,056	-
Developing countries	0.0071	0.0841	16,287	3.07	0.0019	0.0435	16,362	0.67
Developed countries	0.0068	0.0822	21,036	3.00	0.0019	0.0440	21,105	0.91

Notes: t-values are from tests of equal means compared with the first subgroup in each category. Developed countries include EU-15 (excl. Sweden), Norway, North America and Oceania. Developing countries include the rest of Europe, Africa, South America, Asia and Soviet Union. Developing countries include other Europe, Africa, South America, Asia and Soviet Union.

Table C.5: Summary statistics by whether parents have children with adverse health outcomes

	Weight $< 1{,}500$ grams (VLBW)					Neonatal mortality				
	No child		At least one			No child		At least one		
	Mean	N	Mean	N	t-test	Mean	N	Mean	N	t-test
Birth Order										
1	0.3806	521,237	0.3450	4,771	-5.15	0.3813	526,061	0.2554	3,007	-15.77
2	0.4337	521,237	0.3961	4,771	-5.28	0.4342	526,061	0.3213	3,007	-13.22
3	0.1311	521,237	0.1639	4,771	6.09	0.1303	526,061	0.2607	3,007	16.26
4	0.0359	521,237	0.0591	4,771	6.78	0.0356	526,061	0.1068	3,007	12.61
Mother's age										
Below 25 years	0.2020	521,237	0.1962	4,771	-1.01	0.2021	526,061	0.1669	3,007	-5.15
25-35 years	0.7310	521,237	0.6973	4,771	-5.03	0.7307	526,061	0.7210	3,007	-1.19
Above 35 years	0.0671	521,237	0.1065	4,771	8.80	0.0672	526,061	0.1121	3,007	7.78
Mother's marital status										
Single	0.5662	521,039	0.5284	4,769	-5.20	0.5667	525,862	0.4385	3,006	-14.12
Married	0.4121	521,039	0.4353	4,769	3.22	0.4115	525,862	0.5250	3,006	12.42
Divorced	0.0217	521,039	0.0363	4,769	5.36	0.0218	525,862	0.0366	3,006	4.31
Mother's education										
Primary and lower secondary	0.0556	514,515	0.0829	4,706	6.75	0.0559	519,272	0.0637	2,966	1.75
Secondary education and vocational	0.5629	514,515	0.5795	4,706	2.29	0.5629	519,272	0.5823	2,966	2.13
Graduate and postgraduate	0.3814	514,515	0.3377	4,706	-6.32	0.3812	519,272	0.3540	2,966	-3.09
Mother's country of birth										
Sweden	0.9666	521,235	0.9593	4,771	-2.52	0.9665	526,059	0.9528	3,007	-3.54
Developed countries	0.0174	521,235	0.0195	4,771	1.06	0.0174	526,059	0.0246	3,007	2.56
Developing countries	0.0161	521,235	0.0212	4,771	2.43	0.0161	526,059	0.0226	3,007	2.39
Father's age										
Below 25 years	0.0984	521,237	0.0989	4,771	0.11	0.0985	526,061	0.0778	3,007	-4.23
25-35 years	0.7276	521,237	0.6900	4,771	-5.58	0.7275	526,061	0.6871	3,007	-4.77
Above 35 years	0.1740	521,237	0.2111	4,771	6.25	0.1739	526,061	0.2351	3,007	7.90
Father's marital status										

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Single	0.5587	520,378	0.5280	4,765	-4.22	0.5591	525,193	0.4526	3,005	-11.70
Married	0.4133	520,378	0.4382	4,765	3.45	0.4128	525,193	0.5251	3,005	12.30
Divorced	0.0280	520,378	0.0338	4,765	2.19	0.0281	525,193	0.0223	3,005	-2.15
Father's education										
Primary and lower secondary	0.1012	517,595	0.1279	4,721	5.48	0.1013	522,356	0.1286	2,985	4.44
Secondary education and vocational	0.5656	517,595	0.5630	4,721	-0.36	0.5654	522,356	0.5628	2,985	-0.28
Graduate and postgraduate	0.3332	517,595	0.3090	4,721	-3.58	0.3333	522,356	0.3085	2,985	-2.92
Father's country of birth										
Sweden	0.9627	521,219	0.9432	4,771	-5.80	0.9626	526,045	0.9537	3,005	-2.30
Developed countries	0.0214	521,219	0.0314	4,771	3.96	0.0214	526,045	0.0263	3,005	1.65
Developing countries	0.0159	521,219	0.0254	4,771	4.15	0.0160	526,045	0.0200	3,005	1.56

Notes: Summary statistics for selected variables, separately for those parents in the regression sample that never had a child with VLBW or neonatal mortality ("No child") and those parents that experienced VLBW or neonatal mortality in at least one but not all of their children ("At least one"). The latter parents allow for identifying the effect of economic conditions on health outcomes in the fixed-effects regression. t-values are from tests of equal means of parents in the "No child" group compared with parents in the "At least one" group. Developed countries include EU-15 (excl. Sweden), Norway, North America and Oceania. Developing countries include the rest of Europe, Africa, South America, Asia and Soviet Union. Developing countries include other Europe, Africa, South America, Asia and Soviet Union.

Table C.6: Sensitivity to choice of fixed effects

	Parents sample		Mother sample				
	With parents FE (baseline)	Without parents FE	With mother FE (baseline)	+ only 1992-2004	+ excl. stillbirths	+ father known	Without mother FE
VLBW	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unemployment	-0.577**	-0.310	-0.405*	-0.567**	-0.639**	-0.698**	-0.286*
	(0.209)	(0.183)	(0.193)	(0.189)	(0.188)	(0.194)	(0.132)
% change	-13.39%	-7.20%	-8.01%	-10.96%	-13.90%	-15.97%	-5.66%
Mean × 1,000	4.308	4.308	5.050	5.177	4.595	4.374	5.050
N	525,728	525,728	773,138	597,996	594,952	559,958	773,138
Neonatal mortality	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unemployment	-0.246*	-0.006	-0.104	-0.050	-0.213	-0.243*	-0.160
	(0.107)	(0.098)	(0.150)	(0.184)	(0.114)	(0.101)	(0.130)
% change	-11.31%	-0.26%	-1.84%	-0.81%	-8.36%	-11.60%	-2.84%
Mean × 1,000	2.177	2.177	5.629	6.116	2.550	2.096	5.629
N	529,078	529,078	777,702	602,012	598,907	563,496	777,702

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among 18-64-year-old men in the nine months following conception. Percentage changes divide the unemployment effect by the mean level of the outcome in the observations used in the regression. Both coefficients and percentage changes are for a 1-percentage-point increase in the unemployment rate. Coefficients and means are scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents or mothers fixed effects, as indicated in the column header. The parents sample consists of all children born to parents with at least two births (see Subsection 2.3). Analogously, the mother sample consists of all children born to mothers with at least two births. The mother sample is larger than the parents sample because father information is unavailable for a number of babies, in particular those born after 2004, and stillbirths. Columns 4-6 investigate how these data limitations affect the difference between parents and mother fixed effects estimates shown in columns 1 and 3. They progressively restrict the sample to babies born between January 1992 and March 2004, live births and all other babies with known father. In column 2 and 7, we report the baseline regressions without parents and mother fixed effects, respectively, and instead include local-labour-market fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.7: Sensitivity to functional form

Weight < 1,500 grams (VLBW)	With parents FE (baseline) (1)	Without parents FE (2)	With parents FE logit (3)	Without parents FE logit (4)
Unemployment	-0.577**	-0.310	-16.001**	-6.814
	(0.209)	(0.183)	(5.638)	(4.142)
% change	-13.39%	-7.20%	-	-6.77%
N	525,728	525,728	4,766	524,112
Neonatal mortality	(1)	(2)	(3)	(4)
Unemployment	-0.246*	-0.006	-15.597*	-0.233
	(0.107)	(0.098)	(7.082)	(3.913)
% change	-11.31%	-0.26%	-	-0.23%
N	529,078	529,078	3,007	521,317

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among 18-64-year-old men in the nine months following conception. Percentage changes divide the unemployment effect by the mean level of the outcome in the observations used in the regression. Columns 1 and 2 repeat the baseline regression (with parents fixed effects) and the OLS regression without parents fixed effects. Both coefficients and percentage changes are for a 1-percentage-point increase in the unemployment rate. Coefficients are scaled up to express them as per 1,000 infants. Columns 3 and 4 show coeffcients from fixed effects logit and ordinary logit models, respectively. For the ordinary logit model, we alse report percentages changes based marginal effects. This is not possible for the fixed effects logit model because there is no way to consistently estimates the fixed effects in short panels. Columns 2 and 4 omit parents fixed effects and instead control for local-labour-market fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.8: Effects by trimester in the 9 months before, during or after pregnancy

	Before pregnancy			*During* pregnancy			*After* pregnancy		
Trimester	1st	2nd	3rd	1st	2nd	3rd	1st	2nd	3rd
VLBW	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Unemployment	-0.244	-0.221	-0.352*	-0.494**	-0.478*	-0.413*	-0.283	-0.326	-0.233
	(0.201)	(0.204)	(0.178)	(0.179)	(0.187)	(0.185)	(0.181)	(0.182)	(0.161)
Neonatal mortality	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Unemployment	-0.336**	-0.220	-0.144	-0.184	-0.217*	-0.191*	-0.101	-0.120	-0.217
	(0.130)	(0.114)	(0.100)	(0.100)	(0.102)	(0.093)	(0.114)	(0.111)	(0.120)

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among 18-64-year-old men. Coefficients are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.9: Effects of all 3 trimesters at once

	Weight $<$ 1,500 grams (VLBW)	Neonatal mortality
1st trimester	-0.325	-0.045
	(0.272)	(0.196)
2nd trimester	-0.124	-0.137
	(0.338)	(0.247)
3rd trimester	-0.132	-0.059
	(0.239)	(0.146)
N	525,728	529,078

Notes: All coefficients in one column come from a joint regression. Unemployment refers to the unemployment rate among 18-64-year-old men. Coefficients are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.10: Effect of male unemployment by age group

Weight < 1,500 grams (VLBW)	18–24 years	18–30 years	18–40 years	18–64 years
Unemployment	-0.222*	-0.348**	-0.446**	-0.577**
	(0.088)	(0.103)	(0.133)	(0.209)
N	525,728	525,728	525,728	525,728
Neonatal mortality	18–24 years	18–30 years	18–40 years	18–64 years
Unemployment	-0.080	-0.083	-0.103	-0.246*
	(0.050)	(0.065)	(0.081)	(0.107)
N	529,078	529,078	529,078	529,078

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the average unemployment rate among men in the indicated age group in the nine months following conception. Coefficients are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.11: Effect of female unemployment by age group

Weight < 1,500 grams (VLBW)	18–24 years	18–30 years	18–40 years	18–64 years
Unemployment	-0.174*	-0.216*	-0.240	-0.297
	(0.078)	(0.095)	(0.125)	(0.176)
N	525,728	525,728	525,728	525,728
Neonatal mortality	18–24 years	18–30 years	18–40 years	18–64 years
Unemployment	-0.002	-0.004	-0.020	-0.095
	(0.042)	(0.053)	(0.066)	(0.095)
N	529,078	529,078	529,078	529,078

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the average unemployment rate among women in the indicated age group in the nine months following conception. Coefficients are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.12: Effect of male unemployment by region

Weight < 1,500 grams (VLBW)	Municipality	Local labour market	County
Unemployment	-0.365**	-0.577**	-0.149
	(0.134)	(0.209)	(0.291)
N	525,728	525,728	525,728
Neonatal mortality	Municipality	Local labour market	County
Unemployment	-0.130	-0.246*	-0.391*
	(0.072)	(0.107)	(0.166)
N	529,078	529,078	529,078

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among 18-64-year-old men in the nine months following conception. Coefficients are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 21 counties, 72 local labour markets and 283 municipalities in the sample. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.13: Effect of male unemployment by level of aggregation

Weight < 1,500 grams (VLBW)	(1)	(2)	(3)
Municipality	-0.267*	-0.432**	-0.339**
	(0.118)	(0.129)	(0.113)
Other municipalities in local labour market	-0.089		
	(0.142)		
Other municipalities in county		0.247	
		(0.247)	
Other municipalities in county in local labour market			-0.149
			(0.136)
Other municipalities in county out of local labour market			0.143
			(0.241)
N	500,492	522,297	383,944
Neonatal mortality	(1)	(2)	(3)
Municipality	-0.093	-0.085	-0.052
	(0.084)	(0.083)	(0.101)
Other municipalities in local labour market	-0.074		
	(0.103)		
Other municipalities in county		-0.293	
		(0.162)	
Other municipalities in county in local labour market			-0.125
			(0.113)
Other municipalities in county out of local labour market			-0.143
			(0.117)
N	503,760	525,639	386,258

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among 18-64-year-old men in the nine months following conception. Coefficients are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 21 counties, 72 local labour markets and 283 municipalities in the sample. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.14: Effect of male unemployment by region for the different subsamples in Table C.13

Weight < 1,500 grams (VLBW)	Local labour market	County
Baseline - Unrestricted		
Unemployment	-0.577**	-0.149
	(0.209)	(0.291)
N	525,728	525,728
Variation within local labour market (1)		
Unemployment	-0.471*	
	(0.199)	
N	500,492	
Variation within county (2)		
Unemployment		-0.236
		(0.293)
N		522,297
Variation within county, inside and outside of local labour market (3)		
Unemployment		-0.359
		(0.362)
N		383,944
Neonatal mortality	Local labour market	County
Baseline - Unrestricted		
Unemployment	-0.246*	-0.391*
	(0.107)	(0.166)
N	529,078	529,078
Variation within local labour market (1)		
Unemployment	-0.245	
	(0.131)	
N	503,760	
Variation within county (2)		
Unemployment		-0.418*
		(0.167)
N		525,639
Variation within county, inside and outside of local labour market (3)		
Unemployment		-0.454*
		(0.198)
N		386,258

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among 18-64-year-old men in the nine months following conception. Coefficients are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 21 counties, 72 local labour markets and 283 municipalities in the sample. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.15: Effect of female unemployment by region

Weight < 1,500 grams (VLBW)	Municipality	Local labour market	County
Unemployment	-0.142	-0.297	-0.219
	(0.120)	(0.176)	(0.231)
N	525,728	525,728	525,728
Neonatal mortality	Municipality	Local labour market	County
Unemployment	-0.082	-0.095	-0.230*
	(0.072)	(0.095)	(0.103)
N	529,078	529,078	529,078

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among 18-64-year-old women in the nine months following conception. Coefficients are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 21 counties, 72 local labour markets and 283 municipalities in the sample. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.16: Heterogeneous effects during the economic crisis in the early 1990s

Crisis years *Weight < 1,500 grams (VLBW)*	1992-1993 (1)	1992-1994 (2)	1993-1994 (3)	1992-1996 (4)
Unemployment	-0.588**	-0.608**	-0.422	-0.499*
	(0.211)	(0.200)	(0.240)	(0.198)
Unemployment × Crisis	-0.071	-0.220	-0.228	-0.212*
	(0.154)	(0.128)	(0.128)	(0.087)
Mean - non-crisis	4.079	4.121	4.121	4.151
Mean - crisis	5.621	4.919	4.562	4.526
% - non-crisis	-14.4%	-14.75%	-10.25%	-12.03%
% - crisis	-11.71%	-16.82%	-14.26%	-15.71%
N	525,728	525,728	486,310	525,728
Neonatal mortality	(1)	(2)	(3)	(4)
Unemployment	-0.237*	-0.244*	-0.248*	-0.277**
	(0.110)	(0.110)	(0.125)	(0.105)
Unemployment × Crisis	0.063	0.018	0.034	0.082
	(0.079)	(0.064)	(0.082)	(0.055)
Mean - non-crisis	1.879	1.823	1.823	1.780
Mean - crisis	3.889	3.334	3.072	2.728
% - non-crisis	-12.6%	-13.38%	-13.6%	-15.54%
% - crisis	-4.46%	-6.78%	-6.95%	-7.11%
N	529,078	529,078	489,530	529,078

Notes: In each column, all coefficients come from the same regression. Unemployment refers to the unemployment rate among 18-64-year-old men in the nine months following conception. Percentage changes divide the unemployment effect by the mean level of the outcome in the observations used in the regression. Both coefficients and percentage changes are for a 1-percentage-point increase in the unemployment rate. Coefficients and means are scaled up to express them as per 1,000 infants. Controls are parents fixed effects, month fixed effects as well as crisis-specific month fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.17: Effect on labour market outcomes

	No wage				No reimbursements					
	Mother		Father		Mother		Father		Log family income	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
Unemployment 18–64 years	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Unemployment	2.429**	4.206**	0.994	-0.687	2.118**	3.878**	1.730*	-0.543	-4.325	-5.838**
	(0.764)	(0.887)	(0.724)	(0.623)	(0.748)	(0.866)	(0.747)	(0.654)	(3.368)	(2.176)
N	529,074	529,074	527,968	527,968	470,618	470,618	469,648	469,648	428,201	428,201
Unemployment 18–40 years	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Unemployment	2.362**	3.324**	2.007**	0.557	1.988**	2.934**	2.249**	0.365	0.994	-0.476
	(0.779)	(0.729)	(0.648)	(0.516)	(0.638)	(0.619)	(0.520)	(0.441)	(1.129)	(1.218)
N	529,074	529,074	527,968	527,968	470,618	470,618	469,648	469,648	428,201	428,201

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among 18-64-year-old individuals of the indicated gender in the nine months following conception. "No wage" and "No reimbursements" are indicators for parental unemployment (mother or father). "No wage" takes on the value 1 if a gross wage of zero is reported in the statement of income submitted to the tax agency. "No reimbursements" takes on the value 1 if no work-related reimbursements are received. Coefficients are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.18: Effect of parental unemployment ("no reimbursements")

	Baseline	Mother		Father		Both parents	
Weight < 1,500 grams (VLBW)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unemployment	-0.577**	-0.710**	-0.696**	-0.703**	-0.694**	-0.702**	-0.701**
	(0.209)	(0.201)	(0.194)	(0.201)	(0.200)	(0.201)	(0.201)
No reimbursements		-0.006	0.007	-0.009	0.011	-0.032	-0.023
		(0.005)	(0.016)	(0.009)	(0.020)	(0.016)	(0.056)
No reimbursements × Unemployment			-0.010		-0.015		-0.007
			(0.014)		(0.014)		(0.039)
N	525,728	493,360	493,360	492,716	492,716	492,716	492,716

	Baseline	Mother		Father		Both parents	
Neonatal mortality	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unemployment	-0.246*	-0.230*	-0.239*	-0.228	-0.240*	-0.228	-0.233*
	(0.107)	(0.117)	(0.117)	(0.117)	(0.118)	(0.117)	(0.116)
No reimbursements		0.004	-0.005	-0.006	-0.033	-0.007	-0.040
		(0.005)	(0.012)	(0.005)	(0.026)	(0.011)	(0.061)
No reimbursements × Unemployment			0.007		0.020		0.025
			(0.009)		(0.019)		(0.045)
N	529,078	496,523	496,523	495,874	495,874	495,874	495,874

Notes: In each column, all coefficients come from the same regression. Unemployment refers to the unemployment rate among 18-64-year-old men in the nine months following conception. "No reimbursements" takes on the value 1 if no work-related reimbursements and no income from self-employment are received. Coefficients involving the unemployment rate are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.19: Effect of first differences of unemployment

Weight < 1,500 grams (VLBW)	Overall	Male	Female
Unemployment	-0.326	-0.256	-0.267
	(0.212)	(0.190)	(0.211)
N	468,122	468,122	468,122
Neonatal mortality	Overall	Male	Female
Unemployment	-0.114	-0.131	-0.032
	(0.190)	(0.181)	(0.164)
N	471,267	471,267	471,267

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among the indicated gender among 18-64-year-old men. Unemployment is the first-differenced average unemployment rate in the nine months following conception. First-differencing means subtracting the average unemployment rate from the previous nine-months-period. Coefficients are for a 1-percentage-point increase in the unemployment rate and scaled up to express them as per 1,000 infants. Controls are month fixed effects and parents fixed effects. Standard errors clustered at the local labour market level are given in parentheses. There are 72 local labour markets. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.20: Effect on various pollutants

	Nitrogen dioxide (NO_2)	Sulphur dioxide (SO_2)	Particulate Matter (PM_{10})	Ozon (O_3)
Men unemployment 18-64 years	-0.248**	-0.052	-0.488*	-0.202
	(0.118)	(0.054)	(0.255)	(0.187)
% change	-2.08%	-2.90%	-2.80%	-0.33%
Men unemployment 18-40 years	-0.133	-0.045	-0.121	-0.124
	(0.081)	(0.040)	(0.171)	(0.109)
% change	-1.11%	-2.51%	-0.69%	-0.20%
Women Unemployment 18-64 years	-0.106	0.057	-0.177	-0.294
	(0.120)	(0.088)	(0.263)	(0.189)
% change	-0.89%	3.18%	-1.01%	-0.48%
Women Unemployment 18-40 years	-0.056	0.068	0.090	-0.130
	(0.078)	(0.053)	(0.226)	(0.132)
% change	-0.47%	3.80%	0.52%	-0.21%
Mean ($\mu g/m^3$)	11.94	1.79	17.45	61.09
N	1051	789	404	526
N municipalities	108	93	71	66

Notes: Each reported coefficient comes from a separate regression. Unemployment refers to the unemployment rate among 18-64-year-old individuals (men or women) in the nine months following conception. Percentage changes divide the unemployment effect by the mean level of the pollutant in the observations used in the regression. Both coefficients and percentage changes are for a 1-percentage-point increase in the unemployment rate. Controls are season×year fixed effects and municipality fixed effects. Seasons are summer (April to September) and winter (October to March). Pollution data come from the IVL Swedish Environmental Research Institute. Standard errors clustered at the municipality level are given in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.